University of New South Wales
School of Economics

Honours Thesis

The Impact of Organisational Form on Contract Design and Buyer and Seller Interaction

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Declaration

I hereby declare that this submission is my own work and that to the best of my knowledge it contains no material previously written by another person, or material which to a substantive extent has been accepted for the award of any other degree or diploma of a university or other institute of higher learning, except where explicitly referenced in the text. I also declare that the intellectual content of this thesis is the product of my own work and that any assistance that I have received in preparing the project, writing the program or presenting the thesis, has been duly acknowledged.

Nicola Evelyn Cole
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Contents

Abstract

I investigate the impact of organisational form on the contracting behaviour of firms, the responses of agents of different cognitive and informational types to those contracts, and firms' anticipations of agents' responses. Specifically, I address theoretically and empirically the difference in behaviour of for- and not-for-profit firms. I construct a model to predict differences in how for- and not-for-profit firms contract with agents of different cognitive (sophisticated or naïve about the likelihood of outcomes) and informational (informed or uninformed about organisational form) types. I then use this model to design a laboratory experiment. I find that the proportion of sophisticated agents drives differences in misrepresentation while the proportion of informed agents influences chosen advertising. I also find that agents are less likely to accept misrepresentative contracts and that this likelihood is decreasing in agent sophistication.

Chapter 1
Introduction

The contracting behaviour of firms is an important economic question, but the impact of organisational form on this behaviour has been of increasing significance in recent years. In 2018 alone, the Productivity Commission's investigation into the Australian superannuation industry and the Royal Commission into Misconduct in the Banking, Superannuation and Financial Services Industry have produced reports detailing the behaviour and misbehaviour of firms. The Productivity Commission's draft report has found significant differences in the behaviour of for- and not-for-profit firms. Despite this increasing relevance, there has been very little literature on the impact of organisational form on a firm's contracting behaviour, nor on the subsequent behaviour of agents towards these firms.

Some empirical evidence indicates that organisational form (for- or not-for-profit) does influence contracting and business decisions. For instance, Liu and Ooi (2018) find that for-profit firms are more likely to award contracts to close business associates, even when these are not the most profitable. Likewise, the Australian Medical Association indicates that for-profit firms charge much higher premiums and offer less benefits than their not-for-profit counterparts (2018), and numerous contemporaneous news articles suggest the same (Harris (2017), Hamlyn (2018), Letts (2017)).

In markets with complex products, such as the insurance market, there is potential for firms to obfuscate their true pricing behaviour in order to act opportunistically and optimise their own profits. The individuals and businesses who utilise insurance have varying cognitive abilities and access to information regarding the products and companies, impacting their interactions and resulting in suboptimal behaviour. Deetlefs, Dobrescu, Bateman, Newell, Ortmann, and Thorp (2018) and Steffel, Williams, and Pogacar (2016) have modelled the impact of individual characteristics on consumer behaviour in markets with complex goods whilst Behavioural Contract Theory (BCT), as summarised by Koszegi (2014), has provided evidence of the behaviour of firms in markets with complex products, however there is almost no literature explicitly modelling the supply and demand interaction in these markets.

Whilst for-profit firms have incentives to act opportunistically, the same may not be true for not-for-profit firms. Due to their different incentives, the pricing behaviours of for-profit and not-for-profit firms can be compared in order to demonstrate the supply-side variance in markets with complex products. In accordance with BCT, different levels of transparency result in different responses from the users of these products (Gabaix and Laibson, 2006). However, the work of Steffel et al. (2016) suggests, somewhat counter-intuitively, that the motivations of firms may not impact the responding behaviours of the individuals and businesses who may interact with these complex goods.

In this thesis, I contrast the difference in predicted contracting behaviour between firms with for- or not-for-profit organisational structures and the response from agents with varying levels of knowledge regarding expected outcomes (being either sophisticated or naïve) and organisational form (informed or uninformed). I investigate these behaviours and responses by first constructing a theoretical model of the choices of both firms (being choices of advertising, misrepresentation, premium and payout) and buyers (to accept or reject any contract), then using this model as the motivation for a laboratory experiment.

Initially, I model this problem in an oligopolistic environment with the design of contracts differentiated between for and not-for-profit firms by their actual and stated likelihoods, their levels of advertising, their premiums and their payouts. Conceptually, this is similar to Heidhues and Koszegi (2017) wherein a firm has two prices with different levels of visibility. A firm is, in some cases, completely able to obfuscate the true likelihood of an outcome and thus extort the maximum surplus from consumers. However, I introduce a non-distribution-of-profit constraint (Ortmann, 1996) in order to replicate the constraints legally imposed on not-for-profit firms. For simplicity, this is implemented in the model as a zero-maximum-profit constraint.

For buyers, I also incorporate knowledge- (in the style of Grubb (2015)) and cognitive ability-specific elements to the models of buyer behaviour, including sophistication or naïveté regarding the true likelihood of outcomes and informed or uninformed status, being whether a buyer knows the organisational type of firm they are contracting with.

I then test the predictions of this model experimentally using a double-blind, computerised laboratory experiment. In this, I treat some participants as buyers and some as sellers of different types and follow their decisions (which align with options in the model) across a variety of scenarios. Sellers are matched with buyers based on their decisions and the decisions of other sellers in the same experimental group, and matches are only successful if buyers accept the contract specification offered by their matched seller. I utilise the well-established strategy method pioneered by Selten (1967) in order to maximise the collected data.

I hypothesise that the levels of advertising and misrepresentation selected by for-profit firms will be higher than those selected by not-for-profit firms as the benefit receivable by not-for-profit firms is limited by the non-distribution-of-profit constraint and thus there is less incentive to engage in misleading behaviour and less benefit to advertising. In addition, I predict that firms of any type will advertise and misrepresent less when contracting with sophisticated buyers as these buyers are able to readily identify misrepresentation and should reject such contracts at a higher rate, reducing the incentive to firms. Finally, I expect that naïve buyers should be less likely to reject leading contracts as they are unable to easily identify the degree (if any) of misrepresentation.

I find that selected levels of misrepresentation are driven largely by the proportion of sophisticated agents, rather than by the proportion of informed agents. As the proportion of agents with access to information regarding the true likelihood of outcomes increases, the ability of firms to effectively misrepresent diminishes and thus they select lower levels of misrepresentation. In addition, I find that for-profit firms select higher levels of misrepresentation than not-for-profit firms. The experimental correspondence between misrepresentation and sophistication aligns with the hypothesised behaviour of firms when selecting levels of misrepresentation.

Comparatively, I find that selected levels of advertising are influenced to a greater (and more consistent) degree by the proportion of informed agents than by the sophisticated proportion. As agents become more informed about a firms' being for- or not-for-profit, firms invest less in advertising. Interestingly, for-profit sellers invest less in advertising than not-for-profits, which I attribute to the decision to engage more in misrepresentative behaviour and a perceived trade-off between these choices. The relationship between sophistication and advertising is as predicted in the hypotheses, however, the direction of the impact of for-profit status on advertising contradicts expectations.

I also find that the willingness of agents to accept a given contract diminishes in the degree of misrepresentation and in their own degree of sophistication, such that an agent will reject a contract based on both perceived (relative to self-determined norms) and known (relative to known actual likelihoods) misrepresentation. Agents are also more inclined to accept contracts when there is a large proportion of not-for-profit firms in the market, which suggests that agents trust not-for-profit sellers more than for-profit sellers. Agent behaviour is consistent with the theoretical predictions.

My thesis contributes to the limited work on not-for-profit firm behaviour and motivations and interactions with for-profits in markets with complex products and to the literature regarding agent contracting behaviour as a function of knowledge and sophistication. I introduce a new theoretical model and a novel experimental design.

The remainder of my thesis is structured as follows: In Chapter 2, I discuss the existing literature on BCT, for-profit and not-for-profit firms, and insurance. In Chapter 3, I introduce the theoretical model and its predictions. Then, in Chapter 4, I introduce the experimental design, specifications, and derive the hypotheses. In Chapter 5, I outline and discuss the results of the experiment whilst in Chapter 6 I identify and interpret the results, complications with the theoretical model and the design and implementation of the experiment, and areas of further interest and clarification.

Chapter 2
Background Literature

2.1 Behavioural Contract Theory

Combining traditional contract theory with insights from psychology, Behavioural Contract Theory (BCT) emphasises the impact of principal and agent characteristics on behaviours and contracting decisions. In addition to the insights of contract theory, such as effort choices and outside options, behavioural contracts can account for moral hazards, incomplete and asymmetric information, screening, auctions, and exploitative contracts (Koszegi, 2014). Behavioural contracts also focus on the impact of nudges, trust, and default options on agent behaviours (Evans, Dillon, Goldin, and Krueger, 2011) and the potential for principals to exploit faulty beliefs, through obfuscation (Grubb, 2015), mechanism design (Gabaix and Laibson, 2006), and naïvite-based discrimination (Heidhues and Koszegi, 2017).

As epitomised by Koszegi (2014)'s literature review, the BCT literature has focused heavily on the impact of agent characteristics on their behaviours, and on the ability, tendency, and willingness of principals to exploit these characteristics when contracting in order to maximise their profitability. In particular, it has focused on the impact of agents' incomplete information and faulty beliefs on the contracts which principals will offer. Heidhues and Koszegi (2017) found that principals will discriminate specifically on the basis of naïveté, wherein an agent has an incorrect belief regarding the probabilistic distribution of potential outcomes, and also noted that, if principals discriminate completely, this can be social welfare maximising. This is reflected in Kalayci (2016) who found that principals will continue to discriminate thusly even when more principals enter the market, and Grubb (2015) who identified that principals will exploit the non-optimal contracting behaviour of agents with faulty beliefs.

Another emphasis of BCT, and of behavioural economics as a whole, has been the impact of nudges on agent behaviour. Nudges are policy tools implemented in order to encourage, though not force, agents to act in a manner which is welfare maximising. Smith, Goldstein, and Johnson (2013) found that nudges are more transparent than subliminal social advertising and are usually ethical, as they do not prevent an agent from selecting the same action they would have selected if the nudge were not present. This is epitomised by Johnson and Goldstein (2003) who found that twice as many respondents opted to donate their organs after death when this was presented as the default option, rather than the traditional opt-in approach. Smith et al. (2013) justify the tendency for agents to select the default option as either implied endorsement (wherein if an option is the default, it is seen as the best), cognitive efficacy (wherein an agent feels that the effort of investigating other options outweighs the benefit) or least effort (wherein it is simply easier not to change). This effect is magnified when an agent trusts the principal (Deetlefs et al., 2018), which Evans et al. (2011) attribute to an assumption of future reciprocity, though Butt, Donald, Foster, Thorp, and Warren (2015) notes that trust can be difficult to distinguish from disengagedness.

BCT provides the potential for principals to obfuscate contracts in order to exploit the faulty beliefs of agents. Grubb (2015)'s work found that principals will exploit consumers who are overconfident or otherwise naïve by introducing complicated contracts in order to maximise their profit. Kalayci and Serra-Garcia (2015) similarly found that complexity usually led to agents selecting high-benefit, high-cost contracts, even when simpler, cheaper contracts existed, and Kalayci (2015) who found that principals would offer higher prices when they had an option to confuse buyers.

2.2 For-Profits and Not-for-Profits

The BCT literature has not yet sufficiently covered the behaviour of for- and not-for-profit firms. There is, however, significant non-contract literature and empirical evidence suggesting that organisational structure is a significant component of principal behaviour.

The existing literature on not-for-profit behaviour is largely limited to suggestions regarding the modelling of their decisions. Ortmann (1996) endorsed the use of a non-distribution-of-profit constraint to effectively limit profitability, with excess profits usually being donated to a charity or else used for the benefit of agents, whilst Du Bois, Caers, Jegers, Schepers, De Gieter, and Pepermans (2004) and Ortmann (1996) suggested also modelling not-for-profit behaviours as a series of internal principal-agent conflicts. Du Bois et al. (2004) concluded that there was no solid consensus in the literature regarding the motivations of not-for-profits beyond profit not being the key motivation, whilst Ortmann and Brhlikova (2010) noted that the perception of benevolence usually attributed to not-for-profits may not be valid.

Despite the lack of coverage in the literature, there is evidence that organisational structure does significantly impact principal behaviour. In addition to the Productivity Commission and the Royal Commission into Misconduct in the Banking, Superannuation and Financial Services Industry, Liu and Ooi (2018) note that for-profit companies empirically have higher levels of trustee-director affiliation, related-party outsourcing and conflicts of interest compared to not-for-profit companies, and note also that the high agency costs associated with this resulted in reduced investment performance. This is particularly pervasive in the insurance and superannuation industries.

2.3 Insurance and Complex Financial Products

Insurance has been a concern of economics since the very beginning. Indeed, even Adam Smith wrote in The Wealth of Nations "The neglect of insurance [...] is, in most cases, the effect [...] of mere thoughtless rashness and presumptuous contempt of the risk" (Smith (1776) as quoted in Sandroni and Squintaini (2012)). Modern models of insurance derive from Rothschild and Stiglitz's seminal 1976 work, comparing high- and low-risk agents and the contracting decisions under a Cournot-Nash equilibrium. In particular, Rothschild and Stiglitz's modelling insurance using price and quantity competition, with agents unable to purchase arbitrary multiples of contracts, differentiated them from any existing work and has today become the standard insurance model.

Notably, this led to the works of Sandroni and Squintaini, focusing on overconfidence and asymmetric information in the insurance industry. Their 2007 paper focused on the policy implications of behavioural biases in insurance with asymmetric information, finding that it is not possible to weakly improve the welfare of all agents when some agents are overconfident. Spinnewijn (2013) found that contracting with agents differently heterogeneously dependent on their beliefs could result in a socially optimal outcome, even if such a contract did not benefit each individual, echoed in the results of Heidhues and Koszegi (2017) which advocated naïveté-based discrimination.

In recent years, BCT insights have even been used by insurance companies. For instance, Lemonade, an American start-up selling home and renters insurance, has argued that it uses behavioural insights in order to not only attract more customers but also to increase profitability (Harris, 2017). By asking potential claimants to undertake an honesty pledge before lodging a claim, they believe that they are less likely to receive fraudulent claims and so save significantly on administrative processing fees (Harris, 2017). Additionally, by donating excess premiums to charity (Ortmann, 2017), Lemonade are able to position themselves as a benevolent insurer whilst by cancelling existing contracts for new insurees (Harris, 2017), they reduce switching fees and so can easily attract new customers.

2.3.1 Health Insurance

Health insurance is ubiquitous throughout the Western world but regularly includes very complicated contracts. The Australian Medical Association's 2018 Report Card noted that, although health insurance is one of the largest single purchases families make annually, the health insurance offerings are a "complex and confusing array [...], with low benefits, differing definitions, exclusions, and restrictions" (Australian Medical Association, 2018). This has resulted in rising premiums and falling membership rates (Hamlyn, 2018) with profits rising 18 % following a 2016 premium increase (Letts, 2017) even as profit margins decrease. Despite these criticisms, there has been an increasing demand for behavioural insights by the health insurance sector (van Winssen, van Kleef, and van de Ven, 2016), particularly for supplementary insurance which works in addition to some baseline coverage, such as Australia's Medicare system.

Chapter 3
Model

I propose a two period economy ($t = 0, 1$) with a unit measure of agents, and two firms: a not-for-profit firm (NFP) and a for-profit firm (FP). At $t = 0$, agents choose which firm to sign a contract with in order to insure themself against a possible health shock in $t = 1$. Then, in $t = 1$, there are two possible states: $s \in S = \{G, B\}$ with $\Pr(s = B) = \gamma \in (0, 1)$. If $s = B$, then agents receive a health shock with an associated impact of $y = 0$, effectively leaving them unable to earn in that period. If $s = G$, then agents maintain their health with an associated impact of $y = 1$. For simplicity, all agents are endowed with 1 in $t = 0$ and no savings is allowed.

3.1 Agents

The agents' utility $u(y)$ satisfies $u', -u'' > 0$ and $\lim_{y \to 0} u'(y) = \infty$. As a result, the willingness-to-pay for the insurance contract is strictly positive. For the rest of the paper, assume that $u(y) = \ln(y)$.

There are two types of agents: those who know the organisational form of the firm (informed) and those who do not know (uninformed). In addition to what the agents know about the firms, there are two cognitive types: sophisticated and naïve agents. Sophisticates are those who know the set of states S and its distribution. Naïfs know S but have incorrect beliefs on the distribution. They rely on firms to inform them of the distribution of states. The joint distribution of the types is shown in Table 3.1.

	Sophisticated	Naïve
Informed	μ_s^i	μ_n^i
Uninformed	μ_s^u	μ_n^u

Table 3.1: Joint Distribution of Agents

The true utility of an agent of any type is:

$$U = \gamma \ln(a_j) + (1 - \gamma) \ln(1) - p_j$$

Where a_j is the amount paid by the firm in the bad state for a consumer of type

$j \in (s, n)$ and p_j is the associated premium for the contract paying a_j.

3.2 All Firms

All firms are either for-profit or not-for-profit firms.

All firms maximise the same profit function:

$$\pi = \sum_j \sum_k [\mu_j^k \omega_j^k(h, d)(p_j - \gamma a_j)] - c(h)$$

Where $\omega_j^k(h, d)$ is the proportion of consumers of type μ_j^k (where $k \in (i, u)$) who will purchase from the firm, as a function of advertising (h) and misrepresentation (d), and $c(h)$ is the cost of advertising. Advertising increases the likelihood that a given contract will be seen by a potential buyer while misrepresentation is the difference between the actual and stated probability of a given health shock occurring.

3.2.1 Not-for-Profit Firms

A not-for-profit firm is a firm whose profits are used for the sole use of running the business (Brhlikova and Ortmann, 2008). Their profits are constrained by a non-distribution constraint, with any positive profit being either donated to a relevant but third-party charity or used to balance any periods of economic loss. In this way, the expected profit of a not-for-profit firm cannot be greater than zero.

The realisable profit of a NFP firm is thus:

$$\pi^{NFP} = \min\{\pi, 0\}$$

Based on this restriction, the best possible outcome for a NFP firm occurs where, assuming that the NFP offers the same contract to all members: (p_{NFP}, a_{NFP}):

$$\pi = (p_{NFP} - \gamma a_{NFP}) \sum_j \sum_k [\omega_j^k(h, d)\mu_j^k] - c(h) = 0$$

Subject to the incentive compatibility constraint of any agent:

$$\gamma u(a_{NFP}) + (1 - \gamma)u(1) - p_{NFP} \geq \gamma u(0) + (1 - \gamma)u(1)$$

and the participation constraint:

$$\gamma u(a_{NFP}) + (1 - \gamma)u(1) - p_{NFP} \geq 0.$$

From the first order conditions (see Appendix A.1), this is optimised when $a^*_{NFP} = 1$

In order to ensure $\pi \geq 0$, and given that h has a negative impact on profit when profit is otherwise 0, it must be that $h = 0$ and $p_{NFP} = \gamma a_{NFP}$

Thus $p^*_{NFP} = \gamma$.

It can be seen that NFP helps smooth consumption and provides insurance at the actuarially fair price. In essence, NFP guarantees the agent a consumption of 1 regardless of the state and sells the contract at a price $p^{AF} = \gamma$. Note that with logarithmic utility, the guaranteed consumption of 1 is also welfare maximising. The assumption that NFP offers the contract $C^{NFP} = \left\{\left(p^{AF}, 1\right)\right\}$ is without loss of generality. If NFP was endowed with the same contractual instruments as FP, C^{NFP} would be optimal because NFP is not allowed to pocket its profits. As a result, they would be indifferent between C^{NFP} and the optimal contract for FP.

3.2.2 For-Profit Firms

A for-profit firm maximises their profit by engaging in misrepresentation through misstating the probability of $s = B$ as $\Pr(s = B) = \gamma + d$ to mislead naïfs. It also invests $h \in [0, 1]$ in advertisement to create hype or a positive anecdotal impression at a cost of $c(h)$, where $c', c'' > 0$ with $c(0) = 0$ and $\lim_{h \to 1} c'(h) = \infty$. For the rest of the analysis, assume

$$c(h) = \frac{\chi h}{1 - h} \text{ with } \chi > 0.$$

Advertisement is meant to attract agents who are indifferent between FP and NFP. FP designs contract (p, a), where p is the price agents pay to FP and a is the amount FP pays to the agent when $s \neq G$. In practice, advertising acts as a multiplying factor on the likelihood that a given contract is seen by any buyer.

Sophisticated Individuals

For sophisticated agents, misrepresentation should not preclude identification of the actual likelihood, but advertisement can still increase the likelihood that they choose a given contract as it is more likely that this contract would be offered to them. The contract for sophisticates is (p_s, a_s), and the profit from them is

$$\pi_s = \left[\omega^i_s(h, d)\,\mu^i_s + \omega^u_s(h, d)\,\mu^u_s\right](p_s - \gamma a_s),$$

where $\omega^i_s(h, d), \omega^u_s(h, d) \in [0, 1]$ are respectively the proportion of informed and uninformed agents FP attracts. I refer to this as the reaction function. Assume that $\frac{\partial \omega_k^j}{\partial h} > 0$ and $\frac{\partial \omega_k^j}{\partial d} < 0$ for any $j \in \{i, u\}$ and $k \in \{s, n\}$. This is constrained by the participation constraint:

$$U_s \equiv (1-\gamma) u(1) + \gamma u(a_s) - p_s \geq U_0 \equiv u(1) - p_{NFP}$$

which is functionally identical in this case to the incentive compatibility constraint.

Lemma 1 *The optimal* (p_s^*, a_s^*) *is* $p_s^* = p_{NFP} = \gamma$ *and* $a_s^* = a_{NFP} = 1$.

Proof Suppose the downward incentive compatibility and participation constraints are non-binding. It is optimal to set (p_s, a_s) such that $U_s = U_0$. Since $u(c) = \ln(c)$ and $p^{AF} = \gamma$ this implies $p_s = \gamma[u(a_s) + 1]$. From the first order conditions on a_s, (See Appendix A.2.2) FP chooses $a_s^* = 1$, so $p_s^* = \gamma$. Finally, since the optimal contract for sophisticates is the same as the contract from NFP, then incentive compatibility is the same as the participation constraint for naïfs. ∎

Lemma 1 states that FP provides sophisticates with the same contract as the actuarially fair contract that NFP provides. As a result, due to the competition with NFP, FP makes a non-positive profit from sophisticates and provides them with full insurance.

Naïve Individuals

FP misleads naïfs by convincing them that $\Pr(s = B) = \gamma + d$, where $d \in [0, 1-\gamma]$. Let the contract for naïfs be (p_n, a_n), the profit from them is

$$\pi_n = \left[\omega_n^i(h,d)\mu_n^i + \omega_n^u(h,d)\mu_n^u\right](p_n - \gamma a_n),$$

constrained by the participation constraint

$$U_n \equiv (1-\gamma-d) u(1) + (\gamma+d) u(a_n) - p_n \geq U_0,$$

and the incentive compatibility constraint

$$U_n \geq (1-\gamma-d) u(1) + (\gamma+d) u(a_s) - p_s.$$

Lemma 2 *The optimal contract* (a_n^*, p_n^*) *given the optimal misrepresentation is* $p_n^* = (\gamma + d^*)\ln(a_n^*) + \gamma$, *and* $a_n^* = 1 + \frac{d^*}{\gamma}$.

Proof The result follows immediately from the first-order conditions (see Appendix A.2.2) and the fact that the participation constraint binds at the optimum. ∎

This would imply, by Lemma 2, FP exploits naïfs by exaggerating the probability of $s = B$ with $d^* > 0$ and promising a larger payout $a_n^* > 1$ when $s = B$ occurs. As a result, FP is able to price the contract above the actuarially fair price: $p_n^* > \gamma$. In essence, FP exploits the naïfs by overstating the possibility of disasters and promising a larger than necessary payout when disasters occur to raise premiums. It is evident that naïfs suffer a welfare loss of γ by contracting with FP, and sophisticates will not choose this contract.

Note however that, if the selected level of misrepresentation is 0, the premium and payout are exactly identical across all cases.

To implement (a_n^*, p_n^*), FP can present additional insurance for $s = G$ as an add-on feature. It first presents the optimal contract for sophisticates, and then presents additional insurance payouts $a_n^* - 1$ for an added cost of $p_n^* - \gamma$. At this time, FP also presents its assessment of $\Pr(s = B) = \gamma + d^*$.

The firm chooses contract $\{(p_s, a_s), (p_n, a_n)\}$ and misrepresentation (h, d) such that it maximises $\pi_s + \pi_n - c(h)$.

As no profit is gained from the sophisticated individuals, this is equivalent to maximising $\pi_n - c(h)$, which results in the same contracts as above such that

$$\{(p_s, a_s), (p_n, a_n)\} = \left\{(\gamma, 1), \left((\gamma + d^*)\ln(1 + \frac{d^*}{\gamma}) + \gamma, 1 + \frac{d^*}{\gamma}\right)\right\}$$

The optimal misrepresentation (h^*, d^*) is characterised by

$$\left[\mu_n^i \frac{\partial \omega_n^i}{\partial h} + \mu_n^u \frac{\partial \omega_n^u}{\partial h}\right] \left[(\gamma + d^*)\ln(a_n^*) + \gamma(1 - a_n^*)\right] = c'(h^*),$$

$$\left[\mu_n^i \frac{\partial \omega_n^i}{\partial d} + \mu_n^u \frac{\partial \omega_n^u}{\partial d}\right] \left[(\gamma + d^*)\ln(a_n^*) + \gamma(1 - a_n^*)\right] + \left[\omega_n^i(h, d)\mu_n^i + \omega_n^u(h, d)\mu_n^u\right]\ln(a_n^*) = 0.$$

Let the reaction function take the Cobb-Douglas form

$$\omega_k^j(h, d) = \left(\alpha_j^k + h\right)^\rho \left(\beta_j^k - \delta_j^k d\right)^{1-\rho}$$

Where α_j^k is the likelihood a contract is seen even without advertising and h is advertising (thus the reaction function is strictly increasing in advertising but not zero without it), β_j^k is the proportion of agents who would accept a contract which was not misleading, δ_j^k is the direct effect of misrepresenting and ρ is the impact of advertising relative to misrepresentation.

From this model, it is possible to derive specific predictions for some distributions of agents, but not for all.

For instance, when all agents are sophisticated, there is no benefit to the firm from engaging in any level of misrepresentation as this will not increase expected profit, nor in advertising as the expected profit per accepted contract is 0.

$$\mu_n^i = 0, \mu_n^u = 0 \quad \Rightarrow \quad h^* = 0, \quad d^* = 0 \quad \Rightarrow \quad (p_{NFP}, a_{NFP}) = (p_{FP}, a_{FP})$$

It can also be expected that the chosen level of advertising and misrepresentation should be highest when all agents are naïve, as these are the agents who are affected by advertising and misrepresentation, with the levels dependent on the specification of the reaction function ω.

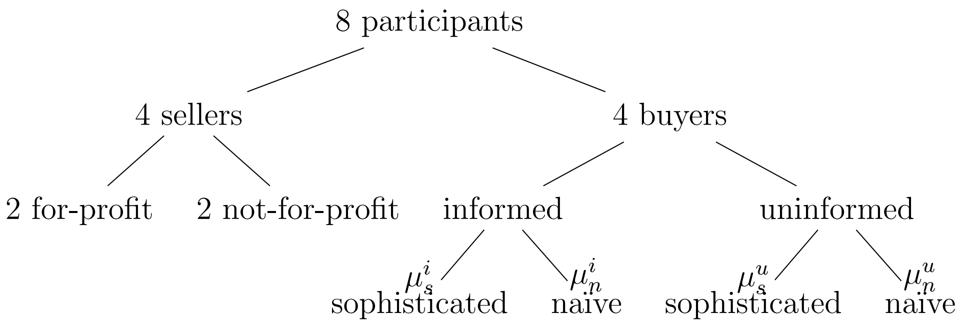
However, the levels of advertising and misrepresentation in the non-border cases, especially due to the specification of the reaction function, which motivates the use of an experiment to estimate the true reaction specification.

Chapter 4
Experiment

In order to determine the true specification of the reaction function and to test the predictions of the model of seller decisions, I undertake a laboratory experiment.

I implement a multi-player bargaining game between sellers and buyers of four hypothetical health insurance contracts, each related to a different, hypothetical disease. Each disease has different probabilities and potential payouts.

Within the experiment, participants are divided into groups of 8, consisting of 2 for-profit sellers, 2 not-for-profit sellers, and 4 buyers, such that:



These allocations determine their roles and the instructions and information they are given.

The use of four sellers and four buyers is sufficient to mimic a competitive market, as shown in Huck, Normann, and Oechssler (2004). As this situation has, to the best of my knowledge, never been studied experimentally, I attempt to follow the recommendations of List, Sadoff, and Wagner (2011) and engage a total of 120 participants, allowing 30 participants (at minimum) for each "cell", being the for-profit and not-for-profit sellers, with the remaining 60 participants (necessary for effective matching) being designated buyers and do not change across the rounds. Due to unavoidable constraints including scheduled participants not attending, I undertook the experiment with 95 participants, 48 buyers and 47 sellers who were subsequently categorised into 23 FP and 24 NFP sellers (with an experimenter acting as a FP seller for one session due to no-shows, and her data being removed from the sample). This addressed in more detail in the Discussion and in Appendix D.

During the experiment, all participants engage in 4 rounds related to a distinct but hypothetical illness. This involves different specifications of payments and actual likelihoods of the illness occurring.

In order to gain the most information possible regarding the behaviour of participants, I implement the strategy method Selten (1967). Participants make contingent decisions for a number of decision nodes. Brandts and Charness (2011) found that the chosen elicitation method for decisions, being either the traditional direct-response method or the strategy method, usually produces the same results, with different results most common when strong emotions are involved, which is unlikely to hold true for this experiment.

Prior to roles being allocated, all participants are given the same contextualising information. Specifically, buyers will understand the decisions available to sellers and vice versa.

Within the context information, there is an abbreviated version of the tables which is available to the buyers and sellers in the experiment itself, although calculated based on different values. The use of these tables allows both buyers and sellers to understand all roles available in the game. The tables are replicated here in Table 4.1 for convenience of comprehension.

Of note, the only difference in the tables offered to a seller who is for- or not-for-profit occurs in the bottom right-hand corner of the tables, where a for-profit seller has a potential expected benefit of 8 whilst a not-for-profit seller has their potential expected benefit capped at 5.

In order to isolate the decision of advertising and misrepresentation, and to minimise cognitive errors, the profits, premiums and payouts are all calculated based on the predictions of the model, such that:

$$p = (\gamma + d)ln(1 + \frac{d}{\gamma}) + \gamma$$

$$a = 1 + \frac{d}{\gamma}$$

$$\mathbb{E}[\pi] = (\alpha_i + h)(p - \gamma a - c(h))$$

Where p is the premium, a is the payout, and $\mathbb{E}[\pi]$ is the profit-per-contract received by a seller, weighted by the likelihood of their contract being seen.

For-Profit Firm Profits

Stated likelihood	Advertising 0	0.2	0.4
0.5	0	0	0
0.7	1	1	2
0.9	3	5	8

Not-For-Profit Firm Profits

Stated likelihood	Advertising 0	0.2	0.4
0.5	0	0	0
0.7	1	1	2
0.9	3	5	5

Buyer Expected Benefits

Contract	Stated likelihood	Premium	Payout
1	0.5	50	100
2	0.7	74	140
3	0.9	103	180

Table 4.1: Context Tables Shown in Experiment

For an actual likelihood (γ) (for this context, $\gamma = 0.5$, sellers can select one contract (consisting of a choice of advertising (h) and stated likelihood ($\gamma + d$) from the table for each of 9 scenarios across 4 hypothetical diseases. Note that the only difference in choice between the for-profit and not-for-profit sellers is that the presented profits for a not-for-profit are capped (any excess profits are donated to charity) such that the objective function is identical. Each buyer can select a variety of contracts (at least one, but potentially all) they are willing to accept for 18 scenarios (3 scenarios regarding the sellers across 6 scenarios regarding the buyer themself) across the same 4 hypothetical diseases.

At the end of each of the four rounds (each based on a given disease), one scenario is drawn for the seller and one for the buyer, and these are used to match participants and to calculate payoffs.

A match is considered successful if the stated likelihood chosen by a seller matches one of the contracts accepted by their matched buyer for that scenario.

What is not shown to participants, either in context or the actual instructions, is the actual expected benefit which is calculated based on the actual γ rather than the stated γ. Participants are told that the expected benefits presented to them are based on the stated, rather than actual, γ but the details of the calculations are not otherwise shared.

Once roles have been allocated, participants are given instructions specific to their roles and assigned information status.

Within each round, each seller chooses a level of h (advertising) and d (misrepresentation) for each of 9 scenarios regarding the distribution of buyers. These ordered scenarios are outlined in Table 4.2 such that the first scenario is such that none of the buyers are informed and none are sophisticated and the sixth is such that half of the buyers are informed and all are sophisticated.

		Buyer Sophistication		
		None	Half	All
Buyer Informed	None	1	2	3
	Half	4	5	6
	All	7	8	9

Table 4.2: Seller Elicitation Questions

Within each round, each buyer is provided with any relevant information they are entitled to and then selects which contracts they will accept for each of 18 scenarios regarding the distribution of sellers, based on what information they have been given. These scenarios are outlined in Table 4.3 such that the first scenario is the case where a buyer is uninformed and naïve and all sellers in the economy are for-profit, the ninth is the case where a buyer is informed and aware they are contracting with a not-for-profit seller but still naïve and all sellers in the economy are not-for-profit and the eleventh is the case where a buyer is uninformed but sophisticated and knows that half of the contracts in an economy are offered by a for-profit seller.

		Seller Type		
		All FP	Half FP	All NFP
Buyer Type	Uninformed, Naïve	1	2	3
	Informed (FP), Naïve	4	5	6
	Informed (NFP), Naïve	7	8	9
	Uninformed, Sophisticated	10	11	12
	Informed (FP), Sophisticated	13	14	15
	Informed (NFP), Sophisticated	16	17	18

Table 4.3: Buyer Elicitation Questions

For buyers who are informed regarding organisational structure, either All FP (the first seller type across the top of Table 4.3) or All NFP (the third seller type) are used as rationality checks, depending on which is relevant. No such rationality check is used for uninformed buyers.

All experimental sessions were conducted using a double-blind design such that participants were unaware of each others decisions and identifying details were removed from decisions such that experimenters were unaware of which participant in a session had made given selections.

The experiment was programmed in z-Tree which automatically de-identifies participants from data. The experiment was conducted in the UNSW BizLab.

4.1 Payouts and Specifications

The probability of a seller being matched to a given buyer is:

$$\alpha_i + (1 - \sum \alpha_j)\frac{h_i}{\sum h_j}$$

such that each seller will be matched with some fixed probability α_i with the remaining probability distributed based on their relative expenditure on advertising.

The payouts received by participants are calculated on a per-round basis such that:

Buyers receive an amount for each round which consists of a fixed endowment (E) less some associated expected loss (γm), which vary across rounds, less the premium paid (p) plus the expected value of the actual benefit (γa):

$$\pi_B = E - \gamma m - m(p + \gamma a)$$

The value of m (the associated loss) is based on industry average payouts and was also used throughout the experiment in order to scale the predictions of the model.

Sellers receive an amount for each round which consists of the price they charge (p) less the benefit they expect to pay (γa) less their cost of advertising (c(h)), multiplied by the number of contracts accepted (N):

$$\pi_S = N * m * (p - \gamma a - c(h))$$

Where the profit-per-contract term $(p - \gamma a - c(h))$ is capped in accordance with the table for not-for-profit consumers.

The specification of the model is calculated as shown in Table 4.4 where γ is the true likelihood of HR A-D occurring, m is a scaling factor, p is the premium and a is the payout when advertising (h) and misrepresentation (d) are both 0.

	When {h, d} = {0, 0}					
Round	γ	m	p	a	π_S	π_B
HR A	0.4	1000	400	1000	0	0
HR B	0.3	200	60	200	0	0
HR C	0.2	1500	300	1500	0	0
HR D	0.1	800	80	800	0	0

Table 4.4: Round Specification

4.2 Hypotheses

The following three testable hypotheses for the behaviour of buyers and sellers in this model were derived from the model.

First, the non-distribution-of-profit constraint should influence the choices of sellers. Specifically, as not-for-profit sellers receive less expected and actual benefit from increased levels of misrepresentation, they will select levels of stated likelihood which are closer to the actual level, relative to the choices by for-profit sellers. In addition, as the profit per expected contract is constrained for not-for-profit sellers, I predict that they will also engage in less advertising as the expected return is lower than that of a for-profit seller.

Hypothesis 1 *Not-for-profit sellers will engage in less advertising and misrepresentation than for-profit sellers as the benefit they receive from both is capped by the non-distribution constraint.*

Second, when contracting with sophisticated buyers, I predict that sellers will select lower levels of misrepresentation as sophisticated buyers are able to identify the degree of misrepresentation and so should not accept such contracts. As this would reduce the potential profitability per accepted contract, I predict that sellers would also select lower levels of advertising as the marginal benefit from one additional accepted contract is lower.

Hypothesis 2 *Sellers will advertise and misrepresent less when contracting for sophisticated buyers as sophisticated buyers can identify misrepresentation and hence are likely to reject such contracts.*

Third, I predict that, as they can identify misrepresentation at no cost and are only disadvantaged by misrepresentative contracts, sophisticated buyers will reject misrepresentative contracts. I predict both that sophisticated buyers will be more likely to reject a misrepresentative contract than a naïve buyer and that a sophisticated buyer will be more likely to reject a contract if it is misrepresentative.

Hypothesis 3 *Sophisticated buyers will identify misrepresentation and hence are likely to reject such contracts.*

Chapter 5
Results

5.1 Sellers

I first regress sellers' choices of advertising on explanatory factors in Section 5.1.1 then regress sellers' choices of misrepresentation in Section 5.1.2. I then regress buyer decisions on explanatory factors in Section 5.2 before, in Section 5.3, explicitly relating all results to the hypotheses.

Due to the small sample size, the results presented below do not cluster standard errors. Each table of results below has a corresponding table in Appendix C wherein the standard errors are bootstrapped with 200 replications, however, this does not impact the statistical significance of the results as presented in this section any meaningful way. Each bootstrapped table is referenced in the footnotes to the tables presented in this chapter.

5.1.1 Advertising

I begin by regressing sellers' choices of advertising (h) on the distribution of sophisticated and informed agents. For this first regression, I treat the distribution of agents (being none, half or all as explained in the experimental methodology, see Table 3.1) as continuous, as can be seen in Column 1 of Table 5.1.

$$h = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \epsilon$$

From this linear regression, an increase in both the proportion of sophisticated buyers and the proportion of informed agents is associated with approximately a 0.1 (-0.103 and -0.099) reduction in the selected level of advertising. In the experimental design, 0.1 is the step between selectable levels of advertising, thus sophisticated and informed agents are associated with a unit decrease in the selected level of advertising. The direction of these effects corresponds with the predictions of Hypothesis 2. Both of these coefficients are statistically significant at the 0.1 % level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.103***	-	-	-	-	-	-	-
	(0.02)							
Half Sophisticated	-	-0.055***	-	-0.055***	-0.024	-0.024	-0.024	-0.023
		(0.02)	-	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
All Sophisticated	-	-0.103***	-	-0.103***	-0.035*	-0.035*	-0.035*	-0.034*
		(0.02)		(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Informed	-0.099***	-	-	-	-	-	-	-
	(0.02)							
Half Informed	-	-0.063***	-	-0.063***	-0.061***	-0.061***	-0.061***	-0.061***
	-	(0.02)	-	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	-	-0.099***	-	-0.099***	-0.096***	-0.096***	-0.096***	-0.096***
	-	(0.02)	-	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
For-Profit Seller	-	-	-0.069***	-0.069***	-0.087***	-0.088***	-0.088***	0.099
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)
Misrepresentation	-	-	-	-	0.393***	0.396***	0.393***	0.402***
	-	-	-	-	(0.05)	(0.04)	(0.04)	(0.05)
Constant	0.499***	0.505***	0.432***	0.539***	0.441***	0.354***	0.343***	0.330***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1503	1503	1503	1503	1503	1503	1503	1503
R^2	0.055	0.056	0.019	0.075	0.118	0.187	0.193	0.376

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For the case where standard errors were bootstrapped with 200 replications, see Table C.10 in Appendix 5

Table 5.1: Determinants of Selected Level of Advertising

Following this, I also present the same regression as above where the proportion of sophisticated and informed buyers is presented as a factor variable, such that half of the potential agents being sophisticated is distinct from all or none being sophisticated. As can be seen in Column 2, seller behaviour does not vary between the case where some sophisticated agents exist and where all agents are sophisticated (the coefficients are consistent with those in Column 1 at -0.103), relative to the base case where no buyers are sophisticated, however, the coefficients when half of the buyers are sophisticated and half are not is approximately half (-0.055) that when all are sophisticated, which is an intuitive result. Similarly, seller behaviour does not vary between the case where all or any buyers are informed (-0.099) relative to the base case where no buyers are informed, while the coefficient when half of the buyers are informed is approximately two-thirds (-0.063) of the coefficient when all of the buyers are informed. In all instances, these coefficients are significant at the 0.1 % level.

Across both of these regressions, the R^2 is very similar (0.055 and 0.056) being that approximately 5.5 % of the variation in advertising choice explained exclusively by the distribution of buyers. Due to the increased information available from factorising buyer proportions, this factorisation has been maintained throughout subsequent regressions.

For completeness, I regress the seller's choice of advertising on the state of being a For-Profit seller, as seen in Column 2 of Table 5.1.

$$h = \beta_0 + \beta_1 \text{FP} + \epsilon$$

It can be seen that, ceteris paribus, being designated a for-profit seller for the experiment is associated with a 0.069 decrease in the choice of advertising which is significant at the 0.1 % level. Simple designation of type explains 1.9 % of the variation in the data ($R^2 = 0.019$).

This direction of causality contradicts in part Hypothesis 1, in which I predict an increase in the selected level of advertising by a for-profit seller as compared to a not-for-profit seller. However, the magnitude of this effect is relatively small, if statistically significant, result, and is likely due to a perceived trade-off between advertising and misrepresentation in the experiment itself. The possibility of a perceived trade-off is expanded upon in detail in the Discussion.

I then regress seller's choices of advertising on the distribution of both sophisticated and informed agents and on their designated type.

$$h = \beta_0 + \beta_1\text{sophisticated} + \beta_2\text{informed} + \beta_3\text{FP} + \epsilon$$

As would be expected due to the lack of correlation and seen in Column 4, all coefficients and significances are unchanged from those in Columns 1 and 2, with the R^2 (0.075) being the sum of the R^2 values from said Columns. While this could be perceived as questionable in a field experiment or empirical investigation, the manufactured nature of this experiment did not allow for any correlation between seller-status and agent-distribution. The external validity of the experimental design is expanded upon in the Discussion.

I then introduce the simultaneous choice of misrepresentation (d) as a control to the regression in Column 4.

$$h = \beta_0 + \beta_1\text{sophisticated} + \beta_2\text{informed} + \beta_3\text{FP} + \beta_4 d + \epsilon$$

As can be seen in Column 5 of Table 5.1, an increase in the amount of misrepresentation is associated with an increase in the choice of advertising, ceteris paribus.

Choices of misrepresentation were available in intervals of 0.1, thus a 0.1 increase in misrepresentation is associated with an increase of 0.04 in chosen advertising, significant to the 0.1 % level.

Controlling for the choice of misrepresentation decreases dramatically the magnitude and statistical significance of the proportion of sophisticated buyers. The impact of having all sophisticated buyers, while still significant at the 5 % level, is barely a third (-0.035) of the effect without this control while having half of the buyers sophisticated is no longer statistically significant even at the 10 % level and is approximately half (-0.024) the prior value.

In contrast, this control barely affects the impact of the proportion of informed agents. The coefficients on half and all buyers being informed have decreased in magnitude only very slightly (by 0.002 and 0.003, approximately 3 % each) but both coefficients are still significant at the 0.1 % level. This suggests that advertising choices are influenced more by the proportion of informed buyers than sophisticated buyers.

The impact of being a for-profit seller increases in magnitude to -0.087 (approximately a 25 % change in magnitude) and remains significant at the 0.1 % level.

Introducing the control for misrepresentation also increases the R^2 value to 0.118, a 0.04 (57 %) increase.

Following this, I introduce round-level fixed effects for each of the four rounds which each player, regardless of type, plays through. This is a logical introduction as scale and probability effects vary depending on the round itself.

$$h = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \beta_4 d + \gamma R + \epsilon$$

As can be seen in Column 6, round-level fixed effects have no observable effect on the coefficients for informed agents or sophisticated agents, nor on the statistical significance of either. Similarly, round-level fixed effects are associated with only very small changes in the coefficients on for-profit sellers and misrepresentation (changes in magnitude of 0.001 and 0.003) and no impact on the statistical significance.

However, introducing these fixed effects increases the R^2 to 0.187, a 58 % increase in the explanatory power of the model.

I also introduce a session-level fixed effect for each of the 5 sessions this experiment was run over (1 small pilot and 4 larger main sessions, as explained in detail in the Discussion), to account for factors such as change in experimenters, timing effects and group distributions (as further discussed in the Discussion).

$$h = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \beta_4 d + \gamma R + \eta S + \epsilon$$

This has almost no observable effect (Column 7) on the coefficient values relative to those in Column 6, with no changes in statistical significance and the only change in coefficient value simply reversing the change in the coefficient on misrepresentation associated with the introduction of round-level fixed effects.

Finally, I introduce an individual-level fixed effect to account for heterogeneity in individual choices.

$$h = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \beta_4 d + \gamma R + \eta S + \lambda I + \epsilon$$

As can be seen in Column 8 of Table 5.1, the introduction of individual fixed effects has very little impact on the coefficients on the agent-type variables or their significances.

A more significant effect is evident for the coefficients on for-profit seller status and misrepresentation. Where the coefficient on for-profit status had been consistently small, negative and very statistically significant for all prior regressions (Columns 3-7), controlling for individual fixed effects results in a positive, less significant (while significant at the 10 % level, this is not significant at the 5 % level) coefficient instead. This may suggest that much of the explanatory power attributed to for-profit status was actually due to the individuals designated for-profit in the experiment. While this is discussed further in the discussion, it is likely that the relatively small experimental study (96 participants resulted in 48 sellers, 24 of whom were designated for-profit) may have resulted in inadvertently grouped characteristics. The coefficient on misrepresentation (0.402) is slightly larger than in Columns 5-7, however, the change is relatively small and the significance is maintained.

Introducing individual-level fixed effects increased the explanatory power of the model by more than 98 % (R^2 of 0.376), which suggests that much of the variation in choice is due to individual characteristics.

The selection of advertising is driven most strongly by the proportion of buyers who are informed, rather than sophisticated, and by individual effects, such as selection of misrepresentation.

5.1.2 Misrepresentation

As was the case for advertising, I begin by regressing seller choices of misrepresentation on the proportion of sophisticated and informed buyers in a given scenario. For this first regression, as shown in Column 1 of Table 5.2, I treat both proportions as continuous variables, being the spectrum from no (0) through half (0.5) to all (1) of the population being sophisticated or informed.

$$d = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \epsilon$$

An increase in the proportion of sophisticated buyers has a negative and statistically significant (to the 0.1 % level) impact on the selected magnitude of misrepresentation, with an increase in the amount of sophisticated agents from none to all (0 to 1) being associated with a reduction in misrepresentation of 0.172 points (where the options presented in the experiment were in units of 0.1 points). Thus, ceteris paribus, a seller should offer a contract roughly two units more misrepresentative when all buyers are unaware of the true likelihood.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.172***	-	-	-	-	-	-	-
	(0.01)							
Half Sophisticated	-	-0.080***	-	-0.080***	-0.073***	-0.073***	-0.073***	-0.073***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-	-0.172***	-	-0.172***	-0.160***	-0.159***	-0.160***	-0.159***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed	-0.008	-	-	-	-	-	-	-
	(0.01)	-	-	-	-	-	-	-
Half Informed	-	-0.004	-	-0.004	0.004	0.004	0.004	0.004
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed	-	-0.008	-	-0.008	0.004	0.005	0.004	0.005
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For-Profit Seller	-	-	0.046***	0.046***	0.054***	0.055***	0.055***	-0.037
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Advertising	-	-	-	-	0.118***	0.128***	0.125***	0.127***
	-	-	-	-	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.275***	0.272***	0.162***	0.250***	0.186***	0.200***	0.240***	0.232***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1503	1503	1503	1503	1503	1503	1503	1503
R^2	0.219	0.220	0.023	0.243	0.278	0.284	0.299	0.465

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

For the case where standard errors were bootstrapped with 200 replications, see Table C.11 in Appendix 5

Table 5.2: Determinants of Selected Levels of Misrepresentation

However, while an increase in the proportion of informed agents is associated with a negative, if very small (-0.008) impact on the level of misrepresentation, as hypothesised in Hypothesis 1, this result is not statistically significant at even the 10 % level.

I also regress selected levels of misrepresentation on the situation where the proportion of sophisticated and informed buyers is presented as a factor variable, with half informed being distinct from half of the impact of all informed buyers.

As can be seen in Column 2, these results align with those in Column 1. The coefficients and significances on the full-proportion cases are identical to those when the proportions are presented as continuous, while the coefficients on half of the population being sophisticated or informed are approximately half that of the full-proportion case (-0.08 and -0.004) with the same significances as the full cases.

Across both of these regressions approximately 22 % of the variation in the model is explained solely by the proportion of buyers, with R^2 values of 0.219 and 0.220. Due to the additional explanatory possibilities, proportions have been treated as factor variables for the duration of this section. For completeness, I regress misrepresentation on designation as a for-profit seller with no further explanatory covariates.

$$d = \beta_0 + \beta_1 FP + \epsilon$$

From this, it can be seen in Column 3 that being designated a for-profit seller results in a 0.046 increase in selected misrepresentation, significant to the 0.1 % level. This matches the predictions of Hypothesis 1.

It is also of note that designation as a for-profit seller explains 2.3 % of the variation in the model (R^2 value of 0.023).

I then combine agent distribution and seller type into one regression such that:

$$d = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \epsilon$$

As was the case for selected advertising, the coefficients (as shown in Column 4) are identical to those from Columns 2 and 3 and the R^2 value (0.243) is the sum of the two prior. This is attributable to the experimentally enforced lack of correlation between buyer and seller type.

Following this, I control for the simultaneously selected level of advertising (h), as shown in Column 5 of Table 5.2.

$$d = \beta_0 + \beta_1\text{sophisticated} + \beta_2\text{informed} + \beta_3\text{FP} + \beta_4 h + \epsilon$$

The choice of advertising has a positive, statistically significant influence on selected misrepresentation. A one-unit increase in advertising (of 0.1 per the model specification) is associated with a 0.012 increase in the amount of misrepresentation selected by the same seller, significant to the 0.1 % level.

Controlling for advertising slightly decreases the magnitude of the effect of the proportion of sophisticated buyers but does not impact the statistical significance. The coefficient on half sophisticated decreases in magnitude by 0.007 to -0.073 while that on all sophisticated agents decreases in magnitude by 0.012 to -0.160.

It is with regard to the (statistically insignificant) impact of informed buyers that controlling for advertising has the largest result. While earlier, in Columns 1, 2 and 4), an increase in informed buyers was associated with a negative (insignificant) effect on misrepresentation choices, once advertising is controlled for, these coefficients maintained a similar magnitude but had a positive effect. However, these results are sufficiently insignificant that this direction is unlikely to be explanatory.

Controlling for advertising also increases the magnitude of the coefficient on being designated a for-profit seller to 0.054, 0.008 larger than in Column 3 or 4, being a 17 % increase, without any loss of significance.

Including a covariate for advertising selection also increases the amount of variation explained by the model by 3.5 percentage points with a new R^2 of 0.278.

I then introduce round-level fixed effects to account for variation in the specification between experimental rounds.

$$d = \beta_0 + \beta_1\text{sophisticated} + \beta_2\text{informed} + \beta_3\text{FP} + \beta_4 h + \gamma R + \epsilon$$

As can be seen in Column 6, introducing these fixed effects has only negligible impacts on the coefficients on buyer-proportion variables or for-profit designation (at most 0.001) and has no impact on the statistical significance.

The only change of note is for the change in the coefficient on advertising, which increases by 0.01 to 0.128 (approximately an 8 % change) without any reduction in statistical significance.

Introducing round-level fixed effects increases the explanatory power of the model by 0.6 percentage points to an R^2 of 0.284.

Following this, I also introduce session-level fixed effects to account for any heterogeneity in experimental sessions.

$$d = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \beta_4 h + \gamma R + \eta S + \epsilon$$

As was the case for the introduction of round-level fixed effects, this has no observable impact on statistical significance for any coefficients and no impact larger than 0.003 on any magnitudes, as can be seen in Column 7.

However, introduction of session-level fixed effects does increase the explanatory power of the model by 1.5 percentage points, with an R^2 value of 0.299.

Finally, I introduce individual-level fixed effects to account for any heterogeneity attributable to the participants themselves.

$$d = \beta_0 + \beta_1 \text{sophisticated} + \beta_2 \text{informed} + \beta_3 \text{FP} + \beta_4 h + \gamma R + \eta S + \lambda I + \epsilon$$

As shown in Column 8, there is no significant change in the coefficients on buyer-proportions, seller designation or selected level of advertising, nor any similar impact on the statistical significances.

The effect of note from introducing individual-level fixed effects is that the impact of being a for-profit seller, which had been consistently positive and statistically significant thus far (from Columns 3 to 7) becomes negative and statistically insignificant at even the 10 % level. This mirrors the effect from introducing individual fixed effects for choices of advertising (as discussed above) and suggests that seller designation may have captured individual variation, especially due to the sample size which was a limitation of this experiment (as discussed in further detail in the Discussion).

Introduction of individual-level fixed effects increases the explanatory power of the model dramatically, with the R^2 increasing to 0.465, a 16.6 percentage point increase in the explanatory power.

The selection of levels of misrepresentation is driven most strongly by the proportion of sophisticated agents and individual tendencies, including choices of advertising.

This is mirrors the determinants of advertising where the proportion of informed buyers (as well as individual effects and choices of misrepresentation) was the driving force.

It is possible that this symmetry could be attributed to a potentially perceived trade-off between choices of advertising and misrepresentation in the experiment, with the benefit from advertising being perceived higher for uninformed individuals and the benefit from misrepresentation being perceived higher for naïve individuals.

5.2 Buyers

All buyers had the option to accept or reject a contract from a table of 5 contracts across 18 scenarios and 4 rounds.

The dependent variable was thus whether a contract had been accepted, and hence a logit regression was used to interpret the results. A logit specification was selected in preference to a probit regression as the coefficients are more mathematically convenient and the results are statistically comparable. As logit coefficients are not directly interpretable, a Marginal Effects table has been included in Abstract C in Table C.1. The coefficients and marginal results will be explicitly discussed throughout the text below.

I begin by regressing acceptance against the magnitude of misrepresentation (d).

$$accept = \tau_0 + \tau_1 d + \mu$$

This results in a coefficient on misrepresentation of -5.074 which is significant at the 0.1 % level, as seen in Column 1 of Table 5.3. This corresponds to a marginal effect of -1.075, which, given misrepresentation is selected in units of 0.1, means that an increase in misrepresentation of one unit (0.1) results in a reduction of acceptance of 10.75 %.

Misrepresentation results in a pseudo-R^2 of 0.082, implying 8.2 % of the variation in acceptance is explained by the level of misrepresentation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Misrepresentation	-5.074***	-	-5.182***	-5.203***	-5.291***	-5.333***	-6.225***
	(0.13)	-	(0.13)	(0.13)	(0.13)	(0.13)	(0.15)
Sophisticated	-	-0.551***	-0.619***	-0.621***	-0.632***	-0.636***	-0.743***
	-	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Informed	-	-0.022	-0.024	-0.024	-0.025	-0.025	-0.029
	-	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Seller Distribution							
Half For-Profit	-	-	-	0.175***	0.178***	0.180***	0.210***
		-	-	(0.04)	(0.04)	(0.04)	(0.05)
No For-Profit	-	-	-	0.319***	0.324***	0.326***	0.381***
	-	-	-	(0.04)	(0.04)	(0.04)	(0.05)
Constant	1.544***	0.770***	1.903***	1.745***	1.367***	1.575***	1.102***
	(0.03)	(0.03)	(0.05)	(0.05)	(0.06)	(0.08)	(0.13)
Round FE	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes
N	15480	15480	15480	15480	15480	15480	15480
Pseudo-R^2	0.082	0.013	0.097	0.100	0.112	0.117	0.223

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For the case where standard errors were bootstrapped with 200 replications, see Table C.12 in Appendix 5

Table 5.3: Determinants of Buyers' Decisions

Following this, I regress acceptance against designation as a sophisticated and/or informed buyer.

$$accept = \tau_0 + \tau_1 \text{sophisticated} + \tau_2 \text{informed} + \mu$$

As can be seen in Column 2, this results in a negative, statistically significant (to the 0.1 % level) coefficient on designation as a sophisticated buyer. This corresponds to a marginal effect of -0.129, thus a sophisticated buyer, in expectation, should be 12.9 % less likely to accept a given contract.

However, designation as an informed buyer, while still negative with a coefficient of -0.022 is insignificant even at the 10 % level. This result corresponds to a marginal effect of -0.005, which is sufficiently small to be of little consequence. It is possible that the impact of this designation is mitigated by awareness of the proportion of for-profit sellers in the market. This is expanded further in discussions regarding Column 4 onwards and in the Discussion.

Without other factors, the variation from buyer designation explains 1.3 % of the variation in the model, with an R^2 of 0.013.

I then incorporate misrepresentation and buyer designation into the same model, as shown in Column 3.

$$accept = \tau_0 + \tau_1 d + \tau_2 \text{sophisticated} + \tau_3 \text{informed} + \mu$$

The coefficient on misrepresentation remains large, negative and highly significant at -5.182. This implies a marginal effect of -1.073, which is only very slightly smaller in magnitude than that without covariates (a 0.1 increase in misrepresentation would result in a 10.73 rather than 10.75 % reduction in acceptance).

The coefficient on designation as a sophisticate similarly remains negative and highly significant at -0.619. This is equivalent to a marginal effect of -0.129 which is the same as the result without misrepresentation (Column 2).

The effect of designation as an informed individual remains small, negative and insignificant with a coefficient of -0.024 and a marginal effect of -0.005.

Combining the models from Columns 1 and 2 increases the pseudo-R^2 to 0.097.

As suggested in the explanation of Column 2 results, I then incorporate the stated seller distribution, relative to the base case of all contracts being offered by for-profit sellers.

$$accept = \tau_0 + \tau_1 d + \tau_2 \text{sophisticated} + \tau_3 \text{informed} + \tau_4 \text{seller} + \mu$$

The coefficients on stated seller distribution are both positive and highly significant (to the 1 % level). As shown in Column 4, relative to a situation where all sellers are for-profit, a buyer is 3.7 % more likely to accept a contract if half of the sellers are not-for-profit (with a coefficient of 0.175 and a marginal effect of 0.037) and 6.6 % more likely if all of the sellers are not-for-profit (with a coefficient of 0.319 and a marginal effect of 0.066). These results suggest that buyers (at least experimentally) trust contracts offered by not-for-profit sellers more than those offered by for-profit sellers, even when a contract is otherwise identical.

The coefficients on misrepresentation, being sophisticated and being informed are almost unchanged from the previous models with coefficients of -5.203, -0.621 and -0.024 and marginal effects of -1.074, -0.129 and -0.005. All significances are also consistent with those in earlier models.

Introducing stated seller distribution to the model slightly increases the amount of variation in the model explained with a new pseudo-R^2 of 0.100.

Following this, I introduce round-level fixed effects to address any variation due to scale effects between experimental rounds.

$$accept = \tau_0 + \tau_1 d + \tau_2 \text{sophisticated} + \tau_3 \text{informed} + \tau_4 \text{seller} + \psi R + \mu$$

From this, the effects from misrepresentation, being sophisticated and being informed, as shown in Column 5, are consistent in marginal effect and significance with previous models with coefficients of -5.291, -0.632 and -0.025 and marginal effects of -1.074, -0.129 and -0.005.

Similarly, the coefficients on stated seller distribution are consistent in magnitude and significance, with scenarios with half or no for-profit firms having coefficients of 0.178 and 0.324 and marginal effects of 0.037 and 0.066, relative to the base case where all firms are for-profit.

Introducing round-level fixed effects increased the explanatory power of the model by 12 % with the pseudo-R^2 increasing to 0.112.

I then introduce session-level fixed effects to account for variation between experimental sessions, such as experimenter effects, time effects or the impact of individuals on a group.

$$accept = \tau_0 + \tau_1 d + \tau_2 \text{sophisticated} + \tau_3 \text{informed} + \tau_4 \text{seller} + \psi R + \phi S + \mu$$

As in prior cases, this has negligible impact on the effect from any of the explanatory variables, in magnitude or significance, as can be seen in Column 6 of Table 5.3. The coefficients on misrepresentation, being sophisticated and being informed are -5.333, -0.636 and -0.025 with associated marginal effects of -1.074, -0.129 and -0.005. The coefficients on stated seller distribution are the same with scenarios for half and no for-profit firms having coefficients of 0.180 and 0.326 and marginal effects of 0.037 and 0.066 relative to the base case where there are no for-profit firms.

The introduction of session-level fixed effects results in a slight increase in the explanatory power of the model, with the pseudo R^2 increasing to 0.117.

Finally, in Column 7, I introduce individual-level fixed effects to account for heterogeneity between buyers.

$$accept = \tau_0 + \tau_1 d + \tau_2 \text{sophisticated} + \tau_3 \text{informed} + \tau_4 \text{seller} + \psi R + \phi S + \nu I + \mu$$

As in the previous instances, this has no meaningful impact on the effect from any of the explanatory variables. The coefficients on misrepresentation, being sophisticated and being informed are -6.225, -0.743 and -0.029 with marginal effects of -1.075, -0.129 and -0.005. The effects of stated seller distribution are similarly unchanged with coefficients on half and no for-profit firms of 0.210 and 0.381 and marginal effects of 0.037 and 0.066. All statistical significances remain unchanged.

However, in this instance, the introduction of individual-level fixed effects is associated with an increase in the explanatory power of the model with the pseudo R^2 more than doubling to 0.223.

5.3 Relation to Hypotheses

With respect to Hypothesis 1, being that not-for-profit sellers will engage in less advertising and misrepresentation than for-profit sellers as the benefit they receive from both is capped by the non-distribution constraint, there is partial experimental support. In all cases which do not control individual-level fixed effects, the level of misrepresentation selected by for-profit sellers was higher than that selected by not-for-profit sellers, which supports the hypothesis. However, the level of advertising selected by a for-profit seller was actually lower than that chosen by a not-for-profit, which contradicts my theoretical prediction.

As expanded upon in the Discussion section, it is likely that this result is, at least partially, due to a perceived trade-off between advertising and misrepresentation which may have been a fault of the experimental design and presentation.

There is general support for Hypothesis 2, being that sellers of any type will advertise and misrepresent less when contracting for sophisticated buyers. In most cases the proportion of sophisticated agents does have a negative and statistically significant effect on the selected level of advertising, this holds less strongly when experimental round and session are controlled for. In all cases, the proportion of sophisticated agents has a statistically significant, negative effect on the selected level of misrepresentation.

Combining the seller results, it appears that the level of misrepresentation is driven largely by the degree of sophistication whilst the level of advertising is influenced most strongly by the proportion of informed agents. Especially when accounting for the perceived trade-off between advertising and misrepresentation, this conclusion seems logical. Misrepresentation is most effective when buyers are not able to identify it, thus it would follow that the level of misrepresentation should decrease mainly with the proportion of buyers who can identify it. Similarly, as advertising has a magnifying effect on the proportion of buyers who see a contract, but not the proportion who accept said contract, it would be logical that the proportion of buyers who know organisational form, and thus already know the seller exists, would have a negative (if relatively small) effect on the choice of advertising.

Finally, with respect to Hypothesis 3, being that sophisticated buyers will reject misrepresentative contracts, there is significant support from the experimental data. Agents of any type are more likely to reject misrepresentative contracts but this effect is stronger and larger in magnitude for sophisticated agents.

Overall, the results largely correspond to the theoretical predictions, albeit with some noise. Despite this noise, I believe these results are robust and would not expect significant changes in direction or significance were this experiment replicated.

Chapter 6
Discussion

There are several areas regarding the model and the experimental design and implementation which require further discussion. I also include a list of prioritised recommendations and suggestions which warrant further investigation.

6.1 Model

The same model, with few adjustments, can apply to any insurance contract where there are clearly defined outcomes with known probabilities. For instance, life insurance with a payout of a upon death (with probability γ) or car insurance with a payout of a_1 for repairs (necessary with probability γ) or a_2 for replacement (necessary with probability α such that the probability of nothing happening is $1 - \gamma - \alpha$) and periodic fee structures p could both be modelled as above with very few modifications.

It is also of note that γ may be heterogeneous between agent types. There is likely to be some causality between a higher personal γ and an agent being sophisticated. For instance, if an agent has a family history of diabetes then they are statistically more likely to develop diabetes themselves and theoretically more likely to be aware of the likelihood of developing diabetes. Due to this, a possible extension to the model could include a type-dependent γ.

An interesting extension to the model would be the extension to heterogeneous cost functions and cost differentials for for- and not-for-profit firms. Building on the work of Leete (2001), this would reflect access to subsidies and existing corporate relationships which may vary between (and indeed, within) firms of different types. While modelling this difference was not the purpose of the model constructed for my thesis, it would be an interesting extension and would be a meaningful contribution to the literature.

6.2 Experiment

6.2.1 Design

It is possible that the contextualisation of the insurance with actual diseases could result in externalities. Participants may have personal preconceptions regarding particular diseases, or may already know the actual lifetime prevalence (approximates of which were used to calculate the payoff tables). This may result in differences in their decisions being more attributable to experimental context rather than the manipulated conditions. However, the same preconceptions would logically occur if the same participant were contracting with a given disease, and thus it is plausible that these externalities reflect actual insurance decisions more clearly than is possible through the use of hypothetical diseases.

As an example, a participant who has family history of cancer may already know lifetime prevalence (which, if they are designated naive, could be problematic information) or may have a perception that their lifetime prevalence is higher than average (which may result in their accepting a contract with significant positive misrepresentations if they believe this to be a more accurate reflection of their personal circumstances). However, these same beliefs would exist if this participant were purchasing a non-hypothetical insurance policy and so these decisions may reflect their actual preferences.

In order to avoid any preconceptions or impact from personal experience, I decided to create four hypothetical diseases (HR A-D). Whilst there is the potential for this choice to result in artificiality ((Wallis and Friedman, 1942), (Harris, 2017)), the overall health insurance context should be sufficiently common to overcome this artificiality.

An extension to the experiment would be to implement three experimental treatments, one completely hypothetical, one with named common diseases and one with common diseases and further information. This extension would both address the potential for artificiality and increase the external validity, and would more completely represent the spectrum of agents.

6.2.2 Implementation

Due to the significant timing restrictions associated with completing a project of this scale within the duration of an honours year, in addition to further timing constraints detailed in Appendix D, the experiment itself was completed in a very short time frame. Two pilot studies of 8 participants were run within a week (and demonstrated no problems) with the four main experimental sessions (each intended to have 24 participants) run on the same day, one day after the final pilot study. It was not feasible to run subsequent experimental sessions at any point after that day. Despite these caveats, the experiment in its implemented form and its results constitute a proof of concept. I am convinced that a replication under better conditions would lead to fundamentally similar but quantitatively stronger results.

When I implemented the experiment, several unexpected and unmitigatable factors impacted the results. Despite allowing for an additional 4 participants in each experimental session (with 28 requested for a session of 24 participants) to allow the experiment to continue in case some participants did not show up, 2 of the main sessions had 5 participants not arrive (leaving each experimental session with a total of 23 participants) while one session had 8 participants not attend (for a total of 20 participants). For the first session, Dr PC Yu, who was unfamiliar with the experiment itself but familiar with the underlying theoretical method acted as a participant. In the third session, where there were again 5 'no-show' participants, the author acted as a participant also. In both cases, the other participants were advised that this was the case but none chose to withdraw in either instance[1]. Dr Yu's decisions were retained in the data due to his lack of familiarity with the experiment itself (a version without Dr Yu's data demonstrated no differences in the results, see Tables C.2 and C.3 in Appendix C[2]), while the decisions of the author were removed from the data. For the last session, only 20 of the 28 invited participants attended and, as a result, four volunteers were paid to leave the experiment and that session was instead run with 16 participants. While it is possible that these factors may have impacted the decisions of participants in that session, any such effect is controlled for by the introduction of session level fixed effects. In addition, one participant left the room for the duration of one round[3] during the third session and the randomly selected values for that round were also removed from the data.

In order to maximise the use of the collected data, the observations associated with the second pilot session were included in the data analysed in the Results chapter. While it is my belief that, given the faults already in the data, this inclusion did not impact the validity of the data, the same tables presented in the Results section in Tables 5.1, 5.2 and 5.3 have been replicated in Appendix C in Tables C.7, C.8 and C.9 with the pilot data excluded. While this exclusion slightly impacted the magnitude of some effects, there was no impact to the sign of coefficients nor the statistical significances, and thus this inclusion is unlikely to have further affected the validity of the presented results.

[1]As the experiment could only be completed with groups of exactly 8, the alternative presented to participants was that 7 other participants could volunteer to leave the session and receive a show-up payment only, with the session then being run with two groups of 8 rather than three.

[2]Note that, as Dr Yu acted only as a seller, only the results related to selections of advertising and misrepresentation were replicated without his data.

[3]This participant left the lab in order to visit the bathroom.

There was significant variation in the speed at which participants completed their tasks. It was not uncommon for there to be differences of more than 10 minutes between the first and last participants finishing one of the four rounds, with at most two participants usually being responsible for the delay. This suggests that the difference in speed was attributable to individual differences rather than the differences in tasks assigned to buyers and sellers. This delay meant that many participants were left waiting for significant periods of time after completion of their tasks as the experimental design was such that no group could progress until all participants across all groups had finished. It is possible that this may have impacted the behaviour of some participants as they may have attempted to complete the task faster to minimise the waiting time for others, which may have reduced the amount of consideration given to selections. Successive rounds did consistently take less time (typically the first round took approximately 12 minutes, this converged to 5 minutes by the fourth round), however, it is unclear whether this is due to long waiting times or a learning effect. Although it is possible to program z-tree to record time, the significant heterogeneity in participant completion time was not expected during the design of the experiment, and the time between pilots and regularly planned sessions was too short to make programming changes, and so was not explicitly recorded by the experiment, and thus cannot be controlled for in the results.

In future sessions, I would both record completion time per participant, in order to be able to control for it, and would limit round duration. By explicitly imposing both upper and lower time limits, such that a screen could not be progressed past in less than a given time (20 seconds), and capping round duration at perhaps 7 minutes would encourage efficient but complete decision making.

In addition, the computer program (z-Tree) consistently froze between experimental rounds in the full experiment for periods of up to 3 minutes, which contributed to the delays in timing. This delay had not occurred during the pilot sessions and hence could not be remedied on the day of the experimental sessions.

The combination of these timing factors resulted in almost all of the session durations exceeding the allocated time, in one case by 20 minutes. One recommended adjustment for further experimental sessions would be the use of 90, rather than 60, minute sessions. This adjustment was not possible on the day of the experiments. This delay resulted in some participants within the session becoming frustrated and concerned about being able to attend scheduled classes, which may have impacted their results. In addition, the delay resulted in subsequent sessions beginning later than planned which may have impacted the decisions made by some participants. It is possible that some participants may have taken less care with their decisions either due to a desire to complete the experiment on time or due to a reduced desire to produce useful results.

Finally, data from the fourth round did not process properly for the two last experimental sessions and hence, while these rounds were undertaken and paid for, the results were calculated with limited data. While not fully understood, this failure is likely attributable to not ensuring all participants pressed "Continue" on the final screen before commencing the next experimental session. However, in prior sessions and the pilots, such failure had not occurred and was not noticed until after all experimental sessions had been concluded. A regression completely excluding the fourth round from all sessions is included in Tables C.4, C.5 and C.6 in Appendix C. While there are some slight differences in the data, any changes in coefficients were only small and there was no impact on statistical significance or direction of effects, thus this failure is unlikely to have biased the results to a large degree.

Another concern regarding the implementation of the experiment is the clarity and understandability of the experimental conditions, instructions and tasks. While no concerns were raised by participants in any round following the provision of the main instructions (included in Appendix B) which were also read to the group, many questions were raised while the experiment was underway. In future experiments, the use of comprehension questions could avoid, or at least reduce, the likelihood of such problems.

In particular, an included rationality check for buyers (where they were told they were contracting with one party in a situation where no parties of that type existed) caused significant confusion and may have impacted the results. As was discussed in less detail in the results section, buyers were asked what their decisions would be for a range of background information and for a series of seller-type designations. An example of where this lead to confusion would be:

Imagine you did not know the likelihood of HR-A and knew that you were being offered a contract by a For-Profit firm.
Which of the following contracts would you accept if:
All contracts were being offered by Not-For-Profit firms.

The presentation of this scenario has been included in Appendix B. While it was believed that a rational participant would act no differently depending on the distribution of seller types, conditional on knowing the seller type they were contracting with, this instead caused significant confusion where participants were unsure which information was correct: stated seller type or potential sellers in the market. It is possible that this could be the reason that being designated an informed buyer was not a significant determinant of buyer decisions (See Table 5.3) as the effect may have been mitigated by stated seller distribution (which was statistically significant). While it is also possible that awareness of seller type has no relevance for buyer decisions, this would contradict existing literature (i.e. Deetlefs et al. (2018), Ben-Ner, Hamann, and Ren (2018)) and rational expectations, especially as seller distribution did have a significant effect. This confusion may also explain the relatively small (though highly significant) effect from seller distribution (the marginal effects were also included in Table C.1 in Appendix C).

A similar confusion may have existed for sellers where there was a perceived trade-off between choices of advertising and misrepresentation. While this trade-off was not an intention of the experimental design, the manner in which tables were presented (for an example, see Appendix B) may have suggested that such a trade-off did occur. This confusion was likely compounded by the use of the model predictions for premium and payout, both functions of advertising and misrepresentation, for the calculation of payouts. While this design was intended as a mechanism by which to minimise the number of decisions and cognitive load required of participants, and to isolate decisions of advertising and misrepresentation, it may have concealed actual preferences.

The prediction of the model was that, in expectation:

$$p^* = (\gamma + d^*)\ln(1 + \frac{d^*}{\gamma})$$

and

$$a^* = 1 + \frac{d^*}{\gamma}$$

Thus the premium charged (p^*) was increasing in misrepresentation (d), as was the payout (a^*).

From this, the expected profit per accepted contract, exclusive of the advertising cost $c(h)$ was:

$$p^* - \gamma a^* = (\gamma + d^*)\ln(1 + \frac{d^*}{\gamma}) - \gamma(1 + \frac{d^*}{\gamma}).$$

This could result in a negative expected profit per accepted contract for any cases where:

$$d > \gamma(e - 1).$$

A negative profit-per-contract was not possible through the specification of the experiment in the cases where $\gamma = 0.4$ or 0.3, but when $\gamma = 0.2$, a negative profit-per-contract was possible for choices of misrepresentation larger than 0.34 (where 0.4 was an available option), and where $\gamma = 0.1$, this was possible for any choice of misrepresentation larger than 0.17, where 0.2, 0.3 and 0.4 were all available choices.

This problem could have been avoided by scaling choices of misrepresentation to the actual likelihood, however, this was not considered prior to the experiment's completion. Also, using smaller units of misrepresentation, such as 0.01 (rather than 0.1) would more accurately reflect the actual behaviour by firms when creating misrepresentative contracts.

In addition, a large proportion of the subject pool from which these participants were drawn has English as a second language, and, most likely, this fact is reflected in the composition of participants in the experiment. In particular, despite it being explicitly mentioned in the instructions, in the context slides and in the experiment itself, at least one participant was unaware that they were making decisions about insurance contracts until the fourth round whereupon they asked the experimenter. This may have resulted in an inability to make choices representative of their actual preferences.

Participants were also aware on a per-round basis of their current payout, which may have impacted their behaviour. At the end of the third round (of four), 28 % of participants had yet to receive any payment (42 % of sellers and 17 % of buyers) while an additional 24 % had received amounts less than \$ 2 (13 % of sellers and 33 % of buyers). While average payouts at the end of the experiment were \$ 20.70, including an adjusted \$ 10 show-up payment, it is possible that the lack of payment may have limited the amount of cognitive effort participants were willing to implement. The work of Gneezy and Rustichini (2000) suggested that, in some cases, it was better to explicitly pay participants nothing (to implement a non-performance-based payment system) than to pay them too little (per decision). While Rydval and Ortmann (2004) found that cognitive ability was more important than the payment amount, their finding does not eliminate the possibility that participants may have implemented less effort depending on their payments thus far.

Similarly, it is possible that decision fatigue had effects on the reliability of participant choices. Not including the context round, sellers made 36 decisions (1 symmetric choice of h and d for each of 9 scenarios, repeated across 4 rounds) while buyers made 360 (5 choices of accept or reject across 18 scenarios, repeated across 4 rounds). While the strategy method, which was used in this experiment, is very common in experimental economics, and usually results in results functionally identical to the non-strategy method (Brandts and Charness, 2011), the number of decisions required may have impacted cognitive capacity and willingness.

It is also possible that the decision to randomly draw only one scenario from 9 or 18, with no guarantee that the scenarios would be evenly distributed within an experimental session, may have impacted the behaviour of participants.

In addition, as so few experimental sessions were run, it is possible that conditions within sessions could have influenced results. The four main sessions were run by two different experimenters (Andreas Ortmann and PC Yu), had different attendance rates (as discussed above in 6.2.2) and started at a slightly different time relative to the expected start time (usually due to the session before). Fixed effects for the sessions were introduced in an attempt to address this, as shown in the Results section (Tables 5.1, 5.2, and 5.3). The session fixed effects all had explanatory power which suggests that part of the variance in the data was due to the sessions themselves, although this could also be due to many other reasons not otherwise controlled for.

This exhaustive list of design and implementation problems suggests that my results quantitatively are very robust and that once the problems have been addressed, I can expect quantitatively stronger results.

The external validity of the results from the experiment in its current form are limited by several factors, in addition to those discussed thus far. It is possible that the strict lack of correlation between for- or not-for-profit firms and the distribution of agents may not reflect economic realities, as theoretically (including work by Gabaix and Laibson (2006) and Heidhues and Koszegi (2017)) as a larger proportion of buyers become informed there is less incentive for new sellers to misrepresent and hence less benefit to being for-profit, and a larger proportion of for-profit sellers should encourage buyers to become more sophisticated in order to be less exploited. Also, the very small sample size and high degree of individual fixed-effects suggest that it is possible that some the results may be due to differences in individual preferences rather than representative of a larger population. As these results correspond in most cases with empirical and theoretical predictions, it is unlikely that these factors undermine the validity of the findings, but further experimentation on a larger scale with more nuanced methodology would be beneficial to the interpretation of results.

6.3 Recommendations

There are several modifications which I believe would improve the validity of the results reported in this thesis. I first suggest several extensions to the model itself to increase its external validity. Following this, I enumerate suggestions to improve the experimental design and implementation. While I do not expect these suggestions to impact the robustness of the results presented in my thesis thus far, they would quantitatively increase the strength of the results.

6.3.1 Model

1. Expand to heterogeneous γ between agents.
   Introducing individual specific likelihoods of illness, likely on some probabilistic distribution, would add complexity but also increase the external validity of the model.

2. Expand to heterogeneous cost differentials between for- and not-for-profit sellers.
   Introducing type-dependent cost functions would reflect differences in access to subsidies and industry connections between for- and not-for-profit firms.

3. Expand to other insurance contract scenarios.
   Expanding the model to other insurance scenarios would increase the applicability of this model and the contribution to the literature.

6.3.2 Experimental Design

1. Exclude or explain rationality check for buyers.
   This would eliminate a significant cause of confusion within the experiment without reducing the robustness of the results.

2. Remove perceived trade-off between advertising and misrepresentation.
   This would isolate the actual preference for levels of advertising and misrepresentation and reduce noise in the results.

   (a) Use proportional choices of misrepresentation between rounds.
   This would smooth the relative magnitude of misrepresentation between rounds, such that a percentage misrepresentation would be implemented instead of a percentage point misrepresentation, increasing consistency between rounds.

(b) Use smaller units of misrepresentation.
Using 0.1 units of misrepresentation for true likelihoods ranging from 0.1 to 0.4 is less reflective of actual behaviour. Using units of 0.01 would increase the external validity of the results and may have a reduced ethical cost to participants.

3. Introduce additional experimental sessions.
While impossible due to time constraints (see Appendix D), the use of more experimental sessions would allow for larger sample sizes, less noise and more efficient clustering possibilities.

4. Contrast hypothetical and actual diseases.
By contrasting treatments with completely hypothetical diseases (HR A-D as in this implementation), known, common diseases (Cancer, Diabetes, Heart Disease, Stroke) and common diseases with context provided (symptoms, life expectancy, risk factors), I could increase the external validity.

5. Reduce variability in per-round payment.
This would prevent variation in effort levels possible from severely heterogeneous payments.

6.3.3 Experimental Implementation

1. Comprehension

   (a) Introduce comprehension questions.
   This would have ensured that participants were aware of the basic context of the experiment and of their roles.

   (b) Expand sessions to 90 minutes.
   This would have allowed more time for participants to become familiar with the experimental design.

   (c) Introduce English fluency requirement.
   A requirement for fluency in English when recruiting subjects would increase the comprehension ability of participants.

2. Programming

   (a) Heterogeneity in completion time

      i. Record time to complete tasks.
      This would allow the time taken per participant to be used as a measure of engagement or ability or, at minimum, to be controlled for during regressions.

ii. Limit round duration to encourage efficient decisions.
Introducing a clearly stated time restriction such that a round could not be completed before 2 minutes (with an additional minimum of 20 seconds per round) or after 7 minutes would induce more efficient decision making.

iii. Allow some groups to progress before others were completed.
This would allow the experiment to progress more smoothly with groups not waiting for other groups to proceed but would not otherwise affect decisions made by groups. This could also lead to a different timing of payoffs which would allow the end of the experiment to run more smoothly.

(b) Computer froze between experimental rounds.
This would reduce frustration from participants and experimenters and allow sessions to run faster and more smoothly.

(c) Data should save without pressing final complete button.
This would reduce the likelihood of losing data and any requirement to check that data had processed before commencing further rounds.

Chapter 7
Conclusion

This thesis investigated the relationship between organisation form, the state of being for- or not-for-profit, and contract design and decisions, particularly choices of misrepresentation and advertising in an insurance contract. I also studied how agents of different types, being informed or uninformed, sophisticated or naïve, responded to these contracts. My thesis builds on the existing literature on organisational form as a determinant of behaviour and contributes to the Behavioural Contract Theory literature, particularly with regards to situations with asymmetric information.

I began by formulating a theoretical model, which predicted different behaviour by for- and not-for-profit firms as a result of the non-distribution-of-profit constraint. I then used this model to design and implement a laboratory experiment in order to investigate the impact of type and information on contractual decisions.

From this experiment, I found that choices of misrepresentation are driven by the ability of firms to successfully misrepresent, being where smaller proportions of the agents are sophisticated, and that choices of advertising are influenced more strongly by whether an agent is informed. The direction of these effects corresponded with the direction of my theoretical predictions however the differences being driven by different factors was beyond the scope of my theoretical model.

In addition, I found that agents are more inclined to reject misrepresentative contracts and that this is increasing in the degree to which agents are able to identify misrepresentation. I also found that agents are more likely to accept contracts if the market distribution of for-profit firms is lower. These results corresponded with the predictions of my theoretical model.

Appendix A
Proofs and First-Order Conditions

A.1 Not-For-Profit Profit Maximisation

$$\pi = (p_{NFP} - \gamma a_{NFP}) \sum_j \sum_k [(\omega_j^k(h,d)\mu_j^k)] - c(h) = 0$$

Constrained by the incentive compatibility constraint:

$$\gamma u(a_{NFP}) + (1-\gamma)u(1) - p_{NFP} \geq \gamma u(0) + (1-\gamma)u(1)$$

And the participation constraint:

$$\gamma u(a_{NFP}) + (1-\gamma)u(1) - p_{NFP} \geq U_0$$

Using that $u(\cdot) = ln(\cdot)$ and that $\ln(1) = 0$, and assuming that $U_0 = 0$, this simplifies to:

$$\pi = (p_{NFP} - \gamma a_{NFP}) \sum_j \sum_k [(\omega_j^k(h,d)\mu_j^k)] - c(h) = 0$$

s.t.

$$\gamma ln(a_{NFP}) - p_{NFP} \geq \gamma ln(0) = -\infty$$

This results in the first order conditions:

$$\begin{cases} \sum_j \sum_k [(\omega_j^k(h,d)\mu_j^k)] = 1 & (i) \\ \gamma(\sum_j \sum_k [(\omega_j^k(h,d)\mu_j^k)]) = \frac{\gamma}{a_{NFP}} & (ii) \\ \gamma ln(a_{NFP}) - p_{NFP} = 0 & (iii) \end{cases}$$

Substituting (i) into (ii), find that $a^*_{NFP} = 1$
Substituting a^*_{NFP} into (iii), find that $p^*_{NFP} = \gamma$

A.2 For-Profit Profit Maximisation

A.2.1 Sophisticates

$$\max \pi_s = \max \left[\omega_s^i(h,d)\,\mu_s^i + \omega_s^u(h,d)\,\mu_s^u\right](p_s - \gamma a_s)$$

Constrained by the participation constraint:

$$U_s \equiv (1-\gamma)\,u\,(1) + \gamma u\,(a_s) - p_s \geq U_0 \equiv u\,(1) - p_{NFP}.$$

Using that $u(\cdot) = ln(\cdot)$, that $p_{NFP} = \gamma$ and that $\ln(1) = 0$, this simplifies to:

$$\max\left[\omega_s^i\,(h,d)\,\mu_s^i + \omega_s^u\,(h,d)\,\mu_s^u\right](p_s - \gamma a_s)$$

$$\text{s.t.}$$

$$\gamma\ln(a_s) - p_s \geq -\gamma$$

This results in the first-order conditions:

$$\begin{cases} (\omega_s^i(h,d)\mu_s^i + \omega_s^u(h,d)\mu_s^u) = 1 & (i) \\ \gamma(\omega_s^i(h,d)\mu_s^i + \omega_s^u(h,d)) = \frac{\gamma}{a_s} & (ii) \\ \gamma ln(a_s) - p_s = -\gamma & (iii) \end{cases}$$

Substituting (i) into (ii), find that $a_s^* = 1 = a_{NFP}^*$
Substituting a_s^* into (iii), find that $p_s^* = \gamma = p_{NFP}^*$

A.2.2 Naïfs

$$\max\pi_n = \max\left[\omega_n^i\,(h,d)\,\mu_n^i + \omega_n^u\,(h,d)\,\mu_n^u\right](p_n - \gamma a_n)$$

Constrained by the participation constraint

$$U_n \equiv (1-\gamma-d)\,u\,(1) + (\gamma+d)\,u\,(a_n) - p_n \geq U_0,$$

and the incentive compatibility constraint

$$U_n \geq (1-\gamma-d)\,u\,(1) + (\gamma+d)\,u\,(a_s) - p_s.$$

Using that $u(\cdot) = ln(\cdot)$ and that $\ln(1) = 0$, this simplifies to:

$$\max\left[\omega_n^i\,(h,d)\,\mu_n^i + \omega_n^u\,(h,d)\,\mu_n^u\right](p_n - \gamma a_n)$$

$$\text{s.t.}$$

$$(\gamma+d)ln(a_n) - p_n \geq (\gamma+d)ln(a_s) - p_s - \gamma$$

Substituting in the predictions for a_s and p_s from and using that and that $\ln(1) = 0$:

$$\max \left[\omega_n^i\,(h,d)\,\mu_n^i + \omega_n^u\,(h,d)\,\mu_n^u\right](p_n - \gamma a_n)$$

s.t.

$$(\gamma + d)ln(a_n) - p_n \geq -\gamma$$

This results in the first-order conditions:

$$\begin{cases} [\omega_n^i\,(h,d)\,\mu_n^i + \omega_n^u\,(h,d)\,\mu_n^u] = 1 & (i) \\ \gamma\,[\omega_n^i\,(h,d)\,\mu_n^i + \omega_n^u\,(h,d)\,\mu_n^u] = \frac{\gamma+d}{a} & (ii) \\ (\gamma + d)ln(a_n) - p + \gamma = 0 & (iii) \end{cases}$$

Substituting (i) into (ii), find that $a_n^* = 1 + \frac{d^*}{\gamma}$
Substituting a_n^* into (iii), find that $p_s^* = \gamma + (\gamma + d)ln(1 + \frac{d^*}{\gamma})$

Appendix B
Instructions

B.1 Context

In this experiment, you will be making decisions about hypothetical insurance contracts.

Your payoffs for each round will depend on your decisions and the decisions of the other participants.

You will be randomly designated either as a seller or buyer of insurance contracts.

If you are a seller, then you will either be designated a For-Profit or a Not-For-Profit seller.

If you are a Not-For-Profit seller, then your potential profit is capped in each round with any excess profit being donated to the Prince of Wales Hospital Foundation.

As a seller, you will choose levels of advertising and stated likelihood for nine scenarios across four rounds. Advertising increases the chance that a buyer sees your contract, stated likelihood can make your contract look more appealing to buyers.

If you are not a seller, then you are a buyer. As a buyer, you will choose whether to accept or reject contracts for eighteen scenarios across four rounds.

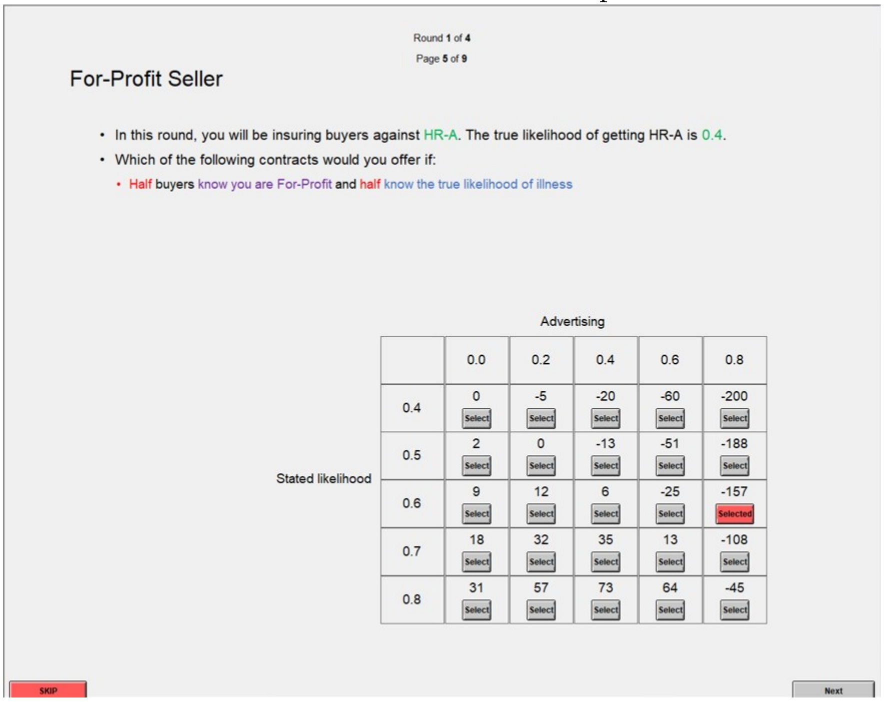
At the end of each round, one buyer scenario and one seller scenario will be played out simultaneously and this is what your payouts will be based on.

A successful match occurs when the contract offered by a seller matches a contract accepted by a buyer they have been matched with. If no match is successful, then the payout for that round will be 0.

All amounts in this experiment are in experimental dollars. Fifteen experimental dollars are equivalent to one Australian dollar.

B.2 Screenshots

The decision screen available to for-profit sellers:


Round 1 of 4

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For-Profit Seller

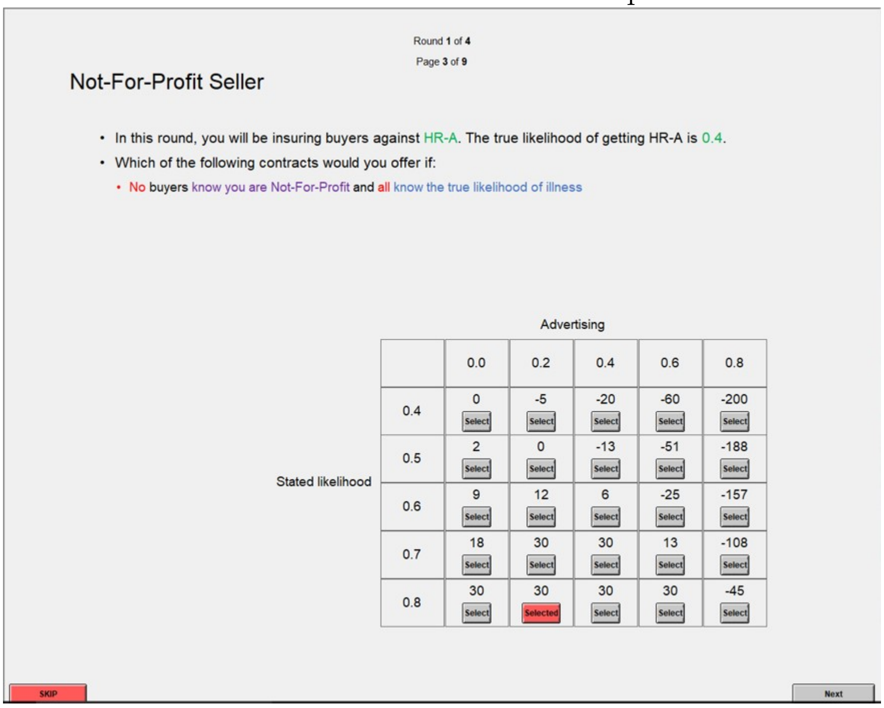
- In this round, you will be insuring buyers against HR-A. The true likelihood of getting HR-A is 0.4.
- Which of the following contracts would you offer if:
  - Half buyers know you are For-Profit and half know the true likelihood of illness

Stated likelihood \ Advertising	0.0	0.2	0.4	0.6	0.8
0.4	0 Select	-5 Select	-20 Select	-60 Select	-200 Select
0.5	2 Select	0 Select	-13 Select	-51 Select	-188 Select
0.6	9 Select	12 Select	6 Select	-25 Select	-157 Selected
0.7	18 Select	32 Select	35 Select	13 Select	-108 Select
0.8	31 Select	57 Select	73 Select	64 Select	-45 Select

SKIP

Next

The decision screen available to not-for-profit sellers:


Round 1 of 4

Page 3 of 9

Not-For-Profit Seller

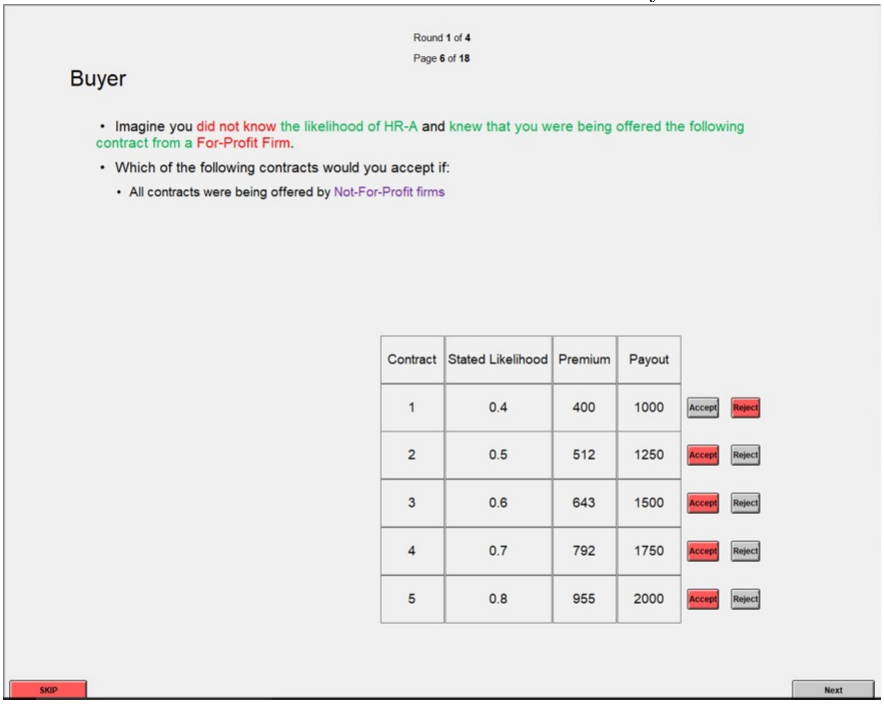
- In this round, you will be insuring buyers against HR-A. The true likelihood of getting HR-A is 0.4.
- Which of the following contracts would you offer if:
  - No buyers know you are Not-For-Profit and all know the true likelihood of illness

Stated likelihood \ Advertising	0.0	0.2	0.4	0.6	0.8
0.4	0 Select	-5 Select	-20 Select	-60 Select	-200 Select
0.5	2 Select	0 Select	-13 Select	-51 Select	-188 Select
0.6	9 Select	12 Select	6 Select	-25 Select	-157 Select
0.7	18 Select	30 Select	30 Select	13 Select	-108 Select
0.8	30 Select	30 Selected	30 Select	30 Select	-45 Select

SKIP

Next

The decision screen available to buyers:



Contract	Stated Likelihood	Premium	Payout
1	0.4	400	1000
2	0.5	512	1250
3	0.6	643	1500
4	0.7	792	1750
5	0.8	955	2000

Appendix C
Further Results and Regressions

C.1 Marginal Effects of Logit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Misrepresentation	-1.075	-	-1.073	-1.074	-1.074	-1.074	-1.075
Sophisticated	-	-0.129	-0.129	-0.129	-0.129	-0.129	-0.129
Informed	-	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005
Seller Distribution	-	-	-	-	-	-	-
Half For-Profit	-	-	-	0.037	0.037	0.037	0.037
No For-Profit	-	-	-	0.066	0.066	0.066	0.066
Round FE	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes
N	15480	15480	15480	15480	15480	15480	15480
Pseudo-R^2	0.082	0.013	0.097	0.100	0.112	0.117	0.223

Table C.1: Marginal Effects on Determinants of Buyers' Decisions

C.2 Experimental Validity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated								
Half Sophisticated	-0.024	-0.024	-0.024	-0.024	-0.024	-0.024	-0.023	-0.023
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-0.035*	-0.035*	-0.035*	-0.035*	-0.035*	-0.035*	-0.034*	-0.034*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Informed								
Half Informed	-0.061***	-0.061***	-0.061***	-0.061***	-0.061***	-0.061***	-0.061***	-0.061***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	-0.096***	-0.096***	-0.096***	-0.096***	-0.096***	-0.096***	-0.096***	-0.096***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For Profit Seller	-0.087***	-0.087***	-0.088***	-0.088***	-0.088***	-0.088***	0.099	0.099
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.05)
Misrepresentation	0.393***	0.393***	0.396***	0.396***	0.393***	0.393***	0.402***	0.402***
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Constant	0.441***	0.441***	0.354***	0.354***	0.343***	0.343***	0.330***	0.330***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
RoundFE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes	Yes
Dr Yu Included	Yes	No	Yes	No	Yes	No	Yes	No
N	1503	1503	1503	1503	1503	1503	1503	1503

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Comparison of Advertising Choice With and Without PC Yu

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated								
Half Sophisticated	-0.073***	-0.073***	-0.073***	-0.073***	-0.073***	-0.073***	-0.073***	-0.073***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-0.160***	-0.160***	-0.159***	-0.159***	-0.160***	-0.160***	-0.159***	-0.159***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed								
Half Informed	0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.004
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	0.004	0.004	0.005	0.005	0.004	0.004	0.005	0.005
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For Profit Seller	0.054***	0.054***	0.055***	0.055***	0.055***	0.055***	-0.037	-0.037
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
Advertising	0.118***	0.118***	0.128***	0.128***	0.125***	0.125***	0.127***	0.127***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.186***	0.186***	0.200***	0.200***	0.240***	0.240***	0.232***	0.232***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
RoundFE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes	Yes
Dr Yu Included	Yes	No	Yes	No	Yes	No	Yes	No
N	1503	1503	1503	1503	1503	1503	1503	1503

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

Table C.3: Comparison of Misrepresentation Choice With and Without PC Yu

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated								
Half Sophisticated	-0.024	-0.023	-0.024	-0.022	-0.024	-0.021	-0.023	-0.016
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
All Sophisticated	-0.035*	-0.047**	-0.035*	-0.044*	-0.035*	-0.043*	-0.034*	-0.032*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Informed								
Half Informed	-0.061***	-0.069***	-0.061***	-0.069***	-0.061***	-0.069***	-0.061***	-0.069***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
All Informed	-0.096***	-0.102***	-0.096***	-0.102***	-0.096***	-0.102***	-0.096***	-0.101***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
For-Profit Seller	-0.087***	-0.089***	-0.088***	-0.091***	-0.088***	-0.091***	0.099	0.100
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.05)
Misrepresentation	0.393***	0.346***	0.396***	0.363***	0.393***	0.369***	0.402***	0.433***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)
Constant	0.441***	0.437***	0.354***	0.368***	0.343***	0.333***	0.330***	0.278***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
RoundFE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes	Yes
Round 4 Included	Yes	No	Yes	No	Yes	No	Yes	No
N	1503	1251	1503	1251	1503	1251	1503	1251

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

Table C.4: Comparison of Advertising Choice With and Without Round 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated								
Half Sophisticated	-0.073***	-0.073***	-0.073***	-0.072***	-0.073***	-0.072***	-0.073***	-0.071***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-0.160***	-0.161***	-0.159***	-0.159***	-0.160***	-0.159***	-0.159***	-0.158***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed								
Half Informed	0.004	0.004	0.004	0.005	0.004	0.005	0.004	0.006
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	0.004	-0.002	0.005	-0.001	0.004	-0.001	0.005	0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For Profit Seller	0.054***	0.051***	0.055***	0.052***	0.055***	0.053***	-0.037	-0.038
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
Advertising	0.118***	0.109***	0.128***	0.119***	0.125***	0.119***	0.127***	0.131***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Constant	0.186***	0.193***	0.200***	0.205***	0.240***	0.246***	0.232***	0.240***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
RoundFE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes	Yes
Round 4 Included	Yes	No	Yes	No	Yes	No	Yes	No
N	1503	1251	1503	1251	1503	1251	1503	1251

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

Table C.5: Comparison of Misrepresentation Choice With and Without Round 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Misrepresentation	-5.203***	-5.326***	-5.291***	-5.404***	-5.333***	-5.442***	-6.225***	-6.346***
	(-39.38)	(-36.97)	(-39.61)	(-37.15)	(-39.71)	(-37.23)	(-41.69)	(-39.04)
Sophisticated	-0.621***	-0.601***	-0.632***	-0.610***	-0.636***	-0.614***	-0.743***	-0.715***
	(-17.45)	(-15.51)	(-17.58)	(-15.61)	(-17.65)	(-15.66)	(-18.95)	(-16.82)
Informed	-0.0244	-0.0195	-0.0248	-0.0198	-0.0250	-0.0199	-0.0292	-0.0232
	(-0.65)	(-0.48)	(-0.66)	(-0.48)	(-0.66)	(-0.48)	(-0.71)	(-0.52)
	(0.04)							
<u>Seller Distribution</u>								
Half For-Profit	0.175***	0.161***	0.178***	0.163***	0.180***	0.165***	0.210***	0.192***
	(4.08)	(3.44)	(4.10)	(3.45)	(4.13)	(3.47)	(4.45)	(3.75)
All For-Profit	0.319***	0.318***	0.324***	0.322***	0.326***	0.325***	0.381***	0.378***
	(7.34)	(6.73)	(7.39)	(6.77)	(7.43)	(6.80)	(8.01)	(7.33)
Constant	1.745***	1.700***	1.367***	1.382***	1.575***	1.650***	1.102***	1.215***
	(32.97)	(29.58)	(23.22)	(22.09)	(18.96)	(17.99)	(8.34)	(8.10)
Round FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	No	No	No	No	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes	Yes
Round 4 Included	Yes	No	Yes	No	Yes	No	Yes	No
N	15480	12960	15480	12960	15480	12960	15480	12960

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

Table C.6: Comparison of Acceptance Choice With and Without Round 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.100***	-	-	-	-	-	-	-
	(0.02)	-	-	-	-	-	-	-
Half Sophisticated	-	-0.054***	-	-0.054***	-0.028	-0.026	-0.027	-0.024
	-	(0.02)	-	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
All Sophisticated	-	-0.100***	-	-0.100***	-0.043*	-0.040*	-0.042*	-0.034*
	-	(0.02)	-	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Informed	-0.089***	-	-	-	-	-	-	-
	(0.02)	-	-	-	-	-	-	-
Half Informed	-	-0.062***	-	-0.062***	-0.061***	-0.060***	-0.061***	-0.060***
	-	(0.02)	-	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
All Informed	-	-0.089***	-	-0.089***	-0.086***	-0.086***	-0.086***	-0.086***
	-	(0.02)	-	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
For-Profit Seller	-	-	-0.091***	-0.091***	-0.102***	-0.104***	-0.104***	0.098
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)
Misrepresentation	-	-	-	-	0.321***	0.336***	0.329***	0.370***
	-	-	-	-	(0.05)	(0.05)	(0.05)	(0.05)
Constant	0.491***	0.498***	0.442***	0.543***	0.462***	0.380***	0.377***	0.383***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1359	1359	1359	1359	1359	1359	1359	1359
R^2	0.049	0.050	0.034	0.084	0.113	0.170	0.178	0.362

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Determinants of Selected Level of Advertising Excluding Pilot

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.177***	-	-	-	-	-	-	-
	(0.01)	-	-	-	-	-	-	-
Half Sophisticated	-	-0.083***	-	-0.083***	-0.078***	-0.077***	-0.077***	-0.077***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-	-0.177***	-	-0.177***	-0.167***	-0.166***	-0.166***	-0.166***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed	-0.009	-	-	-	-	-	-	-
Half Informed	-	-0.005	-	-0.005	0.001	0.001	0.001	0.002
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	-	-0.009	-	-0.009	-0.000	0.001	0.000	0.001
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For-Profit Seller	-	-	0.036***	0.036***	0.044***	0.046***	0.046***	-0.036
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Advertising	-	-	-	-	0.096***	0.106***	0.104***	0.113***
	-	-	-	-	(0.01)	(0.02)	(0.02)	(0.01)
Constant	0.273***	0.271***	0.162***	0.253***	0.201***	0.217***	0.200***	0.194***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1359	1359	1359	1359	1359	1359	1359	1359
R^2	0.235	0.235	0.014	0.249	0.273	0.281	0.289	0.468

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, $^{***}p < 0.01$

Table C.8: Determinants of Selected Level of Misrepresentation if Pilot Excluded

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Misrepresentation	-4.989***	-	-5.096***	-5.109***	-5.192***	-5.232***	-6.157***
	(0.14)	-	(0.14)	(0.14)	(0.14)	(0.14)	(0.16)
Sophisticated	-	-0.552***	-0.618***	-0.620***	-0.629***	-0.634***	-0.746***
	-	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Informed	-	-0.023	-0.026	-0.026	-0.026	-0.026	-0.031
	-	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Seller Distribution	-	-	-	-	-	-	-
Half For-Profit	-	-	-	0.126**	0.128**	0.129**	0.152**
	-	-	-	(0.05)	(0.05)	(0.05)	(0.05)
All For-Profit	-	-	-	0.261***	0.264***	0.267***	0.313***
	-	-	-	(0.05)	(0.05)	(0.05)	(0.05)
Constant	1.505***	0.753***	1.864***	1.739***	1.382***	1.321***	1.158***
	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.07)	(0.13)
Round FE	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes
N	14040	14040	14040	14040	14040	14040	14040
Pseudo-R^2	0.080	0.134	0.095	0.097	0.108	0.113	0.225

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Determinants of Buyers' Decisions without Pilot

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.103***	-	-	-	-	-	-	-
	(0.02)	-	-	-	-	-	-	-
Half Sophisticated	-	-0.055***	-	-0.055***	-0.024	-0.024	-0.024	-0.023
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-	-0.103***	-	-0.103***	-0.035	-0.035*	-0.035*	-0.034*
	-	(0.02)	-	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Informed	-0.099***	-	-	-	-	-	-	-
	(0.02)	-	-	-	-	-	-	-
Half Informed	-	-0.063***	-	-0.063***	-0.061***	-0.061***	-0.061***	-0.061***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	-	-0.099***	-	-0.099***	-0.096***	-0.096***	-0.096***	-0.096***
	-	(0.02)	-	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
For-Profit Seller	-	-	-0.069***	-0.069***	-0.087***	-0.088***	-0.088***	0.099
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)
Misrepresentation	-	-	-	-	0.393***	0.396***	0.393***	0.402***
	-	-	-	-	(0.05)	(0.05)	(0.05)	(0.05)
Constant	0.499***	0.505***	0.432***	0.539***	0.441***	0.354***	0.343***	0.330***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1503	1503	1503	1503	1503	1503	1503	1503

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.10: Determinants of Selected Level of Advertising with Bootstrapped Standard Errors (200 Replications)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sophisticated	-0.172***	-	-	-	-	-	-	-
	(0.01)	-	-	-	-	-	-	-
Half Sophisticated	-	-0.080***	-	-0.080***	-0.073***	-0.073***	-0.073***	-0.073***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Sophisticated	-	-0.172***	-	-0.172***	-0.160***	-0.159***	-0.160***	-0.159***
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Informed	-0.008	-	-	-	-	-	-	-
	(0.01)	-	-	-	-	-	-	-
Half Informed	-	-0.004	-	-0.004	0.004	0.004	0.004	0.004
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
All Informed	-	-0.008	-	-0.008	0.004	0.005	0.004	0.005
	-	(0.01)	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
For-Profit Firm	-	-	0.046***	0.046***	0.054***	0.055***	0.055***	-0.037
	-	-	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Advertising	-	-	-	-	0.118***	0.128***	0.125***	0.127***
	-	-	-	-	(0.01)	(0.01)	(0.02)	(0.02)
Constant	0.275***	0.272***	0.162***	0.250***	0.186***	0.200***	0.240***	0.232***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Round FE	No	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	No	Yes
N	1503	1503	1503	1503	1503	1503	1503	1503

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.11: Determinants of Selected Level of Misrepresentation with Bootstrapped Standard Errors (200 Replications)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Misrepresentation	-5.074***	-	-5.182***	-5.203***	-5.291***	-5.333***	-6.225***
	(0.14)	-	(0.14)	(0.15)	(0.13)	(0.13)	(0.17)
Sophisticated	-	-0.551***	-0.619***	-0.621***	-0.632***	-0.636***	-0.743***
	-	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Informed	-	-0.022	-0.024	-0.024	-0.025	-0.025	-0.029
	-	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
<u>Seller Distribution</u>							
Half For-Profit	-	-	-	0.175***	0.178***	0.180***	0.210***
	-	-	-	(0.05)	(0.04)	(0.04)	(0.05)
No For-Profit	-	-	-	0.319***	0.324***	0.326***	0.381***
	-	-	-	(0.04)	(0.04)	(0.04)	(0.05)
Constant	1.544***	0.770***	1.903***	1.745***	1.367***	1.575***	1.102***
	(0.03)	(0.03)	(0.05)	(0.06)	(0.06)	(0.09)	(0.11)
Round FE	No	No	No	No	Yes	Yes	Yes
Session FE	No	No	No	No	No	Yes	Yes
Individual FE	No	No	No	No	No	No	Yes
N	15480	15480	15480	15480	15480	15480	15480

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.12: Determinants of Buyers' Decisions with Bootstrapped Standard Errors (200 Replications)

Appendix D
Timing and Delays

There were several time constraints (mostly beyond my control) which limited the possibility of running additional experimental sessions or expanding the model further.

The first of these, common to all theses, was the requirement to complete this thesis in a period of under 9 months. While clearly not insurmountable, this did limit the scope of my thesis and restricted the potential for several interesting extensions.

As my model was novel to the limited existing literature, I took time and care when constructing it in an attempt to produce a valid contribution to the literature.

The design of the experiment, which depended heavily upon the formulation of the theoretical model, was hence delayed by the construction of the model and then, being a novel design itself, took further time to design effectively.

In turn, my application for funding was conditional on the design of the experiment being finalised. Due to this, I applied for funding in the Semester 2 round, rather than either of the Semester 1 dates. After submitting my funding request by the due date (31 August 2018), I received a revise and resubmit decision two weeks later (12 September 2018) and finally received approval for funding a week after that (18 September 2018).

My subsequent application for ethics approval was itself dependent on the approval for funding and thus was not submitted until 25 September 2018. A week following my submission, I received another revise and resubmit decision (2 October 2018). This was reviewed two weeks later (in the 16 October 2018 meeting) and I received unconditional approval the following day (17 October 2018). It is important to understand that I was not allowed to do any testing or piloting until I had received that final ethics approval. In fact, I was not permitted to so much as schedule sessions without proof of ethics approval.

As of 17 October 2018, with a thesis due date of 5 November 2018, I had 20 days (inclusive) remaining.

I ran one small (8 person) pilot the following day (18 October 2018), a second pilot the next Monday (22 October 2018) then four full experimental sessions the following day (Tuesday, 23 October 2018).

After this time, with 14 days (inclusive) remaining, it was not feasible to run further experimental sessions.

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