The London School of Economics and Political Science

Identification of Adverse Selection and Moral Hazard: Evidence From a Randomised Experiment in Mongolia

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Declaration

I certify that the thesis I have presented for examination for the MPhil degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

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I can confirm that my thesis was copy edited for conventions of language, spelling and grammar by Parmeet Birk.

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Abstract

Insurance market failures are common in developing countries and one commonly proposed explanation for this is the presence of asymmetric information. In this paper I test for the relative importance of adverse selection and moral hazard for car insurance using a randomised experiment at the largest insurance company in Mongolia, randomly upgrading low coverage buyers to a higher coverage. With this experiment, I find significant ex-ante adverse selection for third party and theft risks, while there is no evidence of ex-post moral hazard for either risk. Moreover, I find no evidence of adverse selection or moral hazard for coverages differing in co-payment rates. I also discuss how certain market features, likely to be perceived as specific to this context, are common in other insurance markets in developing countries, and whether these factors are likely to be driving the results in this paper.

Contents

List of Figures

List of Tables

Identification of Adverse Selection and Moral Hazard: Evidence From a Randomised Experiment in Mongolia

I Introduction

Insurance market inefficiencies are common in poor countries, which are often characterised by thin or even missing formal insurance markets. One potential explanation for this is that informational asymmetries, in particular adverse selection and moral hazard, might be more relevant in developing countries. These informational asymmetries, when more prevalent, can undermine the potential for insurance markets to provide insurance, specially for lower risk individuals (see Rothschild & Stiglitz 1976; Wilson 1977; Pauly 1968; Zeckhauser 1970; Spence & Zeckhauser 1971).

Given the importance of understanding such market failures, a large literature developed through testing for the joint presence of adverse selection and moral hazard by testing whether individuals with higher insurance coverage are also riskier (also called conditional correlation approach, used in Chiappori & Salanié 2000; Dahlby 1983, 1992; Puelz & Snow 1994; Richadeau 1999; Cohen 2005). Yet, adverse selection and moral hazard have considerably different implications in terms of optimal policy and firm behaviour. Firms react to adverse selection by offering menus of policies with higher and lower coverage; while responding to moral hazard by forcing individuals to bear a higher share of costs (Feldstein 1993; Gruber 2006). If significant adverse selection is present in the market, governments may consider making insurance more affordable by, for instance, mandating insurance coverage or offering tax subsidies to insurance providers (Gruber 1994, 2001, 2008); this is certainly not the case where there is moral hazard (see Arrow 1963; Pauly 1968, 1974; Nyman 1999). In this sense, to better understand these insurance market failures and therefore design better policies, it is important to devise tests that distinguish between adverse selection and moral hazard.

This paper presents the results of an experiment implemented at the largest insurance provider in Mongolia, Mongol Daatgal, LLC., aimed at distinguishing between adverse selection and moral hazard for different types of risks covered by a car insurance product. Car insurance makes up an important part of the non-life insurance industry in most countries[1], and is especially relevant for insurance providers in poor countries (for an early review presenting cross country data on insurance markets, see "Insurance in Developing Countries: an Assessment and Review of Developments", UNCTAD 1993); additionally, car accidents are a significant source of risk for individuals in developing countries, as captured by the death rates in road accidents and by data suggesting lower enforcement and financing of policies for road safety (see the Global Status Report on Road Safety 2013, from the World Health Organization)[2]. By looking at Mongolia, I aim to illustrate how informational asymmetries might present themselves in an insurance market with some features similar to those in other developing countries - for instance, providers lacking capacity to design carefully specified contingent pricing, consumers being unaware of, distrustful or inexperienced in insurance, as well as a large degree of informal risk-sharing arrangements[3]. Potentially, these are also explanations for why formal insurance penetration is low in new markets.

More precisely, I look at a context where the typical car insurance product prescribes a coverage against a combination of three different vehicle risks (collision, third party and theft), specifying a co-insurance rate and a premium. For each customer arriving at the firm, the experiment first allows consumers to choose freely among alternative coverages, and then randomly offers free additional coverage for risks (or a lower co-insurance rate) that were not initially purchased. Later on in the experiment, these customers were surveyed on their insurance histories, unclaimed

[1]For instance, for the UK (in 2005) and the US (in 2008) net written premiums account for 26% of net premiums for non-life insurance. Source: https://stats.oecd.org/

[2]In particular, death rates in road accidents amount to 18.3/20.1 per 100,000 in low/middle income countries, while amounting to 8.7 per 100,000 in high income countries, see the Global Status Report on Road Safety, WHO, 2013.

[3]Lack of underwriting expertise and statistical data contributing to bad pricing has been mentioned as issues specific to developing countries in the aforementioned UNCTAD report.

accidents and their experiences with the experimental car insurance.

With this experiment, we can then proceed to test separately for the presence of adverse selection - namely, for whether individuals with higher riskiness tend to self-select into higher coverages - and moral hazard - namely, for whether individuals respond to an increase in coverage by reducing effort and, consequently, becoming more risky. Particularly, the experimental procedure allows me to test for adverse selection by testing for whether customers selecting no coverage against a certain risk but being awarded such coverage by the experiment have lower risk than individuals who have initially chosen to get covered against that risk. If this is the case, it can be concluded that given the incentives to exert effort (after all, both groups of individuals here end up in the same coverage), individuals with higher riskiness tend to buy higher coverage more often. Similarly, to test for moral hazard, I can use the experiment to test for whether there is an increase in riskiness in response to random increases in coverage. If this is the case, we can say that individuals causally respond to upgrades in coverage by having higher riskiness. The above experimental design allows me to distinguish between adverse selection and moral hazard for two of the three risks above (third party and theft).

The tests significantly indicate that there is adverse selection in both third party (TP from here onwards) and theft risks. In particular, holding final contract constant, those who chose TP coverage are 50% more likely to make a claim in TP than those who chose not to get insured against TP but received TP coverage. Similarly, those who self-selected in coverage against theft are around 50% more likely to have incurred theft (over the past three years) than those who opted for insurance excluding theft coverage but ended up covered for theft due to the randomisation. The adverse selection results for TP are robust to using different measures of accidents as well as adding a wide range of controls; on the other hand, the theft results are harder to capture without also using data on history of accidents, primarily due to theft claims being rare.

Overall, there is no robust evidence of moral hazard for either TP or theft. A priori, those who were given coverage upgrades - becoming covered for TP accidents - are naturally more likely to successfully make TP claims simply because they have the right to claim. In this sense, performing the moral hazard test with only claims data would wrongly suggest there is moral hazard simply because such a test would be comparing claims for those who were upgraded in coverage (which are not zero) with claims for those who did not receive upgrades in coverage (which are necessarily zero). To address this, I first look at the sum of claimed and unclaimed accidents - the total number of accidents - as provided by the survey data. Under this measure of riskiness, individuals who were upgraded are as likely to have TP accidents as the individuals who did not get upgrades. A similar result holds for theft. Hence, coverage upgrades do not seem to have a causal impact over the total number of accidents. One might suggest that the firm does not care, however, about unclaimed accidents. To compare upgraded individuals with non-upgraded individuals on the basis of "accidents that would be claimed if they were covered", I eliminate from the previous accident measure (summing claimed and unclaimed accidents) the accidents where insurees did not know the size of the damage, due to the accident being too small to consider. That is, this measure of riskiness considers only claimed and unclaimed accidents for which insurees know the monetary size of the damage, which could arguably capture accidents that insurees would claim. Even when looking at this measure of riskiness, I do not find evidence of moral hazard for either TP accidents or theft.

These results, naturally, could be specific to the context that I am analysing: an experimental product in an insurance market in a developing country where the firm (i) has difficulties writing down contingent prices, (ii) faces relatively little competition, and (iii) provides insurance to individuals who are relatively inexperienced in/distrustful of insurance and often have informal risk-sharing arrangements. Towards the end of the paper, I then offer a discussion on how these particularities of the context might affect the baseline findings.

In particular, it is possible that the sizeable adverse selection result is a consequence of the relative absence of contingent pricing. For instance, age is considered as an important risk factor and employed in pricing for a wide range of insurance products by providers in rich economies. While Mongol Daatgal, LLC., can observe age (among other potential risk factors), it does not adjust individual premiums according to the driver's age. In our context, older insurees both are less risky and demand less insurance for third party risks, therefore the firm's introduction of higher prices for young drivers can potentially weaken adverse selection. If that is the case, then adverse selection is not driven fully by presence of private information, but through the firm's "bad" pricing. Yet, I show that the adverse selection results are largely unchanged once all observables, recorded in the contracts but ignored in the firm's pricing model, are controlled for. Moreover, while some of these variables are significantly correlated with accidents, they still have relatively little predictive power over them. This suggests that either the firm is not collecting enough information about individuals or that despite further data collection efforts, there is still a large unobservable component in individual's inherent riskiness. While it might still be that the introduction of contingent pricing would change the reduced form coefficients (and that to fully answer this question, I would need better knowledge of the structure of demand), overall, the above exercises suggest that for individuals with given observable characteristics, I can still significantly detect adverse selection. This suggests that implementing pricing contingent on currently available individual- or vehicle-specific variables might not go a long way in eliminating problems of adverse selection. This is consistent with recent literature in insurance markets suggesting that improving pricing brings only modest welfare gains (see Einav et al. 2010a, Bundorf et al. 2008; for a review, Levin 2001, Einav et al. 2010c).

Similarly, I consider the possibility that the sizeable adverse selection is present partially due to the market under consideration being relatively uncompetitive, with few large firms and many consumers claiming to not having searched around for alternative providers. I argue that while statistics on market shares suggest less than perfect competition, firms may actually be facing differential competition for different types of coverages offered , even within the same car insurance product. In particular, the firm under consideration seems to face higher price competition for more basic coverages, as it is making losses on these, and has a considerable profit margin for its more comprehensive coverages. Since I find adverse selection both for unprofitable and profitable products, it seems unlikely that results are driven fully by competition. I also argue that even if adverse selection is more likely to be detectable in competitive environments, this is even more relevant to the present study, as failures at the market level due to asymmetric information are likely to be exacerbated by competition (Rothschild & Stiglitz 1976). Finally, I present preliminary exercises to check whether within Mongol Daatgal, LLC., consumers display differential adverse selection depending on their search behaviour. While this is only suggestive and does not imply any causal relationship, it seems that there are no significant differences between these consumers.

Finally, in our setting, the survey data suggests that a considerable group of individuals did not get payouts for claims, would not recommend their current insurance product to their family, and broadly, when asked so, answer that they do not trust the insurance company. With this in mind, I check whether baseline adverse selection and moral hazard interact with individuals' degree of distrust in insurance, measured as an indicator of whether individuals either claim to not trust the company, claim they would not recommend the insurance product to their family, or claim they have not received any payout after making a claim. One might expect that those who trust that insurance pays out as expected might display higher adverse selection than those who distrust insurance, basing their coverage choices more on inherent risk and less on factors related to trust. One might also expect that our measure of moral hazard might underestimate the true extent of moral hazard, if distrustful buyers are present, who are less likely to respond to random upgrades to insurance coverage. Overall, for adverse selection, it seems that those who distrust are both less likely to make claims in case of an accident and adversely select more. As for moral hazard, at least for the co-insurance experiment, it seems that those who are more trusting are more likely to respond to coverage upgrades than those who are less trusting. These results are not robust to all measures of risk or all types of randomisations in this paper, therefore should be taken with care. Moreover, trust is endogenous and likely to depend on both insurance choice and risk, so these results are only suggestive in terms of understanding whether a particular channel through which trust may affect our results are central to baseline results.

Some suggestive evidence can also be provided to understand the moral hazard results. In particular, it might be that I do not find any evidence of moral hazard if accidents or claims are not capturing lack of effort/negligence with enough precision. Overall, I look at different vehicle risks and a priori it is hard to come up with specific preventative activities that productively reduce probability of incurring particular vehicle risks. One could argue that both third party and theft can be reduced by parking the car in more secure locations or installing anti-theft system or other types of systems. Since I can observe these behaviours from the survey data, I run the moral hazard regressions taking as the dependent variable a dummy for preventative care, instead of accidents. In fact, even for this alternative measure of effort, individuals do not seem to display moral hazard, so that results (or lack thereof) are not that likely to be driven by taking accidents as a measure of riskiness.

Also, the moral hazard result might be insignificant due to the significant presence of informal insurance in this market. In fact, in the survey, insurees reported they have financed through informal means around 30% of accidents that were at least partially covered with formal insurance; and around 50% of all accidents in the past three years are financed through other drivers, friends and family. In such a case, it might be that additional coverage by the company is only replacing informal insurance, and we then do not observe moral hazard. To check whether that is the case, I examine whether upgrades in coverage reduce the amount of informal financing for risks an individuals incurs. At least for third party coverage this seems to be the case, while for upgrade in the co-insurance rate (from 10% to 0%) or upgrade to theft coverage this channel does not seem to be central. I also show that the extent to which an individual had access to informal insurance seems to reduce claims through the insurance company. Finally, it seems that those who had access to informal insurance are not systematically different in their adverse selection test statistic from those who did not have access to informal means to insure against losses.

My research is related to a large empirical literature dedicated towards understanding the role of asymmetric information in explaining insurance market inefficiencies (for a comprehensive review, see Einav et al. 2010b). This work owes much to the efforts of Chiappori & Salanié (1997), who describe a set of positive correlation tests for asymmetric information. In competitive markets, a significant positive correlation between coverage and ex-post risk, conditional on all information used by the firm in pricing, would indicate presence of asymmetric information: either consumers have prior information about their exposure to risk (adverse selection), or insurees with higher coverage take less care (moral hazard), or both. In vehicle insurance markets existing empirical results using this conditional correlation approach are mixed: some find evidence of asymmetric information (Dahlby, 1983, 1992; Puelz & Snow, 1994; Richaudeau, 1999; Cohen, 2005; Kim et al, 2009), while others do not (Chiappori & Salanié, 2000; Dionne et al, 2001, 2004).

While positive correlation tests provide many valuable insights into the presence of asymmetric information in many markets, as mentioned before, they do not allow for differentiation between adverse selection and moral hazard. Clearly, for a researcher evaluating effectiveness of some policies, such as whether to make coverage more or less comprehensive, such distinction is necessary. On this, Abbring et al. (2003) exploit panel data on insurance choices and claims to distinguish moral hazard from dynamic selection on unobservables. Under moral hazard, experience rating implies negative occurence dependence, since past claims increase the cost of filing for an additional claim. With this methodology they find no evidence of moral hazard. A second set of papers looks at experimental variation in "actual" versus "self-selected" contracts, for instance Karlan & Zinman (2009) in consumer credit, Finkelstein et al. (2012) in health insurance, Gunnsteinsson (2014) in crop insurance. Methodologically, I use an identification strategy which is very similar to the one in Karlan and Zinman (2009), using the experiment to randomly create a wedge between self-selected insurance contracts versus upgraded actual contracts. The only difference is that while Karlan and Zinman (2009) use randomisation in interest rates (or, prices) - which, in an insurance context, would imply that the moral hazard test statistic could be due to an income effect - I use a randomisation in the amount of coverage.

Finally, while there is a sizeable literature on tests and structural models of asymmetric information in car insurance markets in developed countries (again, see Chiappori and Salanie 2000; Dionne et al. 2001, 2006; Cohen 2005; Kim et al. 2009), there is very little evidence on relevance of asymmetric information in such insurance markets in developing countries, and instead, most of the literature on insurance markets in developing countries focuses on crop insurance and catastrophic risk insurance. However, life, property and health insurance, as mentioned above, are important branches of insurance both for insurance providers (since they account for a significant share of revenues) and individuals in such countries (since they account for frequent and relevant risks faced by them). This paper hopes to contribute to this literature by, first of all, showing evidence on how car insurance markets operate in one developing country, and showing suggestive evidence on which factors peculiar to developing countries could be making the results distinguishable from their counterparts in developed countries. Secondly, this paper does so using a randomised experiment, while not altering the setting, i.e. how the firm operates, how consumers choose between products, and their interactions, which provides an interesting background within which to study issues related to asymmetric information.

The rest of the paper proceeds as follows. Section II describes the institutional background with an emphasis on the informational environment at the firm and the market level and how these features might potentially affect experimental design, interpretation of the results and the external validity. Section III presents experimental design, its implementation, followed by detailed description of administrative and survey data. Here I also discuss the identification strategy. Section IV shows empirical results. Section V presents further robustness checks and discusses the results against the features discussed in Section II. Section VI concludes.

II Context and menu design

I collaborated with the largest insurance firm in Mongolia to implement a randomised experiment on its first-party car insurance product, which only insures against losses to the policyholder's vehicle and therefore excludes third party liability cover. In particular, a typical insurance contract is characterised by a combination of vehicle risks (collision, third party, theft among others), a co-insurance rate and a premium, while the firm set the deductibles at zero for all coverages.

I first present background information relevant to the research design and interpretation of the results. In particular, I briefly discuss the market overview, followed by different distribution channels through which car insurance is sold in Mongolia. I then move on to describing technological and human capital constraints many insurance providers face in Mongolia, preventing costless information sharing and data collection within and between firms, which is often necessary for actuaries to price optimally the insurance products, with the implication that multi-dimensional screening tools often available for insurance providers in rich economies, such as experience rating and reliable longitudinal data needed to price heavy-tailed or rare risks, are not employed at all by insurance providers in Mongolia. Finally, I present the experimental car insurance (product) design.

A Market overview and sales channels

Formal insurance is a relatively new concept in Mongolia, with the only insurance company at the time, Mongol Daatgal, LLC., being privatised in 2003. Despite a large growth in insurance markets (as well as the range of insurance products offered) since then, even in the capital of Mongolia, Ulaanbaatar, car insurance markets are relatively small with only an estimated 10% of vehicles insured in 2011[4]. Still, car insurance is seen as the most popular private insurance by policymakers and insurance providers, with the latter earning most of the revenue through car insurance sales. It should be noted that the experiment was implemented a little less than a year after third party liability (TPL) insurance, which is separate from the car insurance product I consider, became compulsory for all drivers in Mongolia in 2012.

I collaborated with Mongol Daatgal, LLC. (MD from now on), the largest insurance company in Mongolia, earning more than 30% of the market revenue for car insurance since its inception. While the randomisation was implemented in Ulaanbaatar, where 16 other insurance companies operate to sell car insurance along other insurance lines, the market does not seem to be perfectly competitive. For instance, at least for car insurance, the same five insurance companies have been consistently earning around 80% of the total revenue in the period 2009-2012[5]. At the time of the experiment, pricing and further information about insurance products were not readily available online, and often buyers had to visit the branches to find out such information. Finally, among the participants of the survey that I discuss later on, only around 30% report of having compared the product to those from alternative providers.

By law an insurance policy only begins after the buyer physically signs the contract, with the implication that insurance cannot be sold online and therefore all contracts are paper-based. There are three different suppliers of insurance, often operating in entirely different markets: 1) branch managers and agents, 2) brokers, and 3) banks. Branch managers sell insurance at the different branches across the country and receive a fixed salary with a bonus of 1.8% on all generated income, while agents often sell off-site, for instance at gas stations, fairs, vehicle registration offices et cetera. They have no salary and receive a bonus of 15% on generated

[4]Source: 2011 report by the Financial Regulatory Commission of Mongolia; www.frc.mn. The statistics were published just before Third-Party Liability (TPL) insurance became compulsory for all drivers, with the relevant law enforcement commencing in 2012.

[5]The statistics on yearly revenues at the firm level for different insurance types are also available on www.frc.mn.

income (see Appendix B for more details on sources of income for different branch sellers). Since managers and agents often operated and were compensated differently, in turn we might expect the characteristics of insurees to differ depending on who they bought the contract from. Furthermore, I implement the experiment only at the branch level, excluding broker- and bank-sold contracts, as well as corporate contracts. Below I discuss the reasons for focusing only on the branches.

In 2011 banks were permitted to sell car insurance, creating a “parallel” market with the difference that bank insurees are also borrowers (insuring their collateral), with rare cases of non-borrowers buying insurance through banks. Both banks and brokers contract non-exclusively with insurance firms, so those who are buying insurance through banks or brokers typically face a larger choice set and lower prices compared to branch-level insurees. Although most vehicle insurance contracts are sold through banks (which we found out only at the end of the project due to lack of reliable baseline data), at least for first-party vehicle insurance, I decided against implementing the experiment with them for two reasons. Firstly, bank employees are likely to direct insurees toward buying a particular (typically higher) coverage, though this may not be as systematic, depending on bank-insurer agreements and incentives for the bank employee in securing collateral, with the implication that buyers do not necessarily self-select coverage based on their risk profile or preferences. Secondly, MD was not willing to design an identical insurance product for banks in order to remain more competitive, so insurance product offered through banks had a different design and was typically cheaper. Finally, with both banks and brokers, there were concerns on the extent to which the randomisation process would be correctly implemented and monitored reliably.

Table I.1 shows that these different types of contracts (with varying product designs) typically attract different sets of buyers. In particular, the table shows average revenue as well as the claim probability and average claim (in case of accident) for each type of contract[6]. Bank insurees, typically the bank’s borrowers, have a much

[6]I focus here on all car insurance contracts sold in Ulaanbaatar during July 2013-July 2014, to

lower claim probability compared to insurees in the experiment (branch-level)[7]. This is suggestive evidence of presence of adverse selection in first-party car insurance, given that branch-level insurees are likely to demand insurance on a voluntary basis, while bank insurees are likely to be forced to buy insurance, therefore less likely to self-select based on private information.

Finally, it can be seen from Table I.1 that the firm derives around 85% of the profits (excluding non claim-related expenses) from corporate contracts[8]. Typically, vehicles insured under such contracts are corporate-owned vehicles, and are often more expensive and have multiple users. Since company drivers are often non-liable for any damages, moral hazard is likely to be higher, while adverse selection is expected to be lower, for instance, if there are agency problems with the company employee responsible for buying insurance not having the incentive or knowledge of private information needed to buy suitable insurance coverage, if such private information indeed exists.

These point to the possibility that there can be multiple insurance markets (with very different set of consumers) even locally, for a given firm and a given insurance product, with the implication that naturally empirical tests of asymmetric information are almost always likely to be context-specific, and the interesting question is then to understand whether we can learn more about the underlying market features potentially driving the heterogeneous results, abundant in recent empirical literature.

be consistent with the scope of the experiment. Also, brokers are not present there, as the firm did not contract with any brokers during this period.

[7]It should be noted that claim probability for bank contracts is likely to be biased downwards with the dataset used to construct Table I.1, because as can be seen from Table I.1 most bank contracts were sold in May-June 2014, so for most contracts claims data are still being collected. Restricting to the sample of contracts for which complete claims data are available, i.e. those that were sold until March 2014, claim probabilities change to 0.38, 0.27 and 0.49 for corporate, bank and experiment contracts, respectively. The reported pattern is similar.

[8]According to the firm, operational costs are relatively high, estimated to be around 20-40% of the revenue for first-party car insurance, depending on the coverage type.

B Firm's information set and pricing

The firm has been using the same insurance software for at least a decade, which fails to record data on most individual- and vehicle-specific characteristics, typically used for insurance pricing, for instance, age, gender, vehicle type, engine size et cetera[9]. The software only records where the contract was sold (branch), insurance start and end date, car valuation and total premium paid. The firm claims that IT constraints, in combination with constraints in hiring good programmers and actuaries, prevent system-wide data collection, monitoring and hence implementation of pricing contingent on individual-level characteristics[10].

Before the experiment the firm offered a very complicated design for its first-party car insurance product, where the price could also be adjusted (to some extent) by sellers. Since the insurance software did not record the amount of coverage for sold contracts, for instance, types of risks covered, excess, deductible or co-insurance rate, the firm's actuary typically did not have such information at the pricing stage. The actuary would then calculate market share and fit claims distribution for some arbitrary car value intervals and a rough estimate of "amount of coverage" proxied by the ratio of premium paid to car value, and used these to set the premium as a percentage of car value for a given coverage level[11]. In particular, assuming perfectly inelastic demand (hence assuming no effect of prices on the risk pool), the premium for each coverage was set to cover claims with 95% confidence level plus some profit margin. For many other dimensions of the contract, such as deductibles and co-insurance rates, the rate was set arbitrarily based on "experience" of the claims department as well as competitors' products. With this, it can be seen that capacity to screen consumers is quite limited, or at least it is not straightforward to infer the intention to screen from the firm's pricing strategy prior to the experiment.

[9] The firm continued to use the same insurance software until the end of our experiment. In 2013 it started developing a new insurance program, which is still at development stage as of April 2015.

[10] As of 2009 there were no qualified actuaries in Mongolia from internationally recognised institutes, with only 9 actuaries permitted by the Financial Regulatory Commission of Mongolia to practice actuarial science in the whole country. Actuarial or related courses only started in 2009.

[11] The firm tried to collect a sample of claims and contracts to check exact coverage and claim type in 2012, but the sample was small and non-random.

Besides aforementioned lack of contingent pricing, to the best of my knowledge, MD did not employ experience rating for almost any type of insurance, including first-party car insurance, since (i) claim history was only centralised for TPL insurance in 2012, and (ii) the firm does not systematically record past claims for its own insurees. As for other insurance providers, at the time of introducing the experiment no firm had a bonus malus system[12].

The constraints described above are also present to a large extent for other insurance providers in Mongolia, and therefore have several important implications at the market level. Firstly, incomplete, possibly even biased, data are used to price insurance products, including the experimental car insurance product. Secondly, prices reflect risk and demand only to the extent that car values explain risk and demand. I show that for the experimental product, car value is positively correlated with demand for higher coverage, but does not reflect significantly any of the risk measures we use, such as number of claims or claim probability. From discussions with sellers, they never refused insurance or priced based on risk factor(s) other than car value and have had neither the financial incentive nor an explicit instruction from the management to do so, further reinforcing the evidence towards lack of risk-contingent pricing.

C Menu design

First-party vehicle insurance only covers damages to own (insured) vehicle. As mentioned in part B of this section, before the experiment the firm offered a complicated product design with some flexibility for sellers to adjust prices. Moreover, also prior to the experiment MD had been separately working on a new design, with an aim to simplify the design (claiming this is likely to attract those who have little experience with formal insurance), and we keep this new design for the experiment (see Figure II.1).

[12]Some type of bonus malus scheme, not in its usual sense, exists at MD. See Appendix C for more details.

I provide here the firm's definition of each risk label in the experimental product design. Although all labels and definitions were literally translated from Mongolian, the meanings are kept. "Vehicle-related accident" is any loss to the insured vehicle caused by an unintentional action or inaction of the policyholder, other non-excluded drivers or a third party. Typical examples are collision, hitting a garage post and being hit by a stone, a more likely risk near construction sites or on dirt roads. "Natural disaster" includes any other acts of God, such as thunder and hail. "Third-party intentional action" is any loss to the insured vehicle intentionally caused by a third party without the policyholders' or other non-excluded drivers' permission, for instance, when a vehicle is scratched or drawn on, hit-and-run et cetera. Initially, the firm intended to create an insurance line against common risks, for which subrogation is unlikely, and ended up classifying it as "Third-party intentional action"[13]. Therefore, at least to some extent, the product design can reflect the likelihood of recovering losses. Meanwhile, drivers' personal accident insurance pays out up to 5 million Mongolian Tugriks (MNT), or around £1667, for 70% or higher loss of employability or death, as a result of a car accident. There is a separate personal accident cover offered by the firm, with options to increase protection level.

Figure II.1 describes the different plans available in the experimental product design (for simplicity I only provided labels of the most common risks). In particular, the most basic coverage covers against both "Vehicle-related accident" and "Natural disaster", while middle coverage covers additionally against "Third party intentional damage" and "Water leakage in the garage". The highest coverage is comprehensive, additionally covering against "Theft/robbery", "Fire/explosion" and "Driver's personal accident". Since insurees only ended up claiming in collision, third party and theft, labels in the figure reflect this, for simplicity. Amount of coverage is therefore determined by a bundle of risks and a co-insurance rate (also on the product design), which is the fraction of losses financed by the insuree. Premium rate is

[13]Subrogation allows the firm to attempt to recover costs from the faulty party. This cannot be done, for instance, if the faulty party cannot be traced (hit-and-run accident).

Car risks	Low	Middle		High	
Risk 1 (collision etc.)	√	√		√	
Risk 2 (third party etc.)		√		√	
Risk 3 (theft etc.)				√	
PRICING					
Premium (% of vehicle value)	0.8%	1.2%	2.0%	3.0%	3.8%
Co-insurance rate	10%	10%	0%	10%	0%

Figure II.1: Experimental car insurance product design

expressed as a percentage of car value, therefore final price depends on both the car value and the amount of coverage. To illustrate, for instance, product 1 covers 90% of the losses due to “Vehicle-related accident” and “Natural disaster” at a premium of 0.8% of the car value. Finally, I do not allow sellers to flexibly set prices during the experiment in order to keep the same menu design for all consumers.

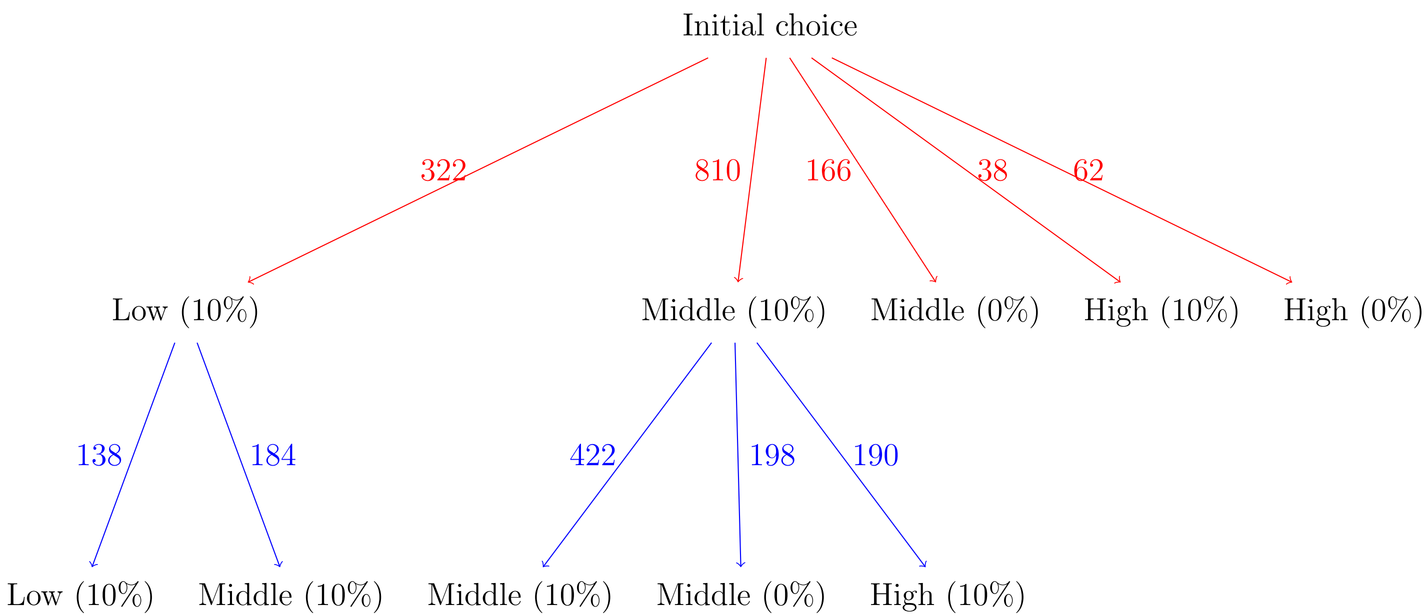

Figure II.2: This figure shows initial selection by buyers into different coverages (red lines), followed by the randomisation assigning some buyers to the same coverage or a higher coverage (blue lines). The numbers along the lines indicate number of contracts sold.

III Methodology

A Experimental design and implementation

In order to identify adverse selection and moral hazard separately, we offer insurance against an additional source of risk to randomly selected buyers, at no additional cost. In this way, we implemented three types of randomisations. A random subset of buyers who self-selected into "Low" coverage are assigned to "Middle" coverage, keeping the same level of co-insurance rate (10%). Similarly, a random subset of buyers, who self-selected into "Middle" coverage, are upgraded to "High" coverage, again keeping the co-insurance rate constant (10%). Finally, "Middle" coverage insurees with 10% co-insurance rate are upgraded to the same type of coverage but without co-insurance rate. The choices and subsequent assignments are shown in Figure II.2.

The experiment was implemented at MD for exactly one year (07/2013-07/2014). The randomisation was implemented at all seven branches of the firm in Ulaanbaatar, employing 51 insurance managers and 12 active agents. The last experimental contract was sold in July 2014, so claims are still being tracked for some contracts. Therefore, in all regressions I use 11 months' worth of claims data for each one-year contract on average. Early cancellations are allowed only if there had been no claims, and to the best of my knowledge, almost all buyers bought one-year insurance and have not cancelled before the contract ended, with the implication that we do not need to control for length of the contract, but length of time for which the contract is valid until the claims data is collected[14].

Since all contracts have to be paper-based and monitoring is costly, especially for agents who sell outside the branches, there were concerns that sellers might try to find out the randomisation outcome before finalising the contract. For example,

[14]The financial department is responsible for the cancellations. In future, it might be possible to check this more closely by merging data sets from the financial and sales departments. Due to some non-unique contract "identifiers" and lack of description against transactions, the merged data may still be inaccurate.

if sellers can easily find out which contracts could be upgraded for free and sell them to friends or family, who might be riskier, then detection of adverse selection and moral hazard can be driven by sellers' fraud, instead of underlying asymmetric information. To better monitor sellers and therefore reduce the extent to which this can happen, the randomisation was implemented through a mobile messaging software, with sellers and buyers sending messages to the server to find out the randomisation outcome (with some specific information about the contract). In this way I can see if sellers found out the randomisation outcome before sales went through, by for example, merging with the transactions data. Also, it is easy to check if a buyer (or another seller) is colluding with the seller by sending messages on multiple contracts. I complemented the program with multiple training sessions and a seller's manual on how to finalise contracts with the interactive software in place (see Appendix E for an outline of the instructions sent out to the sellers).

B Data description

To better monitor the process, the company sent out 2565 bills - company papers on which contracts can be written - to the branches and at the end of July 2014 I managed to track and physically recover almost all of the bills[15]. 1434 were valid contracts, the rest were either invalidated or unused. For the invalidated and unused bills only 39 mobile messages were sent out, so the extent to which sellers attempted to find out the randomisation outcome before the contract is finalised is likely to be low. The messaging scheme also allowed me to detect 35 contracts suspected of biasing the randomisation, so these were eliminated from all regressions, though for almost all regressions coefficient estimates do not change significantly, once they are included[16].

[15]As mentioned before, all contracts have to be paper-based. Branches request a certain number of bills and they distribute the bills according to an ordered numeric bill identifier. I needed to track all bills to include contracts for which messages were not sent, in case there were any. The fact that 1) within the branch distribution of bills is random, 2) the firm's insurance software does not track the bills and 3) difficulties in enforcing data entry, quality of which can be monitored, made this process very challenging.

[16]35 contracts with one or combination of following features: 1) messages were sent at least one day before the contract has started, 2) the randomisation was incorrectly done, and 3) successful upgrades after unsuccessful attempts discovered for some of the above 39 invalid or unused contracts.

I present summary statistics by chosen coverage in Table III.1. Balance tables to check the validity of the randomisation are presented in Appendix VI. Variables used in these tables are contract-specific variables: insuree gender, age (in years) and residence dummies; vehicle age (in years), value (in Mongolian Tugriks MNT)[17], brand dummies, a dummy for whether vehicle is of a lighter colour. I also collected seller-specific characteristics - gender, age, length of employment at MD, a dummy for whether the seller is an agent or a manager - as well as when the contract started, the number of days that the contract has been valid. Table VI.1 shows the differences between those who chose the lowest coverage and were assigned to middle coverage (at 10% co-insurance rate) *versus* those who were kept in the lowest coverage. Similarly, Table VI.2 presents the relevant results for those who chose middle coverage and were assigned to the highest coverage (at 10% co-insurance rate) *versus* those who were kept in middle coverage. Finally, Table VI.3 shows the relevant results for the co-insurance experiment. Overall, on 18 out of 21 pre-randomisation outcomes there is no significant difference between those who were upgraded to a higher coverage versus those who were kept in low coverage. Outcomes on "Days insured" and whether seller is an agent or manager, *seller agent*, enter significantly in two of the three balance tables. In particular, it seems contracts with upgrades were more likely to be sold earlier on in the project by managers, compared to contracts without upgrades. This can be fully explained by distribution of bills, which is random across sellers and branches. In particular, sales are seasonal, with more sales earlier on in the project, and branches with high volume sales (and managers rather than agents) ended up being (randomly) allocated more contracts with upgrades.

1 Administrative data

Administrative data include around 1434 valid contracts matched with 616 claims materials. Contracts have a standard format, while claims materials do not, ranging from five to forty pages, so we identified common documents, such as insurance

[17] 1 GBP ≈ 3000 MNT according to www.mongolbank.mn as of February 2015.

hotline reports, claims application forms and claims department reports. For incomplete claims materials I filled in the gaps using police reports and discussions with claims managers and hotline employees.

All contracts and claims materials are handwritten, and since the company did not record most of the data, I took photos and manually entered the data[18]. In addition to the variables used to construct the balance tables, I also collected data on the amount of coverage - risks covered and co-insurance rate - as well as whether signature of the buyer and the seller were present and whether the upgrade information was correctly entered on the contract. From the claims materials I collected a vast set of variables, including accident type, car value at the time of the accident, claim size, payout, evaluator dummies. Finally, I collected seller-specific characteristics from human resources records at MD.

2 Survey data

At the end of sales, during August-September 2014, I hired six enumerators to contact by phone all insurees, whose contracts indicated residence in Ulaanbaatar and who were not foreigners, to participate in a survey aimed at understanding insurance markets in Mongolia[19]. Enumerators were not given in advance any information about the insuree, except full name, telephone number and home address. Few measures were taken to achieve a high response rate and to reduce the extent to which respondents hid their risky behaviour. At the same time it was important to explain how we obtained their personal information. In our initial calls we informed the interviewees that their contact information was shared by MD as part of a collaborative work to evaluate and improve car insurance market inefficiencies in Ulaanbaatar and while participation is voluntary, this research can be important for policy analysis. They were told that we could meet them wherever and whenever is

[18]Quality of data entry was checked by comparing entered data on some of the variables against administrative data from firm's insurance and claims programs. I also checked a random sample of the entered data against the original contracts, and during these checks no entry errors were found.

[19]43 insurees stated on the contract their residence is outside of Ulaanbaatar and 25 were not Mongolian.

convenient for them and that the interview is expected to take around 30 minutes. Finally, we told them that interviews were part of a larger research, funded externally and undertaken by an independent researcher, and data obtained during the interviews will not be shared with third parties, including MD, and will not be used outside the scope of this research. In the end, we interviewed 553 insurees.

To ensure quality and monitor enumerators, the interviews were conducted using tablets and audio recorded. Before the interview started, enumerators were instructed to read information on the broad research purpose, while addressing privacy concerns, and to ask participants again for their permission to be interviewed. Once we managed to meet the respondent, there was only one case in which the respondent refused to continue with the survey, in all other cases the interviews were completed within one meeting[20]. As for the extent to which risky behaviour was captured by the interviews, a total of around 1200 accidents and detailed information about these accidents were recorded as part of a three-year accident history. Most respondents seemed unconcerned to share such information, and the number of reported accidents suggest that revealing this information did not seem to be a major issue.

The interviewers each ran on average four interviews a day, lasting 45 minutes on average, with the time in between spent repeatedly contacting insurees to schedule interviews[21]. For 21 contracts we interviewed a household member, instead of the insuree. Out of the 21 contracts, 20 drove the insured vehicle and 16 bought the experimental car insurance together with the insuree, with the implication that we can be confident of having interviewed the right person and even if this was not the

[20]Sometimes it proved hard to track the potential interviewee even after obtaining their permission on the phone. Often when enumerators got to the place of meeting, respondents' phones were turned off. We did not take any systematic measures to reduce the incidence of this, except try to confirm the meeting few hours prior to the interview and often ended up showing up at the agreed location, without such confirmation.

[21]Many interviews ended up lasting longer than expected. This is mostly due to respondents reporting a higher number of accidents than we expected, resulting in interviews taking longer to record all the details for each accident. Also, other factors could have contributed. We often met the respondents at their work place, rather than at their homes, where they could be more distracted. More careful respondents often took longer time to respond to detailed questions.

case, the respondent was in a position to answer most of the interview questions. However, it should be noted that even when we interviewed the right person, we realised that sometimes the respondent was not the main driver or even a driver for the insured vehicle (there was only one such case), with the implication that answers to some questions on driving behaviour or vehicle usage present an incomplete picture about true vehicle usage, which could have entered as an important factor in purchasing insurance. With this, we should, to the best of our ability, incorporate information about other drivers of the vehicle (from the survey) as well as the insuree. Finally, in 65 cases, the respondents gave information on a different vehicle than the one that they insured with MD, so in these cases some vehicle-related variables are recorded as missing.

Some insurees had more than one vehicle insured, so the total number of contracts for respondents is 574, a response rate of around 42%. Insurees from 340 contracts refused to participate in the survey, primarily due to time constraints. For the remaining 452 contracts, I was either unable to reach the insuree or they could not participate within the survey time frame. Table VII.1 is the balance table for respondents *versus* non-respondents.

We compare survey respondents *versus* non-respondents on a number of characteristics, including 1) the treatment variable "Upgrade", taking value 1 if insuree is upgraded to a higher coverage and 0 otherwise, 2) riskiness, overall and by risk type, and 3) a range of characteristics collected from the administrative data. Since the respondents make up a non-random sample of all insurees in the experiment, we might be concerned that results for survey respondents are not applicable to other insurees. For instance, if predominantly low risk (or low income or those who were upgraded in the experiment) individuals agreed to participate in the survey, adverse selection and moral hazard results are likely to be biased. From the tables we see that there is a good balance on almost all outcomes, treatment and controls likely to be correlated with risky behaviour, suggesting bias of the estimates is likely to be small. Moreover, regressions to identify adverse selection and moral hazard ran for all insurees and for only survey respondents yield similar estimates, even in magnitude[22].

The two variables that enter significantly in the balance tables are insuree age and car value (also see density plots in Figure VII.1). Younger insurees or those with high car values (likely wealthier) are less likely to participate in the survey. Differences among respondents and non-respondents in car value can be shown to be entirely driven by extreme high values, with the significance disappearing when the top 1% of car values are eliminated from the analysis (see Table VII.2). On the other hand, age explains claim probability (one measure of riskiness I use) to some extent, though for age alone R^2 is very low at around 0.013, and the significant difference among respondents versus non-respondents is not explained by extreme values. Wilcoxon rank-sum test, which compares the median age assuming distribution shapes are identical, also yield significant results. However, if I use the Harrell-Davis estimator in conjunction with a percentile bootstrap, proposed by Wilcox et al. (2014), to compare quantiles of age distributions, all tests reject significant differences across quantiles, though at all quantiles age of respondents is higher than that of non-respondents. The results of this are presented in Figure VII.2 and Table VII.3[23]. In summary, overall, survey respondents are older than non-respondents, however, we fail to see significant differences across almost all outcome, treatment and a range of controls, so bias in estimates from using the survey data are expected to be small.

The survey collects detailed information about individual's riskiness, risky behaviour and attitudes toward risk, including but not limited to driving experience, where they park their cars, whether they have anti-theft system, 3-year history of accidents and detailed information about them (both claimed and unclaimed), pur-

[22]Not all results are displayed here, and additional information can be requested.

[23]The column labeled *p.value* shows the p-value for a single quantile bootstrapping test. As we do multiple tests (one for each quantile), the overall Type 1 error (defaulting to .05) is controlled by the Hochberg method. Therefore, for each p-value a critical p-value is calculated (see column labeled *p_crit*). The column *signif* marks all tests which fulfill this condition and as discussed, all tests reject significant differences.

chase of other types of insurance, evaluation of own driving skills, evaluation of different types of risky driving behaviour and risk aversion. In particular, to elicit risk aversion, we asked insurees to consider a (hypothetical) binary lottery choice between a safe option yielding a certain amount and a risky option, in a sequence each time increasing the safe option by a certain amount. Alternatively, we could proxy for risk aversion, using other types of risky behaviour, such as smoking, business ownership, whether they bought other types of insurance in the past, seat belt usage and consumption/saving behaviour. In addition to the above, we also recorded characteristics not captured in the contracts, such as education, property ownership, employment, income, vehicle usage and whether there are other drivers. Finally, we wanted to capture the extent to which insurees distrust formal insurance, by asking them directly about trust, but also indirectly on whether they would recommend formal insurance to friends/family, whether they had any conflicts in the past with insurance companies, whether they got less than what they claimed for et cetera. Firstly, these variables are necessary for moral hazard test and secondly, they could be used to check whether estimates are heterogeneous across different insurees and ultimately try to understand the underlying forces driving this heterogeneity. Means of most characteristics are presented in Table V.1.

Finally, in addition to insuree interviews, in November 2014 I collaborated with the marketing department at the firm to survey each seller and record for each contract the extent to which he/she knew the insuree. We explained to the sellers that the purpose of the survey was to understand the firm's outreach into new markets. Through this I found out that for around 27% of contracts sellers knew the person they sold insurance to. This can be used to understand whether there was any preferential ex-ante or ex-post treatment for friends, which provides an interesting margin for interpretation of adverse selection and moral hazard estimates.

C Identification strategy

1 Adverse selection test

In a context of insurance with adverse selection, we would expect that individuals who choose higher insurance coverages are also riskier. However, choosing higher insurance coverages also means, often, getting higher insurance coverage and facing different incentives to exert effort.

In order to isolate adverse selection, choices of insurance coverage correlate with risk, from moral hazard - the direct effect of coverage over riskiness, we compare claims behaviour of high coverage buyers versus low coverage upgraded buyers, for a sample of individuals who, after the randomisation, end up with the same (high) coverage. Given that these individuals' final coverage is the same, any difference in riskiness across these two groups of individuals is driven by their different incentives to self-select into different products.

More precisely, for each risk $R \in \{$"TP", "Theft"$\}$, we run the following regression:

$$Risk_{i,R} = \alpha_{i,R} + \beta_{0R} Low\ coverage\ choice_i + \gamma_R X_i + \epsilon_{i,R} \tag{1}$$

on the sample of individuals i who ended up in the same contract covering R. In this regression, $Risk_{i,R}$ is an individual i's proxy of riskiness in risk type R (different proxies are discussed in the next section), *Low coverage choice* is a dummy indicating that individual i chose not to be covered for risk i (or chose higher co-insurance rates, depending on the experiment analysed), and X_i is a set of controls, either from the administrative data, or from combining both the administrative data and the survey data. To test for the presence of adverse selection we are interested in testing whether $\beta_{0R} < 0$: if this is the case, individuals who chose not to be covered for risk R but ended up covered for risk R are less risky than individuals who chose to be covered for risk R and remained covered for this risk. Crucially, since the sample used in this regression is just the sample of individuals who have coverage for risk R (or who have a lower co-insurance rate), we know that individuals' incentives are being held fixed, and that if $\beta_{0R} < 0$, this must be because the individual's riskiness correlates with the way he self-selects into different amounts of coverage.

It is useful to further discuss what types of patterns could create $\beta_{0R} \neq 0$: crucially, $\beta_{0R} < 0$ if the individual is privately informed about his inherent riskiness, and this affects the way he chooses insurance products. Secondly, if other factors affecting coverage choice - say, the individual's risk aversion - are correlated with the individual's inherent riskiness, this will also be reflected in β_{0R}. Thirdly, if different individuals respond differently to insurance coverage (that is, if the extent of moral hazard is heterogeneous), and if they anticipate their responses to insurance coverages at the moment of coverage choice (what has been dubbed "selection on moral hazard", see Einav et al. 2013), this might display itself as a non-zero β_{0R}. Finally, given that the experimental procedure can only randomly upgrade coverages without increases in premium rates, the coefficient β_{0R} captures differences in risk between individuals who have same coverages, but pay different premiums. If the premium level affects effort provision and accident prevention through an *income effect* (that is, differences in the premium might affect effort provision by changing risk aversion and hence changing the marginal disutility from a loss), this will be captured in this adverse selection coefficient. It should be noted, however, that we do not expect this effect to be significant: first, the differences in premium payments across products are not large relative to the individual's income, which suggests the variation in marginal utility and risk aversion due to premium changes should not be too large for this experiment.[24] Secondly, given that we do not capture significant levels of moral hazard for overall accidents, it is unlikely that there are significant differences in effort due to differences in premium levels.

In other words, β_{0R} will capture, in reduced form, whether there are any factors

[24]Back-of-the-envelope calculations suggest that if individuals who self-selected in low coverage were to choose higher coverage, the premium difference would be around 0.3-1.7% of their reported yearly income, depending on which coverage they self-selected into. Here I have only considered the possible high coverages due to the randomisation. Only for upgrade to theft coverage, the premium difference goes above 1% of the salary, which the implication that overall income effects are likely to be small.

driving choices of coverage levels that are correlated with riskiness, given individuals' incentives to exert effort to reduce risks is held constant. This is the usual force we are interested in to understand whether adverse selection has the potential to create market failures: namely, whether individuals' incentives to self-select into different products increase the insurance company's costs of providing insurance coverage (see Chiappori et al. 2006, who dub this "relevant asymmetric information", and Finkelstein & Poterba 2004).

Two other issues are worth discussing on this test of adverse selection: firstly, what would happen to this test if individuals anticipated that the randomisation will take place? If individuals do not anticipate the outcome of the randomisation - as the implementation section suggests, a significant amount of effort was spent on guaranteeing this is the case - individuals might be more prone to selecting low coverages if they anticipate that randomisation will happen, with the hope that their coverages will be upgraded. Notice, however, that qualitatively, at the moment of insurance choice, this is equivalent to selling two products: a low coverage product, that covers a risk with some probability lower than 1, and a high coverage product that covers the risk with certainty. This should make the low coverage product more similar to the high coverage product and reduce the power of this experiment to detect adverse selection. Hence, the possibility that individuals are anticipating that the randomisation will happen should, if anything, act to attenuate the results on adverse selection, and push us towards viewing this test as a lower bound on the correlation between insurance choices and riskiness.

Secondly, at first, the results from this test could be due to the relative absence of contingent pricing: in particular, without contingent pricing, it could be that individuals who are riskier along dimensions which are observable to the firm can afford to self-select into a higher coverage; the correlation might weaken if the firm employed contingent pricing thereby charging a higher price for riskier individuals. This would not be adverse selection in its usual sense, since if this was the only force driving the results, there would be no market failure, and all we would need to do is to force the firm to engage in "better" pricing. I discuss this possibility in more detail in section VI.

2 Moral hazard test

Similarly, in the context of an insurance market in which there is moral hazard, we should expect that higher insurance coverage for a given individual should reduce the incentives of an individual to exert effort, and as a consequence, would increase the individual's riskiness. Despite this, individuals with lower insurance coverage typically have chosen lower insurance coverage, in the sense that a correlation between insurance coverage and risk could be driven by selection effects.

Again, I use the experimental design to distinguish between the moral hazard story and the adverse selection story. More precisely, I compare the riskiness of individuals who were randomly selected to receive an upgrade in coverage with the riskiness of individuals who were not granted this upgrade, in a sample of individuals who self-selected into a lower coverage. More precisely, for each risk $R \in \{\text{"TP"}, \text{"Theft"}\}$, I run the following regression:

$$Risk_{i,R} = \tilde{\alpha}_{i,R} + \tilde{\beta}_{0R} Upgrade_i + \tilde{\gamma}_R X_i + \tilde{\epsilon}_{i,R} \quad (2)$$

on the sample of individuals i who self-selected in the same coverage, that did not cover for risk R. Again, $Risk_{i,R}$ and X_i are the same as in the regression identifying adverse selection, and $Upgrade_i$ is a dummy that takes a value of 1 whenever individual i, who self-selected into no-coverage for risk R (or a higher co-insurance rate), became covered for risk R due to the randomisation. Given that this regression is estimated on a sample of individuals who self-selected into the same product but ended up in different products, by testing for $\tilde{\beta}_{0,R} > 0$, I am testing for whether random upgrades in coverage have a causal impact over riskiness - that is, by testing for $\tilde{\beta}_{0R} > 0$, I am testing for the presence of ex-post moral hazard.

We might over-estimate moral hazard if we only focus on claims data, as those who were kept in lower coverage can still incur accidents while not having the right to claim losses through insurance. Hence, by focusing on only claims data, we will trivially find “moral hazard”, since we'll observe zero accidents for uncovered individuals and positive accidents for covered individuals. Yet, this is not the statistic of interest to discuss moral hazard, since moral hazard actually refers to the impact of insurance coverage over the individual's expected accidents that would be claimed if they were covered. That is, to test for moral hazard, we would like to have a risk measure that captures accidents that would be claimed if the individual was covered.

To do this, I run the regressions for moral hazard only on survey respondents, for whom I observe both claimed and unclaimed accidents (and in particular, this is the sample for which I can at least observe the accidents faced by uncovered individuals). I then first consider a measure $Risk_{iR}$ that sums both claimed and unclaimed risks. This might include both risks that the individual would claim, and risks that would never be claimed. To take this possibility into account, I then consider a new measure of $Risk_{i,R}$ that sums claimed and unclaimed accidents, but excludes accidents for which losses are reported by the individual as too small to claim or accidents having a zero loss size[25].

It is useful to discuss what types of economic forces could drive $\tilde{\beta}_{0R} \neq 0$: firstly, if individuals react to insurance coverages by putting in less effort into accident prevention (ex-ante moral hazard), this should translate into $\beta_{0R} > 0$. Secondly, if individuals who have a higher coverage commit fraud more or less often, or if they claim more often than less covered individuals (ex-post moral hazard), this should translate into $\beta_{0R} \neq 0$. Finally, the coefficient $\tilde{\beta}_{0R}$ will capture the average impact of higher coverage over riskiness for individuals self-selecting into low coverage.

[25] Alternatively I can use additional information collected on knowledge about own coverage and eliminate accidents which the consumer believes to covered against but still decide to not claim. Since almost half of low coverage consumers believed they are covered with higher coverage, most unclaimed accidents would be eliminated from this analysis. Results do not change significantly with this exercise.

With this, enough has been established to move to the empirical results.

IV Empirical results

Tests for adverse selection and moral hazard for the three experiments are presented in Appendix IV. I use three measures of riskiness, generically termed as accident frequency, dummy and loss size. To test for adverse selection I use only claims data, except for theft, for which I also use three-year history of accidents, while for moral hazard I use all accidents (during the contract) with positive losses[26].

In particular, for accident frequency (dummy), I run a Poisson (logit) regression and average partial effects are presented using "mfx" package in R[27]. For loss size, I use $log(Loss\ size + 1)$ due to a mass at zero and perform least squares regression with robust standard errors[28]. Finally, if sample size is very small (around 100 or less), I also conduct exact Poisson and logistic regression estimates and standard errors, used often for small sample analysis, as well as usual Poisson and logistic regressions with bootstrapped standard errors[29].

A Adverse selection

Table VIII.1 presents adverse selection results for third party risk using claims during the experiment as the dependent variable. The point estimates in the first row are all negative and statistically significant. When all controls are accounted for, the

[26]If we expect long-run incidence to be a more accurate representation of risk type, we could just run the adverse selection regressions using three-year history of accidents. Given that past accident frequency are highly correlated with accidents during the contract the results are similar. The downside of doing this is that we are only focusing on survey respondents and we do not have administrative data on their past contracts, hence cannot take into account moral hazard, defeating the intention to distinguish how individuals self-select in different contracts based on their riskiness from moral hazard.

[27]Using Vuong's non-nested hypothesis test, negative binomial distribution is a better fit for accident frequency than poisson distribution (without any additional controls). However, if we include the controls typically included in all regressions, this advantage fully disappears.

[28]Alternatively, we can use either compound Poisson-gamma distribution with a positive mass at zero, often employed in the insurance literature to predict claims, or zero-inflated models, however, these should be avoided with small samples. Another possibility is to estimate accident frequency and loss size independently, however sample size of positive losses is too small for credible estimation.

[29]Results are not reported here, but the estimates are almost exactly the same as with Poisson or logit regressions.

point estimates are slightly higher, while precision remains about the same. These suggest that keeping ex-post moral hazard constant, individuals who self-selected into the lowest coverage are significantly less risky in third party risk than individuals who self-selected into the middle coverage. For insurees choosing middle coverage, average third party claim frequency and probability are 0.175 and 0.156, respectively. Then, columns 1-4 show that those who chose low coverage are at least 50% less likely to have a third party accident than those who chose middle coverage. Columns 5-6 show that those who chose lower coverage yield at least 85% lower costs than those who chose higher coverage[30]. These estimates are in magnitude very large, with the implication that adverse selection is both economically and statistically significant. Similar results hold for 3-year third party accident history, reinforcing the evidence for individuals self-selecting into different coverages based on somewhat persistent private information, even if initially we may be doubtful about the presence of private information on third party accidents.

For theft, firstly, Table VIII.2 presents adverse selection results for theft using only claims as the dependent variable. The coefficients on *Low coverage choice* in the first row are positive - and statistically significant in 4 out of 6 regressions - suggesting advantageous selection. Those who chose the highest coverage did not make any claims against theft, while those who chose the middle coverage had only five theft claims, suggesting that it is hard to capture individuals' riskiness in theft with only claims data. To account for this, I run the same regressions only on survey respondents, changing the dependent variable to three-year history of theft accidents. If the coefficients on *Low coverage choice* are negative and significant, then this would be evidence of adverse selection with those who faced higher theft risk over a longer period in the past being more likely to choose coverage against it. The results are presented in Table VIII.3. Average theft frequency and probability for those who self-selected into the higher coverage are 0.342 and 0.263, respectively.

[30]We run a gamma regression of claim size on the sample of insurees who have claimed at least once (a total of 89 claims). While low coverage insurees claim lower amount it is not significant, suggesting that adverse selection results are driven by claim frequency, rather than how much is claimed. From the firm's perspective, this still translates into lower expected costs for low coverage versus middle coverage buyers.

The point estimates suggest that those who self-selected into lower coverage have at least 50% lower probability of theft risk than those who self-selected into higher coverage, at least in the long run. The adverse selection results above, as with third party risk, are both quantitatively and statistically important. These results firstly suggest that especially for a rare event, such as theft, we may only be able to capture adverse selection if we have data on long-run history of theft risks. Secondly, adverse selection is at least partially driven by unclaimed accidents, which are a lot harder to capture without survey data[31].

On the other hand, we do not find any evidence of adverse selection for higher versus lower co-insurance rate (see Table VIII.4). For the regression, I use claims in both collision and third party as the dependent variable, since all insurees in the sample are covered against both risks but only differ in the extent of risk sharing, the co-insurance rate[32]. None of the point estimates in the first row are significant and the results are robust to including controls. This implies that those who chose insurance with no co-insurance rate are not riskier than those who self-selected in insurance with 10% co-insurance rate.

B Moral hazard

I now move on to moral hazard results. Tables VIII.5- VIII.8 present moral hazard results for third party and theft. To reiterate we have to use all accidents during the contract to test for whether higher coverage incentivises insurees to be more negligent. Tables VIII.5 and VIII.7 therefore use all accidents as the dependent variable. The coefficient on *Upgrade* is not significantly different from 0 and do not change with adding controls, implying no evidence of moral hazard, at least for low coverage insurees. However, the more relevant test for moral hazard, at least from the firm's perspective, might be to include only accidents which are likely to be claimed

[31] To account for smaller sample size I also looked at average partial effects with bootstrapped standard errors and exact Poisson/logistic regression estimates. The results are robust to these adjustments.

[32] Using separately claims for each risk yields similar results. In other words, we do not find any evidence of adverse selection for the co-insurance rate experiment for either risk.

by the insuree. In other words, the usual moral hazard test could be rejected if low coverage insurees who were not upgraded reported as many accidents as those who were upgraded, while actual moral hazard could be high if they are also more likely to have "negligible" losses. Yet excluding accidents that were reported to have "negligible" or zero losses yields similar results (see Tables VIII.6 and VIII.8).

Finally, Table VIII.9 presents moral hazard results for the co-insurance experiment. Since all insurees in the sample are allowed to claim for both collision and third party, claims in both can be used to capture moral hazard[33]. The point estimates in the first row on *Upgrade* are not significantly different from zero (they in fact reduce in absolute value after including controls), implying that there is insufficient evidence to support presence of moral hazard. This is surprising given that increasing insuree's liability in risk taking behaviour is seen as an obvious policy to reduce moral hazard, and it seems that at least ex-post moral hazard is not likely to be affected by contract features along these dimensions, though it should be noted that difference in coverage of 10% versus 0% co-payment rate is too little to detect asymmetries in information. Despite this, the claims department at MD was most interested in these results, as they were convinced that setting the co-insurance rate at 10-20% would significantly deter risky insurees from claiming.

V Discussion

Many of these results are naturally specific to the setting I am studying: an insurance firm in Mongolia that (i) uses relatively little contingent pricing, (ii) faces less than perfect competition in the formal insurance markets, (iii) offers insurance to customers who have access to informal insurance and who (iv) have little trust or experience in formal insurance markets. Secondly, the results are from an experiment that estimates moral hazard only for lower coverage buyers. This section,

[33]Doing the same exercise for all accidents during the contract does not change the results. Also, running the regressions separately for collision and third party suggests there is no evidence of moral hazard for either risk.

despite speculative and not offering definitive conclusions, presents some suggestive evidence on the extent to which these factors are relevant in explaining the findings.

A Contingent pricing

The firm uses only car value in screening consumers, though it observes and can potentially use for pricing a wide range of individual and vehicle characteristics. While there may be other settings in which an insurance firm does not use all the information they hold (for example, see Finkelstein & Poterba, 2014), lack of contingent pricing is quite extreme in our setting and therefore can explain the high degree of adverse selection we observe: after all, if the company does not adjust prices according to (observable) individual risk factors, those with observably higher risk are more incentivised to self-select into higher coverages, artificially exacerbating the coverage-risk correlation. In other words, rewarding "good" buyers and punishing "bad" buyers could reduce the extent to which buyers self-select based on risk alone. Potentially, "mispricing" and more generally fewer tools used to screen or incentivise insurees to exert effort can also lead to over-estimation of moral hazard. If detected adverse selection and moral hazard results are a consequence of pricing issues, this would lead to different, potentially more targeted, policy recommendations. In particular, acquiring better skills or tools for pricing can both reduce adverse selection and moral hazard and hence improve market outcomes. The extent to which this would be the case is outside the scope of this research. Given that there is little evidence of moral hazard in our setting, I discuss here some preliminary evidence on why pricing issues are not likely to explain fully the adverse selection findings in this paper.

Firstly, I check whether the adverse selection results change in a significant way after controlling for the observables recorded in the company's administrative data. Since the firm does not take into account almost any of these factors, as discussed previously, the correlation could be driven by individuals self-selecting on risk in an unconstrained way. Then, if adverse selection is largely driven by observable risk factors among low and high coverage consumers, controlling for these factors should considerably reduce the coverage-risk correlation. Still, as can be seen in all even columns of regression tables, adding these controls has very little effect on the magnitude of the adverse selection test statistic.

One could argue that these risk factors potentially have a non-linear effect on risk and choice, and therefore I should adopt a more general approach in taking into account these observables. I look at three related, but somewhat different, measures of riskiness. I do not present all the tables here, but using different distributional assumptions for the above measures (instead of poisson/logit/log normal for accident frequency, dummy or loss size, respectively) or running regressions adding squared terms yield similar results, suggesting functional form is not likely to drive these core results.

Secondly, a related concern might be that while the results are robust to controlling for observables that the firm might potentially want to price upon, the reduced-form results might fail to detect adverse selection once the insurance company actually adopts pricing based on these observables. In that case potential market failures due to asymmetric information can be corrected through better pricing. It is hard to fully eliminate this possibility, but it seems that the observables in the administrative data have very little predictive power over actual risk (see Table IV.1), with R^2 at around 0.025. The firm could potentially try to collect more information on buyers, but for the purpose of this research, we take these as "private information". So, given factors currently observable by the firm, there seems to be little gain in adopting risk-adjusted pricing to start with, and there is still significant residual explaining demand and risk outcomes.

Despite this suggestive evidence, I do not have enough data to evaluate effectiveness of other tools, such as experience rating, that also have not been taken up by firms.

B Imperfect competition from other insurance firms and adverse selection

I have argued that the market being studied seems to be one in which there is less than perfect competition, especially given that only a few firms are consistently capturing most of the market and consumers seem to engage relatively little in search for alternative insurance providers. Despite that, from Table II.1 we can see that the firm is actually making losses on the lowest two coverages, which around 80% of the buyers purchased, and quite a high profit margin on the highest two products[34]. On the other hand, the firm seems to face an increasing proportion of consumers who search for its more comprehensive packages (see Table V.1). These are puzzling, given that in equilibrium we expect to observe a higher price competition on the products for which consumers search more[35].

Overall, in simple models of insurance markets with asymmetric information only on risk type, the correlation test studied here can be seen as a simple implication of incentive compatibility: whenever there is one product covering more than the other, higher risk individuals have a higher willingness to pay for the high coverage product, and hence, the high coverage product will select higher risk insurees. This would not happen without asymmetric information, and at first, such a positive correlation test should be a valid test of the presence of asymmetric information irrespective of the degree of competition in the market. However, Chiappori et al. (2006) argue that detecting a positive correlation between riskiness and coverage under competitive environments, a priori, would not suggest that such a correlation would also be found under imperfectly competitive environments.

Moreover, one could entertain a few arguments on how the competitiveness of the market might actually underestimate the adverse selection test statistic. Firstly, it might be that in more competitive environments, firms would be pushed towards

[34]Note: the measure of profit does not include operational expenses and other indirect costs, so need to be revised to take this into account.

[35]In this setting, riskier individuals both search more and choose higher coverages, which would drive down profitability of higher coverages, further reinforcing the expectation in equilibrium.

engaging more in contingent pricing to become more competitive, which would in turn reduce the extent of adverse selection. This does not seem to be happening here, since firms have not and are not engaging in price discrimination in response to additional information about individual risk. Secondly, in response to higher competition, firms might engage in better cream-skimming/lemon-dropping strategies in a way that the set of consumers at the firm might become more homogeneous. This can in turn reduce adverse selection detected at a given firm. This also does not seem to be the case here, as firms are not explicitly denying insurance or punishing "bad" risks. Finally, if there is multidimensional asymmetric information, say, both in risk aversion and risk type, it is not entirely clear how firms would react to more competition. Overall, this paper does not have much to add on these channels, given the analysis focuses only on one product within one firm at a given point in time.

Whatever is the margin of competition among firms, one potential explanation for finding adverse selection (which only uses data for buyers in the two lowest coverages) could be that the firm might be facing fierce price competition. If that is the case, then any market failures that might ensue are mainly driven by presence of asymmetric information in a highly competitive market[36]. While I cannot rule out this possibility, it seems that firstly, 1) asymmetric information is especially important to study in competitive markets, where market failures are more likely to be caused by asymmetric information and 2) adverse selection for theft is also significant (despite theft being rare), while theft is only covered by higher coverages for which the firm seems to face less effective competition. Finally, I check whether individuals who ended up choosing MD after searching around for alternative providers display differential adverse selection to those who did not search, by running the following regression for third party risk:

$$\begin{aligned} Risk_{iR} = \alpha_R + &\beta_{0R} Low\ coverage\ choice_i + \beta_{1R} Search_i \\ &+ \beta_{2R} Low\ coverage\ choice \times Search_i + \gamma_R X_i + \epsilon_{iR} \end{aligned}$$

[36] It should be noted that the firm does not have any legal obligations towards offering the most basic coverage, but seems to want to do that to capture large part of the market in this relatively new market, though it is considering to drop third party insurance.

on the survey respondents who ended up being covered for risk R either due to self-selection or due to the randomisation, where $Search_i$ denotes number of alternative providers a consumer surveyed. $\beta_{2R} < 0$ indicates the extent to which those who search around display higher adverse selection compared to those who do not search, which is captured by β_{0R}. Results are presented in Table IX.2. They suggest that adverse selection among "non-searchers" is higher than among "searchers", though this difference is only significant for one measure of risk, claim frequency. Also, "non-searchers" display, at least in magnitude, similar level of adverse selection as the baseline. Naturally search here is likely to be endogenous, so we cannot interpret this as causal impact of search on adverse selection, but it seems that at least within the firm those reporting differential search behaviour do not seem to yield significantly different observed adverse selection.

C Informal insurance

In Mongolia formal insurance markets are relatively new and therefore it is likely that informal risk-sharing arrangements are still important. In the survey, there were 1285 insured and 298 uninsured accidents reported as part of 3-year history of accidents[37]. Around 30% of insured accidents were not taken to the insurance company and 5% rejected by insurance providers. In combination with uninsured accidents, these were either self-financed, financed through friends/family or transfers from other drivers or left unresolved.

Given this, one could argue that adverse selection and moral hazard results presented here are specific to a setting where individuals often resort to insuring against risks through informal means. So, I consider here potential interactions of access to informal insurance with measures of asymmetric information. In theory, the extent to which we over- or under-estimate moral hazard that is relevant to the firm depends on the extent to which formal insurance overcrowds informal insurance (see

[37]Number of insured accidents overwhelm number of uninsured accidents. This is natural if individuals tend to over-estimate their coverage or systematically report being covered when not covered.

Arnott & Stiglitz 1991) or "complements" it. If individuals can insure themselves against risks through friends and family and therefore compensate for low coverage, in other words formal and informal insurance are perfect substitutes, then individuals may not respond to coverage upgrades as much, thus attenuating the moral hazard statistic. This may lead us to not find evidence of moral hazard even when it is present, and potentially lead to less developed formal insurance markets, if they fail to provide incentives for insurees who have access to informal insurance. On the other hand if individuals put a lot less effort in preventing accidents in response to higher coverage, then it might be possible that they demand more informal insurance. Since we do not find any evidence of moral hazard, we check for the first channel by running the moral hazard regression, but replacing the dependent variable $Risk_{iR}$ by the number of accidents (insured) insured through informal means in the duration of the contract. If informal insurance is replacing low coverage by the firm, we should observe that receiving the higher coverage due to the randomisation should decrease contributions from friends and family. Table IX.3 presents the results, suggesting that those who were upgraded have less accidents covered by friends/family, suggesting that lack of moral hazard result can be driven by formal insurance substituting for informal insurance.

Informal insurance might also affect the adverse selection correlation, since the friends and family providing informal sources of insurance might effectively act as a competitor to the formal insurance markets (in particular, they might be a competitor who is better informed about the individual's riskiness than the firm). We check whether those who had better access to informal insurance in the past display differential adverse selection compared to those who had not received informal insurance, by running:

$$\begin{aligned} Risk_{iR} = \alpha_R + &\beta_{0R} Low\ coverage\ choice_i + \beta_{1R} Informal_i \\ &+ \beta_{2R} Low\ coverage\ choice \times Informal_i + \gamma_R X_i + \epsilon_{iR} \end{aligned}$$

on the survey respondents who end up covered for risk R either due to self-selection or due to the randomisation. $Informal_i$ denotes number of accidents insured through informal means prior to purchasing the experimental contract. From Table IX.4 we see that there is no significant difference between those who had more versus less access to informal insurance.

D Trust

Often, in insurance markets in developing countries, insurees claim they do not fully trust the insurance company to pay (see UNCTAD report, 1993), and at least Cole et al. (2013) suggest endorsements of insurance sellers by trusted bodies considerably increases take up of insurance. In the current setting, 7% of consumers explicitly responded in the survey that they do not trust MD, and more than 20% claimed that they would not recommend the insurance product they bought to family members and friends. Finally, 12% of survey respondents suggest they did not get any payouts after claiming at some point in the past.

In this sense, if consumers do not fully trust the insurance company, and in particular, if they do not trust the company to pay out after claiming, consumers might be less willing to reduce effort in response to increases in coverage; at least, they might be less willing to do so than if they expected to receive the payouts with certainty. This could be a potential explanation for our findings of low moral hazard.

Trust could also affect adverse selection: if consumers know correctly that they are not covered under low coverage products (which is not necessarily the case here, see Enkhbayar (2015)), their lack of trust in receiving payouts might undermine their perception of the high coverage product as indeed providing the additional coverage, which might induce them to self-select as if the high coverage product is relatively similar to a low coverage product. If this is the case, it is likely that consumer choices between the two products would be less driven by their risk types, and more driven by other non-risk related factors shifting preferences between the two products. This would then lead us to find lower adverse selection test statistics.

To check for these possibilities, I run the adverse selection regression and the moral hazard regression interacting $Choosing\ low\ ins_i$ and $Upgrade_i$ with a proxy for insuree trust in the insurance company. The proxy for trust I use is a dummy variable indicating whether the survey respondent either said he would not recommend the product to others, that he did not receive payouts or that he does not trust the insurance company.

Again, clearly, these results are only suggestive, and I cannot rule out the possibility that the results are driven instead by reverse causality and omitted variables - it might be that the individual's inherent riskiness drives trust, and it might be that the results are driven by other factors correlated with trust and with self-selection into different products/reaction to upgrades in coverage. Still, the results suggest that these channels are not likely to be significant (see Tables IX.5 and IX.6 for AS and MH results, respectively).

VI Conclusion

Since Akerlof (1970) and Rothschild & Stiglitz (1976), we think of the presence of asymmetric information as arguably a prominent factor in explaining market failures. A large literature followed that attempts to understand whether asymmetric information is present in insurance markets, typically focusing on detailed insurance data that are most abundant in rich economies. Initially, studies emerged looking at correlation between coverage and risk exposure, meanwhile recently empirical studies focus on different types of asymmetric information which may all lead to positive coverage-risk correlation but have largely different policy implications. Also, what seems to be important is that studies especially those using correlation approach often yield conflicting and ambiguous results, with the implication that it might be important to study market features that can explain why we might expect large adverse selection/moral hazard in one market and less of it in other markets.

In this paper, I employ a randomised experiment, randomly assigning individuals who originally chose low insurance coverage to a higher insurance coverage. I do this for a car insurance product at the largest insurance provider in Mongolia. I find with this experiment that adverse selection plays an important role, potentially driving out presumably low risk individuals out of the formal markets. On the other hand, I find no evidence of moral hazard. Within the same product, I also randomly upgrade individuals choosing 10% co-payment rate to insurance without any co-payment rate and surprisingly do not find any evidence of adverse selection or moral hazard, while perhaps with higher differences in co-payment rates informational asymmetries are more likely to be detected. In health insurance markets, recent studies show that co-payment is important in controlling for firm costs through reducing moral hazard, while this seems less likely to be the case in this setting.

The main contribution of the paper is providing evidence of informational asymmetries in a developing country where insurance markets have arguably similar characteristics to those in other developing countries. In particular, I focus on four features that can be common: 1) lack of contingent pricing due to technology/human capital constraints, 2) imperfect competition, 3) lack of trust in insurance, 4) presence of informal risk-sharing arrangements. It might be then important to understand whether these shared features are potentially driving adverse selection and moral hazard results. While it is not possible, given the scope of this research and the experiment, to rule out any of these factors, I discuss given rich survey data how individuals with different degrees of access to informal insurance or levels of trust self-select into different insurance packages. Taking into account some suggestive evidence, it seems unlikely that lack of contingent pricing, at least on the observables that the firm has access to, or imperfect competition is causing large adverse selection. On the other hand, having access to formal insurance seems to substitute for informal insurance, with individuals who have been upgraded from no third party insurance to being covered against third party financing less of the accidents through informal means. This can potentially explain why I did not find any evidence of moral hazard.

In summary, there are few potential avenues for future research, especially within the context of developing countries. New markets in which both consumers and insurance providers are relatively inexperienced presents many margins through which mispricing can affect the extent of asymmetric information. While in this paper the observables to the firm do not seem to matter as much, other types of screening mechanisms have not been discussed, for example experience rating. Many firms, not just in insurance but also in credit markets, have difficulties in incorporating experience rating and do not know how to price well different risks. Also, issues with trust and past experience with insurance would be interesting to analyse within the overall market, not just within one firm at one point in time, which often requires coordination among many firms and the regulatory agencies, that is simply lacking in many developing countries.

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Appendices

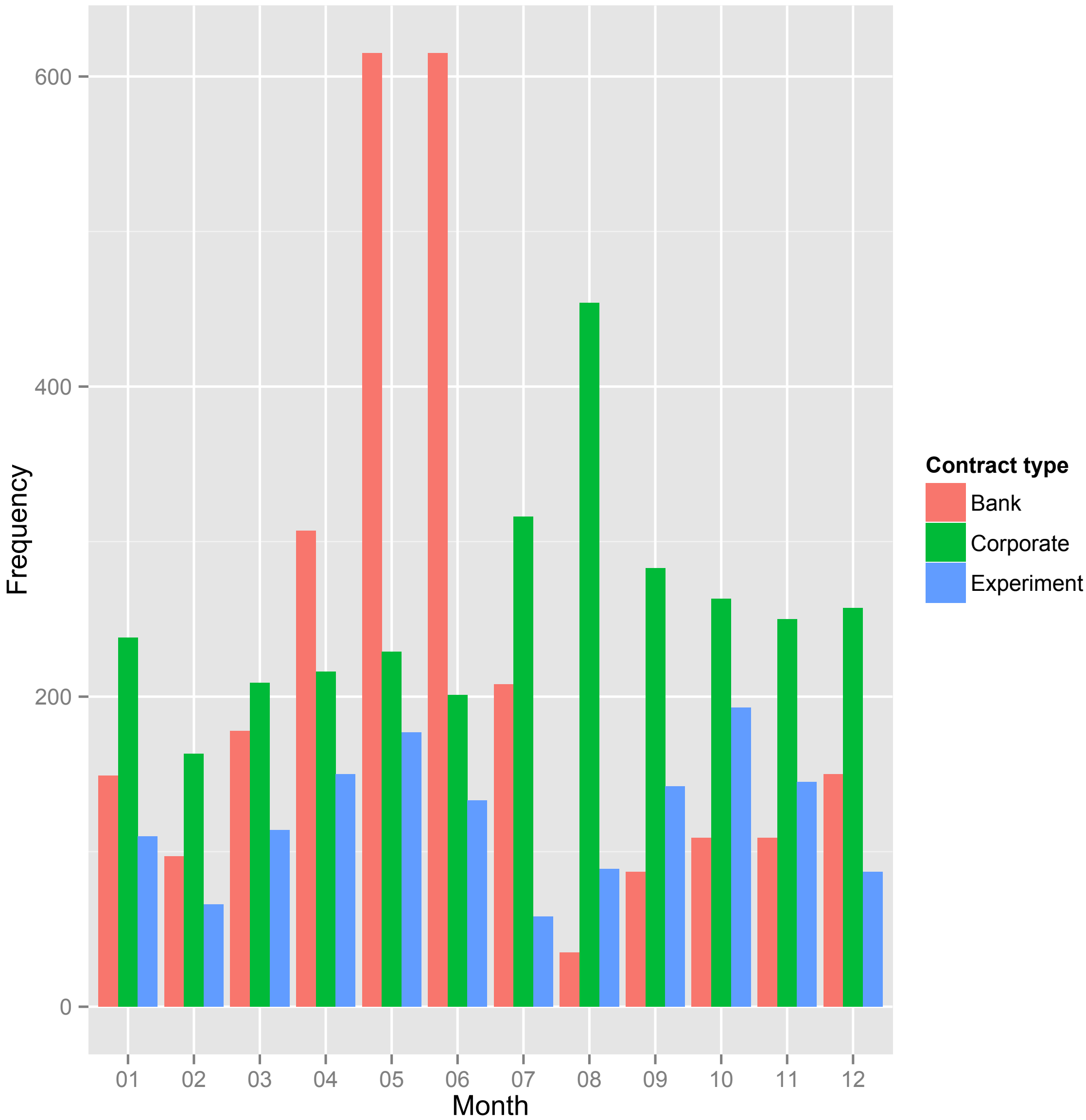
I Context: additional notes

A Existing contract types: corporate, bank vs experimental

Table I.1: Summary statistics on performance of different types of contracts are presented here. I look at contracts that were sold during 07/2013-07/2014 and respective claim behaviour during 07/2013-02/2015. Average profit is calculated as average revenue minus average claim, and excludes any expenses outside claims. Average revenue, claims and profits are converted from Mongolian Tugriks to pounds £, using exchange rate of 1£ = 3000MNT.

Sale source	No	Pr(claim)	Avg rev	Claim size	Payout	Avg profit
Corporate	3172	0.368	252	276	200	179
Bank	2667	0.210	61	188	148	30
Experimental	1434	0.301	70	232	191	12.7

Figure I.1: Number of contracts sold each month during 15/07/2013-15/07/2014, grouped by contract type. July 2013 and July 2014 are both aggregated as July, but can be seen as representative of the whole month as there is very little variation in sales throughout July.



B Existing bonus structure: agents vs managers

Around 100 agents were registered with Ulaanbaatar branches as of July 2013, however, only 10-15 turned out to be active sellers. This is due to a distortive bonus mechanism at the firm, with agents receiving a bonus of around 15% of generated income, while managers receive around 1.8%. Furthermore, with differentiated bonus structure, managers often write contracts in the name of agents', whom they bring in, and collect their bonuses. Banks and brokers can also do so[38]. Due to income-

[38]This prevents researchers from identifying the actual seller and the volume of sales through each channel. Only after the project has started it became clear that most contracts were sold

based bonus system, lack of underwriting skills and limited monitoring, sellers do not underwrite or evaluate risk, that is their incentives are misaligned with those of the firm.

C "Bonus malus" within the contract

Uncommon type of "bonus malus" exists at MD, with the current coverage reducing with each additional claim, but only in the duration of the current contract. Claims in the current contract do not affect next period pricing. A brief explanation is the following. An insuree can claim any amount up to the vehicle valuation. The firm promises to pay out an amount equivalent to:

$$\text{Loss val} * (1 - c) * \min\left(1, \frac{\text{Initial car val} - \sum \text{Past claims}}{\text{Car val at the time of accident}}\right) \tag{3}$$

where c is the co-insurance rate. Hence with each claim the coverage within the same contract decreases, with the insuree being allowed to "top-up" insurance.

D Claiming process

Generally, an insuree may need to provide different sets of materials in the claim application, depending on the type of risk incurred. The insurance company will only consider claim applications if no road traffic laws are violated by the insuree and the insuree must inform the firm hotline of the accident while remaining at the incident location[39]. The hotline team arrives at the location to inspect the situation, make notes and inform of the set of documents the insuree needs to provide for a claim application. Police reports are often requested to determine the faulty party, so that the insurance company can rebate the claim from the faulty third party (subrogation). The insuree may then choose the damage evaluator, who reports both the loss and vehicle valuation at the time of the incident. These enter in the payout equation (3). Survey respondents were distrustful of and dissatisfied with the loss and the car valuations, suspecting firms might be colluding with damage

through banks and brokers.

[39]This is in line with police requirements to not move from the location of the accident due lack of/low quality road traffic cameras, even on the main roads.

evaluators. From discussions with interviewers, it was clear that many insurees also thought that they could only claim once during the coverage period, so they delayed claiming until loss size was justifiable.

E Seller's manual with randomisation in place

All sellers underwent training and were given a manual on how to finalise insurance sales with the randomisation in place. Few points from the manual worth mentioning are:

1. Sellers should explain carefully what each coverage insures against.
2. Once the contract is signed and payment is finalised, both the seller and the buyer send a text message to a server using their mobile phones (with some additional information in the messages) and if the process is accepted by the server, they both receive messages with the randomisation outcome.
3. The seller should note the randomisation outcome on the contract and have the insuree sign in the box next to it.
4. If an insuree is upgraded to a higher coverage, the seller should explain carefully what this implies.
5. If the message is sent before the contract starts or before a contract is finalised, then the employee faces a punishment equivalent to a month's minimum wage per message.

II Firm's performance: by contract type

Table II.1: This table shows sales and profitability of type of insurance cover (initial choice) for the experimental product. In particular, it shows the total number of contracts sold, average profit, revenue and claim size (in pounds), calculated at the end of the project.

Coverage	Low	Middle (10%)	Middle (0%)	High (10%)	High (0%)
No of contracts	333	833	168	38	62
Profit	-12	-21	17	186	198
Revenue	34	56	93	237	288
Claim	46	77	76	52	91

III Summary of administrative data: by coverage choice

Table III.1: This table summarises variables collected in the administrative data, for each coverage as chosen initially: low, middle (10% or 0% co-insurance) and high (10% or 0% co-insurance) coverages.

Coverage	Low	Middle (10%)	Middle (0%)	High (10%)	High (0%)
Number of contracts	322	810	166	38	62
Age	39.45	38.82	37.52	43.11	37.00
Male	0.68	0.63	0.62	0.45	0.47
Car value (£)	4272	4660	4717	7914	7591
Car age	11.67	10.66	10.71	9.61	9.89
Light colour	0.69	0.73	0.75	0.68	0.66
Number of upgraded	184	388	0	0	0
Upgrade rate	0.57	0.48	0.00	0.00	0.00
No SMS	0.16	0.16	0.98	1.00	1.00
seller age	34.54	33.97	35.64	32.40	30.69
seller Male	0.27	0.25	0.27	0.42	0.34
seller experience	3.13	4.11	5.72	4.03	4.03
seller agent	0.14	0.18	0.11	0.13	0.07
buyer friend	0.24	0.26	0.34	0.29	0.29
Residence, brand and branch dummies					
Bayangol (resid)	0.16	0.23	0.21	0.18	0.15
Bayanzurkh (resid)	0.20	0.17	0.16	0.26	0.31
Chingeltei (resid)	0.12	0.12	0.12	0.18	0.08
HanUul (resid)	0.15	0.14	0.18	0.13	0.16
Sukhbaatar (resid)	0.11	0.12	0.14	0.18	0.13
Songinokhairkhan (resid)	0.20	0.19	0.18	0.05	0.15
NonUB (resid)	0.06	0.03	0.02	0.00	0.03
NA (resid)	0.01	0.01	0.01	0.00	0.00
Asian (brand)	0.95	0.96	0.97	0.92	0.95
European (brand)	0.04	0.04	0.03	0.05	0.02
US (brand)	0.01	0.01	0.00	0.03	0.03
NA (brand)	0.01	0.00	0.00	0.00	0.00
Bayangol (branch)	0.07	0.10	0.06	0.05	0.05
Bayanzurkh (branch)	0.10	0.08	0.10	0.16	0.19
Chingeltei (branch)	0.10	0.08	0.19	0.05	0.10
HanUul (branch)	0.29	0.16	0.16	0.11	0.08
Sunkhbaatar (branch)	0.06	0.10	0.12	0.11	0.13
Songinokhairkhan (branch)	0.20	0.20	0.13	0.13	0.13
Central (branch)	0.19	0.27	0.25	0.40	0.32

IV Predictive power of observables for claim frequency

Table IV.1: Robust standard errors in parentheses. This table shows OLS regression estimates of claim frequency (aggregated across all types of vehicle risks) on a number of observables recorded in the administrative data.

	Claim frequency
	OLS
Age	-0.006***
	(0.002)
Male	-0.027
	(0.045)
Car value (MNT)	-0.000
	(0.000)
Car age	-0.016***
	(0.006)
Light colour	0.006
	(0.049)
Bayanzurkh (resid)	-0.051
	(0.062)
Chingeltei (resid)	0.046
	(0.081)
HanUul (resid)	-0.061
	(0.069)
Sukhbaatar (resid)	0.075
	(0.081)
Songinokhairkhan (resid)	-0.016
	(0.066)
NonUB (resid)	0.005
	(0.132)
Europe (brand)	-0.222**
	(0.091)
US (brand)	-0.262**
	(0.121)
NA (brand)	-0.473***
	(0.073)
Observations	1364
R^2	0.025

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

V Summary of survey data: by coverage choice

Table V.1: This table shows means of a number of variables collected in the survey, for each coverage choice.

Coverage	Low	Middle (10%)	Middle (0%)	High (10%)	High (0%)
Number of respondents	114	346	61	20	16
Drive to work	0.70	0.76	0.83	0.75	0.56
Safe work parking	0.54	0.51	0.54	0.60	0.67
Safe home parking	0.46	0.51	0.33	0.45	0.69
Anti-theft system	0.22	0.19	0.13	0.16	0.08
Driving experience	15.07	11.83	7.93	10.15	8.19
Engine size (cc)	2362	1968	1946	1936	1873
Daily usage (km)	50.43	44.97	42.04	27.84	34.42
Risk loving	47.98	48.03	50.08	46.50	45.00
Smoker	0.25	0.28	0.33	0.20	0.12
Use mobile while driving	0.70	0.76	0.80	0.80	0.75
Business ownership	0.44	0.41	0.39	0.50	0.38
Seatbelt usage	0.90	0.87	0.85	0.95	0.94
Last month over spend	0.45	0.40	0.46	0.55	0.69
Last year over spend	0.26	0.28	0.20	0.40	0.50
No of other types of insur	1.51	1.34	1.51	1.50	1.94
No of properties	1.36	1.30	1.28	1.50	1.25
No of vehicles	1.53	1.44	1.41	1.50	1.62
University education	0.59	0.66	0.72	0.70	0.69
Loan	0.45	0.43	0.36	0.55	0.31
Car loan	0.17	0.15	0.13	0.16	0.08
Hh income (£/month)	396	427	382	513	508
Insuree income (£/month)	367	391	367	597	440
Averse to risky acts	45.47	44.63	43.23	44.60	47.50
Averse to breaking law	15.85	15.88	15.95	15.55	15.50
No of drivers in household	1.96	2.00	1.93	2.10	2.00
No of drivers	1.64	1.66	1.57	1.47	1.85
Paid insurance alone	0.89	0.87	0.83	0.84	0.56
Bought in person	0.87	0.87	0.76	0.84	0.69
Trust	0.79	0.72	0.67	0.68	0.62
No of insured risks	0.35	0.44	0.43	0.45	0.75
No of uninsured risks	0.76	0.94	1.10	1.05	1.06
Search for other provider(s)	0.22	0.34	0.38	0.53	0.62
Did not claim when insured	0.47	0.45	0.49	0.43	0.25

VI Balance tables: by experiment type

Table VI.1: Balance table for Experiment 1: those who stayed in the lowest coverage *versus* those who were upgraded to middle coverage (at 10% co-insurance rate). The first column shows the pre-randomisation variables. I have included seller-specific characteristics and days insured to see if upgraded contracts were sold differentially in terms of seller type or timing throughout the project.

	No upgrade	Upgrade	std.diff	z	
Male	0.65	0.70	0.12	1.03	
Age	39.47	39.33	-0.01	-0.10	
Car age	11.56	11.64	0.02	0.18	
Car value	11552985.07	13821954.17	0.13	1.19	
Bayangol (resid)	0.15	0.16	0.03	0.30	
Bayanzurkh (resid)	0.22	0.19	-0.08	-0.68	
Chingeltei (resid)	0.10	0.13	0.08	0.70	
HanUul (resid)	0.15	0.15	-0.01	-0.09	
Sukhbaatar (resid)	0.12	0.11	-0.01	-0.13	
Songinokhairkhan (resid)	0.20	0.20	-0.01	-0.08	
NonUB (resid)	0.05	0.06	0.02	0.20	
Asia (brand)	0.95	0.95	0.02	0.22	
Europe (brand)	0.03	0.04	0.06	0.56	
US (brand)	0.01	0.01	-0.03	-0.26	
NA (brand)	0.01	-0.00	-0.19	-1.70	.
Light colour	0.63	0.71	0.17	1.51	
seller_male	0.28	0.25	-0.08	-0.68	
seller age	35.08	34.01	-0.10	-0.88	
seller experience	3.30	2.98	-0.07	-0.62	
seller agent	0.19	0.09	-0.28	-2.43	*
Days insured	148.79	175.05	0.27	2.38	*

Table VI.2: Balance table for Experiment 2: those who stayed in middle coverage *versus* those who were upgraded to the highest coverage (at 10% co-insurance rate). The first column shows the pre-randomisation variables. I have included seller-specific characteristics and days insured to see if upgraded contracts were sold differentially in terms of seller type or timing throughout the project.

	No upgrade	Upgrade	std.diff	z	
Male	0.61	0.64	0.08	0.86	
Age	39.02	38.62	-0.03	-0.39	
Car age	10.65	10.67	0.00	0.05	
Car value	13995352.80	13638082.90	-0.02	-0.25	
Bayangol (resid)	0.25	0.22	-0.06	-0.68	
Bayanzurkh (resid)	0.16	0.19	0.08	0.95	
Chingeltei (resid)	0.11	0.13	0.08	0.90	
HanUul (resid)	0.15	0.13	-0.05	-0.54	
Sukhbaatar (resid)	0.12	0.12	-0.01	-0.09	
Songinokhairkhan (resid)	0.20	0.17	-0.07	-0.76	
NonUB (resid)	0.02	0.03	0.10	1.11	
Asia (brand)	0.95	0.96	0.07	0.81	
Europe (brand)	0.04	0.02	-0.11	-1.29	
US (brand)	0.01	0.02	0.05	0.62	
Light colour	0.74	0.74	-0.00	-0.04	
seller_male	0.22	0.24	0.05	0.53	
seller age	34.70	33.04	-0.16	-1.79	.
seller experience	4.02	4.42	0.08	0.89	
seller agent	0.25	0.12	-0.30	-3.43	***
Days insured	164.80	177.07	0.12	1.36	

Table VI.3: Balance table for Experiment 3: those who stayed at 10% co-insurance rate in the middle coverage *versus* those who were upgraded to no co-insurance rate within the same cover of risks. The first column shows the pre-randomisation variables. I have included seller-specific characteristics and days insured to see if upgraded contracts were sold differentially in terms of seller type or timing throughout the project.

	No upgrade	Upgrade	std.diff	z	
Male	0.61	0.65	0.09	1.08	
Age	39.02	38.85	-0.01	-0.17	
Car age	10.65	10.84	0.05	0.60	
Car value	13995352.80	13726779.93	-0.02	-0.19	
Bayangol (resid)	0.25	0.22	-0.07	-0.82	
Bayanzurkh (resid)	0.16	0.17	0.04	0.45	
Chingeltei (resid)	0.11	0.13	0.07	0.79	
HanUul (resid)	0.15	0.14	-0.03	-0.34	
NonUB (resid)	0.02	0.03	0.11	1.33	
Sukhbaatar (resid)	0.12	0.10	-0.08	-0.91	
Songinokhairkhan (resid)	0.20	0.21	0.03	0.34	
Asia (brand)	0.95	0.97	0.11	1.26	
Europe (brand)	0.04	0.03	-0.06	-0.76	
US (brand)	0.01	0.00	-0.12	-1.42	
Light colour	0.74	0.71	-0.06	-0.75	
seller_male	0.22	0.30	0.19	2.23	*
seller age	34.70	33.42	-0.12	-1.42	
seller experience	4.02	4.03	0.00	0.02	
seller agent	0.25	0.12	-0.32	-3.76	***
Days insured	164.80	195.55	0.30	3.48	***

VII Balance tables: survey respondents versus non-respondents

Table VII.1: This table examines whether respondents *versus* differ in observable characteristics to non-respondents. A large set of characteristics are included, in particular whether an individual was randomised to higher coverage and her risk measures.

	Survey respondents	Non-respondents	std.diff	z	
Upgrade	0.44	0.41	-0.06	-1.14	
Claim freq	0.29	0.29	0.00	0.09	
Claim size	153348.33	132113.29	-0.03	-0.50	
Collision freq	0.17	0.20	0.06	1.08	
Third party freq	0.11	0.09	-0.08	-1.45	
Theft freq	0.01	0.00	-0.01	-0.11	
Male	0.61	0.63	0.04	0.72	
Age	40.37	37.82	-0.22	-3.93	***
Car age	10.97	10.77	-0.05	-0.95	
Car value	12747790.16	15393225.15	0.15	2.73	**
Bayangol (resid)	0.24	0.19	-0.13	-2.35	*
Bayanzurkh (resid)	0.18	0.19	0.00	0.05	
Chingeltei (resid)	0.13	0.11	-0.05	-0.97	
HanUul (resid)	0.12	0.16	0.11	1.96	.
Sukhbaatar (resid)	0.12	0.12	0.00	0.04	
Songinokhairkhan (resid)	0.20	0.18	-0.06	-1.11	
NonUB (resid)	0.00	0.05	0.31	5.60	***
Asia (brand)	0.96	0.95	-0.03	-0.61	
Europe (brand)	0.03	0.04	0.02	0.36	
US (brand)	0.01	0.01	0.01	0.18	
NA (brand)	0.00	0.00	0.06	1.18	
Light colour	0.72	0.71	-0.04	-0.67	
seller_male	0.27	0.25	-0.06	-1.06	
seller age	33.54	34.50	0.09	1.66	.
seller experience	4.03	4.11	0.01	0.24	
seller agent	0.15	0.16	0.02	0.37	
Days insured	173.92	175.69	0.02	0.31	
buyer friend	0.27	0.27	-0.01	-0.20	

Table VII.2: This table examines whether respondents *versus* differ in observable characteristics to non-respondents, excluding the highest 1% of car values. A large set of characteristics are included, in particular whether an individual was randomised to higher coverage and her risk measures.

	Survey respondents	Non-respondents	std.diff	z	
Upgrade	0.44	0.42	-0.05	-0.97	
Claim freq	0.29	0.30	0.01	0.17	
Claim size	153889.24	134355.89	-0.03	-0.45	
Collision freq	0.17	0.21	0.07	1.20	
Third party freq	0.11	0.09	-0.08	-1.49	
Theft freq	0.01	0.00	-0.00	-0.08	
Male	0.62	0.63	0.03	0.56	
Age	40.40	37.59	-0.24	-4.31	***
Car age	11.00	10.98	-0.01	-0.13	
Car value	12404748.85	12941602.14	0.05	0.86	
Bayangol (resid)	0.24	0.19	-0.13	-2.34	*
Bayanzurkh (resid)	0.19	0.19	0.01	0.10	
Chingeltei (resid)	0.13	0.12	-0.05	-0.84	
HanUul (resid)	0.13	0.15	0.08	1.50	
Sukhbaatar (resid)	0.12	0.12	0.01	0.12	
Songinokhairkhan (resid)	0.20	0.18	-0.05	-0.95	
NonUB (resid)	0.00	0.05	0.31	5.65	***
Asia (brand)	0.96	0.96	-0.01	-0.26	
Europe (brand)	0.03	0.03	0.00	0.05	
US (brand)	0.01	0.01	-0.00	-0.03	
NA (brand)	0.00	0.00	0.07	1.19	
Light colour	0.73	0.72	-0.02	-0.39	
seller_male	0.27	0.25	-0.06	-1.08	
seller age	33.56	34.53	0.09	1.67	.
seller experience	4.04	4.08	0.01	0.13	
seller agent	0.15	0.16	0.02	0.39	
Days insured	174.08	176.25	0.02	0.38	
buyer friend	0.27	0.26	-0.02	-0.38	

Figure VII.1: Density plots of age and car value: for all buyers versus just survey respondents.

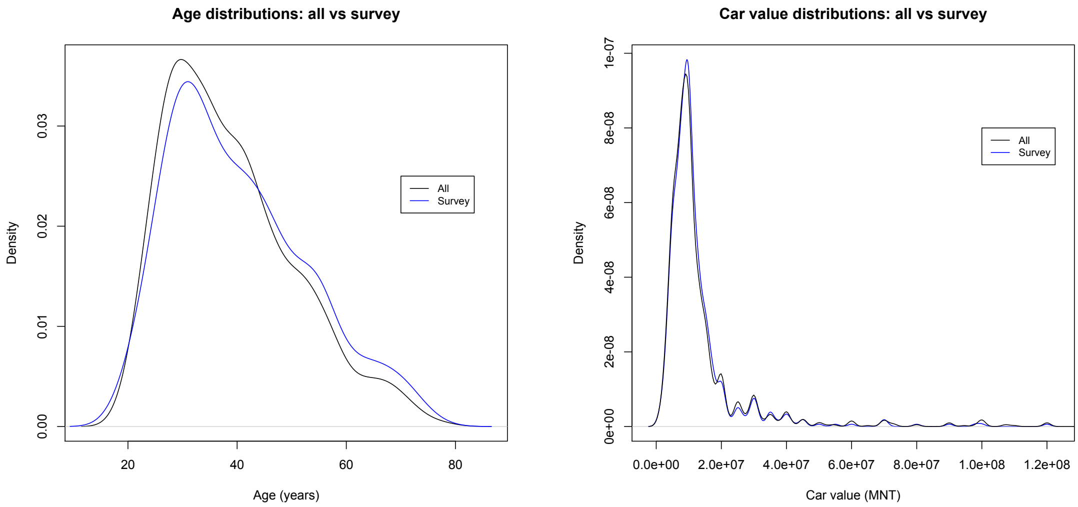
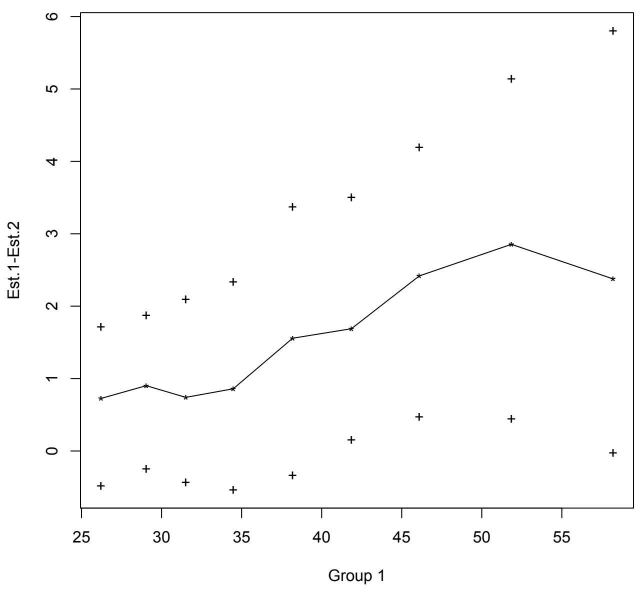


Table VII.3: This table presents multiple tests comparing requested quantiles for age distributions of two groups: group 1 includes survey respondents and group 2 includes all buyers (with non-missing age observations). The test uses Harrell-Davis estimator in conjunction with a percentile bootstrap. The results were obtained using WRS package in R.

q	n_1	n_2	est_1	est_2	est_1-est_2	$ci.low$	$ci.up$	p_{crit}	$p.value$	$signif$
0.10	569	1410	26.22	25.49	0.73	-0.48	1.71	0.02	0.22	NO
0.20	569	1410	29.04	28.14	0.90	-0.24	1.87	0.01	0.15	NO
0.30	569	1410	31.51	30.77	0.74	-0.44	2.09	0.03	0.22	NO
0.40	569	1410	34.48	33.62	0.86	-0.54	2.33	0.05	0.26	NO
0.50	569	1410	38.19	36.63	1.56	-0.34	3.37	0.01	0.11	NO
0.60	569	1410	41.86	40.17	1.69	0.16	3.50	0.01	0.03	NO
0.70	569	1410	46.10	43.68	2.42	0.47	4.19	0.01	0.01	NO
0.80	569	1410	51.85	49.00	2.85	0.44	5.14	0.01	0.02	NO
0.90	569	1410	58.20	55.82	2.38	-0.03	5.80	0.01	0.06	NO

Figure VII.2: This table presents differences between quantiles of age distributions for survey respondents (Group 1) versus all buyers (Group 2). In particular, it shows the confidence intervals for each quantile: upper and lower confidence intervals are denoted by + signs. X-axis and Y-axis denote age (in years) and difference in quantiles, respectively.



VIII Reduced form tests

A Adverse selection

Third party

Table VIII.1: Standard errors are in parentheses. The dependent variables are third party claim frequency, probability and size. Row 1 of columns 1, 3 and 5 show estimates of effect of riskiness on coverage choice, controlling for $log(Days\ insured)$ only. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller characteristics.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Low coverage choice	-0.095***	-0.089***	-0.081***	-0.069**	-1.006***	-0.872**
	(0.029)	(0.031)	(0.026)	(0.028)	(0.322)	(0.345)
log(Days insured)	0.152	0.232	0.086	0.154	0.812	1.231*
	(0.164)	(0.185)	(0.127)	(0.145)	(0.663)	(0.647)
Other controls	NO	YES	NO	YES	NO	YES
Observations	606	595	606	595	606	595
R^2					0.013	0.064
Log Likelihood	-261.524	-237.225	-232.233	-210.932		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Theft

Table VIII.2: Standard errors are in parentheses. The dependent variables are theft claim frequency, probability and size from the administrative claims data. Row 1 of columns 1, 3 and 5 show estimates of effect of riskiness on coverage choice, controlling for *log*(*Days insured*) only. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for most covariates. As theft is rare we exclude here controls on car colour, residence and brand dummies.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Low coverage choice	0.025**	0.027**	0.025**	0.027**	0.255**	0.356*
	(0.011)	(0.012)	(0.011)	(0.012)	(0.116)	(0.214)
log(Days insured)	0.458	0.535	0.447	0.497	1.534**	1.316**
	(0.554)	(0.633)	(0.536)	(0.579)	(0.691)	(0.648)
Other controls	NO	YES	NO	YES	NO	YES
Observations	228	223	228	223	228	223
R^2					0.011	0.049
Log Likelihood	-21.801	-18.916	-21.709	-18.588		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table VIII.3: Standard errors are in parentheses. The dependent variables are 3-year history of theft frequency, dummy and loss size. The regression is only ran on survey respondents. Row 1 of columns 1, 3 and 5 show estimates of effect of riskiness on coverage choice controlling for *log*(*Days insured*). Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Acc frequency		Prob of acc		Acc size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Low coverage choice	-0.327*	-0.468*	-0.266**	-0.317**	-1.480	-2.213
	(0.193)	(0.270)	(0.123)	(0.130)	(1.428)	(1.464)
log(Days insured)	0.050	0.170	0.005	0.033	1.397	1.698
	(0.721)	(0.757)	(0.511)	(0.520)	(6.003)	(6.706)
Other controls	NO	YES	NO	YES	NO	YES
Observations	97	97	97	97	97	97
R^2					0.013	0.088
Log Likelihood	-80.157	-76.163	-55.735	-52.450		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Co-insurance rate

Table VIII.4: Standard errors are in parentheses. The dependent variables are collision and TP claim frequency, probability and size. Row 1 of columns 1, 3 and 5 show estimates of effect of riskiness on coverage choice controlling for $log(Days\ insured)$. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Low coverage choice	0.027	0.037	0.060	0.058	0.510	0.525
	(0.073)	(0.076)	(0.050)	(0.051)	(0.574)	(0.569)
log(Days insured)	1.388***	1.410***	0.630**	0.611*	3.468***	2.348
	(0.461)	(0.477)	(0.313)	(0.326)	(1.283)	(1.595)
Other controls	NO	YES	NO	YES	NO	YES
Observations	364	356	364	356	364	356
R^2					0.012	0.060
Log Likelihood	-337.076	-324.021	-232.071	-223.144		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

B Moral hazard

Third party

Table VIII.5: Standard errors are in parentheses. The dependent variables are third party accident frequency, probability and loss size **during the contract** from the survey data. Row 1 of columns 1, 3 and 5 show estimates of effect of high coverage on riskiness controlling for $log(Days\ insured)$. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Acc frequency		Prob of acc		Acc size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	-0.099	-0.133	-0.091	-0.104	-0.698	-0.390
	(0.094)	(0.124)	(0.078)	(0.079)	(0.827)	(0.804)
log(Days insured)	0.062	0.108	0.050	0.086*	0.251	0.631
	(0.057)	(0.066)	(0.048)	(0.050)	(0.462)	(0.400)
Other controls	NO	YES	NO	YES	NO	YES
Observations	113	113	113	113	113	113
R^2					0.008	0.224
Log Likelihood	-62.981	-51.358	-55.927	-42.619		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table VIII.6: Standard errors are in parentheses. The dependent variables are third party accident frequency, probability and loss size **during the contract** from the survey data. Here accidents with non-positive losses are excluded. Row 1 of columns 1, 3 and 5 show estimates of effect of high coverage on riskiness controlling for $log(Days\ insured)$. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Acc frequency		Prob of acc		Acc size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	-0.083	-0.074	-0.076	-0.055	-0.698	-0.390
	(0.086)	(0.102)	(0.073)	(0.072)	(0.827)	(0.804)
log(Days insured)	0.038	0.077	0.027	0.055	0.251	0.631
	(0.050)	(0.058)	(0.042)	(0.043)	(0.462)	(0.400)
Other controls	NO	YES	NO	YES	NO	YES
Observations	113	113	113	113	113	113
R^2					0.008	0.224
Log Likelihood	-56.996	-44.956	-50.479	-37.720		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Theft

Table VIII.7: Standard errors are in parentheses. The dependent variables are theft accident frequency, probability and loss size **during the contract** from the survey data. Row 1 of columns 1, 3 and 5 show estimates of effect of high coverage on riskiness controlling for $log(Days\ insured)$. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Acc frequency		Prob of acc		Acc size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	0.044	0.042	0.038	0.024	0.549	0.347
	(0.036)	(0.040)	(0.032)	(0.026)	(0.394)	(0.391)
log(Days insured)	0.084**	0.060*	0.067*	0.045*	0.384***	0.329***
	(0.042)	(0.037)	(0.035)	(0.027)	(0.128)	(0.120)
Other controls	NO	YES	NO	YES	NO	YES
Observations	250	249	250	249	250	249
R^2					0.032	0.130
Log Likelihood	-49.689	-34.224	-42.844	-28.143		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table VIII.8: Standard errors are in parentheses. The dependent variables are theft accident frequency, probability and loss size **during the contract** from the survey data. Here accidents with non-positive losses are excluded. Row 1 of columns 1, 3 and 5 show estimates of effect of high coverage on riskiness controlling for $log(Days\ insured)$. Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Acc frequency		Prob of acc		Acc size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	0.038	0.034	0.044	0.028	0.549	0.347
	(0.033)	(0.038)	(0.031)	(0.023)	(0.394)	(0.391)
log(Days insured)	0.070*	0.054	0.059*	0.042	0.384***	0.329***
	(0.038)	(0.035)	(0.033)	(0.026)	(0.128)	(0.120)
Other controls	NO	YES	NO	YES	NO	YES
Observations	250	249	250	249	250	249
R^2					0.032	0.130
Log Likelihood	-44.054	-27.847	-39.960	-22.147		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Co-insurance rate

Table VIII.9: Standard errors are in parentheses. The dependent variables are collision and third party claim frequency, probability and size. Row 1 of columns 1, 3 and 5 show estimates of effect of higher coverage on riskiness controlling for *log*(*Days insured*). Row 1 of columns 2, 4 and 6 show such estimates additionally controlling for all covariates recorded from the contracts as well as seller groups.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	0.039	0.003	0.069*	0.043	0.823	0.465
	(0.059)	(0.059)	(0.041)	(0.040)	(0.523)	(0.538)
log(Days insured)	1.213***	1.422***	0.515**	0.578**	3.729**	4.488***
	(0.353)	(0.388)	(0.229)	(0.229)	(1.472)	(1.444)
Other controls	NO	YES	NO	YES	NO	YES
Observations	620	608	620	608	620	608
R^2					0.012	0.069
Log Likelihood	-564.941	-534.714	-387.594	-361.246		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table VIII.10: This table summarises the various tests for adverse selection and moral hazard for different vehicle insurance contracts. * implies results hold for subset of risk measures, not all.

	"Third party"	"Theft"	Co-insurance rate
Adverse selection	YES	YES*	NO
Moral hazard	NO	NO	NO

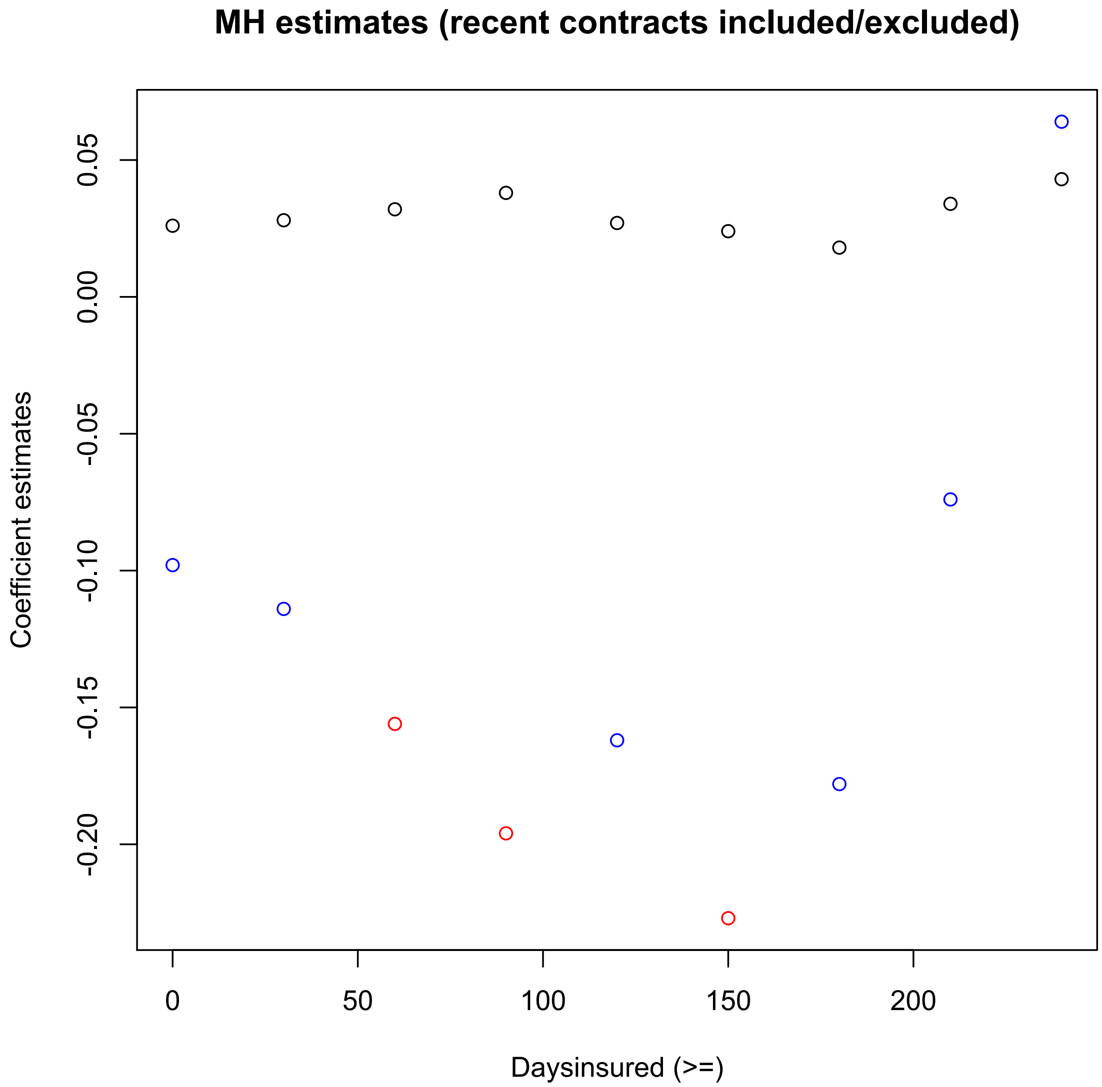


Figure VIII.1: This figure shows moral hazard results for third party and theft excluding later contracts for which we have incomplete claims data. X-axis indicates the extent to which we include later contracts, going to the right would result in including only very early contracts. The Y-axis denote the estimates of moral hazard, effect of higher contract on riskiness. Black round dots are for theft, while blue and red are for third party (red denotes significant, while blue denotes estimates not significantly different from 0).

IX Tables for discussion

Table IX.1: This table shows how adverse selection interacts with search intensity, where search intensity is measured by number of firms a consumer evaluated before choosing the product under consideration. For third party the regression is ran on low coverage buyers and middle coverage consumers (irrespective of co-insurance rate) to ensure higher sample size.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
TP						
Low coverage choice	-0.104**	-0.111**	-0.070	-0.071	-0.761	-0.683
	(0.053)	(0.055)	(0.048)	(0.048)	(0.540)	(0.569)
Search	0.011	0.014	0.018	0.019	0.243	0.267
	(0.015)	(0.015)	(0.013)	(0.013)	(0.193)	(0.179)
Low coverage choice × Search	0.043	0.071*	0.031	0.047	0.347	0.498
	(0.039)	(0.043)	(0.034)	(0.034)	(0.546)	(0.514)
log(Days insured)	0.019	0.018	-0.006	-0.005	-0.043	-0.051
	(0.026)	(0.027)	(0.021)	(0.021)	(0.246)	(0.244)
Other controls	NO	YES	NO	YES	NO	YES
Observations	382	381	382	381	382	381
R^2					0.013	0.073
Log Likelihood	-188.068	-172.547	-159.979	-146.993		
Theft						
Low coverage choice	-0.292	-0.409	-0.167	-0.281	-0.741	-1.605
	(0.258)	(0.334)	(0.165)	(0.182)	(1.774)	(1.661)
Search	0.059	0.090	0.098	0.091	1.020	1.029
	(0.074)	(0.078)	(0.068)	(0.070)	(0.916)	(0.937)
Low coverage choice×Search	-0.022	-0.033	-0.064	-0.047	-0.604	-0.517
	(0.088)	(0.092)	(0.072)	(0.074)	(1.023)	(1.060)
log(Days insured)	0.049	0.096	0.047	0.053	0.545	0.708
	(0.074)	(0.082)	(0.054)	(0.054)	(0.581)	(0.626)
Other controls	NO	YES	NO	YES	NO	YES
Observations	95	95	95	95	95	95
R^2					0.047	0.099
Log Likelihood	-77.414	-73.374	-51.659	-48.651		
Co-insurance rate						
Low coverage choice	0.192	0.239	0.166*	0.163*	1.769	1.790
	(0.140)	(0.146)	(0.089)	(0.092)	(1.089)	(1.095)
Search	0.077	0.077	0.077*	0.075*	0.918*	0.894*
	(0.053)	(0.056)	(0.041)	(0.044)	(0.535)	(0.515)
Low coverage choice×Search	-0.015	-0.002	-0.024	-0.018	-0.069	0.049
	(0.063)	(0.069)	(0.048)	(0.051)	(0.681)	(0.670)
log(Days insured)	0.078	0.121	-0.002	-0.003	-0.074	-0.108
	(0.077)	(0.086)	(0.045)	(0.049)	(0.532)	(0.611)
Other controls	NO	YES	NO	YES	NO	YES
Observations	150	150	150	150	150	150
R^2					0.072	0.119
Log Likelihood	-137.765	-131.920	-89.925	-86.527		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table IX.2: This table presents tests of moral hazard, by considering as the dependent variable alternative measures of effort. In particular instead of using accidents as measure of effort, I use a dummy for whether an individual engages in activities that are likely to reduce TP and theft risks: taking value 1 if individuals typically parks the car in secure locations and/or have anti-theft system, or 0 otherwise.

	TP	
	Safe activities	
	logistic	
	(1)	(2)
Upgrade	-0.063	-0.029
	(0.105)	(0.101)
log(Days insured)	-0.013	-0.037
	(0.060)	(0.042)
Other controls	NO	YES
Observations	92	92
Log Likelihood	-62.479	-53.752
	Theft	
Upgrade	-0.007	-0.042
	(0.072)	(0.069)
log(Days insured)	0.026	0.016
	(0.036)	(0.026)
Other controls	NO	YES
Observations	204	203
Log Likelihood	-133.375	-115.355
	Co-insurance rate	
Upgrade	0.046	0.018
	(0.070)	(0.067)
log(Days insured)	0.059	0.056
	(0.040)	(0.026)
Other controls	NO	YES
Observations	217	216
Log Likelihood	-143.276	-129.901

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table IX.3: This table shows the extent to which coverage upgrades affect informal insurance. Informal insurance is measured through number of accidents that are naturally covered by insurance, but were instead insured through other means.

	TP					
	Informal freq		Prob of informal		Informal size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Upgrade	-0.174**	-0.289*	-0.152**	-0.200***	-1.466*	-1.301*
	(0.088)	(0.161)	(0.074)	(0.071)	(0.752)	(0.746)
log(Days insured)	0.048	0.105*	0.046	0.093*	0.200	0.523
	(0.050)	(0.063)	(0.044)	(0.052)	(0.431)	(0.368)
Other controls	NO	YES	NO	YES	NO	YES
Observations	113	113	113	113	113	113
R^2					0.037	0.232
Log Likelihood	-52.761	-42.570	-48.560	-35.602		
			Theft			
Upgrade	0.020	0.017	0.014	0.005	0.246	0.065
	(0.032)	(0.034)	(0.028)	(0.024)	(0.342)	(0.354)
log(Days insured)	0.064*	0.043	0.049*	0.032	0.293***	0.245**
	(0.035)	(0.030)	(0.030)	(0.023)	(0.111)	(0.103)
Other controls	NO	YES	NO	YES	NO	YES
Observations	250	249	250	249	250	249
R^2					0.017	0.112
Log Likelihood	-45.777	-31.110	-38.777	-25.202		
			Co-insurance rate			
Upgrade	0.002	0.006	0.031	0.038	0.592	0.799
	(0.074)	(0.078)	(0.056)	(0.053)	(0.667)	(0.636)
log(Daysinsured)	0.066	0.162***	0.047	0.088**	0.509*	0.809***
	(0.046)	(0.054)	(0.033)	(0.037)	(0.293)	(0.299)
Other controls	NO	YES	NO	YES	NO	YES
Observations	268	267	268	267	268	267
R^2					0.012	0.146
Log Likelihood	-208.793	-178.435	-148.209	-125.246		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table IX.4: This table shows how adverse selection interacts with past informal insurance, where *Informal* is number of accidents that were financed through means other than formal insurance. For third party the regression is ran on low coverage buyers and middle coverage consumers (irrespective of co-insurance rate) to ensure higher sample size.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
TP						
Low coverage choice	-0.090*	-0.087*	-0.061	-0.056	-0.812	-0.626
	(0.049)	(0.052)	(0.044)	(0.046)	(0.619)	(0.642)
Informal	-0.099**	-0.088*	-0.082**	-0.076*	-0.801***	-0.722***
	(0.048)	(0.049)	(0.039)	(0.039)	(0.267)	(0.273)
Low coverage choice × Informal	0.025	0.029	0.022	0.021	0.250	0.030
	(0.186)	(0.177)	(0.139)	(0.128)	(0.853)	(0.908)
log(Days insured)	0.022	0.016	-0.004	-0.006	-0.038	-0.036
	(0.026)	(0.027)	(0.020)	(0.020)	(0.233)	(0.228)
Other controls	NO	YES	NO	YES	NO	YES
Observations	393	392	393	392	393	392
R^2					0.005	0.060
Log Likelihood	-194.152	-179.208	-164.635	-152.584		
Theft						
Low coverage choice	-0.683*	-0.779	-0.198**	-0.186*	-1.445	-1.557
	(0.410)	(0.581)	(0.100)	(0.108)	(1.243)	(1.191)
Informal	0.286***	0.275	1.739	2.610	4.586***	4.870***
	(0.103)	(0.280)	(235.292)	(782.528)	(1.009)	(1.125)
Low coverage choice× Informal	0.325**	0.405	0.087	0.242	2.606**	2.190
	(0.137)	(0.318)	(287.193)	(900.411)	(1.210)	(1.344)
log(Days insured)	0.084	0.111	0.085	0.060	0.729**	0.662
	(0.077)	(0.101)	(0.057)	(0.048)	(0.361)	(0.428)
Other controls	NO	YES	NO	YES	NO	YES
Observations	97	97	97	97	97	97
R^2					0.493	0.550
Log Likelihood	-55.558	-51.835	-27.563	-22.279		
Co-insurance rate						
Low coverage choice	0.085	0.127	0.079	0.060	1.329	1.198
	(0.127)	(0.133)	(0.089)	(0.092)	(1.252)	(1.279)
Informal	-0.384**	-0.358**	-0.177*	-0.194**	-1.345***	-1.366***
	(0.175)	(0.174)	(0.093)	(0.098)	(0.427)	(0.456)
Low coverage choice× Informal	0.229	0.205	0.095	0.117	0.261	0.296
	(0.187)	(0.186)	(0.100)	(0.103)	(0.640)	(0.677)
log(Days insured)	0.054	0.072	-0.017	-0.017	-0.268	-0.253
	(0.080)	(0.085)	(0.048)	(0.050)	(0.532)	(0.600)
Other controls	NO	YES	NO	YES	NO	YES
Observations	155	155	155	155	155	155
R^2					0.067	0.117
Log Likelihood	-136.726	-131.985	-91.986	-87.795		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table IX.5: This table shows how adverse selection of third party interacts with the degree of distrust, where *Distrust* is a dummy that takes value 1 if individual claims any one of the following: 1) would not offer the product to others, 2) explicitly report not of trusting the company, 3) had in the past accidents for which claims were not paid out. For third party the regression is ran on low coverage buyers and middle coverage consumers (irrespective of co-insurance rate) to ensure higher sample size.

	TP					
	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
Low coverage choice	-0.076	-0.057	-0.039	-0.025	-0.597	-0.331
	(0.057)	(0.065)	(0.053)	(0.058)	(0.781)	(0.797)
Distrust	-0.100**	-0.114**	-0.069*	-0.080**	-0.935*	-1.105**
	(0.046)	(0.048)	(0.040)	(0.040)	(0.518)	(0.526)
Low coverage choice × Distrust	-0.190***	-0.191***	-0.161***	-0.162***	-0.916	-1.188
	(0.024)	(0.024)	(0.020)	(0.020)	(0.865)	(0.920)
log(Days insured)	0.014	0.016	-0.014	-0.011	-0.143	-0.106
	(0.029)	(0.031)	(0.022)	(0.023)	(0.259)	(0.261)
Other controls	NO	YES	NO	YES	NO	YES
Observations	339	338	339	338	339	338
R^2					0.018	0.080
Log Likelihood	-171.016	-157.083	-142.717	-131.545		
	Theft					
Low coverage choice	-0.396	-1.220	-0.395**	-0.491***	-3.644*	-5.226***
	(0.298)	(0.814)	(0.162)	(0.118)	(2.142)	(2.019)
Distrust	-0.049	-0.519	-0.163	-0.256*	-3.881	-5.285**
	(0.246)	(0.418)	(0.169)	(0.142)	(2.654)	(2.687)
Low coverage choice×Distrust	0.062	1.080	0.187	0.392***	4.186	6.884**
	(0.349)	(1.205)	(0.233)	(0.135)	(3.006)	(2.858)
log(Days insured)	0.035	0.056	0.020	-0.006	0.305	0.088
	(0.087)	(0.095)	(0.059)	(0.059)	(0.705)	(0.770)
Other controls	NO	YES	NO	YES	NO	YES
Observations	83	83	83	83	83	83
R^2					0.047	0.232
Log Likelihood	-73.455	-64.100	-49.203	-41.043		
	Co-insurance rate					
Low coverage choice	0.085	0.218	0.113	0.143	1.374	1.804
	(0.134)	(0.149)	(0.095)	(0.095)	(1.408)	(1.387)
Distrust	-0.583**	-0.530*	-0.244*	-0.256**	-2.878**	-2.933*
	(0.276)	(0.280)	(0.128)	(0.130)	(1.463)	(1.516)
Low coverage choice×Distrust	0.463	0.154	0.055	0.015	0.379	-0.192
	(0.642)	(0.474)	(0.188)	(0.192)	(1.989)	(1.963)
log(Days insured)	0.059	0.080	-0.011	-0.006	-0.190	-0.065
	(0.081)	(0.093)	(0.048)	(0.052)	(0.562)	(0.663)
Other controls	NO	YES	NO	YES	NO	YES
Observations	137	137	137	137	137	137
R^2					0.065	0.134
Log Likelihood	-123.907	-116.650	-82.392	-77.052		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.

Table IX.6: This table shows how moral hazard interacts with the degree of distrust, where *Distrust* is a dummy that takes value 1 if individual claims any one of the following: 1) would not offer the product to others, 2) explicitly report not of trusting the company, 3) had in the past accidents for which claims were not paid out. The regression is ran on low coverage buyers.

	Claim frequency		Prob of claim		Claim size	
	Poisson		*logistic*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)
TP						
Upgrade	-0.111	-0.174	-0.107	-0.142	-0.358	-0.288
	(0.119)	(0.164)	(0.098)	(0.092)	(1.230)	(1.208)
Distrust	-0.279	-0.290	-0.244**	-0.263***	-2.302*	-2.910**
	(0.182)	(0.199)	(0.123)	(0.093)	(1.346)	(1.365)
Upgrade × Distrust	0.258	0.683	0.176	0.282	-0.633	1.183
	(0.652)	(1.329)	(0.278)	(0.199)	(1.543)	(1.603)
log(Days insured)	0.054	0.133*	0.042	0.099*	0.068	0.875*
	(0.067)	(0.080)	(0.053)	(0.059)	(0.527)	(0.488)
Other controls	NO	YES	NO	YES	NO	YES
Observations	97	97	97	97	97	97
R^2					0.073	0.306
Log Likelihood	-58.795	-47.523	-51.258	-36.913		
Theft						
Upgrade	0.060	0.068	0.036	0.021	0.459	0.204
	(0.052)	(0.063)	(0.044)	(0.036)	(0.559)	(0.576)
Distrust	-0.027	-0.004	-0.009	0.008	-0.205	-0.170
	(0.052)	(0.068)	(0.043)	(0.047)	(0.340)	(0.354)
Upgrade × Distrust	0.040	0.040	0.048	0.049	0.673	0.794
	(0.121)	(0.132)	(0.103)	(0.089)	(1.060)	(1.085)
log(Days insured)	0.104**	0.074	0.086**	0.058*	0.512***	0.398***
	(0.049)	(0.046)	(0.044)	(0.035)	(0.169)	(0.147)
Other controls	NO	YES	NO	YES	NO	YES
Observations	211	210	211	210	211	210
R^2					0.044	0.151
Log Likelihood	-49.728	-33.766	-39.939	-25.574		
Co-insurance rate						
Upgrade	0.163	0.193	0.146*	0.163**	1.805*	1.999*
	(0.120)	(0.127)	(0.081)	(0.080)	(1.054)	(1.059)
Distrust	-0.073	-0.074	0.002	-0.005	0.006	-0.038
	(0.119)	(0.126)	(0.080)	(0.082)	(0.978)	(1.013)
Upgrade × Distrust	-0.220	-0.257*	-0.180*	-0.194**	-2.441	-2.698
	(0.144)	(0.139)	(0.098)	(0.095)	(1.683)	(1.831)
log(Days insured)	0.095	0.086	0.017	0.011	0.224	0.145
	(0.061)	(0.065)	(0.037)	(0.038)	(0.434)	(0.437)
Other controls	NO	YES	NO	YES	NO	YES
Observations	230	229	230	229	230	229
R^2					0.022	0.069
Log Likelihood	-212.669	-203.448	-144.488	-137.210		

Notes: significance at 1%, 5% and 10% levels denoted by ***, **, *, respectively.