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The Effect of Contract Type on Insurance Fraud

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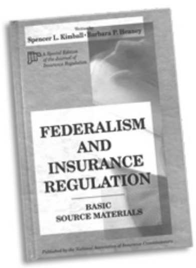

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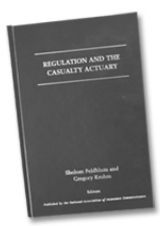

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The Effect of Contract Type on Insurance Fraud

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Abstract

This paper analyzes the possible effects of different types of insurance contracts upon fraudulent behavior. In particular, we are interested in the tempering effect upon fraud that may be found in contracts that are structured on experience-rated premiums (bonus-malus). We use an experiment to observe the level of fraud, under two different contractual environments: bonus-malus and audits.

Our experimental results are the following: First, we found that fraud was a prevalent feature in claiming behavior, although not capitalized upon by all or to the full extent possible. Second, contrary to the theoretical expectation, the amount of fraud was not statistically significant between pure bonus-malus contracts and pure audit contracts. Third, the experience of having had a bonus-malus contract appears to have a significant tempering effect upon the amount of fraud that is committed in contracts with audits and a fixed premium.

Our results may have implications for insurance regulation when fraud is an issue. The way an insurance consumer's contracts are structured over time appears to impact upon fraudulent activity, which likely has effects upon insurance providers and other insurance consumers. Thus, there is scope for external regulation of exactly which intertemporal structures of contracts should be allowed to be offered.

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Introduction

The economics of insurance fraud has developed along several interesting theoretical lines, led principally by the so-called "costly state falsification" and the "costly state verification" perspectives. Picard (2013) provides a detailed survey of the economic analysis of insurance fraud. While there are many different types, or classifications, of insurance fraud (including fraudulent misrepresentation of information by the insured either concerning a loss or at the time of contracting, fraud by third parties and fraud by insurance providers), perhaps the easiest to understand is simple claims build-up and fictitious claiming by insured individuals.[1] In the present paper, we are concerned only with the issue of claims build-up and fictitious claiming in an environment of costly state verification.

Fraud is also of interest to insurance regulators, who are interested in how the presence of fraud by one insured individual might affect the welfare of other insured individuals and the insurance providers themselves. The point of contact between an insurance provider and an insured individual is the insurance contract that determines what happens if an insured loss should occur. Decisions taken by the insured with the intention of receiving a greater indemnity than what is actually owed under the contract are considered to be fraudulent. In an environment of costly state verification, attempts by the provider to control such fraudulent activity imply a cost, which is then either passed on to other insurance consumers in the form of inflated premiums, or borne by the insurer in the form of reduced profits (and correspondingly, perhaps, a greater probability of bankruptcy). In that way, fraud by one insured impacts the welfare of other insureds and providers, and the mechanism under which the impact is spread are the contracts between insureds and the provider. Thus, if fraud is more prevalent under one contract design than another, it may well be in the interests of a regulator to consider which contracts to allow, and which to forbid.

Insurance fraud is also necessarily an intertemporal problem. Tennyson (2002) studied the connection between the attitude of insureds to fraud and the experience that the insured has with insurance products and markets. In that paper, it is found that insureds with more experience in the claims process tend to have a lower tolerance for insurance fraud. On the other hand, Tennyson (1997) also finds that insureds who consider their premiums to be too high are more likely to find fraud to be acceptable. This is a psychological perspective on fraud that attempts to discover how insureds might rationalize fraudulent behavior. The present paper also takes an intertemporal point of view of insurance fraud, but it concentrates exclusively on the economic incentives that the contracts between insureds and providers create for possible fraudulent behavior by insureds. However, somewhat

1. Claim build-up is when an insured loss happens, and the insured individual claims for an amount that is greater than the actual value of the loss. Fictitious claiming is build-up from an original loss equal to zero. Thus, fictitious claiming is a special case of claim build-up, and so, in the present paper, we only refer to "claim build-up," although, by so doing, we do not rule out that the build-up is from a loss of zero.

more in line with Tennyson's work, we are also interested in whether insureds take full advantage of the opportunities for claim build-up, or if rather there is some psychological issue that tempers the level of fraudulent claiming behavior.

The present paper considers the problem of fraudulent claiming in insurance markets when it is costly for the provider to verify the value of the true loss. We are interested principally in the following questions:

- Are there insurance products in the market that, although not designed specifically with the objective of reducing fraud, are in fact having such an effect?
- Is it possible that the design of certain common insurance products leads to a greater level of fraud by insurance consumers?

The paper studies the effect of different types of insurance contracts upon the fraudulent behavior of insurance consumers. Above all, we are interested in comparing two particularly common contract types: those with premiums that vary in function of claim histories (bonus-malus contracts); and those with fixed premiums but with audit systems in place for attempting to detect, and punish, fraudulent behavior. If it can be shown that fraud is reduced under one type of contract as compared to another, then there is scope for regulators to insist upon those being the contract formats that are predominately used.

Moreno et al. (2006) put forward the thesis that, even in complete absence of audit and threat of punishment for detected fraud, bonus-malus contracts have a built-in tempering effect upon fraud, and, indeed, that this tempering effect may even be more important than that provided by traditional systems of audits and threats of punishment. If this is, in fact, true, then we might validly consider that the expensive-to-run audit systems that are currently in common use in insurance companies all over the world may be an inefficient mechanism for the fight against insurance fraud. In this paper, we consider if this tempering effect can be found experimentally, and we also consider if there might exist experience effects as an insurance consumer's contract is changed from bonus-malus to audit.

The theoretical insights from Moreno et al. (2006) have not been contrasted with empirical data. Although bonus-malus premiums take into account many factors (claim history, administrative expenses, marketing, etc.), we are not aware of any empirical study that considers the impact that experience rated premiums may have upon insurance fraud.

The present paper fills this void using experimental methods. The paper adds to a small tradition of economics papers that use laboratory experiments to study insurance fraud. Miyazaki (2008) considers the issue of how the level of deductible may affect insurance fraud, and Dean (2004) looks at the ethical perception of individuals to insurance fraud.[2]

Experimental Design

The experiment was carried out in the Experimental Economics Lab at the Universidad Pablo de Olavide, in Seville, Spain. The experiment was run digitally, and was programmed in z-Tree (Zurich Toolbox for Readymade Economic Experiments), which is the standard software for experimental economics. The subjects were all undergraduate students in business and economics at the University Pablo de Olavide.[3] In all, 154 subjects took part in the experiment.

Our experimental design was careful to offer, in as much as is possible in a laboratory environment, a similar choice game as individuals would face in the real world, at least as far as is relevant for the decision on fraudulent claiming behavior, because one of our objectives was that the results of our experiment concerning the effects of contract type on fraud could be as real-world relevant as possible.[4] Specifically, our subjects were offered voluntary choices as to the type of insurance contract that they would have (rather than having a particular contract imposed upon them with no degree of choice); the insurable risk was partially determined by chance and partially by the subject's own skills and abilities (rather than being pure chance) as, for example, is the case in real-world automobile insurance; and whether a submitted claim was fraudulent was kept as purely private information to the claimant (unless the fraud was uncovered using an audit). All of these are important features of real-world insurance environments that may affect the decision on fraudulent claiming.

2. Lammers and Shiller (2010), in unpublished work, have considered the tempering effect of bonus-malus contracts upon insurance fraud, although their experimental design makes their analysis much more related to public goods games than insurance games.

3. Using students as experimental subjects, rather than actual insurance consumers, is of course extremely common and accepted. For the case of insurance, Schoemaker and Kunreuther (1979) found that the use of students did not imply any significant restriction on the applicability of results to a wider set of consumers.

4. Of course, it is impossible to replicate in a laboratory environment losses of the scale that might be subject to real-world insurance contracts. We are also restricted only to monetary losses, rather than losses of other assets or health. However, we see no reason why the size and type of loss should alter the basic rationale behind the incentives that might be available to an insured for claim build-up. In that way, the experimental design was careful to replicate the same sort of environment *in as far as the decision to commit fraud is concerned*, as exists in the real world.

Layout of the Experimental Laboratory

We were also careful that the two contractual treatments offered (as much as is possible) the same incentives to subjects in terms of the benefits to them of fraudulent claiming. Thus, so long as any relevant personal traits of subjects are on average similar, any differences in fraudulent claiming across subject groups in our experiment are due to the contractual format and nothing else.

One good reason for using experimental methods to look at insurance fraud is the fact that, in real-world environments, fraud is by nature impossible to measure. One can only know how much *discovered* fraud exists, but never how much *undiscovered* fraud goes on.[5] However, using an experimental insurance market, the amount of fraud that occurs, regardless of whether it is discovered, can be directly observed. Our experiment generated a complete data set on the relationship between true losses and claimed losses for both of the contract type treatments that were run. Of course, this is only of interest if the data from the experiment are actually relevant to real-world data, in which case it becomes of primary importance that the insurance market that is used in the experimental design retains as many of the features of real-world insurance markets that are relevant to the research question, which, in this case, is how different are the levels of fraud over different contracts.[6]

5. While there are a good number of empirical efforts to estimate undiscovered fraud (see Weisberg and Derrig, 1993; Derrig and Ostaszewski, 1995; Brockett, Xiaohua and Derrig, 1995; Belhadji, Dionne and Tarkhani, 2000; Viaene et al., 2002; Artís, Ayuso and Guillén, 2002; and Caudill, Ayuso and Guillén, 2005), any such exercise is fraught with difficulties and can never really claim to have measured the true level of fraud.

6. Nevertheless, it must be stressed that our objective was to measure fraud over different contracts, and *not* to compare the levels of fraud in our experiment to the levels of fraud in the real world. In order to study fraud at all, we needed an experimental design such that we could be relatively certain that fraud would indeed be committed. To that end, we offered contracts with a *built-in incentive for fraudulent claiming*, clearly not something that would be done in the real world. Thus, the levels of fraud in our experiment are likely to be significantly larger than the

With this in mind, the experimental design included the following features:

- Participants were allowed to freely choose the level of deductible, including not purchasing insurance at all.
- The loss environment for the insurable risks depended, in part, upon the personal skills and abilities of the insured individual, as well as upon an exogenous risk.
- The amount of fraud that an insured committed was kept as purely private information for that insured during the entire experiment (unless the case was an audit game, and the audit discovered fraud). That is, during the experiment no-one except the insured individual himself ever observed his/her fraudulent claiming behavior.

The insurance game that we used as our experimental design was the following: At stage 1 of the experiment, subjects were made to play an effort game four times over, which would be exactly the same game that would be used to determine their insurable loss in the posterior insurance game. The only difference is that, at stage 1, the effort game was used to allocate money to the subjects—money that later on was put at risk but where the risks could be insured. Specifically, the money earned in stage 1 was a decreasing function of the time they took to complete the game. In that way, the stage 1 tasks had a double objective: firstly, it was a manner in which money could be allocated to individuals in such a way that each individual would consider it money legitimately earned rather than just "house money"; and, secondly, it gave the individuals valuable experience in the task that they would have to perform later as an insurable risk (so they learned something about their ability level, and they developed a subjective estimate of their own private loss distribution).

Next, subjects were fully informed that they would have to replay the same effort game, again four times over, but that now they would suffer a monetary loss that would be an increasing function of the time taken to resolve the game. By making the insurable loss scenarios depend in part on the effort of the subject (greater effort translates to a lower loss), we wanted to avoid subjects feeling that a large loss was pure bad luck, or worse, purely unfair, as this may incite fraud (Tennyson, 1997). At this stage 2, the subjects were offered insurance products that they could purchase against their potential losses. Subjects were offered one of three choices: 1) a contract with deductible; 2) a contract with full coverage; or 3) to not insure at all. Again, by offering choice over insurance products, we wanted to simulate a real-world environment (free choice among contracts is always possible in real-world insurance markets), and we wanted to avoid the

levels of fraud in real-world insurance markets. However, the levels of fraud here are still comparable over the different contracts in exactly the same way as they would be in the real world.

feeling that a deductible contract that is imposed upon (rather than chosen by) the subject is unfair.[7]

There were two treatments in the experiment. In treatment 1, the insurance contracts offered had bonus-malus premiums for the first two rounds of insurance games, but with no audit system and, thus, no punishment for fraud. For the second two rounds of games in treatment 1, the contract was shifted to one in which the premium was fixed and an audit system came into force. The reason for the change to audit will be explained in detail below. In treatment 2, the contracts had fixed premiums and an audit system with punishment for uncovered fraud for all four rounds of insurance games. The premium due for the insurance product chosen was deducted from the subject's earnings from stage 1. Once they had chosen their insurance products, the subjects were made to play out the four effort games, now representing a loss to their stage 1 earnings. Subjects alone observed their true loss, and were able to submit a claim of any size to the insurer; that is, the only type of fraud possible was submitting a claim that is greater than the actual loss suffered.[8]

Finally, subjects' final payments were calculated, they were paid individually and they left. It is important to note that all subjects in the experiment were only identified to the experimenters by a number. The computer system calculated the final payoff to each subject number, and when they were paid, the person making the payments had no idea of how the payoff amount was generated (i.e., whether it involved fraud). The experiment gave a total guarantee to the subjects that their decisions were completely anonymous, only observed by them. In particular, only the subject himself or herself ever observed the true loss suffered in each insurance game.[9]

The essence of our design is the following. The only structural difference between the two treatments is in the first two rounds of insurance games. In treatment 1, these two rounds had bonus-malus premiums but no audit or

7. In a well-defined sense, this aspect of the experiment attempts to overcome the purely psychological effect of insureds finding fraud to be more acceptable when the contract is overpriced or unfair that is discussed by Tennyson (1997). If the insureds are given an unfettered voluntary choice of contracts, rather than being railroaded into a particular contract, they are more likely to be happy with the contractual conditions that they have freely chosen.

8. We specifically did not include any social loss resulting from fraudulent claiming. While fraud may lead to generally higher premiums for all subjects, this effect in any real-world setting is likely to be minimal to the extreme for any one policyholder (which, of course, is not to say that the sum of all the individual effects might not be a valid concern to all policyholders as an aggregate group, and, hence, a concern to insurance regulators). It is not plausible that individuals considering fraud would take it into account, and including such an effect in the design would have made the experiment significantly more cumbersome and costly to run in terms of cognitive effort by the subjects.

9. Of course, the true loss was revealed to the experimenters if an audit was carried out, as would be the case in a real-world audit. It is also true that, because all data was collected digitally, the experimenters also saw the true loss of each subject once the experiment had finished and all subjects had left. However, even then, the true loss was only associated with a subject's number, and never his or her name or identity.

punishment for fraud. In the second two rounds in treatment 1, the contract was switched to having a fixed premium and audits to control for fraud. On the other hand, in treatment 2, both the first two rounds and the second two rounds had constant premiums and audits; that is, treatment 2 had no bonus-malus component at all. The change from bonus-malus to audit in treatment 1 was included with the sole objective of being able to close off the bonus-malus game. Under a bonus-malus system, any fraud is "punished" only by an increased premium in the next round of play, and so there is an end-game problem: In any last period, no punishment is possible and fraud would be rampant. In order to overcome this end-game problem, we ran two extra rounds in treatment 1 using audit and not bonus-malus, because audit does not suffer from an end-game problem. It must be stressed that this change to audit was <u>not</u> included in order to capture some real-world feature, and although it was only a solution to an end-game problem, it turned out to generate some surprising results.[10]

Using this design, we are able to directly compare the levels of fraud in at least three particular ways:

- Most easily and directly, we can calculate the aggregate level of fraud over the four rounds of play in the two treatments. Because the treatments differ only in that one had a bonus-malus component, differences in the aggregate amount of fraud across treatments must be due to the inclusion of the bonus-malus component in treatment 1.
- By comparing only the first two rounds across treatments, we can see which contract type, bonus-malus or audits, does a better job at controlling for fraud. This comparison is in perfect equality of conditions, and the parameters of the games were set such that the comparison is perfectly meaningful.
- We can also use our design to consider how prior experiences in either a bonus-malus or an audit contract affects fraud in a latter contract with audit. This is done by comparing the second two rounds of games across the treatments (treatment 1 in this case is preceded by bonus-malus, while treatment 2 is preceded by audit).

We stress that the experiment is not interested at all in the question of decision-making in regard to insurance demand, only in the question of decision-making in regard to insurance fraud. We have already discussed the reason why we used loss scenarios that depend on both a random element and a subject's ability. Of course, by using this loss generating mechanism, we are unable to know

10. Of course, there are clearly situations in which the contract switch effect may be observed in the real world. An individual with a poor bonus-malus track record may voluntarily decide to switch to an audit contract, either within the same company or by changing companies. Our result around the contract switch may also be interpreted as providing possible policy advice to insurers. That is, if an insured's bonus-malus contract involves excessive claiming, then perhaps he/she should be moved to an audit contract. There is no reason at all why such a clause could not be written into a bonus-malus insurance contract.

exactly the probability of loss, or how the probability of loss is dependent upon the subject's perception of the stochastic element and the subject's ability. We also cannot know how factors such as risk aversion translate into different contract choices. However, we stress that *none of that is relevant to the decision that we are interested in studying*. Once a subject has an insurance contract, and once the subject has suffered a loss, all that is important for the fraud decision are the consequences of the fraud decision (governed by probability of audit or degree of increase in premium, level of fine, etc.) for that particular insurance contract. We have complete control over all of the parameters that are relevant for that decision. Thus, we have absolutely no need at all to know what a subject's ability in the effort task actually is, or what his subjective belief on the distribution of the difficulty of the effort task was.

It may also be argued that risk aversion is a relevant element when the fraud decision is made. We did not elicit risk preferences in the experiment, as doing so would have been an added task that would increase the duration and difficulty of the experiment for subjects, and which would have given us no additional information that would be of use for studying the question at hand. Our analysis is across treatments, and thus across subject pools. While it is true that risk aversion may determine whether a given individual decides to commit more or less fraud, we are not interested in attempting to model how much fraud a given individual alone commits as a function of relevant parameters. We are only interested in *the comparison of the average fraud of a group of individuals against the average fraud of another group of individuals in another treatment*. In essence, we are not interested in *explaining* individual fraud decisions, we are only interested in *measuring* fraud on average under two different treatments. Because all of our analysis is in averages over treatments, it makes little sense to wonder how risk aversion might intervene in the choices of individuals. All that is needed is that there not be any significant differences over the entire set of subjects in one treatment compared to the entire set of subjects in the other treatment. For example, if it were true that every subject in one treatment were uniformly more risk averse than every subject in the other treatment, then risk aversion would be a relevant aspect that would need to be controlled for. But we, as innumerable other experimental economists before us, make the assumption that by randomly assigning subjects to treatments, the general characteristics of the two populations are sufficiently similar for their choices to be comparable without recourse for controlling for specific characteristics.[11] Thus, we have no need for controlling for such things as individual risk aversion or ability in the effort task.

11. Indeed, we note that, in both of the treatments, subjects were offered the same two insurance contract formats (one full coverage and the other with a deductible). The decision to take a full coverage contract over the deductible one was taken by about 63% of the subjects in *both* treatments. Because risk aversion is certainly a relevant factor when choosing deductibles, it would seem that there was, *on average*, no significant difference in this characteristic over the two treatments. Likewise, we have no need to wonder about the possibility of different preference functionals over individuals (expected utility, prospect theory, rank-dependent utility, reference points, etc.), as at no point do we model individual fraud decisions. Because there is no

Table 1: Bonus-Malus Treatment

Game 1	*Game 2*	*Game 3*	*Game 4*
Bonus-malus	Bonus-malus	Fixed premium (audit)	Fixed premium (audit)

Table 2: Audit Treatment

Game 1	*Game 2*	*Game 3*	*Game 4*
Fixed premium (audit)	Fixed premium (audit)	Fixed premium (audit)	Fixed premium (audit)

Stage 2 Treatments

In treatment 1 (the bonus-malus treatment), the contract format that was offered to subjects contained a bonus-malus component. (See Table 1.) That is, the premiums that were paid for each insurance round depended upon the amount of the claim that the subject had submitted in the previous round. If a claim was submitted, the premium for the next game was increased. Of course, as mentioned above, there is a significant end-game problem with this treatment, because if the experiment were to end after round 2 with no next round in which the premium could be increased, we would find that fraud in round 2 would likely be extreme. For that reason, two more insurance rounds were added but with audits rather than bonus-malus. Thus, in treatment 1, the first-round of play had an initial premium, and the premium in round 2 was increased if a claim was submitted in round 1. The premium in round 3 was again increased if a claim was submitted in round 2, but the premium in round 4 was held at the level of round 3. The first two rounds had no audit mechanism at all, but in rounds 3 and 4 subjects faced the possibility of having their claim audited. The switch to audit contracts for the last two rounds avoids the end-game problem.

The second treatment is the audit treatment. (See Table 2.) In this treatment, again four rounds of insurance games are played in order to provide a meaningful comparison with the bonus-malus treatment, but now all four rounds are subject to auditing, and in no round does the premium paid depend upon the claim history. In treatment 2, the premium was held constant throughout the four games.

reason to suppose that individuals in one treatment were significantly different to those in the other treatment in as far as their preferences go, there is no need to include an analysis of preferences.

The Effort Task

Both of the stage 2 treatments were preceded by the money allocation stage, which was identical for both treatments. In this phase of play, the subjects had to carry out an effort task, and the money that they earned was an increasing function of their success in that task. Specifically, they had to type into their computer keyboards a sequence of 20 characters, against a stopwatch. The faster they managed to type the sequence correctly, the more money they earned. This effort task was exactly the same as was used in the insurance games, but in those games, the time taken to type the sequence correctly was the determining factor for the amount of the loss. The idea behind using the same kind of effort task for both phases was to ensure that both the gains and the losses depended on the subject's level of skill and ability, just like in real-world insurance problems (above all, for example, in automobile insurance), and also to give the subjects experience in their ability so that they could make an informed decision when deciding which insurance contract to purchase. We were also careful in giving each subject four *different* sequences, of different levels of difficulty, to type during stage 1, so that they understood that when the insurance games were played, there was an exogenous risk factor that determined the difficulty of the sequence that they would have to type.[12]

In order that there was the opportunity for not suffering any loss, there was a 10-second grace period for each sequence in stage 2. That is, money was only begun to be lost 10 seconds after the subject was shown the sequence and could actually start typing. Thus, if the subject typed the sequence in 10 seconds or less, the corresponding loss amount was set at zero.[13] The amount of loss was set at half of the time taken in round seconds to complete the sequence (less, of course, the 10-second grace period). In order to keep things finite, there was also a maximum limit set on the time for typing of 40 seconds (including the first 10 seconds). If the sequence had not been typed correctly by 40 seconds, then the loss was set automatically at 15 (i.e., at 40 seconds minus the 10-second period of grace, divided by two).

Before each stage of the experiment was carried out, subjects were given full instructions, both on screen and verbally of the whole experiment. Examples were given of the task that was about to be carried out, and subjects had the opportunity to ask questions and get further explanations. Before beginning a stage of the

12. The difficulty of the sequences was achieved by introducing more complex characters. For example, a simple sequence might be aaafffiiitttppbbbrrr, a sequence of medium difficulty could be something like wr3ifo900Pvm207Hfm1q, and a sequence of a rather high level of difficulty would contain more complex characters like Y&b,+?2eT}>39@.rS*/6. All characters of all sequences are visible on the Spanish computer keyboard.

13. By explicitly having the possibility of no loss, the fraud decision could be either a claim build-up (when a positive loss is exaggerated in the claim) or loss fabrication (when no loss occurred, but a loss is claimed). However, as we have already discussed above, loss fabrication (fictitious claiming) is nothing more than claim build-up from a point of zero true loss, and so the difference between these two types of fraud is minimal and unimportant at best.

experiment, all subjects were asked if they understood exactly what they would be asked to do, and the experiment did not proceed unless all subjects agreed that they understood.

Once stage 1 had finished, each participant was shown the details of their performances in the four sequences that they had just attempted, along with the amount of money that they had accumulated (which was between €44 and €56 for each participant). They were then told that they would move on to stage 2, in which they would have to resolve four more sequences, and it was clearly explained how their performance on those next four sequences would translate into losses against their accumulated funds. It was also clearly explained to them that, if they would like, before stage 2 began, they could purchase insurance against the possible losses that might be suffered. The contracts that were on offer were clearly explained, with several examples, and again subjects were given full opportunity to ask questions. The experiment did not continue until all subjects were comfortable with the insurance arrangements from which they would be able to choose. It was made clear to the subjects that, by not insuring, there was a chance that all of their money would be lost, but that by insuring they would always leave the experiment with some money.[14]

When stage 2 of the experiment was explained to the subjects, it was made clear that each subject alone would observe his or her true loss. No one else would know how much each subject actually lost, and that the subject was free to choose the amount to report to the insurer as the loss. However, we were careful not to use the term "fraud" in the explanations to avoid connotations of illegal activity in the minds of the subjects. This was done purposely so that the subjects got the idea that they could present fraudulent claims if they wanted. Of course, the experiment would not have been successful if no fraud was actually committed, and indeed we set the parameters of the experiment such that a strictly positive monetary gain (in expected value) was always possible by committing fraud.

Of the 154 subjects in the experiment, 99 did the bonus-malus treatment and 55 did the audit treatment. All but one subject purchased insurance of some type, and the great majority of subjects did commit fraud at least once.

Parameter Values

The parameters of stage 2 of the experiment were calculated such that the expected earnings from a false claim were equated over the two treatments. The parameters also satisfied the condition that, in expected value, a subject with a loss equal to the mid-point of the loss distribution, and who declares losses honestly, would always earn more by insuring than by not insuring, and this subject would also earn the same from the two insurance formats (deductible or full coverage).

14. We obviously wanted to avoid subjects not insuring. A non-insured subject by definition has no opportunity to commit fraud and, thus, would not add data of any use to us.

The experiment uses several parameters. At stage 1 of the experiment, subjects carried out four effort tasks involving copying a sequence of 20 characters correctly on their keyboards. Each subject began each effort task with €14. If the character sequence was correctly typed within 10 seconds, then nothing was deducted from that €14 budget. However, for each second beyond the first 10 seconds that the task took, the subject lost €0.1, up to a total of €11 if the subject was unable to copy the sequence in less than 40 seconds.

Hence, a subject's earnings in effort task *i* is given by:

$$M_i = 14 - 0.1T_i$$

where $T_i \in [0,30]$ is the time taken to complete the sequence (after the first 10 seconds).

After all four effort tasks of stage 1, the earnings of each individual was a number between €56 (for the fastest typists, who managed to copy all four sequences in less than 10 seconds each), and €44 (for the slowest typists, who took at least 40 seconds to type each sequence). Concretely, at the end of stage 1, the total earnings of each individual is given by:

$$M = \sum_{i=1}^{4} M_i = 56 - 0.1\sum_{i=1}^{4} T_i$$

In stage 2, once again, the subject is faced with the task of copying four character sequences correctly against a clock, exactly as in stage 1. The stage 2 game is exactly the same as the stage 1 game, with a 10-second grace period in which the subject may begin to copy the sequence without penalty, and then for each second that the task takes beyond the first 10 seconds, a loss of €0.5 was suffered (up to a maximum of 30 seconds; that is, a maximum penalty, or loss, of €15 per game). Hence, in stage 2, the losses were numbers between zero and €15 per game; that is, between zero and €60 in total over the four games.

In stage 2, each subject had the choice between not insuring, or purchasing one of two insurance contracts against the potential losses. The insurance contracts offered were exactly the same in both treatments. One was full coverage, in which the entire amount of loss would be paid back as indemnity, and the other had a deductible of €5 (any loss under €5 would not be insured, and losses $T > 5$ have an indemnity of only $T - 5$). We should recall that a loss of €5 implies that the individual has taken 20 seconds to copy the sequence of characters. For simple character sequences, this is reasonably easy to achieve, so there was a very real possibility that subjects understood that they could suffer losses below the deductible. Each type of contract was priced according to a couple of simple rules (explained below), but the premium for full coverage was greater than the premium for the deductible contract. During the phase in which the experiment was explained to subjects, several examples were given of how the contracts worked, and subjects were able to ask questions regarding the contracts. Once a contract was chosen, that same contract was used for the four loss scenarios in stage 2 of the experiment (i.e., subjects could not change their insurance arrangement during stage 2).

In the bonus-malus treatment, the first two games *G*1 and *G*2 of the total of four games (*G*1, *G*2, *G*3, *G*4) that make up the treatment, had bonus-malus contracts. That is, the premium paid in game 2 depended upon the claim made in game 1, and the premium paid in game 3 depended upon the claim made in game 2.[15] The claims presented in the first two games of this treatment were not subject to audit, but the claims made in the second two games of this treatment were subject to audit.

Thus, in this treatment there are four premiums,[16] one for each game, described by the vector $p^{bm} = (p_1^{bm}, p_2^{bm}, p_3^{bm}, p_4^{bm})$. The first ($p_1^{bm}$) is given exogenously, and the others are calculated endogenously as follows:

$$p_2^{bm} = p_1^{bm} + \lambda_1 C_1 \text{ and } p_3^{bm} = p_2^{bm} + \lambda_2 C_2 = p_1^{bm} + \lambda_1 C_1 + \lambda_2 C_2 = p_4^{bm}$$

where C_i is the claim presented in game *i*, for *i* = 1,2, and where $\lambda_i > 0$ for *i* = 1,2.

The contract in game 3 and game 4 is transformed into one with premiums that are independent of the claims presented, but in which claims may be audited. The probability of audit was set at 0.5 (decided by a publicly observed coin flip). If an audit was carried out, and if fraud was detected, then a fine was imposed upon the subject. The fine was calculated as a proportion γ of the amount of fraud committed, and the indemnity paid was equal to the indemnity due for the true level of loss, given the subject's contract.

One of the principal objectives of the experiment was to consider how claiming is affected by the type of contract (either bonus-malus or audit). In particular, which of the two contract types better controls for fraudulent claiming. To that end, it is necessary that the parameters of the experiment be set such that the comparison of fraud over the two contract types is meaningful and relevant. In order to achieve this, we set the parameter values so as to equate the expected financial gain from a false claim in each contract environment. The parameters were also set such that for an individual with an average loss of €7.5 per game (that is, a subject who on average takes 25 seconds to copy a character sequence,

15. The premium paid in game 1 was set at an initial value, and the premium paid in game 4 was equal to the premium in game 3.

16. Whether the premiums are actuarially fair depends on the probability distribution of losses, which is an individual characteristic that we had no interest in measuring. Surely, for some individuals, the premiums were loaded, and for others they could have been fair, or even better than fair. This would also be the case in any real-world insurance environment, when premiums are calculated on average population loss estimations.

which we consider to be a subject of average ability in the loss game),[17] and who is honest (i.e., who does not commit fraud), has a greater expected value in the game from insuring than from not insuring, and insuring with full coverage is equally attractive (in expected value) as insuring with the deductible.

There are eight fundamental parameters in stage 2 of the experiment:

1. The proportional increase in the premium suffered when a loss is claimed in game 1 in the bonus-malus treatment, denoted by λ_1.
2. The proportional increase in the premium suffered when a loss is claimed in game 2 of the bonus-malus treatment, denoted by λ_2.
3. The probability of inspection in the second two games of the bonus-malus treatment, and in all four games in the audit treatment.
4. The fine applied for discovered fraud in the second two games of the bonus-malus treatment and for all four games of the audit treatment, denoted by γ.
5. The initial premium (the premium paid in game 1) of the full coverage contract of the bonus-malus treatment, denoted by $p_{1,f}^{bm}$.
6. The initial premium (the premium paid in game 1) of the deductible contract of the bonus-malus treatment, denoted by $p_{1,d}^{bm}$.
7. The premium paid for the full coverage contract in each of the four games of the audit treatment, denoted by p_f^a.
8. The premium paid for the deductible contract in each of the four games of the audit treatment, denoted by p_d^a.

The values that were used for these eight parameters were the following:

1. $\lambda_1 = 0.3$.
2. $\lambda_1 = 0.45$.
3. Probability of audit = 0.5
4. $\gamma = 0.8$.
5. $p_{1,f}^{bm} = 3$.
6. $p_{1,d}^{bm} = 0.25$.
7. $p_f^a = 6.375$.
8. $p_d^a = 1.375$.

[17] It is quite surprising how our estimate of what is average ability actually corresponded to the reality of the experiment, where the average loss times were 25 seconds and 24.8 seconds in the two treatments, respectively!

Table 3: Descriptive Statistics, Bonus-Malus Treatment

Nº participants: 99.
Nº participants insured: 98 (98.99% of the total) [48 women (48.5%)]
Type of insurance
Full coverage: 62 (63.27%)
Deductible: 36 (36.73%)
Nº observations (4 x 99): 392
Nº insured participants with opportunity to commit fraud: 98 (100%)
Nº insured participants that never committed fraud: 22 (22.45%);
Women: 12 (54.55%); Men: 10 (45.45%)

	Ave.	*SD*	*Median*	*Min*	*Max*
Loss suffered (euros)	35.05	9.09	34.00	9.00	54.50
Claim (euros)	40.75	10.15	41.50	15.00	60.00
Possibility of fraud (euros)	24.74	8.76	26.00	5.50	40.00
Possibility fraud ÷ (60 or 40) (%)	48.95%	20.66%	47.50%	9.17%	100.00%
Fraud (euros)	7.15	6.68	5.25	0.00	27.00
Fraud ÷ Possibility fraud (%)	28.94%	26.41%	24.12%	0.00%	100.00%
Nº times with possibility of fraud	3.51	0.75	4.00	1.00	4.00
Nº times with possibility of fraud ÷ 4	87.76%	18.74%	100.00%	25.00%	100.00%
Nº times fraud	1.88	1.39	2.00	0.00	4.00
Nº times fraud ÷ Nº times possible (%)	51.02%	36.22%	50.00%	0.00%	100.00%
Under claiming	38	2.42%	of total claims		

With these numbers we achieve the following:[18]

- There is an incentive (in expected value, for an average subject) to insure.
- There is an incentive (in expected value, for an average subject) to commit fraud.[19]
- The incentive to commit fraud is (in expected value, for an average subject) equal over all the environments in our two treatments (contract with full coverage or with deductible in both bonus-malus and audit).

18. See the appendix for more details on the calculations behind these parameter values, and to see why they do indeed fulfill the stated conditions.

19. We purposely give an incentive to commit fraud, as without such an incentive we may not have much fraud to measure. All that this incentive does is to exaggerate the ability that exists in any real-world insurance environment of subjects to use the insurance contract for opportunistic behavior. However, because we are not interested in comparing our measured fraud with the real world, but, rather, only to measure fraud over two different treatments under conditions that replicate this feature, in as much as is possible reality, does not detract from our results. All insurance contracts, both in our experiment and in the real world, provide both a risk-reducing mechanism and an ability to earn income for opportunistic subjects.

Table 4: Descriptive Statistics, Audit Treatment

Nº participants: 55
Nº participants insured: 55 (100% of total) [27 women (49.09%)]
Type of insurance:
Full coverage: 35 (63.64%)
Deductible: 20 (36.36%)
Nº insured participants with opportunity to commit fraud: 53 (96.36%)
Nº participants that never committed fraud: 8 (15.09%); Women: 4 (50%); Men: 4 (50%)

	Ave	*SD*	*Median*	*Min*	*Max*
Loss suffered (euros)	38.45	10.89	39.50	20.00	60.00
Claim (euros)	46.98	9.74	48.00	27.00	60.00
Possibility of fraud (euros)	21.38	10.64	20.50	0.00	39.00
Possibility fraud ÷ (60 or 40) (%)	43.13%	25.59%	40.00%	0.00%	97.50%
Fraud (euros)	9.34	8.09	8.50	0.00	31.50
Fraud ÷ Possibility fraud (%)	45.29%	32.79%	44.44%	0.00%	100.00%
Nº times with possibility of fraud	3.16	1.01	3.00	0.00	4.00
Nº times with possibility of fraud ÷ 4	79.1%	25.4%	75.0%	0.0%	100.0%
Nº times fraud	2.30	1.31	3.00	0.00	4.00
Nº times fraud ÷ Nº times possible (%)	69.65%	35.96%	75.00%	0.00%	100.00%
Under claiming	8	0.91%	of total claims		

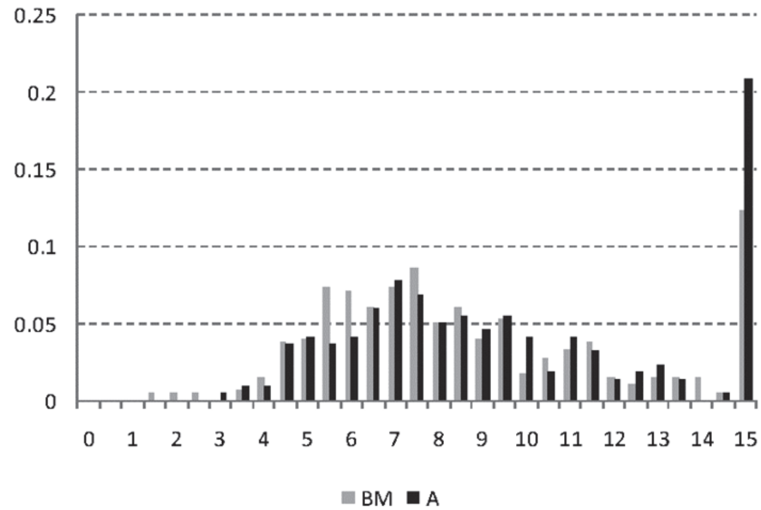
Summary of Results

The average earnings of the subjects was €18.21. (Recall that they only took about 45 minutes to carry out the experiment.)

We have collected a huge amount of data from the experiment, but, here in this paper, we will only report on the data concerning fraud. Specifically, we need to bear in mind that not all subjects, in each insurance game, actually had the opportunity to commit fraud. Some subjects had the maximum possible loss, and, in those cases, fraud becomes impossible. Thus, it is relevant to study the amount of fraud relative to the amount of fraud that is in fact possible.

The summary descriptive statistics from the experiment are reported in Table 3 and Table 4.

Figure 1: Relative Frequencies of Losses Suffered

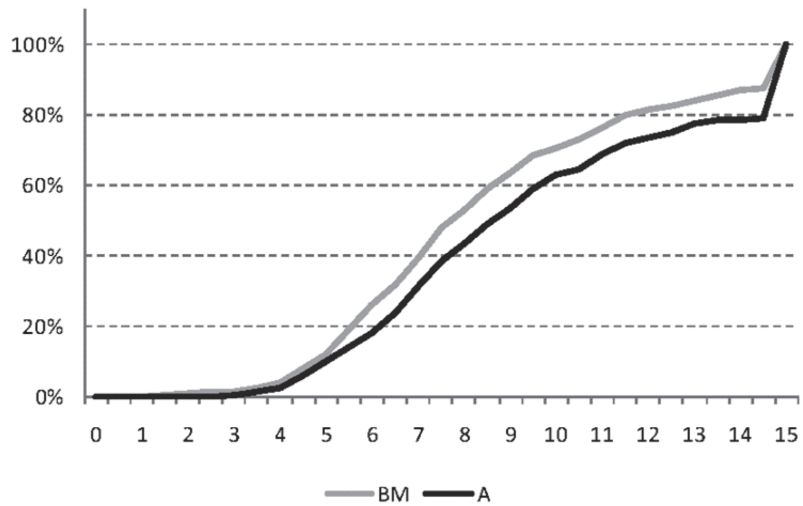


Of particular note, in both treatments, the split between full coverage and deductible contract format was almost identical, with about 63% of subjects choosing full coverage and 37% choosing the deductible. It is also notable that, according to statistics from MAPFRE (one of the principal insurers in Spain), the actual fraction of individuals who choose full coverage contracts in Spain in 2008 (the latest year for which we have statistics) is 66.02% (for men, 65.23%; for women, 67.72%), and the fraction actually choosing a deductible format is 33.98% (for men, 34.77%; for women, 32.28%).[20] Thus, at least in as far as contract choice goes, our experiment replicates reality to a high degree of approximation, which we feel gives credence to the fact that our experimental design does indeed provide a good reflection of reality.

The amount of fraud that is possible depends on the loss suffered and the insurance contract that a subject has. Say the true loss in any particular game is x, which is a number between zero and 15. Under a full coverage contract, claiming honestly involves claiming an indemnity of x. A fraudulent claim is a claim greater than x. But because the maximum loss is known to be 15, the greater is x the less fraud is possible (and, of course, if $x = 15$ there is no option for fraud at all). The greatest possible level of fraud in a full coverage contract is 15. On the other hand, under a deductible contract, a loss of x that is honestly claimed for gives an indemnity of 0 if $x < 5$ and an indemnity of $x - 5$ if $x \geq 5$. The possibility for fraud is again decreasing in the amount of true loss, and the maximum possible fraud is now 10 per game.

20. This data refers to automobile insurance, for claims against damage to the vehicle.

Figure 2: Cumulative Relative Frequencies of Losses Suffered Over the Two Treatments

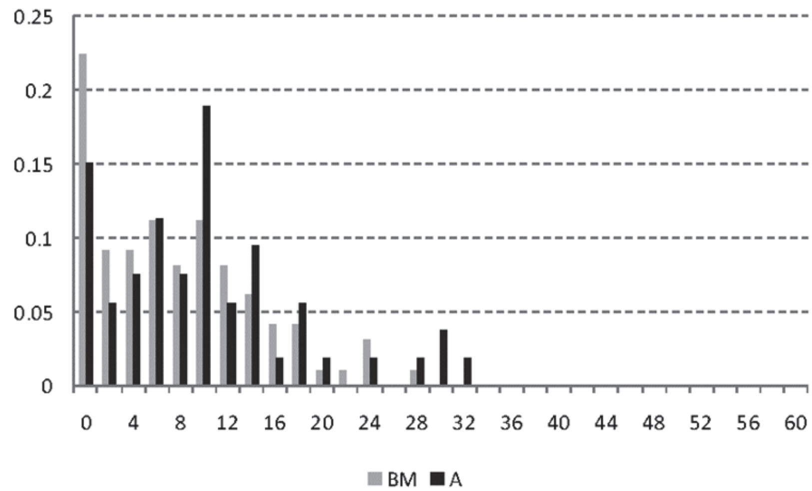


Interestingly, it turned out that in the bonus-malus treatment, there was a greater possibility for fraud than in the audit treatment. This outcome is pure chance: The subjects in the bonus-malus treatment were, on average, better at copying the character sequences than were the subjects in the audit treatment. However, it is also a result that favors our thesis. As will be shown, our results point to less fraud, on average, in the bonus-malus treatment, in spite of there being a greater possibility of fraud in that treatment.

Figure 1 shows the relative frequencies of the per game loss amounts suffered in the two treatments for all four games, from which we can infer the possibility for fraud.

The fact that there was a greater potential for fraud in the bonus-malus treatment can be even more easily seen in Figure 2, which shows the cumulative relative frequencies of losses per game suffered in each of the two treatments (the cumulative data from Figure 1). As can be seen, the curve corresponding to the bonus-malus treatment lies above the curve of the audit treatment, implying that, for any given loss amount, there was a greater frequency of subjects at or below that loss amount in the bonus-malus treatment.

Figure 3: Relative Frequency of Total Fraud Committed



Analysis of Fraud in the Aggregate

Against the graphs of the possibilities of fraud as indicated by losses suffered, we have in the next two graphs an indication of the total amount of fraud that was indeed committed in aggregate (i.e., over the four games in each treatment). In Figure 3 and Figure 4, we show each level of fraud that was committed, expressed as a percentage of total claims, over the entire four-game sequence for each treatment.

In Figure 3, we can see each level of fraud that was committed in each of the two treatments expressed as a percentage of the aggregate amount of claims. The information in Figure 3 is displayed in Figure 4 in cumulative terms. We can see in Figure 4 that there was less fraud, on average, in the bonus-malus treatment than in the audit treatment, because, for any given amount of fraud, a higher percentage of subjects committed that level of fraud or less in the bonus-malus treatment.

We also report the amount of fraud that was committed in each treatment in aggregate relative to the amount of fraud that was possible, conditional upon fraud actually being possible (i.e., conditional upon the loss that is actually suffered being less than the maximum possible loss). This is done in Figure 5 and Figure 6. To understand Figure 5, take the columns at 0.5 on the horizontal axis. The audit treatment column has height 0.15, indicating that a proportion 0.15 (15%) of all claims where fraud was possible had a level of fraud committed that was 50% of the total amount of fraud that would have been possible to commit.

Figure 4: Cumulative Relative Frequency of Total Fraud Committed

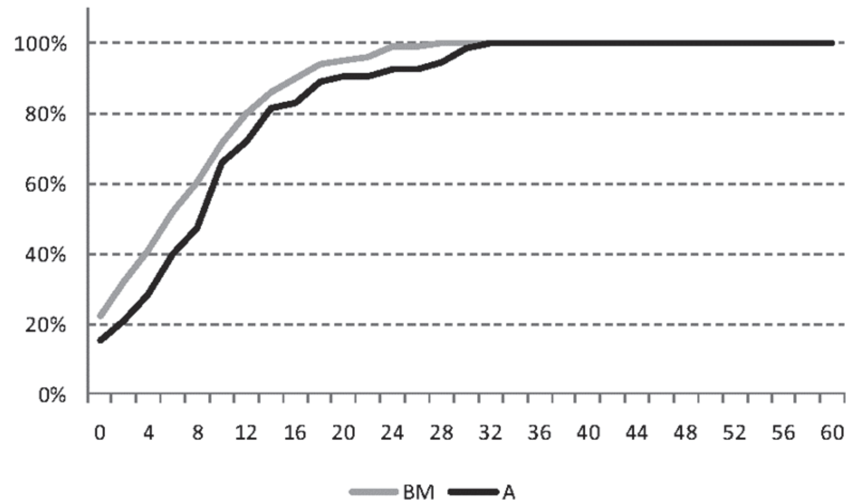


Figure 5: Relative Frequency of Fraud Committed as a Percentage of Fraud Possible, When Fraud Was Possible

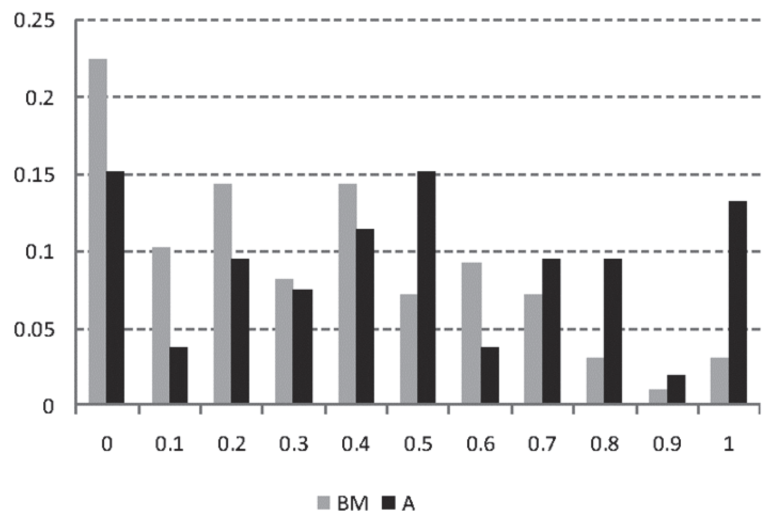
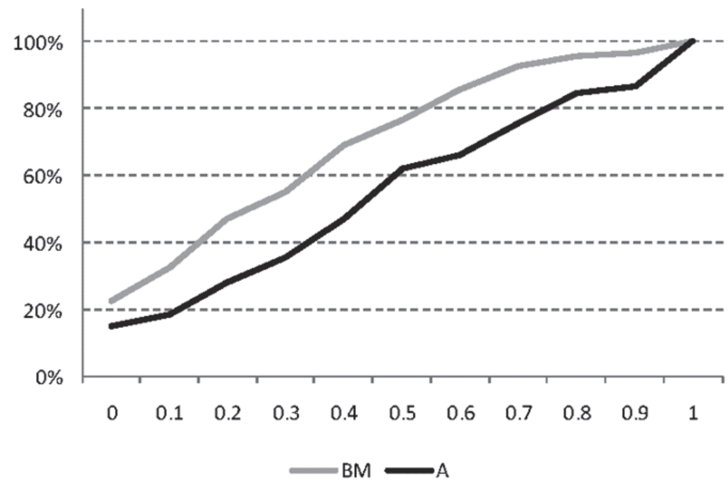


Figure 6: Cumulative Relative Frequency of Fraud Committed as a Percentage of Fraud Possible, When Fraud Was Possible



Once again, we see that, on aggregate, there was significantly less fraud relative to the level of fraud that was potentially possible in the bonus-malus treatment as compared to the audit treatment. Of course, this is to be expected becuase there was both a greater possibility of fraud, as well as less fraud committed in the bonus-malus treatment. However, it is more relevant to compare fraud committed to fraud possible, rather than just to look at the absolute amounts of fraud alone.

Disaggregated Analysis of Fraud

Table 5 reports the results of the experiment in relation to fraud disaggregated by games. The white columns are the number of times that fraud was committed as a percentage of the times in which it was possible to commit fraud (conditional upon fraud indeed being possible). The grey columns report the monetary amount of fraud committed as a percentage of the total amount of fraud that was possible to commit (again conditional upon fraud being possible).

The data on the averages from the above two tables are also collected in the following two graphs. (See Figure 7 and Figure 8.)

Table 5: Fraud Committed by Game

Bonus-Malus Treatment

	Game 1 (BM)		Game 2 (BM)		Game 3 (Audit)		Game 4 (Audit)	
	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud
Average	62.65%	40.38%	58.14%	35.82%	39.08%	18.64%	54.55%	31.95%
Median	100.00%	28.57%	100.00%	16.72%	0.00%	0.00%	100.00%	10.00%
SD	48.67%	41.69%	49.62%	40.86%	49.08%	30.04%	50.08%	38.97%

Audit Treatment

	Game 1 (Audit)		Game 2 (Audit)		Game 3 (Audit)		Game 4 (Audit)	
	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud	Times Fraud	Quantity Fraud
Average	54.76%	29.01%	72.34%	48.03%	80.49%	51.79%	72.73%	49.01%
Median	100.00%	15.97%	100.00%	45.45%	100.00%	46.67%	100.00%	49.50%
SD	50.38%	34.87%	45.22%	40.02%	40.12%	39.44%	45.05%	42.56%

Figure 7: Fraud Quantity as a Percentage of Fraud Possible, Conditional Upon Fraud Being Possible

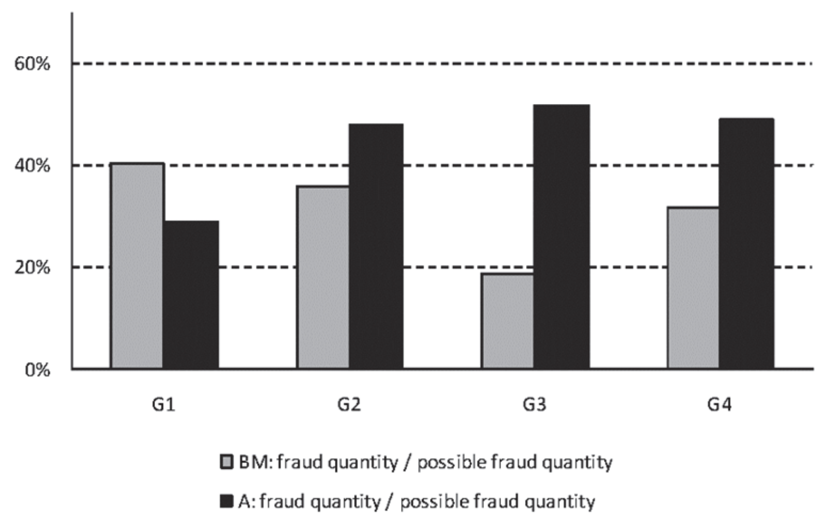
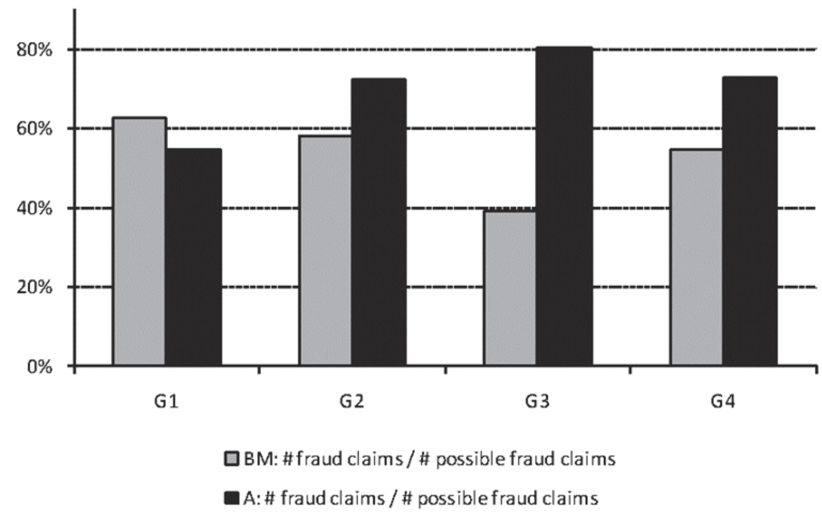


Figure 8: Number of Fraudulent Claims as a Percentage of Possible Fraudulent Claims, Conditional Upon Fraud Being Possible



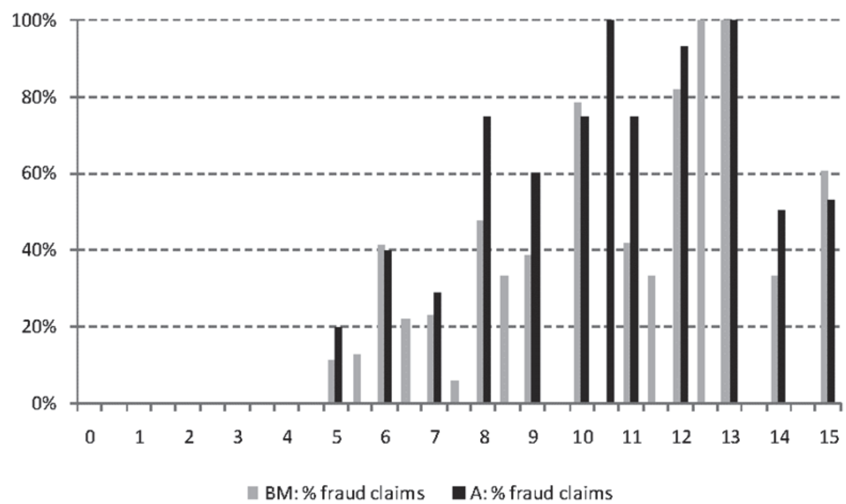
We note that when looking at the averages alone, with the exception of the first game, the levels of fraud in the bonus-malus treatment are always lower than those of the audit treatment. This observation leads us to believe that there does indeed exist a tempering effect upon fraud that is found in bonus-malus contracts as compared to audit contracts, a result that might be of interest to insurance regulators interested in reducing fraud. Indeed, when we look at the data from the two treatments aggregated over each block of two games (game 1 and game 2 on the one hand, and game 3 and game 4 on the other), and over the entire sequence of four games, we see (in Table 6) that the average amounts of fraud committed as a percentage of the total possibility of fraud, is always lower for the bonus-malus treatment than for the audit treatment.[21]

21. As an aside, it is also interesting to note that, in spite of the result from Tennyson (2002) that a greater experience with the insurance market (and the claims process) leads to a lower tolerance for fraud, in the pure audit treatment, the more experienced are the subjects (on average), the more prevalent is their average fraud (with the exception of the move from game 3 to game 4, where a small decline is seen).

Table 6: Euro Amount of Fraud as a Percentage of Possible Fraud

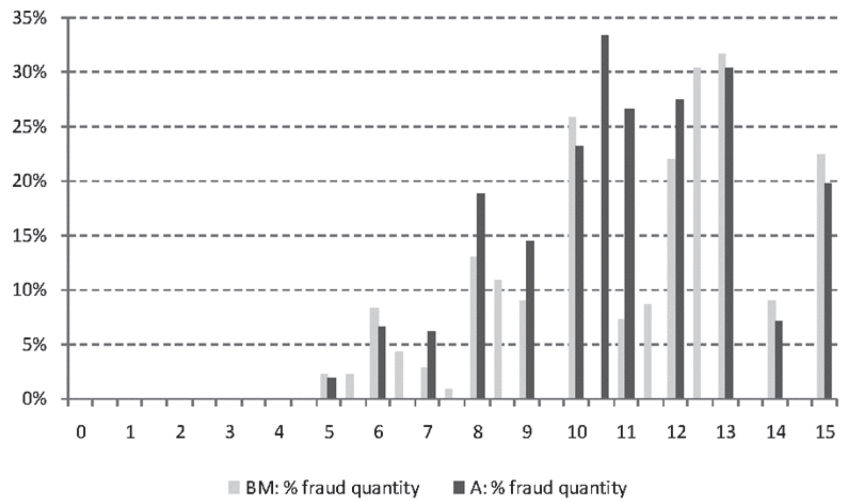
	Games 1 and 2	Games 3 and 4	Games 1 to 4
Bonus-Malus Treatment	36.98%	25.22%	28.94%
Audit Treatment	41.28%	49.40%	45.29%

Figure 9: Number of Cases of Fraud as a Percentage of Number of Claims



Nevertheless, the averages reported in this table for game 1 and game 2 across the two treatments are not statistically different at the 5% level (even though the average in the bonus-malus treatment is smaller), as is also the case for the averages for game 1 and game 2 and for game 3 and game 4 within the audit treatment. However, the average for game 3 and game 4 in the bonus-malus treatment is statistically significantly smaller (again, at the 5% level) than both the average for game 1 and game 2 in the bonus-malus treatment, and for the average of game 3 and game 4 in the audit treatment. Finally, the average overall of the games (game 1 through game 4) in the bonus-malus treatment is also statistically significantly smaller (at the 5% level) than the average overall of the games in the audit treatment.

Figure 10: Quantity of Fraud as a Percentage of Total Quantity of Claims



We also looked at the amount of fraud committed in function of the level of claim. This is interesting because, had this been a real-world scenario, the only observable data point in each game for each subject would have been the level of claim submitted (except, of course, in the cases in which an audit is randomly carried out). Thus, we wonder if the level of claim alone discloses any information on the level of fraud. The results are displayed in the following two graphs. Take Figure 9, which shows the data on the number of cases of fraud. A column at a claim of x shows the percentage of total claims of size x that were fraudulent. In Figure 10, we see the data on the quantities of fraud. A column at claim size x in this graph, shows the percentage of the total amount of claims (each of size x) that were fraudulent.

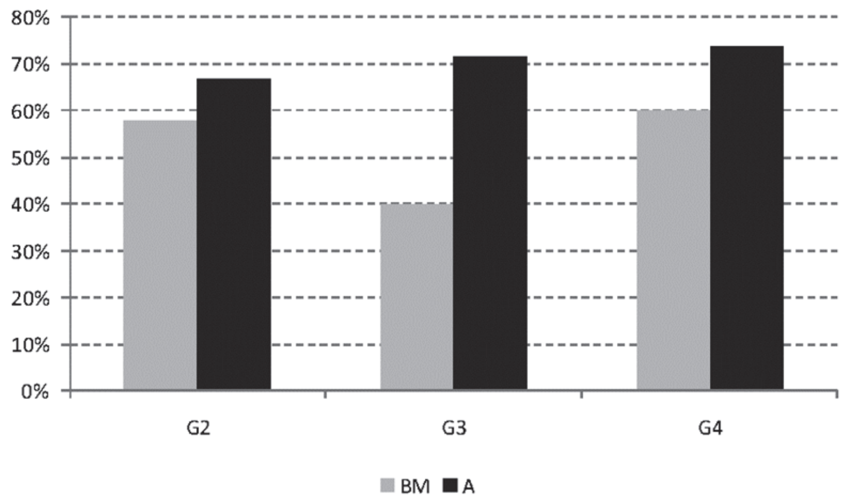
From these two graphs, we can see that (at least as a first approximation), the greatest chance that a claim is fraudulent appears to happen when the claim is (more or less) between about 10 and about 13. We also note that there is no obvious difference over the two treatments in relation to the information on fraud that is disclosed by the level of the claim submitted.

Another way in which our experimental results point to the differences in the incentives for fraud offered by bonus-malus and audit contracts is the following. Behaviorally, it appears from our results that subjects' reaction in terms of fraud in a given period may depend upon what happened in the previous period. In our experiment:

- In the bonus-malus treatment, game 2, the percentage of cases in which fraud was committed by those subjects with both the possibility to commit fraud and who had suffered an increase in premium from game 1 was 57.83%.
- In the bonus-malus treatment, game 3, the percentage of cases in which fraud was committed by those subjects with both the possibility to commit fraud and who had suffered an increase in premium from game 2 was 39.76%.
- In the bonus-malus treatment, game 4, the percentage of cases in which fraud was committed by those subjects with both the possibility to commit fraud and who had been audited in period 3 was 60.00%.
- In the audit treatment, game 2, the percentage of cases in which fraud was committed by those for whom fraud was possible and who had been audited in game 1 was 66.67%.
- In the audit treatment, game 3, the percentage of cases in which fraud was committed by those for whom fraud was possible and who had been audited in game 2 was 71.43%.
- In the audit treatment, game 4, the percentage of cases in which fraud was committed by those for whom fraud was possible and who had been audited in game 3 was 73.68%.

As can be seen in these numbers, it would appear that there is a clear difference between a bonus-malus contract and an audit contract in terms of what happens in the game previous to the current game. If an individual in a bonus-malus game claims a loss in game *i*, that individual appears to be less likely to submit a fraudulent claim in game *i*+1. On the other hand, if an individual in an audit game is audited in game *i*, that individual appears to be more likely to submit a fraudulent claim in game *i*+1. The data is presented graphically in Figure 11.

Figure 11: Percentage of Cases in Which Fraud is Committed, Conditional Upon Fraud Being Possible, and Upon Having Been Either Audited or Having Had a Premium Increase in the Immediately Preceding Game



Looked at from this perspective, the data appears to be rather clear. The increase in premium from a bonus-malus contract has led to a decrease in the incidence of fraud from 57.83% to 39.76%, while the audit has the opposite effect, increasing fraud from 39.76% to 60% in the bonus-malus treatment, and from 66.67%, to 71.43% and then to 73.68% in the audit treatment.[22]

Conclusions

The results of our experiment do not support the hypothesis that in a pure bonus-malus scenario there is any more or less fraud than in a pure audit scenario, because the across treatment comparison of the first two games does not reject (at the 5% level of significance) the null hypothesis that the average levels of fraud committed are, in fact, different.[23]

22. One possible explanation for this effect, at least in as far as the audit games are concerned, is that conditional upon having just suffered an audit, subjects underestimate the probability of suffering another audit. That is, a subject who was just audited believes (erroneously) that it is now less probable that a second audit will occur, and thus is willing to commit a greater level of fraud.

23. We conjecture that if the bonus-malus sequence had been longer than only two sequential games, the difference in the levels of fraud over the pure bonus-malus games

However, interestingly, the experiment does support the hypothesis that, when an insurance consumer has an audit contract, a prior experience in bonus-malus contracts does significantly reduce the incidence of fraud compared to only having had prior experience in audit contracts. Thus, the presence of a bonus-malus component in an insurance consumer's relationship with the insurer appears to have a significant tempering effect upon the amount of fraud committed. This result may be of interest to insurance regulators; i.e., if insurers are made to offer their consumers a bonus-malus element in their contracts early on, then fraud may be reduced in later audit-type contracts.

Finally, the experiment also supports the hypothesis that when deciding their level of fraud in period i, insurance consumers might take into account what has happened in period $i - 1$, and, in this case, it certainly appears that bonus-malus premium increments in period $i - 1$ are much more effective in curtailing fraud in period i than are audits in period $i - 1$.

The regulatory implications of our results relate to the structure of contracts that should be offered to insurance consumers, above all their intertemporal structure as periods go by. It normally happens that fraud by one insurance consumer negatively impacts the other consumers contracted by the same insurer, as the insurer raises premiums to cover for the losses from fraud. Thus, it is unlikely that insurers suffer all of the final consequences of fraud as reduced profit. In that case, insurers may not have a strong incentive to try to reduce fraud, especially if it is costly for them to do so. In the end, those who are most disadvantaged may be the rest of the insurance consumers, who are worse off when fraud by others happens than if less fraud were to occur. Therefore, there is scope for external regulation of exactly what the intertemporal structure of contracts should be.

Our results suggest that there is benefit to the industry of making a bonus-malus component compulsory during contract renewals early on in the relationship between the individual and the insurer. Also, because our research has focused on the behavior of insurance consumers, we have not taken into account the costs to insurers of auditing claims, which are, of course, important. Our research suggests that one can obtain at least similar fraud savings using bonus-malus and no audit, as can be achieved using a costly audit process (and no bonus-malus). Thus, again it appears that having a mandatory bonus-malus component might well be efficient.

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compared to the pure audit games would have been significant. This conjecture is based upon the observation in our data that the fraud in the two pure bonus-malus games followed a decreasing trend, while the fraud in the pure audit games followed an increasing trend.

Of course, the actual values of fraud that are observed in our experiment may not be directly comparable with those that might happen in the real world, because we are limited in our experimental design to relatively small losses, purely monetary losses and possibly a rather high probability of loss. However, they do tell us important information about fraud. We have also deliberately included within the contracts that we offered an incentive for fraud, in order that some fraud should be committed. Our objective is to compare fraud across two different contract formats, not in reducing the absolute amount of fraud that happens. To that end, the parameter values that we have had to choose for the contracts offered were dictated *only* by the need for the incentives offered under each contract to be as comparable as possible, and never for the parameters to be comparable to any real-world scenario. It would thus be erroneous to attempt to conclude from our experiment how much real-world fraud actually happens, or to attempt to provide clear guidance as to the parametrical structure of particular insurance contracts in the real-world. Nevertheless, we can conclude from the experiment that there appears to be a difference between bonus-malus contracts and pure audit contracts in as far as the level of fraud that each incites is concerned. This is the overriding message that our analysis provides.

Appendix

On the calculation of the parameter values in the experiment

We define an "average" subject to be one for whom the expected loss is[24]

$$P_E = 7,5 \text{ euros}$$

(This implies that the subject takes, on average, 25 seconds to complete each character sequence correctly.) If the subject does not insure, then the total expected loss over the four games would be:

$$PT_E = 4P_E = 30$$

Let us now assume that the subject does decide to purchase insurance, and that he or she does so under a full coverage contract. Suppose further that the subject is honest in each of the four games (i.e., does not commit fraud). In this case, the subject claims exactly the loss suffered, and, under a full coverage contract, this will also be his or her indemnity. Therefore, this subject expects to claim an indemnity of $I = P_E = 7,5$ in each of the games. The total cost of this will be the sum of the premiums that are paid over the four games.

$$CT_{CC} = p_{CC}^1 + p_{CC}^2 + p_{CC}^3 + p_{CC}^4 = p_{CC}^1 + p_{CC}^1 + \lambda_1 P_E + 2(p_{CC}^1 + \lambda_1 P_E + \lambda_2 P_E)$$

Now, what if the subject decides instead to buy the deductible contract (which has a deductible of 5). Again, assume that the subject does not commit fraud. Now the total cost will be:

$$\begin{aligned} CT_F &= p_F^1 + \min(5, P_E) + \left(p_F^1 + \lambda_1 (P_E - 5)_+ + \min(5, P_E)\right) + \\ &+ 2\left[p_F^1 + \lambda_1 (P_E - 5)_+ + \lambda_2 (P_E - 5)_+ + \min(5, P_E)\right] = \\ &= 20 + p_F^1 + \left(p_F^1 + 2,5\lambda_1\right) + 2\left[p_F^1 + 2,5\lambda_1 + 2,5\lambda_2\right] \end{aligned}$$

[24] Our estimation was reasonably correct, because the average loss per game over the entire experiment was €8.77.

The two contract formats are equally desirable compared to the option of not insuring if:

$$CT_{CC} = CT_F < PT_E$$, that is, if:

$$20 + 4p_F^1 + 7{,}5\lambda_1 + 5\lambda_2 = 4p_{CC}^1 + 22{,}5\lambda_1 + 15\lambda_2 < 30 \qquad [1]$$

We now look at the expected earnings when fraud is committed under a full coverage contract. As above, assume that the average subject suffers a loss per game of $P_E = 7{,}5$, but that now he claims $R \geq 7{,}5$. If this fraud happens in the first, second, third or fourth game, the expected earning is, respectively:

$$B_{CC}^1(R) = (R - 7{,}5) - 3\lambda_1(R - 7{,}5) = (1 - 3\lambda_1)(R - 7{,}5)$$
$$B_{CC}^2(R) = (R - 7{,}5) - 2\lambda_1(R - 7{,}5) = (1 - 2\lambda_1)(R - 7{,}5)$$
$$B_{CC}^3(R) = B_{CC}^4(R) = -p\gamma(R - 7{,}5) + (1 - p)(R - 7{,}5) = (1 - p - \gamma p)(R - 7{,}5)$$

In the same way, the expected gain from committing fraud in a deductible contract in, respectively, the first, second, third or fourth game (this is the gain with respect to the option of no fraud, under which the indemnity would have become $I = R - 5$ instead of $I = 7{,}5 - 5 = 2{,}5$) is:

$$B_F^1(R) = (R - 7{,}5) - 3\lambda_1(R - 7{,}5) = (1 - 3\lambda_1)(R - 7{,}5)$$
$$B_F^2(R) = (R - 7{,}5) - 2\lambda_1(R - 7{,}5) = (1 - 2\lambda_1)(R - 7{,}5)$$
$$B_F^3(R) = B_F^4(R) = -p\gamma(R - 7{,}5) + (1 - p)(R - 7{,}5) = (1 - p - \gamma p)(R - 7{,}5)$$

Thus, the incentive to commit fraud is the same in each game (and the incentive is also positive) if:

$$1 - 3\lambda_1 = 1 - 2\lambda_2 = 1 - (1 + \gamma)p > 0 \qquad [2]$$

The parameters of the experiment need to fulfil the two conditions [1] and [2]. We take $p = 0{,}5$ so that the audit lottery can be easily understood and carried out, and so condition [2] becomes:

$$3\lambda_1 = 2\lambda_2 = 0{,}5(1 + \gamma) < 1$$

which clearly holds for $\lambda_1 = 0{,}3$, $\lambda_2 = 0{,}45$ and $\gamma = 0{,}8$.

With these same parameter values, condition [1] becomes:

$$20 + 4p_F^1 + 2{,}25 + 2{,}25 = 4p_{CC}^1 + 6{,}75 + 6{,}75 < 30$$

that is:

$$4p_F^1 + 11 = 4p_{CC}^1 < 16{,}5$$

which again holds with $p_{CC}^1 = 3$ and $p_F^1 = 0{,}25$.

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Cummins, J. David and Richard A. Derrig, eds., 1989. *Financial Models of Insurance Solvency*, Norwell, Mass.: Kluwer Academic Publishers.

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