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Demand for Multi-Year Catastrophe Insurance Contracts: Experimental Evidence for Mitigating the Insurance Gap

Abstract

People often fail to insure against catastrophes, even when insurance is subsidized. Even when insuring homes, many homeowners still underinsure the full value of their assets. Some researchers have suggested using long-term insurance contracts to reduce these insurance gaps. We examine insurance decisions in a computer-administered experiment that makes several contributions to our understanding of insurance decisions. First, we provide additional evidence showing that many people prefer long-term insurance. Offering this type of insurance may thus increase insurance penetration. Second, we find that underinsurance can result from the reluctance to update the sum insured if there are costs involved with this updating. Long-term insurance contracts that automatically consider price changes over time can thus also deliver a reduction in the insurance gap. Third, we find that once people have made a decision, they tend to repeat it, demonstrating a strong preference for the status quo. Our research suggests that using this status quo bias may allow insurance companies to further increase insurance demand. As previously demonstrated, our results confirm that subsidies are ineffective in increasing insurance penetration.

JEL-Codes: C910, D810, G220.

Keywords: individual decision-making, choice under risk, disaster insurance, underinsurance, status quo, subsidies.

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https://sites.google.com/view/thomasdudek/research

1 Introduction

Many homeowners are not fully insured against catastrophic natural hazards such as earthquakes, floods or storms. In many cases, only a minority of owners are covered when a large disaster hit a community or a city. Indeed, even in high-income high-insurance-penetration countries like the United States, many hazard risks are underinsured. For example, fewer than 13% of homes are covered for earthquakes in earthquake-prone California (Nguyen & Noy, 2020), and only about 30% of homeowners in the city of Houston were insured when their homes were flooded by Hurricane Harvey in 2017 (Smiley et al., 2021). This insurance gap is not a uniquely American phenomenon; indeed, it is true globally for practically any catastrophic natural hazard that is not insured by a mandatory public insurance scheme. Since there are many reasons why mandatory full-cover public programs are enacted, a significant policy effort has been directed at narrowing this insurance gap through voluntary means.

The existing literature has established that both supply and demand constraints are associated with these insurance gaps. Universally, supply constraints are overcome through either public insurance schemes (e.g., the Earthquake Commission in New Zealand), public-private partnerships (e.g., the National Flood Insurance Program in the US), or regulation that forces insurance companies to offer specific insurance contracts (e.g., the FloodRE program in the UK). Here, in contrast, we focus on the demand for insurance, and test several policy instruments that have been proposed to narrow this insurance gap. In particular, we conduct a set of experiments that aims to investigate the efficacy of these proposed instruments by simulating insurance decisions that homeowners typically must consider.

The first policy we examine is long-term contracting. Researchers have proposed that offering long-term (i.e., multi-year) insurance contracts may increase insurance take up. It may do so because people may value the certainty multi-year contracts provide, and also because it prevents contracts from inadvertently, or maybe purposefully, lapsing (Kleindorfer et al., 2012; Kunreuther & Michel-Kerjan, 2015; Papon, 2008). Kunreuther and his colleagues have also suggested that multi-year insurance may increase take-up by catering to people with diverse preferences. As such, it will potentially increase the pool of people who might consider purchasing insurance.

The second policy instrument we examine are insurance premium subsidies. Subsidies have similarly been proposed to increase insurance take-up by making insurance more affordable. Some governments indeed offer catastrophe insurance at subsidized rates – one notable example is the previously mentioned National Flood Insurance Program in the US. However, the evidence on the effectiveness of subsidies is frequently questioned as some have argued that subsidies only have a minor impact on insurance take-up decisions (e.g., Kousky & Kunreuther, 2014; O'Donoghue, 2015).

Another issue that raised concern is that the amount of cover is often lower than the value of assets at risk so that the insurance policy does not fully cover homeowners' losses from a catastrophic event (Holzheu & Turner 2018). One possible reason for this under-insurance is that the value of assets increases over time, but often the amount covered by an insurance contract when it is rolled-over does not increase sufficiently to account for the increased value of assets or the increased costs of reconstruction. Thus, the third policy instrument we examine is a way to keep the insured amount updated over time to reflect any change in asset values.

We study insurance choices and the associated policy interventions in a computer-administered online experiment. In it, participants were asked to assume the position of homeowners. They first provide information about their risk attitudes and personality and then are asked to make repeated insurance purchase decisions. The design of the experiment allows participants to choose from three insurance options: no insurance, insurance for one year, or a fixed-cost insurance for two years. Disasters are triggered randomly with a known probability of occurrence (10 percent each year). Additionally, participants may experience an increase in their house value after the first year (with a known probability of 50 percent). This random increase of the house value may lead to underinsurance in the second year if the participants choose not to update their sum-insured. Participants have the choice to have their house value assessed after the first year of each round, which, if chosen, also automatically updates their sum-insured to correspond with this new assessment.

Our experiment thus looks at three interventions in the market for insurance that can potentially narrow the insurance gap: long-term insurance, premium subsidies, and updates of the sum-insured. This study makes several contributions. First, we find a strong preference among our participants for long-term insurance contracts. Almost one half of the insurance periods were covered by multi-year insurance contracts. Second, we find that subsidies do not affect insurance take-up at all. Third, we find a significant and robust effect of the first period's insurance choice on the following periods' insurance choices. This observation may be a reflection of an inertia bias (see Handel (2013) for health insurance), or it may be the case that people consistently make choices that are commensurate with their true preferences. Last, we show that many people are not willing to pay for a house value assessment to update their contractual sum-insured, even if it is available to them at a highly subsidized price. Consequently, some of our participants end up underinsured (i.e., a sum-insured below the maximal potential damage) in the second year. Over time, this reluctance to update the insurance cover may lead to significant and growing underinsurance.

In the next section we discuss the related literature more thoroughly, highlight our contributions and the hypotheses we test. In section 3, we describe the details of our insurance experiment. In sections 4 and 5, we describe our results, and the last section presents our policy conclusions and some caveats about the interpretation of our findings.

2 Related literature and contributions

Multi-year insurance contracts may have several advantages, as discussed by Kunreuther (2008) and Kleindorfer, Kunreuther, and Ou-Yang (2012): Long-term home insurance may encourage loss-mitigating investments by the owners by locking-in discounts for implementing them; reduce transaction costs for both the owners and the insurer; reduce the owners' uncertainty about potential premium increases or insurance withdrawal because of the insurer's re-assessment of the risk; reduce the cost of re-insurance; and with these it may also reduce the need for public financial assistance after a loss event to those who were under- or non-insured. Multi-year insurance may also be in the interest of insurers, as it locks in the demand for their product for multiple periods. Furthermore, in competitive markets that offers both types of insurance contracts, (1) some *current clients* with varying preferences might prefer to purchase long-term over short-term contracts; (2) some *potential clients* might choose long-term insurance contracts if those were offered, whereas in the current system they rather stay uninsured; and (3) *insurers* may benefit from cost-reductions in marketing and administrative costs and from an increased diversification among the home insurers if the offering of long-term insurance caters to people with a preference for such contracts who might otherwise reject insurance altogether.

Experimental studies similar to ours have found that many choose to purchase long-term contracts even if those are priced above the actuarially fair premium (Kunreuther & Michel-Kerjan, 2015; Papon, 2008). Their results indicate that people tend to have more periods covered with insurance (i.e., leading to a higher overall take-up rate), and many buy long-term insurance instead of annual insurance if such contracts are offered in addition to annual contracts. A preference for multi-year insurance has also been found by Doherty et al. (2021), who studied farmers' demand for insurance with a discrete choice experiment.

These and other experimental studies have shown that premium loadings may reduce insurance take-up overall, which is not always observed in real-world data (Hendel & Lizzeri 2003; Jaffee et al., 2010; Kleindorfer et al., 2012; Robinson et al., 2019). However, few experimental studies have specifically explored the opposite, the effect of premium subsidies on insurance demand, even though many governments offer subsidized contracts (Kunreuther & Pauly 2004; Marquis et al., 2004; O'Donoghue, 2015; Stabile, 2001). Previous findings, mostly based on survey data, suggest that insurance take-up is weakly associated with insurance subsidies. Yet, it is difficult to decompose the effect of subsidies on insurance take-up from survey data since the decision (not) to insure could also have been influenced by many other factors that could not be controlled for or observed. Experimental studies are better-suited to identify, in a more controlled environment, whether subsidies are effective and economically viable policy instruments to increase insurance take-up rates.

This paper thus contributes to our understanding of the effectiveness of subsidies on overall insurance take-up and on long-term insurance take-up specifically. Some experimental studies that explored the effect of premium differences on insurance take-up show a weak responsiveness of the cost of premiums on insurance take-up. A small sample experimental study Laury et al. (2009) shows that people respond to premium loadings (ranging from 80% to 400% of actuarially fair insurance). This study, however, was not "designed to elicit the critical values for load required to run the horse race between alternative choice models" (Laury et al., 2009, p. 37). Robinson et al. (2019) also experimentally investigated the effect of discounted insurance premiums framed as government compensations and found no significant difference between actuarially fair and subsidized insurance (50% and 75% of actuarially fair price). They found that people primarily responded with fewer insurance purchases when the insurance loading was very high (400% of actuarially fair price). Overall, the effect of government compensations appears small, suggesting that subsidies have little effect on overall insurance demand. Another recent experimental study has approached this question by studying premium loadings and found that people are unresponsive to such loadings (Osberghaus & Reif, 2021). None of these studies looked at multi-year insurance contracts. Furthermore, the two experiments that looked at insurance subsidies also varied many other treatment variables (e.g. different loss probabilities and included a very high loading) that might have confounded their results. More systematic evidence on insurance subsidies should improve our understanding of the potential effectiveness of subsidies on insurance take-up rates overall and on take-up of long-term insurance in particular. [2]

Since the two most related studies that examined multi-year insurance experimentally also only investigated premium *loadings* (Kunreuther & Michel-Kerjan, 2015; Papon, 2008), this paper contributes by looking at the effect of *subsidies* on insurance take-up overall and on multi-year insurance decisions. Since our study uses a controlled experiment that only varies the premium (40% vs. 100% of actuarially

[2] There is a significant body of research that has looked at the regulatory regimes for crop and automobile insurance, especially when these entail regulated prices. The conclusion from these appears to be that premium subsidies have several unintended consequences, such as overall welfare reductions, unintended wealth redistributions, and moral hazard (Du et al., 2016; Jaffe & Shepard, 2020; Lusk, 2016; Weiss et al., 2010).

fair premiums), we are better able to distill the effect of subsidies on insurance take-up than was possible in previous studies. Our goal is to inform policymakers of the effectiveness of widely used policies offering insurance at subsidized rates.

Additionally, these two experimental papers (Papon, 2008; and Kunreuther and Michel-Kerjan, 2015) have not explored their participants' characteristics extensively to shed light on who prefers long-term insurance. It may be of particular interest to learn whether there are specific types of people who could benefit more from help in the insurance decision-making process. To gain insight into the association between people's characteristics and insurance choices, we collect more demographic data than most previous studies have, and we collect data on personality traits, which was not collected in other studies.

An additional contribution of our work here is that we measure risk attitudes in several ways, thus improving our understanding of individual insurance decisions. We use multiple measures of risk attitudes because recent research has shown that a single measure of risk attitudes is likely inadequate to capture risk preferences accurately (Holzmeister & Stefan, 2020; Pedroni et al., 2017). Our attitudinal measures include a constant relative risk aversion (CRRA) parameter, a self-reported willingness to take risks, and locus of control. All these measures have been shown to correlate with risky financial decisions in general and with insurance decisions specifically (Antwi-Boasiako 2017; Dohmen et al. 2011; Dudek 2021; Falk et al., 2016; Harrison & Ng, 2016, 2018; Robinson & Botzen, 2020; Salamanca et al., 2020).[3]

We additionally explore two behavioral phenomena that are often discussed in research on insurance decisions. On the one hand, the availability heuristic suggests that people who can recall a certain experience or event easily from memory (as they have experienced it recently) judge such an event to happen more likely again (Tversky & Kahneman 1973, 1982). This heuristic distorts probability judgments upwards and thereby affects decisions under risk (Keller et al., 2006; Tversky & Kahneman 1982). This should lead more people to buy insurance after a disaster. Researchers who studied insurance take up rates after real-world disasters have indeed shown that take up rates indeed increase, but that people eventually cancel their newly purchased insurance policies and revert back to their old no-insurance status quo (Browne & Hoyt, 2000; Gallagher, 2014; Michel-Kerjan et al., 2012). Both buying insurance and not renewing it later can be explained by the availability heuristic.

[3] While Kunreuther and Michel-Kerjan included a single measure of risk tolerance in their analyses and showed that those less tolerant to risks are more likely to purchase long-term insurance, Papon's (2008) study did not include any measure that might capture risk attitudes.

On the other hand, the gambler's fallacy suggests that people tend to believe that a random event – such as a disaster – will not happen again if it has just happened (Croson & Sundali 2005; Sundali & Croson 2006; Tversky & Kahneman 1974). This fallacy thus distorts probability judgments downwards thus should lead to the exact opposite behavior to the availability heuristic: people will be less likely to buy or more likely to cancel existing insurance policies after an event.

Research so far has failed to conclusively explain people's reactions to the occurrence of a disaster. Whereas some found that the availability heuristic increases insurance demand after disasters (Kunreuther & Pauly 2018; Turner et al., 2014); others found that the gambler's fallacy decreases insurance demand after disasters (Jaspersen & Aseervatham 2017); and yet other studies have found no effect of disasters on insurance demand at all (Camerer & Kunreuther 1989; Reynaud et al., 2018). Intriguingly, some have even argued that both phenomena may play a role in determining the demand for insurance after disasters, but that these may depend on the timing or geographical proximity to the disaster (Kamiya & Yanase 2019; Yin et al., 2016). Our results add additional evidence to this literature.

Finally, to the best of our knowledge, no one has investigated how potential changes in the value of the asset might affect (multi-year) insurance decisions and the emergence of under-insurance. As Holzheu and Turner (2018) have pointed out, people tend not to update their insurance policies even when there may have been additions or changes to the house itself, there were changes in housing regulations, or economic circumstances that affect the value of the house changed (e.g., cyclical movements in market values). People may become underinsured because they are unwilling to undertake the effort of updating their insurance policies (which may also involve some financial costs). Holzheu and Turner (2018) point out that the reasons for the occurrence of underinsurance among those who do purchase insurance have been barely investigated.

To summarize, we investigate three hypotheses and offer some insights about all of them. H1 focuses on premium subsidies; H2 examines inertia (or status-quo bias) in insurance decisions on the intensive margin; and, in contrast, H3 looks at the extensive margin and the role of past insurable events in determining current demand for insurance. More formally,

H1a: Subsidies increase insurance take-up because they make insurance cheaper.

H1b: Subsidies increase the likelihood of purchasing house value re-assessments because they make these re-evaluations cheaper.

H2: Underinsurance occurs over time because people fail to update their insurance policies.

H3: People change their demand for insurance following an insurable disaster because the occurrence of a disaster distorts probability judgments (either upward or downward).

3 Experimental design and data

The experiment was programmed with the LIONESS Lab software (Arechar et al., 2018) and run partially in the lab and partially online on Prolific Academic. The experiment has two stages. In the first, we elicit the participants' attitudes to risk and some measurements of their personality traits. In the second part, the participants make decisions in an incentivized insurance task that aims to simulate real-world decisions that homeowners make with respect to their home insurance.

3.1 Part 1: Measurement of risk attitudes and personality

Before participants make insurance decisions, they first answer sixty binary lottery choices shown in form of pie charts. We use these to elicit a constant relative risk aversion (CRRA) parameter in the fashion of Harrison and Ng (2016) (see Appendix A.1). Following the CRRA elicitation, participants self-reported their willingness to take risks in general and in financial matters on 11-point Likert scales (Dohmen et al. 2011; Falk et al. 2016, see Appendix A.2). Thereafter, participants self-reported their locus of control (Rotter, 1990) by stating their level of agreement with ten statements on 7-point Likert scales (Appendix A.3). Locus of control measures people's perception on how their own efforts or outside circumstances affect their lives and has been shown to correlate with financial decisions and insurance decisions specifically (Antwi-Boasiako, 2017; Dudek, 2021; Salamanca et al., 2020). Finally, participants self-report their big five personality traits by classifying themselves on five items per trait on 7-point Likert scales (Donnellan et al., 2006, as shown in Appendix A.4). Personality traits also have been shown to correlate with risk attitudes and financial decisions (Baker et al., 2019; Bucciol & Zarri 2017; Conlin et al., 2015; Davey & George, 2011; Gambetti & Giusberti, 2019).

3.2 Part 2: The insurance decision task

Following the trait measurements, participants perform the home insurance decision task. Our experimental design resembles that of Kunreuther and Michel-Kerjan (2015), Papon (2008) and Osberghaus & Reif (2021) but also has some distinct differences. The former two studies have investigated long-term insurance choices, while the latter investigates the effect of insurance pricing and disaster experience on insurance demand in an annual insurance simulation. Therefore, we briefly mention some similarities and differences between our design and the two experimental designs that investigated long-term insurance here and expand on those in Appendix B.1. Overall, we closely follow the suggested design features of insurance decision experiments described by Jaspersen (2016).[4]

We ran two versions of the experiment. In one version we offered actuarially fairly priced insurance (Ins_{fair}) and in another version we offer subsidized insurance (Ins_{subs}). The subsidized version charged participants 40% of the actuarially fair price for full sum-insured (i.e., no deductibles) and also charged only 40% of the price for house value assessment that is charged in the actuarially fair version of our experiment. Apart from the price difference, the experimental design is the same in both versions.

Our design most closely resembles Kunreuther and Michel-Kerjan (2015) experimental design in which participants made insurance decisions in 30 periods (so up to 60 decisions). Papon's (2008) experiment simulated 12 independent years and randomly allocated participants into different treatments that either made only annual or only long-term contracts available. Both studies investigated long-term insurance contracts and offered these either at actuarially fair premiums or with a premium loading (which varied between the studies and treatments).[5] Osberghaus & Reif (2021) experimentally studied annual insurance contracts and offered those at a premium, before potential government compensations. This last study investigated insurance pricing effects via government assistance, similar to the experimental study by Robinson et al. (2019). Both studies found little impact of premium loadings on insurance demand.

Our insurance decision task comprises six experimental periods; each of these periods accounts for two years (i.e., up to 12 years of decisions). Each period starts with the same house value (Experimental currency, EC 500,000) and initial cash balance (EC 55,000).[6] Participants can pay for insurance from their cash balance. Each period participants face the same decisions whether to fully insure a house, which reimburses the participant if a disaster occurred. A disaster damages the house by half of its current house value.

Figure 1 illustrates the decision process for one period. The decision tree was also shown to participants to help them with their understanding of the decisions involved in this experiment. The numbers here correspond to those in the subsidized experiment.

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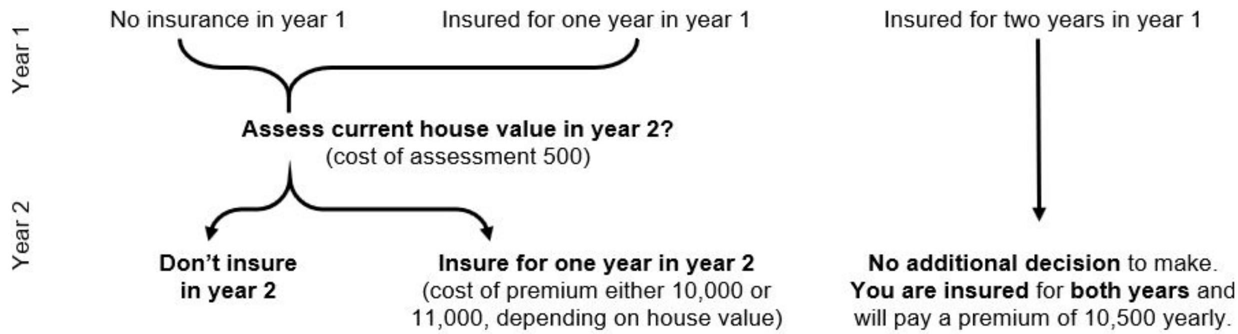
[4] We have not asked participants to perform a task before they receive their endowment, which might affect people's decisions in the experiment, as some might think that the endowment is purely "house money". The house money effect has been shown to influence risk taking (Cárdenas, De Roux, Jaramillo, & Martinez, 2014; Harrison, 2007; Thaler & Johnson, 1990). We do not think this effect has strongly biased our design, however, since the choices in our experiments were very similar to the choices made by participants in the two most closely related experimental studies of insurance decisions (Kunreuther & Michel-Kerjan, 2015; Papon, 2008), who also did not make people perform a task before the insurance decisions to make them perceive they "earned" the endowment.

[5] See more details of similarities and differences in Appendix B.1.

[6] For remuneration, EC 25,000 = $1.

Fig. 1 Insurance decisions in year 1 and year 2 of each period



This figure illustrates the choice options within one experimental period. In year 1, participants can decide not to insure, to insure only the first year, or insure both years with a long-term fixed price contract. Those participants who choose no insurance or only insure for one year have the option to have their house value assessed in the second year, before they decide whether to insure for another year or not in the second year.

Insurance decisions. Participants can choose (1) not to insure annually, (2) to insure annually with an annual contract (in either of the two years), or (3) to insure with a multi-year contract at a fixed price without the option to cancel after the first year. As shown in Fig. 1, if participants decide to insure with a multi-year contract, they have no further decisions to make in year 2 of that period. But those who did not insure or insured with an annual contract in the first year can decide whether to insure in the second year. Everyone who insures with a multi-year contract is automatically covered in full in both years, even if the house value increases. This feature makes the multi-year contract slightly more expensive annually but is priced according to the expected premium of two annual contracts over both years within a period, including the assessment fee (see below). As such, both the two annual contracts with a house assessment and the single two-year contract on average cost the same.

House value assessment. Those who insured with an annual contract *or* did not insure in the first year can decide whether to pay for a house value assessment, so that the new insurance contract in the second year reflects the current true value of the house. Those who insure in the second year again but did not update their contract might remain under-insured due to the new, increased house value. To avoid additional complexity of the insurance choice analyses as a result of this house value assessment, we offered the assessment at the same subsidized rate as the premiums, whereas it is not subsidized in the actuarially fair version of the experiment.

Our aim with this mechanism is to find out if and how many people are willing to pay for an assessment of their current house value, which automatically updates the insurance contract, rather than whether people reinsure or find the resulting increase in a premium unfair or surprising. In fact, we tell participants that there is a 50% chance of the house value increasing by 10% after the first year. But participants are not automatically informed whether the house value increased. Instead, participants have to actively choose to get their house value assessed. If they choose to have the house value assessed and the value increased, the premium for next year's full sum-insured will reflect this increase, as well. This option is only available to those who did *not* insure *or* insured with an *annual* contract in year 1.

Our design does not allow us to identify causal effects of a house value increase on insurance decisions, since participants only receive the information about their house value *if* they chose an annual or no contract in year 1 of any period *and* paid for an assessment of their home value in the beginning of the second year. We are the first to experimentally investigate insurance updating, but at the same time we wanted to have an experimental design resembling the design of the two most closely related experimental investigations of long-term insurance so that we may compare our results to these studies. Our experimental design allows us to gain an insight into the occurrence of home underinsurance based on participants' insurance choices in a controlled environment.

Figure 2 illustrates the experimental process of one period in a different format. This figure was also shown to participants to help them with their understanding of the insurance decision task. Table 1 describes the experiment's main variables.

Fig 2 Experimental process

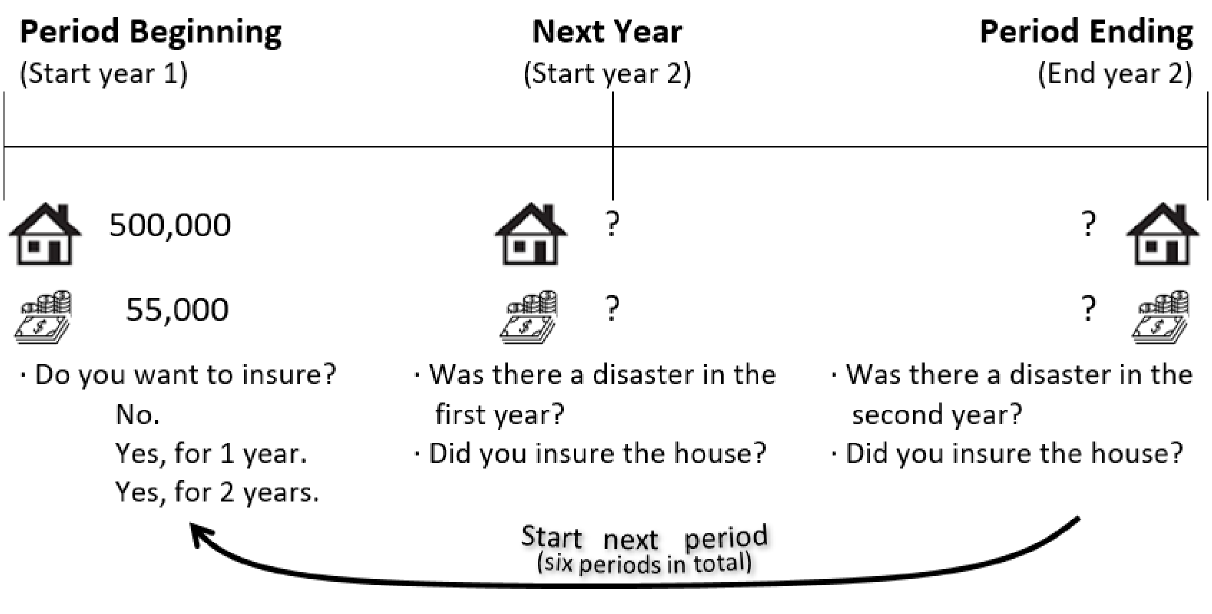


Table 1. Insurance decision task variables

Variable description	Actuarially fair experiment	Subsidized experiment

Home value in year 1.	HV1 = 500,000	HV1 = 500,000
Home value in year 2, depending on the house value increase after year 1.	HV2 = 500,000 or 550,000	HV2 = 500,000 or 550,000
Probability that the house value increases by 10% after year 1.	p_{HV} = 50%	p_{HV} = 50%
Cash balance is the endowment at the beginning of each period that participants can use to pay for insurance.	CB = 55,000	CB = 55,000
Premium for insurance in year 1.	C_1 = 25,000	C_1 = 10,000
Premium for insurance in year 2, depending on the house value increase after year 1 and the choice of the participant whether to assess the house value.	C_2 = 25,000 or 27,500	C_2 = 10,000 or 11,000
Premium for the long-term insurance contract fixed for both years.	C_{LTI} = 26,250	C_{LTI} = 10,500
Probability of a disaster in any year. Fixed for each year and independent of the year or period.	p_D = 10%	p_D = 10%
Cost of a house value assessment after year 1.	A = 1,250	A = 500
Damage caused by the disaster in year 1 or year 2, depending on the current house value.	D = 250,000 or 275,000	D = 250,000 or 275,000

3.3 Incentivizing participants with the random problem selection mechanism

Participants are incentivized in both the lottery choice and the insurance decision task by being told that all their choices have a chance of being paid out for real so that participants should choose carefully according to their own preferences. EC 25,000 in the experiment are worth $1, while the first risk preference elicitation task directly shows the payments in dollars (ranging from as low as $0.00 up to $35.00), so that participants could potentially earn up to $57.20 in total.[7] The final wealth from the insurance experiment is the sum of the house value and the cash balance (less insurance premiums) at the end of the randomly selected period.

The computer randomly draws one insurance period and one of the preference elicitation choices for payment at the click of a button at the end of the study, so that participants do not receive payment information until the very end (after a demographic survey). This type of Random Problem Selection Mechanism (RPSM) is widely used in experimental economics and is recognized as an effective mechanism to incentivize participants (Azrieli et al., 2018, 2020; Camerer et al., 1999). The two most closely related

[7] The house is worth EC 500,000 ($20), the initial cash balance is 55,000 ($2.20), so that those who did not experience a disaster and did not pay for insurance in the randomly selected period might potentially earn $22.20 from the insurance experiment. But those who did never insure and experienced two disasters within the randomly selected period for payment might end up receiving only $2.20 if they also did not earn anything in the preference elicitation task.

studies that investigated long-term insurance decisions used a similar RPSM to incentivize their participants (Kunreuther & Michel-Kerjan, 2015; Papon, 2008).[8]

4 The participants and their decisions: Descriptive Statistics

Table 2 and 3 show descriptive statistics of the demographics and insurance choices by period, separated by the two experimental versions (Panel A Ins_{fair}=actuarially fair, Panel B Ins_{subs}=subsidized). We have a total of 413 participants from New Zealand (n=218) and Australia (n=196) of whom 51% are female. 150 participated in the experiment online in which we offered an actuarially fair insurance premium (Ins_{fair}) and 263 participated in the experiment in which we offered a subsidized premium (Ins_{subs}). Of those who participated in the subsidized version, 174 participated online via the platform Prolific and 89 participated in the same experiment in a computer room at our university's campus. Of the 413 participants, 20% are students, most of whom took part in the lab study on campus. For comparison, Papon (2008) only had a sample of 64 undergraduate students who were split between 2 treatments, and Kunreuther and Michel-Kerjan (2015) had a total sample of 445 adults who were assigned to 8 treatments randomly.

We imputed missing demographic information for 28 participants (8 online, 20 in the lab) in the subsidized experiments, and for 15 participants of the actuarially fair experiment based on statistical analyses of the subsample whose demographic information was available. The missing information problem is more severe in the lab data collection, because here we were not able to substitute any information with demographic data from any other source.[9] We include dummy indicators for each missing demographic variable to account for this imputation.

[8] Our RPSM is arguably better incentivized than those used in the other two studies. Kunreuther and Michel-Kerjan (2015) paid only 1 out of 100 (i.e., in total 5) participants randomly based on a random selection of one of the choices made in the experiment. Similarly, Papon (2008) grouped eight participants and only one out of those (i.e., in total 8) participants got paid randomly based on one of the 12 periods' outcomes. Thus, both other studies potentially reduced the RPSM's effectiveness – although there is some evidence available that paying one or all participants makes little difference to the choices people make (Charness et al., 2016; Laury, 2005; Robinson & Botzen 2020). In our study, every participant gets paid based on one of the six insurance periods' decision outcomes. This difference in our experimental design to the two other related studies allows us to gain further suggestive evidence whether or not paying one or paying all participants makes a large difference.

[9] We could substitute demographic information for the online data collection because Prolific makes such information available to researchers at no cost.

Table 2 Descriptive statistics of demographics

Panel A. Act. fair experiment Ins_{fair}	Mean	Std. Dev.	Min.	Max.
Female	0.52	0.50	0	1
Age in years	34.70	11.20	20	70
Educ in years	16.34	2.63	9	22
White ethnicity	0.66	0.47	0	1
Income ($'000s)	76.62	50.23	0	160
Student	0.14	0.35	0	1
Unemployed	0.09	0.28	0	1
In a partnership	0.64	0.48	0	1
Homeowner	0.39	0.49	0	1
Household size > 2	0.42	0.49	0	1
Panel B. Subsidized experiment Ins_{subs}	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Female	0.50	0.50	0	1
Age in years	32.11	10.03	20	65
Educ in years	16.05	2.65	9	22
White ethnicity	0.69	0.46	0	1
Income ($'000s)	82.08	53.40	0	160
Student	0.22	0.41	0	1
Unemployed	0.11	0.32	0	1
In a partnership	0.46	0.50	0	1
Homeowner	0.41	0.49	0	1
Household size > 2	0.41	0.49	0	1

This table shows summary statistics of the demographic information used as independent variables for all participants in the actuarially fair experiment (Panel A, N=149) and in the subsidized experiment (Panel B, N=263). All variables are explained in detail in Appendix B.2.

Table 3 Descriptive statistics of insurance choices overall

Panel A. Act. fair experiment Ins_{fair}	Mean	Std. Dev.	Min.	Max.
Times insured (0-12)	8.93	4.11	0	12
Times long-term insurance (0-6)	1.84	2.44	0	6
Times insured annually (0-12)	3.95	4.12	0	12
Insurance choice (0, 0.5, 1, or 2)	1.16	0.79	0	2
Panel B. Subsidized experiment Ins_{subs}	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Times insured (0-12)	9.26	3.49	0	12
Times long-term insurance (0-6)	2.09	2.51	0	6
Times insured annually (0-12)	3.71	3.68	0	12
Insurance choice (0, 0.5, 1, or 2)	1.24	0.78	0	2

This table shows summary statistics of the insurance choices for all participants in the actuarially fair experiment (Panel A, N=149) and in the subsidized experiment (Panel B, N=263) across all six insurance periods. *Times insured* measures how many out of 12 years were insured. *Times long-term insurance* measures how often out of 6 periods a person chose the biennial insurance contract. *Times insured annually* measures how often a person specifically chose annual insurance. *Insurance choice* is our main dependent variable that takes the values 0 if the participant did not insure in a period at all, 0.5 if only one of two years was insured, 1 if both years were insured annually, and 2 if both years were insured with the long-term insurance contract.

5 Results

All results are shown including the missing demographics and missing dummy indicators, unless noted otherwise. We obtain the same results when we exclude people whose demographic information we imputed, which indicates that the imputation does not affect our results (see Appendix C.1). We overall find no robust statistically significant relationship between the participants' insurance choices in the experiments and their demographic information with one exception: home owners were more likely to either insure only one year or both years with a multi-year contract (but not both years annually). We never imputed homeownership status because this was a mandatory question to answer. We thus see no reason to exclude specific participants based on the availability of their demographic information.

The dependent and independent variables are described in detail in Appendix B.2. For our results discussed here, we combine the data from the subsidized online (N=174) and laboratory (in-person, N=89) experiments. We conducted the lab version primarily to have more control and to see if we find some stark differences to the results from the online data, but the online and the lab experiment choices were very similar and all regression estimates point in the same direction. Overall, we found no significant differences. Hence, the results discussed throughout compare the subsidized (Ins_{sub}) and the actuarially fair (Ins_{fair}) insurance versions without further discussing results for the two samples of the subsidized version separately. Additional analyses for the two sub-samples of the subsidized version are available in the appendix (C.4.1 and C.4.2).

5.1 Analyses of insurance choices overall

We start our discussion of the results by looking at the overall choices made in the actuarially fair and the subsidized experiment versions.

5.1.1 Descriptive analyses of insurance choices

Fig 3 Insurance choices in both experimental versions over time

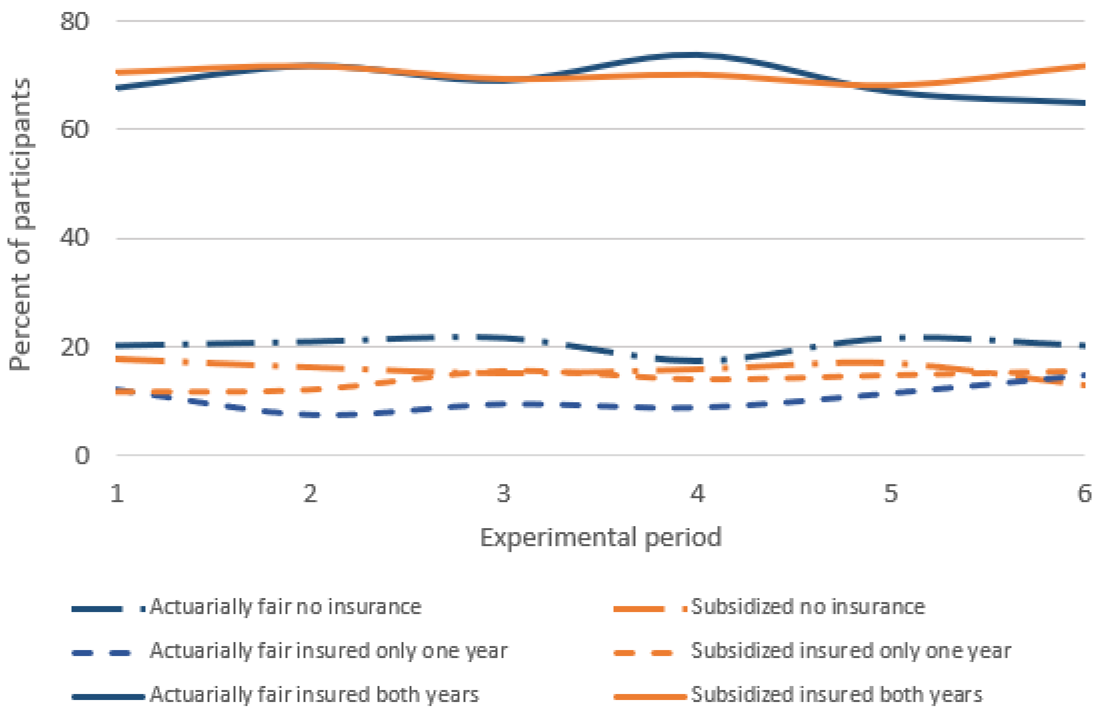


Figure 3 shows for each experimental version the percent of participants who did not insure at all within a period (dotted line), who insured only one of the two years within a period (dashed line), and who insured both years annually or biennially (solid line) within a period. Compared to the actuarially fair version, in the subsidized version fewer people did not insure (on average 20% versus 15%), fewer insured only one of the two years (14% versus 11%), fewer insured both years annually (28% versus 24%), but more insured both years with a long-term contract (42% versus 46%). All these differences are, however, rather small. The majority of the participants insured both years either annually (between 22% and 30%) or biennially (between 35% and 48%) in both versions. Similar to the results in Papon (2008) and Kunreuther and Michel-Kerjan (2015, henceforth KMK), our participants typically either insured in full (both years, on average 70%) or did not insure (on average 18%). The rest of the decisions were for insurance of only one of the two years (on average 12%).

Since Papon applied a 50% loading to all insurance types, he found that a much lower percentage of participants insured overall (36% never insure and only 36% fully insure, see Fig. 3 of his paper). Our results are more comparable to KMK's design in which they offered all contracts at an actuarially fair price. In the appendix, we therefore show results from KMK's second and last row of their Table 2 in our table's last column for a comparison (Table A-2), where we also show the results corresponding to those figures from difference-in-means tests for the average choices made between the actuarially fair and the subsidized insurance experiment (Panel C). We tested the hypothesis that participants in the subsidized experiment are more likely to insure, i.e. a larger fraction of participants should be insured in the subsidized experiment because insurance is much cheaper. But neither the t-tests nor Wilcoxon ranksum tests indicate that this is true; in fact, in most cases we cannot reject the null hypothesis of no differences.

The share of participants who chose long-term insurance in the subsidized experiment slightly increased, while it slightly decreased in the actuarially fair experiment over time. This diverging development results in a statistically significant difference in choices between the two experimental versions only in the sixth (last) period of the experiment. We hence find only weak evidence that subsidies matter and increase insurance take-up, but this take-up only manifests after a while. Perhaps, it took participants some time to learn that the insurance is relatively cheap in the subsidized experiment.

Furthermore, we test difference-in-means of the number of years (out of 12) in which a participant was insured overall. These tests also show no significant difference between the actuarially fair and subsidized experiments (one-sided t-test statistic -0.858, p-value <= 0.197; Wilcoxon rank-sum z statistic -0.204; p-value <= 0.839). We see that subsidies and loadings do not matter nearly as much as economic theory predict (or common sense, for that matter).

An insurance premium that only costs 40% of the actuarially fair price would require an extremely high risk tolerance for those who choose not to insure. In fact, anyone who is risk neutral and even people who are risk loving to some degree should choose insurance in both years according to expected utility theory. Our results support previous findings showing that people are quite unresponsive to differing insurance premiums. Hence, we conclude that subsidizing insurance to significantly increase take-up will likely be an ineffective and costly policy. We therefore reject hypothesis H1a and conclude that subsidies by themselves do *not* increase insurance take-up.

5.1.2 Regression analyses of insurance choices

We look at five alternative choices: no insurance; insurance in only the first *or* only the second year out of two years within a period; annual insurance in both years of a period; and biennial insurance. These insurance choice alternatives are coded as our main dependent (categorical) variable that we use for our multinomial logistic regression analyses in the next section.[10] The multinomial logistic regressions include

[10] This dependent variable takes on four values (within each of the six periods): 0 if no insurance was chosen in any of the two years, 0.5 if a participant only insured either in the first *or* the second year but not both, 1 if both years were insured annually, and 2 if both years were insured with the multi-year insurance contract.

risk attitudes, personality, and demographic characteristics as explanatory variables. Our main variables of interest here are the participants' previous period's insurance choice and whether there was a disaster in a previous period. With these, we are trying to capture habitual decision-making (either resulting from inertia or from consistency in decisions) and the effect of previous disaster experience on following insurance decisions (i.e., whether the availability heuristic or gambler's fallacy dominate, if any). The results are very similar with and without controls for risk attitudes, personality traits and demographic information. We have already shown previously that subsidies did not affect average insurance choices significantly (except, perhaps, in the last period). The table thus only shows the results for the entire sample from both insurance experiments (actuarially fair and subsidized combined) and includes a dummy variable for the experimental version. In the appendix we show the results separated by both versions of the experiment, supporting the results shown here (Appendix C.4.2 and C.4.1). All regression coefficients are compared to the (omitted) choice of no insurance in both years. The coefficients in the table show relative risk ratios.

Table 4 Main results from multinomial logistic regressions

Both experiments combined, N=413 (x 6 periods = 2,478 obs.)			
Panel A.	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
Insured previous period	1.169*** (0.191)	2.497*** (0.183)	2.451*** (0.184)
Disaster previous period	-0.293 (0.196)	-0.656*** (0.186)	-0.547*** (0.171)
Subsidized	0.396 (0.249)	0.082 (0.258)	0.359 (0.225)
Pseudo R-squared		0.083	
Panel B. (including risk attitudes)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
Insured previous period	1.146*** (0.192)	2.455*** (0.186)	2.418*** (0.186)
Disaster previous period	-0.306 (0.198)	-0.671*** (0.186)	-0.561*** (0.172)
Subsidized	0.325 (0.251)	0.002 (0.262)	0.285 (0.227)
Pseudo R-squared		0.091 (Δ Pseudo R^2≈0.008)	
Risk attitudes incl. LoC		Chi^2-test≈22.48, p-value ≤ 0.007	
Panel C. (including big five traits)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
Insured previous period	1.154*** (0.194)	2.457*** (0.186)	2.407*** (0.185)
Disaster previous period	-0.292 (0.197)	-0.674*** (0.185)	-0.545*** (0.171)
Subsidized	0.320 (0.265)	-0.008 (0.285)	0.329 (0.243)
Pseudo R-squared		0.096 (Δ Pseudo R^2≈0.005)	
Risk attitudes incl. LoC		Chi^2-test≈18.30, p-value ≤ 0.032	
Big five personality traits		Chi^2-test≈13.96, p-value ≤ 0.528	
Panel D. (including demographics)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
Insured previous period	1.158*** (0.198)	2.373*** (0.191)	2.313*** (0.190)
Disaster previous period	-0.353* (0.201)	-0.664*** (0.188)	-0.546*** (0.175)
Subsidized	0.243 (0.282)	-0.072 (0.305)	0.269 (0.267)
Pseudo R-squared		0.126 (Δ Pseudo R^2≈ 0.030)	
Risk attitudes incl. LoC		Chi^2-test≈17.18, p-value ≤ 0.046	
Big five personality traits		Chi^2-test≈17.44, p-value ≤ 0.293	
Demographics		Chi^2-test≈275.97, p-value ≤ 0.000	

This table shows results from a multinomial regression analysis of the insurance choices made by participants. The dependent variable takes the values 0, 0.5, 1, and 2 for the choice alternatives no insurance (0,0), insurance chosen in only one of the two years (0,1 or 1,0), annual insurance purchased in both years (1,1) and biennial (long-term) insurance (2,2), respectively. The base category is no insurance in either of the two years and so the results are all shown in comparison to this omitted alternative (0,0). The regressions include additional covariates step-by-step. Panel A has no additional covariates except dummies for the insurance periods. In panel B., we add risk attitudes, which include a self-reported risk tolerance, a constant relative risk aversion parameter, and locus of control. In Panel C., we add the big five personality traits (openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism). Lastly, in Panel D., we add a set of demographic characteristics as explanatory variables: two categorical variables of income (standardized and squared), a female dummy, two categorical age variables (standardized and squared), a white ethnicity dummy, two categorical variables of years of education (standardized and squared), one dummy each indicating whether someone is (i) a student, (ii) unemployed, (iii) in a partnership, (iv) a home owner, and whether someone is (v) living in a household that is larger than 2 people. New Zealand is a dummy indicating whether the participant is a New Zealand resident, Disaster/Insured previous period are each a dummy variable indicating if the participant experienced a disaster or was fully insured in the previous period. Standard errors clustered on the individual are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

First, we find a highly statistically significant positive relationship between current insurance choices and the previous period's choice ($p<0.01$), which supports our assumption that habits drive some of the choices, defaulting people into the same decisions. People either make very consistent decisions or impose a status-quo bias on themselves with their first period's choice. We find that the first period's choice is primarily predicted by a person's willingness to take risks, their locus of control and to some extent by the New Zealand dummy (Appendix C.4.3 and C.4.4). All following periods are largely a repetition of the first period's choice, although our measures of risk attitudes and the occurrence of a disaster in the previous period still predict insurance choices in following periods, as discussed next.

Second, our data suggests that people tend to decline insurance after they experienced a disaster in a previous period. This behavior can be best explained by the gambler's fallacy heuristic. In our design, we attempted to mitigate potential effects of distorted probability judgments by clearly stating that the chance of a disaster is always 10% and independent of each year and period. Providing this information, however, does not seem to remove the effect of a disaster experience on choices. To compare to the two most related studies: While KMK (2015) found no support for either behavioral phenomenon, Papon (2008) found support for the availability heuristic. Our findings here tend to support hypothesis H3 in favor of the gambler's fallacy: People do *not* insure after the occurrence of a disaster (in insurance experiments). This is also supported by Osberghaus & Reif's (2021) recently published experimental investigation of insurance choices.

Furthermore, we find that the big five personality traits do not predict insurance choices. But the overall predictive power of demographics on insurance choices is strong (judging from the Chi^2-test statistic≈275.97, p-value ≤ 0.000). Significant demographic predictors of insurance decisions in our experiment are gender, squared income, age, years of education, squared education, and homeowner status. Some coefficients appear statistically significant at $p<0.1$ or $p<0.5$, which is to be expected with the inclusion of 20 independent variables, but these findings are not robust across both of the experimental versions, nor across different model specifications. However, they never become significant in the opposite direction. For more extensive results, see Appendix C.4.

In summary, risk attitudes (including locus of control) are significant predictors of decisions made in the first period, but thereafter people tend to repeat the choice they made the first time. Yet, risk attitudes remain important predictors to some extent in periods after the first. The experience of a disaster tends to reduce insurance demand in the following period, which supports a gambler's fallacy interpretation. Big five personality traits do not predict insurance choices significantly, and only few of the demographic variables are (not robustly) significant predictors of insurance choices in the experiment. Subsidies never show a significant effect on insurance choices and thus appear to be (statistically) irrelevant for insurance decisions. We thus reject our hypothesis H1a and find support for hypothesis H3.[11]

5.2 Under-insurance and asset value adjustments

In this section we are looking at the choices of (only) those people who insured with an annual contract in a specific period, i.e., for some people we use data from only some of the periods, whenever they decided not to insure both years with multi-year contract. In total, only 244 out of 413 participants chose *not* to insure both years with multi-year contract. The aim of this section is to analyze potential occurrence of underinsurance in year 2 and how a house value increase might affect insurance choices in the second year. We try to understand whether subsidies can reduce such underinsurance overall, and whether subsidies reduce the occurrence of underinsurance over time. This investigation will examine hypotheses H1b and H2.

5.2.1 Descriptive analyses

Table 5 shows an overview of those people who did not insure with a biennial contract in a specific period. The table provides a first insight into why people may be underinsured even though they choose to insure their house.

[11] H1a: Subsidies increase insurance take-up because they make insuring cheaper (incentives).
H3: People change their insurance demand following a disaster because the occurrence of a disaster distorts probability judgments (either upward or downward).

Table 5 Overview of insurance choices among annual insurers

Panel A. Whole sample		Period	1	2	3	4	5	6
	Number of participants	N=	217	219	227	232	244	235
1	Insured in year 1		110	123	127	134	134	140
2	Insured in year 2		121	124	128	144	144	139
3	…and did *not* assess the house value		66	47	53	52	49	54
4	*Row 3 divided by row 2*		54.5%	37.9%	41.4%	36.1%	34.0%	38.8%
Panel B. Ins_{fair}		Period	1	2	3	4	5	6
	Number of participants	N=	80	82	89	86	90	96
1	Insured in year 1		40	44	49	53	49	58
2	Insured in year 2		42	47	51	54	50	52
3	…and did *not* assess the house value		22	15	20	17	11	16
4	*Row 3 divided by row 2*		55.0%	34.1%	40.8%	32.1%	22.4%	27.6%
Panel C. Ins_{subs}		Period	1	2	3	4	5	6
	Number of participants	N=	137	137	138	146	154	139
1	Insured in year 1		70	79	78	81	85	82
2	Insured in year 2		79	77	77	90	94	87
3	…and did *not* assess the house value		44	32	33	35	38	38
4	*Row 3 divided by row 2*		62.9%	40.5%	42.3%	43.2%	44.7%	46.3%
Panel D. Difference in means t-tests					p-values			
1	Insured in year 1		0.877	0.565	0.829	0.362	0.910	0.828
2	Insured in year 2		0.462	0.873	0.824	0.863	0.403	0.198
3	…and did *not* assess the house value		0.167	0.311	0.918	0.512	0.124	0.105

This table shows the number and percentage of participants who chose annual insurance in either the first and/or the second year. Panel A shows the data for the whole sample (participants from both experimental versions), Panel B shows data from the actuarially fair experiment and Panel C shows the data for the subsidized experiments. Panel D shows p-values of difference-in-means t-tests, testing whether the mean choices in year 1 and year 2 are different between the participants who took part in the actuarially fair and the subsidized experiment. No mean-differences are statistically significant. Row 1 of each panel shows the number of people who insured in the first year and row 2 shows the number of people who insured in the second year, out of the total of people who insured either in year 1 or 2 with an annual contract. Row 3 shows the number of people who insured in year 2 and did *not* assess the house value.

First, we note that on average about 40% of the participants who insured in the second year of a period did *not* choose to have the new house value assessed (between 34% and 55% in specific periods, see Panel A, row 4 of Table 5). Recall that the annual insurance contract covers only the initial house value *unless* a participant assesses the house value after the first year. These choices indicate that some people who insured their home run the risk of being under-insured merely because they declined the opportunity to update their insurance sum-insured. We analyzed the choices in both experimental versions separately and, again, find no significant differences in the periodic insurance decisions of those who decided to insure with annual contracts (Panels B and C of Table 5). All p-values between the insurance choices of the participants in the subsidized and the actuarially fair experiments are larger than 0.1, and most even larger than 0.5 (see Panel C), rendering the mean differences indistinguishable from zero. We thus reject hypothesis H1b: Subsidies do *not* increase the likelihood of a house value assessment; again, supporting our previous findings that price incentives have no statistically distinguishable effect on insurance choices.

Furthermore, we found that more participants of the subsidized experiment than of the actuarially fair experiment chose *not* to assess the current house value in the second year (Table 5). The difference in choices is not statistically significant, however.

Assuming that our data is to some degree representative of real-world behavior, and further assuming that people who do *not* assess the house price *and* insure after the first year expect to be fully reimbursed for their damages, we would have at least 34% of the participants underinsured in year 2 (i.e., less than 100% of their house value is insured), even though they chose to insure in that year. This finding supports H2: Underinsurance may occur over time because people do not update their insurance policies. Inertia might be one reason why this happens, and this suboptimal behavior is likely not preventable merely with financial incentives (subsidies that make the house assessment cheaper). In real-life the decision to update one's contract likely is more complex, which plausibly drives the number of those who are underinsured even higher. Our estimates thus likely represent a lower bound of the effect of an *un*willingness to assess one's house value on underinsurance. These findings suggest that insurance contracts should include a mechanism that updates the sum-insured automatically, e.g., by increasing the sum-insured yearly by a reconstruction cost index or a similar index. Some insurance companies include such a feature, but not all do. One reason why insurers do not offer such automatic updates might be because insurers commonly know that homeowners will not update their policies and that this over time could lead to lower payouts for insurance companies. Perhaps automatic or prompted updates lead to other adverse effects unknown to us, such as clients being unwilling to re-insure after an update that leads to a higher insurance premium.

A t-test on each of the demographic and personality characteristics on the choice whether to assess the house price showed that the people are similar in most observed characteristics in both versions of the experiment: Openness to experience is the only statistically significantly different characteristic between those who chose to reveal the house value and those who did not (p-value ≤ 0.043), otherwise in both experiments all other independent variables' p-values are larger than min. 0.20. The t-tests hence indicate that the choice to reveal the house value is not strongly related to a specific trait or characteristic of participants except perhaps openness to experience.

The results here provide two takeaways: First, the results indicate that some people simply are reluctant to pay for their house value assessment and thus are prone to become underinsured. We find that between 34% and 55% of those who purchase insurance in the second year might be underinsured just because they were not willing to pay for a house assessment. Second, and more importantly, participants' choices are unaffected by price for the house assessment *and* insurance. These results reject hypothesis H1b, but support hypothesis H2.[12]

5.2.2 Regression analyses

A logistic regression analysis of the choice whether or not to assess the house price and whether or not to insure in the second year after the participant chose to assess the house price gives us some additional insight into the factors that drive those choices (Table 6). Note that the results here might be biased by sample selection. The sample is much smaller than the overall sample because we are only looking at people who insured either one of the two years with an annual contract.

First, we see that people who insured in the first year are more likely to pay for the house value assessment. Perhaps these people intend to insure their house in the second year again anyway and want to make sure they are fully covered. Second, we see again that the first year's insurance decision is the best predictor of the second year's insurance decision. Home insurance decisions are not significantly affected by a revealed increase of the house value (i.e., after the participants decided to have their house assessed). Third, we again find some weak evidence that a disaster in the first year reduces the likelihood of choosing insurance in the second year among those who insured with only annual contracts. These estimates, however, are only marginally significant with a p-value<0.1, and only once we control for demographic data. Throughout all estimations, the point estimates on this dummy are always negative. This finding again lends support to a gambler's fallacy interpretation of insurance behavior.

[12] H1b: Subsidies increase the likelihood of purchasing house value assessments because they make these re-evaluations cheaper (incentives).
H3: Underinsurance occurs over time because people do not update their insurance policies (inertia).

Table 6 Logistic regressions of the choice to assess and to insure in year 2

Dependent variable →	*Assessed house*	*Insured year 2*	*Assessed house*	*Insured year 2*	*Assessed house*	*Insured year 2*	*Assessed house*	*Insured year 2*
↓Independent variables	**Panel A**		**Panel B** (incl. risk attitudes)		**Panel C** (incl. big five traits)		**Panel D** (incl. demographics)	
House value increased		-0.005 (0.218)		-0.007 (0.218)		-0.026 (0.219)		-0.013 (0.231)
New Zealand	0.391* (0.229)	-0.123 (0.262)	0.356 (0.231)	-0.068 (0.260)	0.307 (0.238)	0.008 (0.268)	0.347 (0.244)	0.003 (0.282)
Insured first year	0.399** (0.175)	2.112*** (0.215)	0.421** (0.176)	2.082*** (0.212)	0.435** (0.172)	2.083*** (0.210)	0.535*** (0.173)	2.180*** (0.232)
Disaster first year	-0.223 (0.200)	-0.365 (0.280)	-0.219 (0.201)	-0.348 (0.273)	-0.191 (0.200)	-0.341 (0.271)	-0.198 (0.202)	-0.546* (0.284)
Pseudo R^2	0.026	0.173	0.028	0.184	0.040	0.191	0.059	0.238
Δ Pseudo R^2	n/a	n/a	0.002	0.011	0.012	0.007	0.019	0.047
Chi-squared test for risk attitudes incl. LoC	n/a	n/a	Chi^2 ≈1.45,	Chi^2 ≈5.34	Chi^2 ≈0.99	Chi^2 ≈3.90	Chi^2 ≈0.99	Chi^2 ≈4.71
	n/a	n/a	p-value ≤ 0.693	p-value ≤ 0.148	p-value ≤ 0.806	p-value ≤ 0.273	p-value ≤ 0.804)	p-value ≤ 0.194)
Chi-squared test for big five personality traits	n/a	n/a	n/a	n/a	Chi^2 ≈7.79	Chi^2 ≈4.23	Chi^2 ≈7.63	Chi^2 ≈3.97
	n/a	n/a	n/a	n/a	p-value ≤ 0.168	p-value ≤ 0.521	p-value ≤ 0.178	p-value ≤ 0.553
Individuals (Obs.)	229 (1,374)	130 (782)	229 (1,374)	130 (782)	229 (1,374)	130 (782)	229 (1,374)	130 (782)

This table shows logistic regression results. The dependent variable is either a dummy variable that indicates whether someone assessed their house or a dummy variable that indicates whether someone insured their house in the second year (after they revealed the house value). The regressions include additional covariates step-by-step. Panel A only includes three dummy variables that indicate whether someone is a New Zealand resident, insured in the first year of a period, and whether they had a disaster in the first year of a period as covariates plus controls for period fixed-effects. In panel B., we add risk attitudes, which include a self-reported risk tolerance, a constant relative risk aversion parameter, and locus of control. In Panel C., we add the big five personality traits (openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism). In Panel D., we add a set of demographic characteristics as explanatory variables: two categorical variables of income (standardized and squared), a female dummy, two categorical age variables (standardized and squared), a white ethnicity dummy, two categorical variables of years of education (standardized and squared), one dummy each indicating whether someone is (i) a student, (ii) unemployed, (iii) in a partnership, (iv) a home owner, and whether someone is (v) living in a household that is larger than 2 people. Standard errors clustered on the individual level are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

6 Conclusions and Caveats

Following earlier research showing a preference for long-term insurance (Kunreuther & Michel-Kerjan 2015; Papon 2008), this paper further investigated long-term insurance decisions and the effect of subsidies on insurance take-up in an incentivized experiment. 413 participants took part in the computer-administered experiments. As in previous work, participants have a strong preference for being fully insured. Of our participants, on average 18% chose no insurance, 12% chose to insure only one year (of a two-year period), 26% chose to insure both years with an annual insurance contract and 44% chose to insure with a biennial insurance contract.

One finding from our study is that inertia or consistency in choices seem to matter more – or at least explain insurance decisions in our experiment better – than personal characteristics. Those who insure in the first year of the first period tend to insure in following periods, and people often make the same type of insurance choice repeatedly (over multiple periods). We present robustness analyses in Appendix C, confirming that the dummy variables capturing whether someone chose insurance in a previous period is the strongest predictor of insurance choices, and that this first period's choice is significantly predicted by risk attitudes (including locus of control).

In support of previous research, we show that insurance subsidies have a negligible effect on insurance take-up. Subsidized insurance might merely be a wealth transfer from some taxpayers to those few who receive subsidized insurance – often those who likely could afford such insurance without subsidies (Ben-Shahar & Logue, 2016), without increasing insurance take-up overall.

As with most experimental studies, our results are limited in their interpretation. We cannot be sure that our experimental study elicited actual insurance choices, although some of our findings hint that this is the case. For example, we see that, similar to real-world findings, insurance subsidies have little impact on insurance choices. We furthermore find, in line with previous studies investigating insurance choices, that people either insure fully (i.e. both years) or do not insure at all, and that many people prefer long-term insurance contracts. These findings support that our insurance experiment simulated insurance behavior at least to some degree similar to other (experimental) studies. What speaks against an interpretation of our results representing real insurance choices is that, as in many insurance experiments, participants were inclined not to insure after a disaster. This is not often observed from real-world data. The availability heuristic is most often observed in real-world data of insurance decisions, while the gambler's fallacy is more often observed in real-world data of other financial decisions and in experimental insurance decisions.

So, what do we learn from our insurance experiment? First, we see that many people prefer long-term insurance contracts. Offering different insurance contracts of varying durations could cater to different preferences and thus increase insurance take-up at the extensive margin. Second, we find that some might insure but become under-insured over time if contracts have no mechanism to update the sum-insured over time. Automatic adjustments of the contract's sum-insured could be a solution to increase insurance take-up at the intensive margin.

For governments, perhaps the most important findings might be (a) that people tend to repeat their previous insurance choices and (b) that subsidies are expensive and ineffective. If governments can convince people to insure and then remind them of their previous choice, this could increase insurance take-up at very little cost. Behaviorally-informed insurance contracts and information provision (designed as ‘nudges’) could increase overall insurance take-up, while also mitigating underinsurance over time.

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