Personality economics:
An investigation of how personality develops and how it predicts decisions

by

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Thomas Dudek

Abstract

This thesis is the product of three research papers, of which each one forms a paper of this thesis. In the first paper, I study how people's personality evolves and whether it is shaped by family dynamics, specifically by the sex of one's siblings. Researchers developed a good understanding of the importance of personality for people's lives but know very little how this personality is shaped. The first paper investigates whether growing up with a sister instead of a brother might be a cause of different personality development. In the second paper, I study two specific personality traits, locus of control and risk tolerance, as predictors of decisions under risk. Although we know risk is a crucial part of our lives, we still have not determined how to define and measure risk attitudes properly. The second paper delves deeper into this topic and shows how risk tolerance and locus of control predict risky decisions in an experiment and in real-world choices and gives some additional insight into the measurement of risk attitudes. In this paper I also studied other personality traits, which turned out to have no important role with regards to decisions under risk. In the third and last paper, I study property insurance decisions with data collected in an experiment. I designed and coded the experiment and collected this data partially in a computer lab and partially online. This project investigates what behavioral and financial factors influence property insurance decisions, especially the choice to insure with fixed-price long-term contracts.

1 Introduction: Research motivation and findings

The first paper aims to answer the question whether a sibling's sex affects a person's development of their personality. Personality is an important predictor of people's economic, social, and physical well-being (Almlund, Duckworth, Heckman, and Kautz 2011; Borghans, Duckworth, Heckman, and ter Weel 2008; Heckman, Stixrud, and Urzua 2006). With personality being so important in one's life, psychologists have long been interested in how personality develops. Early childhood experiences have been theorized as the most influential, with long-lasting effects on someone's development of traits, skills, cognition and more (Erikson 1963; Freud 1940/1949; Piaget 1970). Therefore, researchers studying personality development have put an emphasis on people's childhood and early adolescence (see Bornstein 2014 for a recent review of the literature). Siblings are a central part of the childhood family environment, which is often believed to play a crucial and long-lasting role for personality development. Two theories in psychology predicting opposing effects have motivated our research. For example, since women are on average less tolerant to risks, growing up with a sister may make boys less risk tolerant if they imitate their sister (social learning theory) or more risk tolerant if they differentiate themselves from their sister (sibling differentiation theory). Both theories have received considerable support in the literature. In this paper, I estimate the effect of siblings' sex on risk tolerance, trust, patience, the big five personality traits, and locus of control, aiming to give the most comprehensive analysis of siblings' sex effects thus far. Our identification rests on the assumption that, once parents have another child, it is random whether they will have a son or a daughter. This randomness allows us to estimate the causal effect of siblings' sex by comparing personality traits of people who grew up with a sister with those who grew up with a brother. For our analysis, I combine data from 8 surveys collected in 7 different countries for a total sample size of 42,220 people. Overall, I find no evidence that a sibling's sex affects any of the traits I look at.

The second paper investigates decisions under risk generally, and how to measure two personality traits related to risky decisions with the aim of better being able to explain decision-making. Because most decisions people face involve risk, social scientists have studied how to define and measure attitudes towards risk for a long time. Many decades of research have led to a multitude of risk elicitation methods. Yet, recent research shows that few of those measures are reliable and valid (Holzmeister and Stefan 2020; Pedroni et al. 2017). In this paper, I study locus of control (LoC) in combination with a self-reported willingness to take risks (WTR) as a predictor of risky decisions. I show that LoC predicts insurance choices in an incentivized experiment and among the same participants predicts socioeconomic outcomes. But while WTR predicts risky lottery choices in our data, LoC does not. Overall, our results suggest that the omission of LoC might bias the estimated effects of WTR in studies of decisions involving risk. Furthermore, a constant relative risk aversion (CRRA) parameter derived from lottery choices is not a significant predictor of insurance decisions inside the laboratory but is related to some socioeconomic outcomes outside of the laboratory. In our study, LoC adds significant additional predictive power beyond what is explained by WTR. Since LoC and WTR explain decisions involving risks independently when included in the same model (I found no significant interaction effect) and additionally are significantly correlated with each other, both traits seem to be important in understanding human decision-making under risk.

The third paper examines people's insurance decisions, another field of decision-making under risk and uncertainty. Governments often need to provide financial assistance after a severe disaster strikes a region. This need for government assistance can be mitigated with adequate insurance coverage. Yet, people often fail to purchase insurance, even if it is subsidized (Kousky and Kunreuther 2014; O'Donoghue 2015). Several behavioral reasons have been investigated why people fail to insure (Browne and Hoyt, 2000; Gallagher, 2014; Keller, Siegrist, and Gutscher 2006; Jaspersen and Aseervatham 2017; Kunreuther and Pauly 2018; Kamiya and Yanase 2019). I review the literature related to behavioral biases and add to this body of knowledge. To mitigate underinsurance generally and the negative effects stemming from behavioral biases and heuristics specifically, some researchers suggested using long-term insurance contracts to increase insurance penetration (Kunreuther 2008; Kleindorfer, Kunreuther, and Ou-Yang 2012). I examine insurance decisions in a computer-administered online experiment that makes several contributions. First, I confirm findings from previous studies showing that many people prefer long-term insurance. Offering this type of insurance might increase coverage overall. Second, I find that underinsurance can result from outdated insurance contracts that are rolled over. Long-term insurance contracts that consider price and value changes of an insured property over time may thus be helpful in mitigating this underinsurance. Third, I find people repeat the choices they made in a previous period. It seems that once people decided, the decisions become "sticky", so that the first period's choice largely explain future insurance choices. Fourth, I find New Zealanders, where house insurance penetration is extremely high, are more likely to insure their house in the experiment, especially with multi-year contracts. Our research suggests that reminding people of their previous decision and letting them know what many others are doing might be an effective and cheap way to increase insurance penetration and mitigate underinsurance that develops over time, while insurance premium subsidies seem to be costly and ineffective.

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Piaget, J. (1970). *Carmichael's Manual of Child Psychology*. New York: Wiley.

2 The effect of siblings' sex on economic preferences and personality traits[1]

By Thomas Dudek, Anne Brenøe and Jan Feld.

Abstract

Children spend a lot of time with their siblings and these interactions may shape their personalities. For example, since women are less tolerant to risks, growing up with a sister may make boys less risk tolerant if they imitate their sister or more risk tolerant if they differentiate themselves from their sister. In this paper, we estimate the effect of siblings' sex on risk tolerance, trust, patience, the big five personality traits, and locus of control. Our identification rests on the assumption that, once parents have another child, it is random whether they will have a son or a daughter. This randomness allows us to estimate the causal effect of siblings' sex by comparing personality traits of people who grew up with a younger sister with those who grew up with a younger brother. For our analysis, we combine data from 8 surveys collected in 7 different countries for a total sample size of 41,531 people. Overall, we find no evidence that siblings' sex affects any of the outcomes we look at.

Keywords Personality development · Adolescence to adulthood · Risk attitudes · Trust · Patience · Big five personality · Locus of control · Social learning theory · Sibling deidentification/differentiation

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[1] This paper is an older draft version of an updated, more extensive paper that we submitted with an additional co-author (Julia Rohrer) to the journal *Psychological Science*.

2.1 Introduction

Economic preferences and personality traits are important determinants of people's economic, social, and physical well-being. For example, risk attitudes are associated with saving and investment decisions (Guiso and Paiella 2005, 2008; Meier and Sprenger 2010; Meier and Sprenger 2013) and risk and health related behaviors, such as smoking, alcohol consumption and gambling (Anderson and Mellor 2008; Ida and Goto 2009). Trust is associated with cooperative behavior and may affect how well an economy thrives (Dincer and Uslaner 2010; Tov and Diener 2009; Zak and Knack 2001). Furthermore, the big five personality traits (openness, conscientiousness, extraversion, agreeableness, and neuroticism) are associated with educational attainment and achievement, job choice, income (Almlund, Duckworth, Heckman, and Kautz 2011; Borghans, Duckworth, Heckman, and ter Weel 2008; Kerr, Kerr, and Xu 2018) and work performance (Barrick, Mount, and Judge 2001; Penney, David, and Witt 2011). Despite the importance of these traits for life outcomes, it is not well understood how economic preferences and personality traits form.

In this paper, we investigate how siblings' sex affects risk tolerance, trust, patience, the big five personality traits, and locus of control (hereafter *traits*). Our identification strategy exploits that once parents decide to have another child, it is random whether they will have a son or a daughter. The randomness of a child's sex allows us to estimate the causal effect of siblings' sex by comparing traits of people who grew up with a younger sister with those who grew up with a younger brother. We use data from 41,531 respondents who participated in panel surveys from seven countries across four continents (USA, UK, Netherlands, Germany, China, Indonesia, and Australia).

Our study is motivated by two opposing psychology theories that help to understand how siblings' sex may affect traits. First, the theory of sex typing states that children differentiate themselves from their siblings and thus develop more *distinct* skills or traits (Bossard and Boll 1956). According to this theory, for example, a man should be more risk seeking if he has a sister instead of a brother, because he distinguishes himself from his sister by developing traits typically more pronounced in men (e.g., higher risk tolerance). Second, the theory of sex role learning states that children imitate their siblings and thus develop more *similar* skills or traits (Brim 1958). According to that theory, a man should be less risk tolerant if he has a sister instead of a brother, because he observes and learns from his sister and thereby develops traits typically more pronounced in women (e.g., lower risk tolerance).

Our study is further motivated by an emerging literature that shows that siblings' sex affects several economic and social outcomes. For example, Vogl (2013) examines data from Bangladesh, India, Nepal, and Pakistan and finds that having a younger sister increases the likelihood of women being married early and reduces their educational attainment and earnings. Similarly, Peter, Lundborg, Mikkelsen, and Webbink (2018) study Swedish registry data and find that men with a sister are also more likely to get married and have lower earnings, while women with a sister have a lower educational attainment and are more likely to get pregnant. Patacchini and Cools (2019) find in a US sample that women's earnings are higher when they have sisters only. In a different US sample, Rao and Chatterjee (2018) find that men's earnings decrease with the number of sisters, but they find women's earnings are unaffected by their siblings' sex. Brenoe (2021) studies Danish administrative data and finds that women with sisters (i) choose occupations that have a higher share of men at work, and (ii) are more likely to have a partner who works in an occupation with a higher share of female workers. These findings indicate that a siblings' sex affects people's decisions and thus their socioeconomic outcomes, but we do not know why.

Early research on sibling sex effects suggests a possible mechanism is the effect of sibling sex on personality. For example, girls with sisters develop less feminine interests than girls with brothers while boys' interests are unrelated to their siblings' sex (Grotevant 1978); boys with brothers tend to be more masculine compared to boys with sisters or no siblings (Sutton-Smith, Roberts, and Rosenberg 1964); and girls with older brothers appear more interested in more masculine activities requiring physical skill or strategy compared to girls with a sister or no siblings at all (Sutton-Smith and Rosenberg 1970).

The only previous study that has looked at the effect of siblings' sex on economic and social preferences in a similar fashion to our study indicates that the effect of siblings' sex on traits in fact is a plausible mechanism driving the effect of siblings' sex on outcomes. Detlefsen, Friedl, Lima de Miranda, Schmidt, and Sutter (2018) experimentally elicited measures of risk tolerance, trust, and patience in a sample of 312 high school students. Their point estimates suggest that, for example, first-born men with a younger sister are significantly more risk tolerant. They find similar but weaker effects for trusting behavior: both men and women with sisters are more trusting. The large effect sizes found in their study suggest that we should detect more effects of siblings' sex by looking at more outcomes in a larger sample.

Surprisingly, however, our results show no statistically significant effect of siblings' sex on any of the traits we analyzed. All our point estimates range from -5 percent to +5 percent of a standard deviation. They are precisely estimated with 95 percent confidence intervals allowing us to rule out effects smaller than -10 percent and larger than +10 percent of a standard deviation for all traits. As a comparison to Detlefsen et al.'s findings, we look at risk tolerance: Detlefsen et al. (2018) showed a statistically significant effect of +46 percent of a standard deviation on men's risk tolerance if they have a sister, while we find a statistically insignificant effect of +2 percent of a standard deviation. Moreover, with our 95 percent confidence interval we can rule out effects as small as +6 percent of a standard deviation. The lack of significant effects in our analysis suggests that findings of Detlefsen et al. (2018) may be a chance occurrence or are particular to their specific sample. Our analysis is based on the largest and most diversified sample examining sibling sex effects thus far, which should allow us to identify tiny effects of a sibling's sex on traits. The fact that we did not find an effect suggests that siblings' sex effects on a person's traits are not driving the previously identified siblings' sex effects on outcomes such as earnings and fertility.

2.2 Data

We use data from 41,531 respondents who participated in eight panel surveys from seven countries across four continents. The surveys we use are the National Longitudinal Study of Youth 79 (USA), the British Household Panel Study/United Kingdom Household Longitudinal Study (UK), the Millennium Cohort Study (UK), the Longitudinal Internet studies for the Social Sciences (Netherlands), the German Socioeconomic Panel (Germany), the China Family Panel Studies (China), the Indonesia Family Life Survey (Indonesia) and the Household, Income and Labour Dynamics in Australia Survey (Australia). Our dependent variables are self-reported measures of risk tolerance, trust, patience, the big five personality traits, and locus of control.

Many surveys measure traits in different ways. The most common measure of risk tolerance, patience, and trust is a question asking the respondent to rate themselves on a scale from 0 to 10 (or some deviation such as a scale from 1 to 4 or 1 to 5), where 0 means "not at all willing to take risks /patient/trusting" and 10 means "very willing to take risks/patient/trusting". Some datasets, however, measure risk tolerance and patience with methods typically used in experiments: survey participants' select gambles or select a required monetary amount in order to wait for a certain period of time. Yet another method of eliciting risk tolerance is to ask participants how much of their own money they are either willing to invest or actually investing in risky assets (e.g. on the stock market).[2] The big five personality traits and locus of control are always measured using self-reported ratings of one's agreement

[2] Self-reported measures of risk tolerance, patience, and trust on various scales are available in BHPS/UKHLS, HILDA, IFLS, LISS, MCS, NLSY79, and SOEP. The big five personality traits and locus of control are available in BHPS/UKHLS, LISS, HILDA, and SOEP. Experimental methods to elicit risk tolerance and patience are found in IFLS, LISS, and NLSY79. We derived risk tolerance from self-reported investment behavior in CFPS and HILDA.

or disagreement with different statements (ranging either from 1 to 5 or 1 to 7), such as “I am the life of the party”.

Table 1 shows summary statistics of all traits we study. Because some of the surveys use different scales, we have harmonized all economic preferences to a common scale from 0 to 10 and all other personality traits to a common scale from 1 to 7, because these are how those traits are measured in most of the sruveys. For our analyses, we standardize all traits to have a mean of zero and standard deviation of 1 within each of the surveys. Difference-in-means *t*-tests show that our sample has similar sex differences as shown in the literature. For example, men have a higher risk tolerance, and women score higher on conscientiousness and agreeableness (Falk et al. 2018; Vianello, Schnabel, Sriram, and Nosek 2013).

Table 1. Difference-in-means of Men’s and Women’s Average Traits

	(1)	(2)	(3)	(4)	(5)
Standardized personality traits	Overall Mean	Standard deviation	Mean Women	Difference Men-Women	Distinct people
Risk Tolerance (0-10)	3.30	3.03	2.908	-0.817***	34,309
Trust (0-10)	5.32	2.56	5.284	-0.080***	6,123
Patience (0-10)	4.42	3.31	4.309	-0.232***	12,690
Openness to Exp. (1-7)	4.45	1.14	4.473	0.046***	24,975
Conscientiousness (1-7)	5.11	1.14	5.224	0.238***	25,019
Extraversion (1-7)	4.62	1.23	4.677	0.114***	24,957
Agreeableness (1-7)	3.96	1.47	4.025	0.130***	24,940
Neuroticism (1-7)	3.71	1.27	3.768	0.132***	25,030
Internal Locus of Control (1-7)	5.19	1.14	5.178	-0.032**	17,886
Typical Female Personality Index	-0.04	1.00	0.162	0.414***	36,864

Column (1) shows the overall mean, column (3) shows women’s mean in traits. Column (4) shows the difference between men and women for each. The significance in the difference-in-means from a t-test is denoted as *** p<0.01 ** p<0.05 and shown in column (4). Observations vary by trait as shown in column (5).

Our identification rests on the assumption that the sex of their next born child is random. To test this assumption, we regress a younger sister dummy on one pre-determined characteristic of the older siblings. We repeat this exercise for several pre-determined characteristics and dummy variables for each survey. If sibling’s sex is random, we expect no significant relationships between the younger sibling’s sex and the pre-determined characteristics. We show our balance checks and provide a more thorough description of these in appendix A.2 and conclude in support of our assumption that overall, the sex of the next child is as good as random, which allows us to causally identify the effect of a younger sibling’s sex on the older sibling’s personality (natural experiment).

2.3 Empirical strategy

Our aim is to estimate the causal effect of a sibling's sex on traits. In such an analysis, selection bias is a valid concern as parents' decisions to have another child may depend on the sex and personality of the existing child (see Peter et al. 2018; Vogl 2013). However, once parents decide to have another child, it is random whether they will have a son or a daughter (as we have shown in our balance checks). We therefore estimate the following model

$$Trait_{it} = \beta\, younger\ sister_i \;+\; \gamma' \boldsymbol{X}_{it} \;+\; u_{it}\ , \qquad (1)$$

where $Trait_{it}$ represents a vector of economic preferences and personality traits of the older sibling *i* at time t; $younger\ sister_i$ is a binary variable equaling 1 if the younger sibling is female; X_{it} includes covariates for the repsondents' age, the age difference between them and their siblings, their parents' age at the birth of the older sibling, and a set of dummy variables for the datasets used; and u_{it} is the error term. Our parameter of interest is β, which shows the causal effect of having a younger sister (instead of a younger brother) on the older sibling's traits.

We perform our analyses separately for men and women to allow for potentially different effects by the older sibling's sex. To account for the fact that we observe many people multiple times in our dataset with their outcomes measured in different survey waves we cluster our standard errors at the individual level. In a separate specification, we use the average outcome per person as the dependent variable. This specification leads to very similar results. For ease of interpretation, we standardize each of our dependent variables to have a mean of zero and standard deviation of one.

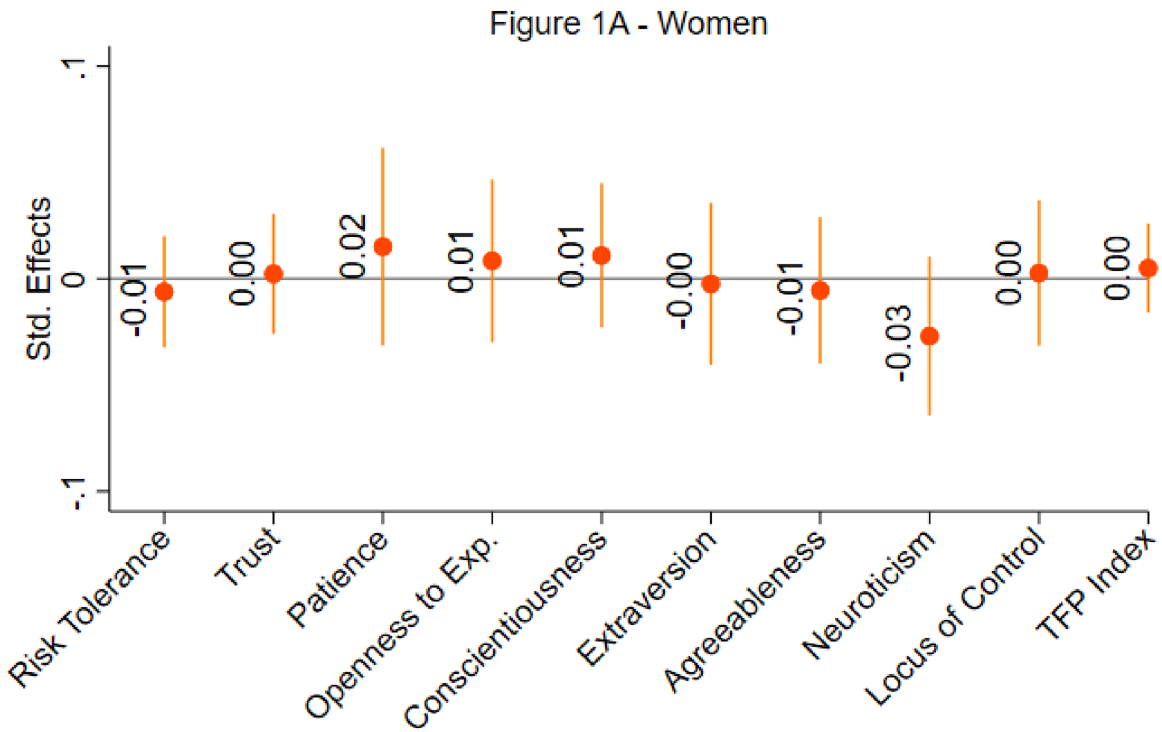
2.4 Results

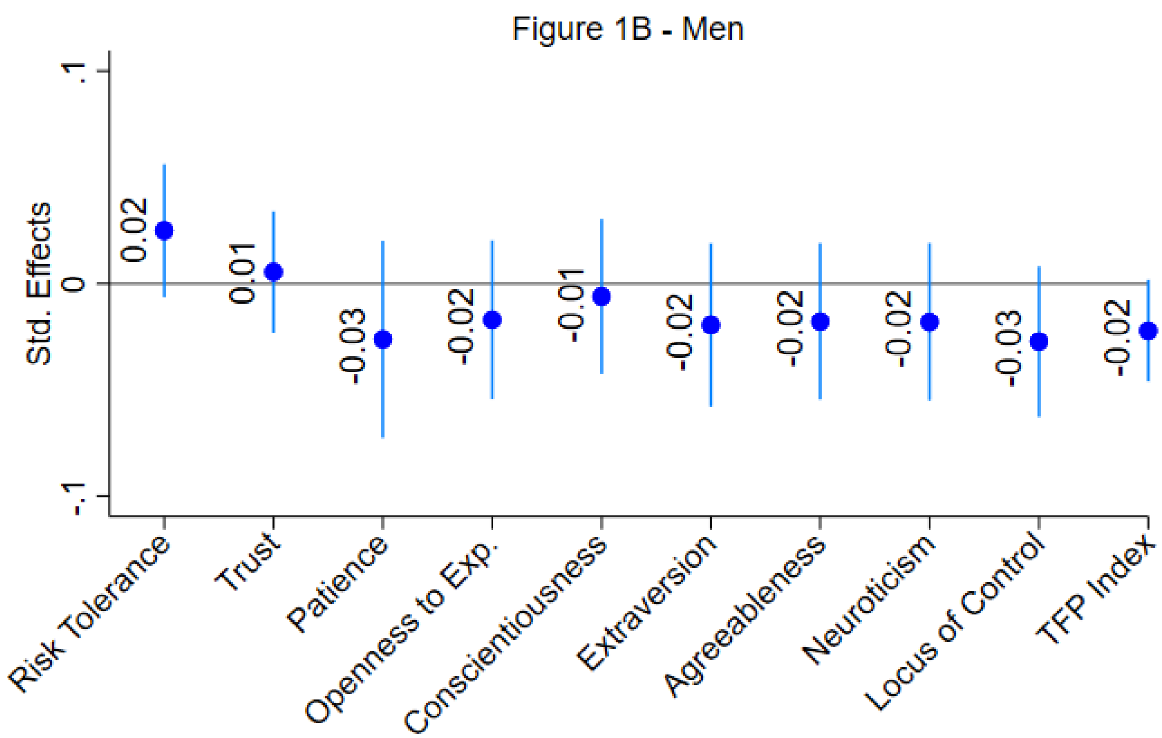
Figure 1 shows our estimates of the effect of siblings' sex on men's and women's economic preferences and personality traits. For both sexes, we find no statistically significant effects. All point estimates are small, between -5 percent and +5 percent of a standard deviation. These results are precisely estimated with the 95 percent confidence intervals allowing us to rule out effects of smaller than -10 percent and larger than +10 percent of a standard deviation for all outcomes – which are effect sizes that are typically considered small in the psychology literature.

Our results differ from findings of Detlefsen et al. (2018) who show, for example, that men are 46 percent of a standard deviation more risk tolerant when they have a younger sister compared to a younger brother. They also show statistically significant effects on both men's and women's trust and trustworthiness, but with smaller effect sizes.

There are three factors that can explain these differences in results. First, Detlefsen et al. (2018) rely on a younger sample (high school students compared to adults) and the effect of siblings' sex may have washed out by the time people have moved away from their siblings. However, an analysis of our data with a sub-set of our sample of people aged 16 and younger does not change our results significantly. Second, they use incentivized methods to elicit risk tolerance while we rely on self-reported surveys. While incentivized methods might measure economic preferences more precisely than self-reported measures, we believe that any differences in precision should be more than offset by our larger sample size (42,220 compared to 325). Third, the findings of Detlefsen et al. (2018) might either be a chance finding or a finding that is specific to their sample and not generalizable. Our more diversified and much larger sample size, on the other hand, suggests more generalizable results.

Figure 1. The effect of having a younger sister on personality traits





2.5 Conclusion

We combine data from 41,531 respondents of eight longitudinal surveys collected in seven countries to estimate the effect of siblings' sex on economic preferences and personality traits. Our results show that women's and men's traits are not significantly affected by their younger sibling's sex, with all our point estimates being small and precisely estimated. Recent research shows that siblings' sex affects marriage decisions, educational attainment (Peter et al. 2018; Vogl 2013), occupational choices and income (Brenoe 2021; Patacchini and Cools 2019; Rao and Chatterjee 2018). Our analysis suggests the sex of one's sibling is not a mechanism for these findings.

These findings may come as no surprise as gender differences in traits are fairly small in the first place. For example, in our data men are 0.82 points more risk tolerant than women on an 11-point scale from 0 to 10. That is a difference of about 28% and this difference is already the most notable and biggest gender difference in our data. All other traits have smaller differences between men and women (see Table 1 above). Furthermore, a potential influence of a sibling's sex on traits may have washed out over time and with additional environmental exposures, such as the school environment (Alison Booth, Cardona-Sosa, and Nolen 2014; Allison Booth and Nolen 2009) or whether one experienced an earthquake or lived through an economic crisis (Cameron and Shah 2015; Guiso, Sapienza, and Zingales 2018; Malmendier and Nagel 2011). The effect of environmental factors on traits may yet be another reason why we do not find any siblings' sex effects on traits (Tucker-Drob and Briley 2014; Vukasović and Bratko 2015). If siblings' sex has an effect, it may be only visible among very young children, potentially even only among those who have a small age difference between each other. This may be the case because children may affect each other only very early on and possibly even more so because children with a lower age difference develop together from an earlier stage of their lives and for a longer time. Yet, in our analyses neither the age difference between the siblings nor the age at which the trait was elicited show a statistically significant effect. Thus, we conclude that these are also likely unimportant factors in the development of one's traits.

Finally, if siblings' sex makes a difference, we simply might be unable to reliably identify such an effect unless we have much better data that also allows us to observe the parents' traits before the birth of each of their children and over time. Furthermore, we would need to know much more about the experiences made by the children and their exposure to life-shaping events together with their traits over time. We additionally would like to note here that it may be that there are other traits or factors unobserved in our data that are affected by a sibling's sex which drive the siblings' sex effects on outcomes. Our findings leave the question open to what we may attribute previous findings of siblings' sex effects on other outcomes than traits.

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3 More evidence on the risk elicitation puzzle: Can locus of control help where most measures of risk attitudes cannot?

By Thomas Dudek and Eric R. Ulm.

Abstract

Because most decisions people face involve risk, social scientists have studied how to define and measure attitudes towards risk. Many decades of research have developed a multitude of risk elicitation methods. Yet, recent research shows that few of those measures are reliable and valid predictors of risky decisions. In this paper we study locus of control (LoC) as a potential predictor of risky decisions. We show that LoC predicts insurance choices in an incentivized experiment and among the same participants predicts socioeconomic outcomes. Furthermore, LoC adds significant additional predictive power to another self-reported measure of willingness to take risks (risk tolerance). Interestingly, LoC and risk tolerance explain decisions involving risks independently when included in the same model and additionally are significantly correlated with each other. But while risk tolerance predicts risky lottery choices, LoC does not. Furthermore, CRRA is not a particularly reliable predictor of risky decisions inside the laboratory but predicts socioeconomic outcomes outside of the laboratory. Overall, our results suggest that the exclusion of LoC might bias the estimated effects of risk tolerance in studies of decisions involving risk and, thus, LoC might be an important variable in understanding decisions involving risk.

Keywords Individual decision-making · Choice under risk · Disaster insurance · Risk tolerance · Constant relative risk aversion (CRRA) · Locus of control · Personality traits · Experimental economics

3.1 Introduction

Social scientists have attempted to understand what determines human decisions under risk and uncertainty for many decades. Risk is involved in almost all decisions humans face, but it is especially prevalent in financial decisions. Recent studies have suggested that most measures of risk attitudes are inadequate (Beauchamp, Cesarini, and Johannesson 2017; Charness, Garcia, Offerman, and Villeval 2020; Csermely and Rabas 2016). These studies identified shortcomings, such weak correlations between repeated risk elicitations with the same measure (lack of test-retest validity), weak correlations between different measures (lack of convergent validity), and their inability to explain real-world decisions (lack of predictive validity). This problem has been recently described as the "risk elicitation puzzle" (Holzmeister and Stefan 2019; Pedroni et al. 2017). The question remains open how we should define and measure risk attitudes appropriately so that we are able to capture true underlying traits that can be elicited reproducibly and that explain decisions with uncertain outcomes in controlled and uncontrolled (natural) environments.

Locus of control (LoC) "refers to the degree to which people expect that a reinforcement or an outcome of their behavior is contingent on their own behavior or personal characteristics versus the degree to which they expect that the reinforcement or outcome is a function of chance, luck, or fate" (Rotter 1990, p. 489). On the lower end of the scale (external LoC), people tend to attribute most of their own life's outcomes to factors outside of their own control, i.e., to chance, luck, or fate, or even social and environmental circumstances. On the upper end of the scale (internal LoC), people tend to attribute most of their own life's outcomes to controllable factors such as one's own actions, effort, or skills. Locus of control hence measures a belief about how much effect one can have over the risk that influences one's life. Rotter himself has derived the measure of locus of control from studies that observed decisions involving chance and luck in his seminal article (Rotter 1966). Since then, many social scientists have shown that locus of control explains risky decisions and socioeconomic outcomes that involve risk. For example, a self-report of LoC was associated with a self-report of one's income, savings, smoking behavior, or self-employment status (Cobb-Clark 2015; Xue, Kidd, Le, Kirk, and Martin 2019).

But since most findings that associate LoC with decisions that involve uncertain outcomes are primarily based on correlations of self-reported measures, many economists may remain skeptical in using LoC as a control variable in research on risky decisions if there are no controlled studies that show that LoC predicts these decisions in contexts where they are subject to appropriate incentives. We are among the first to show experimentally that LoC is a predictor of incentivized insurance choices.

In this paper, we employ three different measures that each have been shown to explain risky decisions in previous research. But rather than only relying only on survey data, we show that locus of control (LoC) explains incentivized insurance decisions in a computer-administered experiment independent of a self-reported willingness to take risks (WTR). We offer the participants, who are New Zealand and Australian residents, the choice between no insurance, annual insurance, and non-cancellable long-term insurance. Our results show that internal LoC is specifically a significant predictor of purchasing long-term insurance in our experiment. If we assume that buying long-term insurance is related to giving up some of one's own decision autonomy, then people scoring higher on (internal) LoC might be less inclined to give up this autonomy (or control). As has been discussed by Rotter (1966), once someone learned that their own actions result in specific outcomes, this manifested belief could lead to a person with such beliefs being less likely to give up autonomy or to be more likely to engage in risky activities. People with an internal LoC may believe that chance, luck, or fate (i.e., risk) plays a less important role in their lives' outcomes than it actually does. Purchasing non-cancellable long-term insurance is plausibly related to giving up autonomy and to the belief whether chance plays a significant role in one's life.

Our study credits locus of control as a potential measure explaining risky decisions with a predictive validity, as we show that LoC correlates with incentivized risky decisions, even when we control for other measures of risk attitudes. We used a similar approach to an experimental study of incentivized risky choices that validated a self-reported willingness to take risks (WTR) measure (Dohmen et al. 2011). LoC and WTR are significantly associated with each other and independently explain risky choices inside and outside the lab. But we find no significant interaction between LoC and WTR when we include an interaction term in regressions, which suggests that the two measures indeed seem to capture distinct attitudes towards risks. A constant relative risk aversion (CRRA) parameter does not predict these insurance choices in our study. However, both LoC and CRRA correlate significantly with some risky decisions outside of the lab. But only the self-reported willingness to take risks also predicts lottery choices framed as such, while locus of control does not predict these. We hence provide evidence that multiple measures of attitudes toward chance in life outcomes may be needed to explain risky decisions or to control for risk attitudes appropriately. Nonetheless, our results suggest that additional research will be required to give more conclusive answers to the risk elicitation puzzle.

Our study contributes to the literature in several ways: First, our study alludes to the measurement of risk attitudes specifically. Some scientists regard risk attitudes as a single stable trait applicable to different contexts, although research has shown that risk attitudes change depending on the decision-context and that a single measure is likely not able to capture general or context-specific attitudes towards risks (Einav, Finkelstein, and Cullen 2010; Hanoch, Johnson, and Wilke 2006; Nicholson, Soane, Fenton-O'Creevy, and Willman 2005; Schoemaker 1993; Vosgerau and Peer 2019; E. Weber, Blais, and Betz 2002).

Second, our study presents and validates a new measure of locus of control. We use ten statements to measure LoC based on longitudinal surveys. However, we deviate from those measures slightly and replace some statements that have been shown to have a low factor loading onto the locus of control construct (Piatek and Pinger 2016). Our ten statements make up a measure that loads exclusively onto one factor, so that we have one unidimensional measure of external versus internal locus of control, which is an improvement over most measures of locus of control in longitudinal surveys.[3] Using fewer statements makes the measure a less accurate predictor of insurance choices, so that our measure of locus of control appears to require these ten statements for a better measurement of the underlying locus of control trait.

Among the same participants of the experiment, we show that our measure of LoC is associated with higher income and a lower likelihood to be unemployed, thereby supporting previous research findings and thus suggesting that our measure of LoC captures a similar trait to other studies' measures of LoC. Additionally, in our data, locus of control positively predicts home ownership – another decision over a potentially risky asset. Since LoC predicts insurance decisions in our incentivized experiments and other socio-economic outcomes as expected, we provide evidence that LoC might be an important predictor of decisions involving risk. To reduce some measurement error in LoC, we partialled-out sex and age differences (following a similar approach to Beauchamp et al., 2017). Our results do not change qualitatively whether we use a measurement error corrected LoC.

Third, we show that locus of control can explain some of the variation in socio-economic outcomes and risky decisions beyond the variation explained by the self-reported willingness to take risks *and* the big five personality traits. Since LoC and WTR are statistically significantly correlated and predict choices with uncertain outcomes independently, we propose that studies of decision-making under risk should include both measures or they might otherwise have biased estimates.

[3] The original locus of control scale was developed so that it is a single, unidimensional instrument. This unidimensionality, however, has often been questioned and challenged by researchers who rely on longitudinal survey data (Piatek and Pinger 2016). Our measure of LoC achieves this unidimensionality with an eigenvalue of 4.14 on the first factor, and an eigenvalue of only 0.64 on the second factor.

Our study is not the first to examine LoC as a predictor of insurance decisions. To the best of our knowledge, two prior studies examined this direct association with survey data (as opposed to an incentivized decision-experiment as ours) and one study investigated this association in an incentivized experiment. We discuss the literature relating LoC to risky decisions more thoroughly in section 3.2. This is followed by a description of our methods and data in section 3.3. Section 3.5 presents the results and section 3.6 discusses these results and concludes the paper.

3.2 Related literature

Irrespective of how salient or large the risks are, every financial decision we make involves risks. Because of the ubiquity of risk in the decisions people face, social scientists – and among those primarily psychologists and economists – have studied how to *define and measure* someone's attitude towards risk. Without the appropriate measures of risk attitudes, one cannot study decision-making properly. But how exactly to measure risk attitudes appropriately is an empirical question that can only be answered with observed data. Many ways have been suggested how to measure risk attitudes so that they are measurable reliably over time and can explain decisions involving uncertain outcomes in many different contexts. Both psychologists and economists have approached this scientific endeavor differently.

In economics, mathematical models have been established as the prevailing normative or descriptive models of risky decisions (e.g. expected utility theory, Bernoulli 1738, 1954 and von Neumann and Morgenstern 1944; von Neumann and Morgenstern 2007; rank-dependent utility, Quiggin 1982; and prospect theory, Tversky and Kahneman 1992). Economists have taken these mathematical models as the basis to develop their measures of risk attitudes derived from choices typically presented as a lottery type (Anderson and Mellor 2008, 2009; Eckel and Grossman 2008; Holt and Laury 2005) or as an elicitation in form of a willingness to pay or to accept a payment for some product, service, or action (e.g., Hey, Morone, and Schmidt 2009).

One most note that such models have been developed to primarily extract the marginal utility of money and not as a measure of general risk attitudes. Since these measures of attitudes are supposed to extract a parameter that explains one's marginal utility of money, the measures prime people towards financial decision-making and thus might only elicit risk attitudes within the financial domain specifically. Nevertheless, some economists have treated such elicited risk attitudes as universal and used those risk attitudes to explain risky decisions in non-financial domains, although research has shown that risk attitudes are not the same across contexts (e.g., Anderson and Mellor 2008; Weber et al. 2002). Some newer methods do not impose a financial domain and thus might elicit more general risk attitudes (e.g., Crosetto and Filippin 2013).

Psychologists have taken an approach less anchored in the mathematical decision-models mentioned above, and they also do not primarily aim to understand people's marginal utility of money, so that these models are potentially able to capture more general risk attitudes than those derived from, e.g., lottery choices. Psychologists define attitudes broadly as a context-dependent tendency of thinking or feeling about something that is based on prior experience with people, situations and events, and is deeply rooted in evaluation of people, situations or events. (Eagly and Chaiken, 2007; Greenwald, Brock and Ostrom, 2013; Shrigley, Koballa Jr. and Simpson, 1988). Following this very broad definition of attitudes from psychologists, we believe that someone who thinks or feels they can control risky outcomes might as well have a specific attitude towards risks. So far, locus of control has not been considered specifically as a measure of risk attitudes.

Among the most prominent behavioral measures of risk attitudes from psychology are the Balloon Analogue Risk Task (Lejuez et al. 2002), the Columbia Card Task (Figner et al. 2009), the Game of Dice Task (Brand et al. 2005) and several others (Bechara et al. 2005; Levin and Hart 2003; Rogers et al. 1999). Psychologists also developed a multitude of self-report methods that either elicit traits related to risk taking (Eysenck et al. 1985; Zuckerman et al. 1978) or directly elicit risk taking with survey questions (Blais and Weber 2006). Locus of control is measured with survey questions, as well.

Beyond the criticism that most measures might be more suitable to elicit *financial* risk attitudes, another criticism often raised is that most methods mentioned here are highly mathematical and thus might be difficult to interpret or understand for some people (Mata et al. 2018). It is possible that these mathematical methods elicit risk attitudes that are confounded with other variables (e.g., intelligence, knowledge, other skills). We refer the interested reader to a more thorough discussion of the relationship between risk attitudes and other traits to Canavari et al. (2019) and Almlund et al. (2011). More important with regards to this study, recent research has seriously challenged many of those methods as inappropriate and incapable to elicit attitudes towards risks, mainly because the different measures of risk attitudes are internally invalid, unreliable and/or do not predict real-world decisions (Beauchamp et al. 2017; Charness et al. 2020; Csermely and Rabas 2016; Frey et al. 2017; Mata et al. 2018).

Internal validity is measured primarily in three ways: (a) by calculation of the correlations between several measures (convergent validity), i.e., how well different measures of risk attitude explain each other; (b) by calculation of an internal reliability coefficient (Cronbach's alpha, Cronbach 1951; Tavakol and Dennick 2011); and (c) by calculation of correlations of the same measure over time (test-retest correlation, i.e., temporal stability). Correlations across different methods (convergent validity) show how reliable measurements of a truly observable risk attitude are and are deemed as sufficient when they are larger than 0.4. A Cronbach's alpha above 0.7 is regarded as a sufficient reliability coefficient for measures that aggregate multiple statements to elicit one trait, although some accept lower levels between 0.5 and 0.6. Generally, the higher the alpha the more internally reliable the measure is. And test-retest correlations above 0.4 are interpreted as capturing the same trait well-enough over time, so that one can infer this trait to be a fairly stable, truly observable trait, whereas larger correlations indicate a better reliability of the measure. Predictive validity is identified by the measure's explanatory power of real-world behaviors, choices, and outcomes. If the measure is associated statistically significantly (e.g., p-value<0.05) with the outcome variable and explains a large share of its variation (e.g., R-squared>0.1), the measure is said to have a good predictive validity.

Since there has been mounting evidence that many of the methods to elicit risk attitudes are inconsistent and do not explain real-world outcomes well, a recent study coined the term "risk elicitation puzzle" and thereby reignited the scientific endeavor to study how one should measure risk attitudes appropriately so that we have a measure that is internally valid and explains risky decisions reliably (Holzmeister and Stefan 2020; Pedroni et al. 2017).

On the other hand, we have a very good understanding about locus of control's (LoC) reliability, internal and predictive validity. For example, we know that LoC is a stable and repeatedly measurable trait, at least in the short-term (Cobb-Clark and Schurer 2013; Preuss and Hennecke 2018). LoC has an acceptable alpha coefficient and reaches acceptable levels of test-retest correlations (giving LoC a certain internal validity). In the appendix (section B.5) we show test-retest correlation matrices and alpha coefficients from three longitudinal surveys on which we based our own LoC measure. The tables provide suggestive evidence for the temporal stability and internal reliability of our measure of locus of control. Indeed, our measure's alpha is even higher than the one from any of the other surveys. For researchers of decisions that involve risk (or uncertain outcomes), it is useful to have a measure that has been shown to have such desirable psychometric properties (*internal validity* and measurement *reliability*).

Beyond the previously mentioned criticism and shortcomings of the mathematically derived methods of risk attitudes, another criticism is that they generally take a long time to measure and are typically financially incentivized, which makes them more expensive than survey measures. The time and cost involved in measuring risk attitudes with such methods also makes them rather unfeasible for use in longitudinal surveys. Locus of control, on the other hand, is not incentivized, does not require a good understanding of numbers, and can be measured much more quickly. But since people are typically not paid for reporting their "true" locus of control trait, LoC has only received little attention from economists who study risky decisions. This is so because some social scientists argue that, without proper incentives, people will not take the tasks or questions they are confronted with seriously (Azrieli et al. 2020; Camerer et al. 1999; Harrison et al. 2005). This is often true, although this notion has been recently challenged by some studies. Contesting studies find that non-incentivized measures are even better or at least equally good predictors of real-life decisions (Dave et al. 2010; Lönnqvist et al. 2015). In fact, the self-reported measure of a willingness to take risks also is not incentivized (Dohmen et al. 2011; Falk et al. 2018), and yet is widely used by both economists and psychologists. This measure has been shown to be a reliable and valid predictor of decisions involving risk inside and outside of the lab and across countries (Dohmen et al. 2017; Dohmen et al. 2011; Falk et al. 2018; Josef et al. 2016), which is a similar finding and approach to this paper relating decisions with uncertain outcomes to locus of control.

Moreover, locus of control (LoC) has been linked to many risky decisions in a broad range of contexts, crediting LoC with a certain *predictive validity*. Predictive validity ensures that the measure is not just a statistical artifact or a merely theoretical measure but that it in fact explains decisions in real world contexts. For example, LoC predicts a higher likelihood of choosing self-employment (Caliendo, Fossen, and Kritikos 2014), choosing jobs with performance-related payment (Heywood, Jirjahn, and Struewing 2017), and job search effort (McGee and McGee 2016; McGee 2015). Locus of control is also linked to educational attainment (Coleman and DeLeire 2003; Jaik and Wolter 2016). One study has shown that LoC is associated with a higher likelihood of adopting new technologies (Abay et al. 2017). These and other studies show that LoC is an important trait with regards to the labor market (Cobb-Clark 2015; Wang et al. 2010; Xue et al. 2019). Furthermore, locus of control has been associated with many health-related behaviors such as smoking and alcohol consumption (Caliendo and Hennecke 2020; Cheng et al. 2016; Cobb-Clark et al. 2014). All these outcome variables have been also associated with the self-reported willingness to take risks measure.

More related to this paper, LoC has also been shown to predict financial decisions such as risky investments and savings inside and outside of an experiment (Cobb-Clark et al. 2016; Pinger et al. 2018; Salamanca et al. 2020; Schäfer 2016). For example, a higher score on locus of control (LoC) is associated with less diversified investment choices in an experiment (Pinger et al. 2018) and higher equity holdings in longitudinal survey data (Salamanca et al. 2020). Both outcome variables are associated with larger financial risks.

The most related literature to ours has also shown that locus of control is associated with insurance decisions. Antwi-Boasiako (2017) has used survey data to show that people scoring higher on LoC are more likely to own home insurance. As the author argues, people who believe that they control their own destiny might be more inclined to take actions aligned with this belief, e.g., by purchasing insurance. This study, however, did not include additional measures of risk attitudes. Robinson and Botzen (2020) provide supportive evidence of the positive association between insurance demand and internal locus of control in experimental data, even after controlling for other measures of risk attitudes. Their results, however, suggest that internal locus of control is only significantly related to insurance demand when the chance of a disaster is relatively high. Further support for these two findings is provided by Dudek (2021), who uses survey data to show that internal LoC is positively associated with personal (health and life) insurance choices, which again is robust to the inclusion of self-reported measures of risk attitudes and the big five personality traits.

Overall, the literature suggests that locus of control can predict risky real-world behavior in various contexts, inside and outside of the lab, often better than most of the other measures of risk attitudes discussed previously. Additionally, LoC can be measured more quickly and cheaper than behavioral measures.

3.3 Methods and descriptive statistics

Our experiment starts with the elicitation of risk attitudes (a constant relative risk aversion parameter, a willingness to take risks and locus of control) and the big five personality traits. Participants make insurance decisions *after* they finished all trait and attitude elicitations. We designed the experiment specifically this way to avoid any potential reverse causality problems. A measurement of risk attitudes (or of any trait) after the experiment might be affected by the experiment's decisions or outcomes and thus must be considered as a bad control variable (Angrist and Pischke, 2008, p. 64-68).

3.3.1 Experimental design

We conducted two versions of our insurance experiment online on the research platform Prolific Academic. Apart from the insurance premium, the experimental design is the same in both versions of our experiment. The experiment's instructions (apart from the premium) and advertisements posted on

Prolific also were the same. In one version we offered actuarially fairly priced insurance (Ins_{fair}) and in another version we offered subsidized insurance (Ins_{subs}). The first time we mention the price (premium) is in the instructions of the experiment, i.e., after we measured participants' risk attitudes and personality traits, therefore participants should not have had a different incentive to enter the study based on the different experimental versions.

We discuss some of the experiment's design features in more detail in the appendix. Importantly, we show that participants are similar (between the two versions) and make statistically indistinguishable choices in both versions of our experiments, which likely renders the experimental versions and features irrelevant for the current paper. To answer whether locus of control is a valid predictor of risky decisions beyond or instead of other measures of risk attitudes, the insurance features should not matter. We hence combined the data from both experiments and primarily discuss the data and our results as if they came from one large experiment.

In the experiment, participants essentially have four options to choose from: They can either not insure in any of two years within an experimental period; they can insure only one of the two years; they can insure both years with an annual insurance contract; or they insure both years with a long-term insurance contract.

3.3.2 Risk attitudes and personality

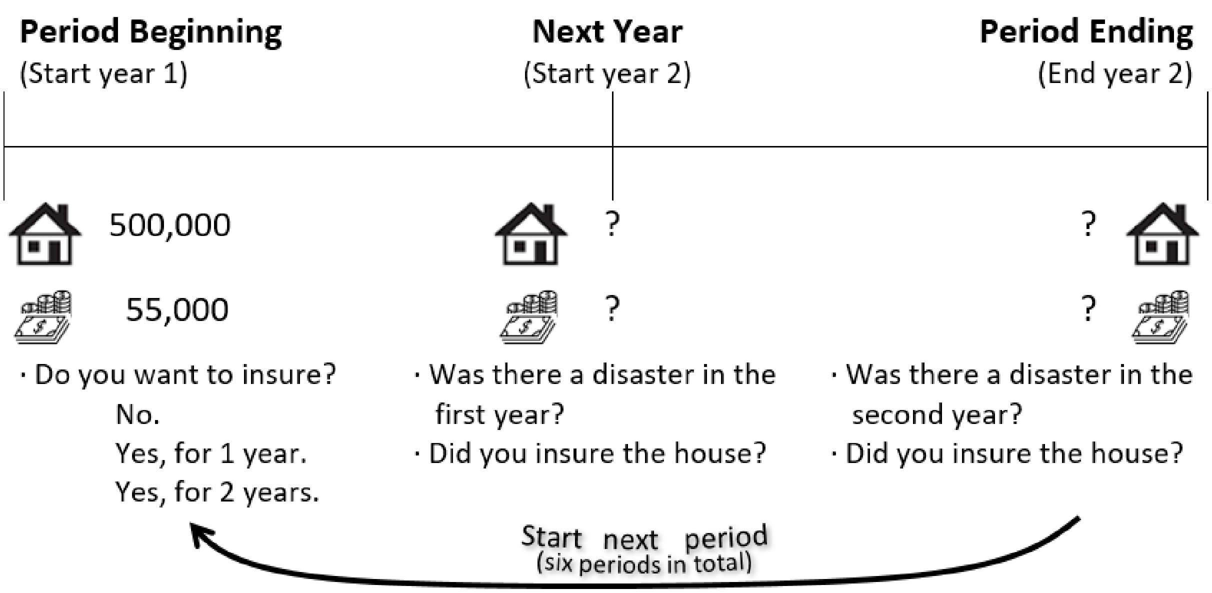
Before the insurance decision task, participants first make 60 binary lottery choices that we use to elicit a constant relative risk aversion (CRRA) parameter in the fashion of Harrison and Ng (2016) and then self-report their willingness to take risks in general and in financial matters on an 11-point scale (Falk et al., 2016). After the elicitation of people's propensity to take risks, we measure locus of control (10-item questionnaire) and the big five personality traits (25-item questionnaire, 5 items per trait). To measure these traits, we ask people to self-report their agreement or disagreement with multiple statements on a Likert scale from 1 to 7. We adopt statements from widely studied longitudinal surveys such as the National Longitudinal Survey of Youth 79 Children/Young Adults (NLSY79 CH/YA), the Household Income and Labour Dynamics in Australia (HILDA) Study, and the German Socio-Economic Panel (SOEP). All measures are described in more detail in Appendix B. At this stage we need to note that our CRRA parameter likely cannot explain the insurance decisions in our experiment and that we should not expect high correlations, because the experiments have no proper risk-return trade-off since they are offered at a subsidized or actuarially fair premium. With such an insurance design, everyone who is classified as risk averse according to the CRRA parameter (100% of our participants) should choose full insurance coverage. We discuss this in more detail in sections 3.5 and 3.6.

After the elicitations of risk attitudes and personality, participants perform the home insurance decision task. We closely follow the suggested design features of insurance decision experiments described by Jaspersen (2016).[4]

3.3.3 Insurance decision task

See Figure 2 for an illustration of the experimental process as described in detail below. This figure was also shown to participants during the instructions. Participants make insurance decisions in six experimental periods. Each period accounts for 2 years (so that we can have up to 12 decision points per participant).

Figure 2. Experimental process.



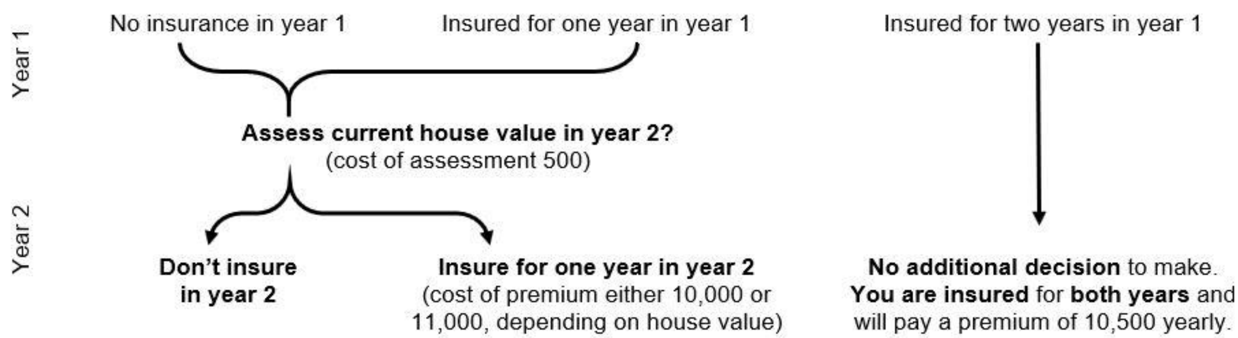
In the insurance decision task, people can choose between three insurance options *in the first year of each period*: they can choose (1) not to insure, (2) to insure with an annual contract, or (3) to insure

[4] The only feature we have not included is a short task before the insurance decision task to make the participants feel like they earned the endowment. Some research has shown that an endowment may feel like a windfall payment to some participants, which might have an effect on risk-taking behavior (see Thaler and Johnson (1990), G. Harrison (2007) and Cárdenas, De Roux, Jaramillo, and Martinez (2014) for related experimental studies). However, the studies mostly related to ours also have not included such a task before participants were given an endowment and these studies found very similar insurance choices to those in our study (Kunreuther and Michel-Kerjan 2015; Papon 2008).

biennially (with a long-term contract) at a fixed price without the option to cancel after the first year. If participants decide to insure with a biennial contract, they have no further decisions to make in year 2 of that period, as they are then insured for a fixed price in both years. But those who did not insure or insured with an annual contract can decide whether to insure in the second year.

We tell participants that there is a 10% chance of an occurrence of a disaster and that this is independent of each year and period. Since we conducted the experiments online, the computer randomly draws a number from 1 to 100 and triggers a disaster only if the drawn number is less than 11. Participants are told that this is a random computer-generated process, and they receive information about the drawn number (i.e., whether a disaster happened) after every year to build more trust in the random mechanism. We additionally showed participants the decision tree in Figure 3 to help them with their understanding (the numbers here correspond to those in the subsidized experiment). The contract premiums are all either actuarially fair (no loading) or subsidized (40% of the actuarially fair price) for full coverage (no deductibles).

Figure 3. Insurance decisions in year 1 and year 2 of each period.



This figure shows the decision-tree of participants at the outset of each period. One period involves decisions for two years. In year 1, a participant can choose between (a) no insurance, (b) insurance for one year, and (c) insurance for two years. After the first year, those who did not insure (a) or insured only the first year (b), can choose whether to have their current house value assessed. This decision is not investigated in the current study. If the house value increased after the first year (this happens with a 50% chance and results in a house value increase of 10%), insurance for the second year becomes more expensive – in line with the increased house value. Participants can then decide again whether to insure. Participants who insured with a biennial contract, are automatically insured for two years at a fixed premium, which takes into account potential house value increases and covers those, as well.

We tell participants that there is a 50% chance for the house value to increase in year 2 and that this increase brings along an increase in the premium if participants pay for a house value assessment in the second year. Participants are not automatically informed of a potential house value increase. Instead, participants must 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 coverage will reflect this increase, as well. This option is only available to those who did *not* insure *or* insured with an *annual* contract in the first year of any period. Everyone who chooses to insure with a long-term contract is automatically covered in full in both years, even if the house value increases. This feature makes the long-term contract slightly more expensive annually but is priced according to the expected premium of two annual contracts over both years within a period.

To make the incentives similar among the two experimental versions (and across periods), we offered the house assessment at the same subsidized rate as the premiums, whereas it is not subsidized in the actuarially fair version. Since the subsidies nor the house value changes affected insurance decisions, we do not discuss these features any further in this paper. Choices in our experiment are similar across both versions (actuarially fair and subsidized) and are not far from the choices made in a closely related paper of insurance decisions (Kunreuther and Michel-Kerjan 2015). We show evidence for the similarity of choices and of participants in both experimental versions in Appendix C (Table C-4).

3.3.4 Incentivizing participants with the random problem selection mechanism

Participants are incentivized in both the lottery choice risk elicitation task (60 pie charts that present varying risky payoffs, see Appendix B.1 for more details) and the insurance task by being told that all their choices have a chance of being paid out so that they should choose carefully according to their own preferences. Paying according to the so-called random problem selection mechanism (RPSM) has been recognized as incentivizing participants so that they make choices more in line with real-world behavior and preferences, albeit not without criticism (Azrieli et al., 2018, 2020; Camerer et al., 1999). The computer randomly draws one of 60 pie charts and one of the six insurance decision periods according to which participants get paid. The payment for the insurance decision task is the sum of the house value and their bank account (less any paid insurance premiums) at the end of a randomly selected period. Every participant gets paid based on one of the 60 lottery choices *and* one of the six insurance decision periods.

3.4 Data

3.4.1 Dependent variables

The main dependent variable in our analyses of the insurance experiment is a categorical variable that takes the value 0 if the participant did not insure any of the two years within a period (denoted as alternative 0,0; not shown in the table); takes the value 0.5 if a participant only insured one of two years in the experiment (denoted as alternative 0,1 or 1,0); takes the value 1 if the participant insured both years with an annual insurance contract (1, 1); and takes the value 2 if the participant insured both years with a biennial insurance contract (2, 2). We analyze this dependent variable with multinomial logistic regressions. Table 2 shows the percentage of participants who chose each of the insurance alternatives by experimental version for each period. In the appendix we show this table in a more extensive format and compare our results to a related paper (Table C-4).

Table 2. Descriptive statistics of insurance choices by period for both experimental versions.

Panel A. Ins$_{fair}$	(1) Period 1	(2) Period 2	(3) Period 3	(4) Period 4	(5) Period 5	(6) Period 6	(7) Average
(0, 0)	20.1%	20.8	21.5	17.4	21.5	20.1	20.2
(0, 1)	6.7	4.7	5.4	4.7	6.0	5.4	5.5
(1, 0)	5.4	2.7	4.0	4.0	5.4	9.4	5.1
Only one year	12.1	7.4	9.4	8.7	11.4	14.8	10.6
(1, 1)	21.5	26.8	28.9	31.5	27.5	29.5	27.6
(2, 2)	46.3	45.0	40.3	42.3	39.6	35.6	41.5
Both years	67.8	71.8	69.1	73.8	67.1	65.1	69.1%
Panel B. Ins$_{sub}$	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Average
(0, 0)	17.8%	16.3	15.2	15.9	17.0	12.9	15.8
(0, 1)	7.6	5.7	7.6	8.7	9.1	8.7	7.9
(1, 0)	4.2	6.4	8.0	5.3	5.7	6.8	6.1
Only one year	11.7	12.1	15.5	14.0	14.8	15.5	14.0
(1, 1)	22.3	23.5	21.6	25.4	26.5	24.2	23.9
(2, 2)	48.1	48.1	47.7	44.7	41.7	47.3	46.3
Both years	70.5	71.6	69.3	70.1	68.2	71.6	70.2%

This table shows the percentage of participants who chose each of the insurance options in the actuarially fair experiment (Panel A, N=149) and in the subsidized experiment (Panel B, N=263) for each of the six insurance periods separately. Recall, a value 0 and 0.5 indicate no insurance or only one out of two years was insured, 1 indicates both years were insured annually, and a value 2 indicates they were insured biennially in any specific period.

The main dependent variable in our analyses of the lottery choices is a dummy variable that indicates whether the participant chose the safer pie chart out of two shown pie charts within a pair of lottery choices. It takes the value 1 if the participant chose the safer pie chart, and 0 otherwise. We analyze this dependent variable with logistic regressions. We apply three different classifications of what determines a safe choice. First, we compare the two pie charts within each pair by their standard deviation. The lower standard deviation (SD) classifies the safe option. Second, we compare the pie charts by their coefficient of variation (the standard deviation multiplied by 100 divided by the expected value). The lower coefficient of variation (CV) classifies the safe option. The CV is a dimensionless measure and has been shown to be a measure that explains risky choices better than the SD or expected value (Weber, Shafir, and Blais 2004). Third, we compare the pie charts by their expected value (the sum of each outcome probability multiplied by its corresponding payment). The expected value (EV) is the standard measure to identify the utility maximizing (i.e., "best") option within a choice set of options and is typically used in economic risky analyses. A risk-neutral participant should always choose the option with the higher EV.

As we can see from Table 3, participants on average made different choices and thus would be classified differently based on the SD, CV, or EV.

Table 3. Descriptive statistics of safe lottery choice variables

	Mean	Std. Dev.	Min	Max
Safe Choice SD	0.48	0.50	0.00	1.00
Safe Choice CV	0.51	0.50	0.00	1.00
Safe Choice EV	0.50	0.50	0.00	1.00
Times Safe Choice SD	29.78	6.82	11.00	50.00
Times Safe Choice CV	30.73	6.41	14.00	49.00
Time Safe Choice EV	28.60	4.08	18.00	41.00

This table shows descriptive statistics of the dependent variables derived from the choices made by participants in the lottery choice task (60 pairs of pie charts that offer varying payoffs with different probabilities).

The main dependent variables in our analyses of real-world choices are years of education, income (categorical variable), and three dummy variables measuring the participant's home ownership status, unemployment status, and partnership status. The dummy variables take the value 1 if the status is true, and 0 otherwise.

Table 4. Descriptive statistics of demographics

	Mean	Std. Dev.	Min.	Max.
Education in years	16.2	2.65	9.00	22.00
Income (categorical)	5.33	2.42	1.00	9.00
Unemployment status	0.10	0.30	0.00	1.00
Partnership status	0.52	0.50	0.00	1.00
Homeownership status	0.40	0.49	0.00	1.00

This table shows summary statistics of all participants of the demographic information that we collected with survey questions and used as dependent variables in our result section 3.5.3, but which function solely as covariates in all other results sections.

3.4.2 Independent variables

We have a total of 413 participants from New Zealand (n=221) and Australia (n=192) of whom 50% are female. 149 participated in the actuarially fair version and 264 participated in the subsidized version of our experiment. Of the 413 participants, 22% are students. Our participants' median age is between 26 and 35 years.[5] The entire study took participants on average 45 minutes and participants were paid on average GBP 17.63 (SD=4.70, min.=10.74, max.=34.00). We standardize all our independent variables (traits and non-dummy demographic variables) to have a mean of zero and standard deviation of one. Table 5 shows summary statistics of personality traits. Additional statistics for the demographic data as well as descriptions of all variables are available in Appendix C.3. We find some significant

[5] We measured age in intervals: 1=*18-22*; 2=*23-25*; 3=*26-30*; 4=*31-35*; 5=*36-40*; 6=*41-50*; 7=*51-60*; 8=*61-70*; 9=*70+*. (Mean=3.75 Std. Dev.=1.83) Age in years takes the median value for those whose exact age we did not know.

differences in some of the big five personality traits between the samples of the two experimental versions (see Table 5).

Table 5. Descriptive statistics of personality traits

	Mean Combined	Std. Dev.	Mean Ins_{fair}	Mean Ins_{subs}	Difference in Means	p-value
Internal Locus of Control (LoC)	0.00	1.00	-0.02	0.01	-0.03	0.766
Risk tolerance (R)	0.00	1.00	-0.06	0.03	-0.10	0.345
Financial risk tolerance (FR)	0.00	1.00	0.04	-0.02	0.07	0.524
Principal component risk (WTR)	0.00	1.00	0.00	0.00	0.00	0.970
Constant relative risk aversion (CRRA)	0.00	1.00	-0.07	0.04	-0.12	0.257
Openness to Experiences (O)	0.00	1.00	0.24	-0.14	0.38	0.000
Conscientiousness (C)	0.00	1.00	0.18	-0.10	0.27	0.007
Extraversion (E)	0.00	1.00	-0.26	0.15	-0.41	0.000
Agreeableness (A)	0.00	1.00	0.09	-0.05	0.14	0.166
Neuroticism (N)	0.00	1.00	-0.07	0.04	-0.11	0.264

Summary statistics for standardized personality measures and a t-test for all personality measures for difference-in-means of participants in the actuarially fair and the subsidized version of the experiment.

It is well known that measures of personality traits and risk attitudes suffer from measurement error. To reduce measurement error due to age and sex differences, we partialled out age and sex effects by first regressing each trait on the respondents' age, age squared and sex and then predicted the residuals of these regressions (see Beauchamp et al. (2017) for a similar approach). The predicted residuals represent our measures of personality traits (broadly defined, including risk attitudes, the big five traits and locus of control).

The personality measurements' pairwise correlations are shown in Table 6. Cronbach's alphas for the big five traits and locus of control are shown in parentheses in the first column of the table. We describe in detail how we measured each trait in Appendix B. All our traits have acceptable internal reliability (alpha) ratios (where these are measurable).

Table 6. Correlation matrix of personality traits

	CRRA	R	FR	WTR	LoC	O	C	E	A	N
CRRA	1									
R	-.026	1								
FR	-.072	.575***	1							
WTR	-.05	.882***	.886***	1						
LoC (α=0.87)	-.029	.178***	.170***	.185***	1					
O (α=0.84)	-.005	.009	-.004	-.011	.052	1				
C (α=0.73)	.01	.041	.007	.020	.22***	-.062	1			
E (α=0.82)	.004	.333***	.200***	.29***	.235***	.063	.022	1		
A (α=0.68)	-.067	.218***	.089	.161***	.295***	-.066	.255***	.169***	1	
N (α=0.77)	.085	-.203***	-.141**	-.187***	-.341***	.089	-.197***	-.149**	-.188***	1

Pairwise correlation matrix for personality traits. CRRA=Constant Relative Risk Aversion; R=general willingness to take Risks; FR=willingness to take Financial Risks; WTR= Principal component of R and FR; LoC=Locus of Control; O=Openness to experiences; C=Conscientiousness; E=Extraversion; A=Agreeableness; N=Neuroticism. Cronbach's alphas for each trait are shown in parentheses, where possible. *** p<0.001; ** p<0.01; *p<0.05.

Two of our main traits under investigation – locus of control and willingness to take risks – are positively correlated, which is consistent with other studies that explored these two traits (Pinger et al. 2018; Plunkett and Buehner 2007; Salamanca et al. 2020). We also find some significant correlations between our measures of risk attitudes and big five personality traits. Note that none of the self-reported traits correlate significantly with the CRRA parameter. This fact suggests either of two things: CRRA does not measure an underlying general risk attitude and instead captures people's marginal utility derived from money as it is supposed to, or our CRRA parameter might be measured incorrectly. Recall, as we discussed in the literature review, that many previous studies have shown that CRRA parameters elicited with similar methods to the one we use have low to no correlations with other measures of risk attitudes and the method we used has been applied in other insurance papers successfully (Harrison and Ng 2016, 2018). We therefore do not think our CRRA elicitation is problematic.

43 participants had one or more items of demographic information missing (insurance choices, locus of control, or risk tolerance are never missing). We imputed demographic information for those participants based on the information of other participants with regressions. We proceeded stepwise, starting with the variable that had the least missing information (age) and moving on to the second least, then third least missing and so on until we filled in all missing data. We include dummy variables for each demographic variable that had some missing information. For example, if a participant's income was missing, we imputed income based on all other available demographic information in the whole sample and assigned the missing income dummy a value of one for this participant.

3.5 Results

3.5.1 Risk attitudes and incentivized insurance choices

The following results are shown for the whole sample, i.e., all participants from both experiments, including those people whose demographic information we imputed.

Table 7 shows the main results from different multinomial logistic regression model specifications, which are separated with a panel header on the top left corner of each panel following the previous model's pseudo-R-squared. The goal is to find out what measure explains insurance choices in an incentivized experiment best. Recall, the three main explanatory variables we are interested in are are: Locus of control (LoC), a willingness to take risks (WTR) derived as the main principal component from two self-reported instruments, and a constant relative risk aversion parameter (CRRA) derived from 60 binary lottery choices (see Appendix B for more information). All coefficients show the relative risk ratio (i.e., the change in probability) that someone chooses a different alternative than no insurance in both years of a period (alternative 0,0=base outcome) for a one standard deviation change in any of the shown variables in the first column of the table.

To identify partial correlations and rule out that our estimates are biased by other observable characteristics, in Table 7, we show our results after we sequentially included a dummy in the models that indicates if the data comes from the subsidized experiment (Panel B); a dummy that indicates if the participant is a New Zealand (vs. Australian) resident (Panel C); a dummy that indicates if the participant insured in the previous period and a dummy that indicates if the participant experienced a disaster in the previous period (Panel D). We then add demographic characteristics as covariates (Panel E), and finally include the big five personality traits as additional controls to rule out that our results may be driven by these traits (Panel F). Previous research has indicated that the big five personality traits are associated with risky (financial) decisions (Baker, Kumar, and Goyal 2019; Bucciol and Zarri 2017; De Bortoli et al. 2019; Gambetti and Giusberti 2019; Mayfield, Perdue, and Wooten 2008; Zaleskiewicz 2001). Including these traits in one model specification rules out the possibility that the coefficients on our measures of risk attitudes might be mediated by the big five personality traits.

All results tables only show coefficients of the traits for which we removed measurement error, but we also checked whether the results differ if we use the measures without partialled-out age and sex differences and found that the measurement error-corrected personality traits are slightly better predictors of insurance choices in our experiment than the raw measures of personality traits. Yet, overall we find no qualitative difference between these results.

Table 7. Insurance decision results

Panel A (i.period)	**Alternative 0,1 or 1,0**	**Alternative 1,1**	**Alternative 2,2**
CRRA	0.163 (0.121)	0.203* (0.119)	0.200* (0.105)
WTR	-0.149 (0.113)	-0.341** (0.134)	-0.264** (0.118)
LoC	-0.187 (0.126)	-0.247* (0.141)	-0.216 (0.133)
Pseudo R-squared		0.013	
Panel B (+ subsidized)			
CRRA	0.146 (0.121)	0.197* (0.120)	0.189* (0.106)
WTR	-0.147 (0.112)	-0.339** (0.134)	-0.263** (0.118)
LoC	-0.192 (0.125)	-0.248* (0.141)	-0.220* (0.133)
Pseudo R-squared		0.016	
Panel C (+ New Zealand)			
CRRA	0.151 (0.122)	0.205* (0.120)	0.199* (0.108)
WTR	-0.148 (0.112)	-0.339** (0.134)	-0.263** (0.118)
LoC	-0.206 (0.128)	-0.277* (0.143)	-0.261* (0.135)
Pseudo R-squared		0.018	
Panel D (+ previous period)			
CRRA	0.163 (0.123)	0.202* (0.109)	0.197** (0.095)
WTR	-0.139 (0.112)	-0.308** (0.122)	-0.235** (0.105)
LoC	-0.186 (0.116)	-0.238* (0.122)	-0.220** (0.111)
Pseudo R-squared		0.087	
Panel E (+ demographics)			
CRRA	0.069 (0.143)	0.092 (0.133)	0.134 (0.113)
WTR	-0.120 (0.116)	-0.303** (0.126)	-0.260** (0.110)
LoC	-0.174 (0.123)	-0.328** (0.132)	-0.268** (0.116)
Pseudo R-squared		0.119	
Panel F (+ big five personality)			
CRRA	0.048 (0.146)	0.076 (0.133)	0.127 (0.111)
WTR	-0.077 (0.121)	-0.235* (0.126)	-0.249** (0.111)
LoC	-0.100 (0.136)	-0.237* (0.142)	-0.266** (0.123)
Pseudo R-squared		0.124	

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. Besides of the personality measures, panel A includes period fixed effects. Panel B adds a dummy that indicates whether the participant took part in the subsidized experiment, panel C adds a dummy indicating whether the participant is a New Zealand resident, panel E adds demographic controls, and panel F adds the big five personality as additional covariates. 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. 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.

Our results from the experiment indicate that both a higher willingness to take risks (WTR) and a higher internal locus of control (LoC) predict a lower insurance take-up (more risk taking). We also see that the CRRA parameter is a weak predictor of insurance choices. In fact, significance on the CRRA parameter disappears in some of our robustness analyses once the big five personality traits or demographic controls are accounted for, while significance on WTR and LoC increase (or only become apparent) when we include these covariates. Most importantly, we see that both WTR and LoC explain insurance choices independently (a robustness check in which we interacted both variables showed no significant interaction). Our study thus supports that self-reported measures of risk attitudes are good predictors of risky insurance decisions with real financial implications, while the CRRA parameter derived from lottery choices is not.

We provide additional robustness checks for the two samples separated in Appendix C along with further other robustness checks (Appendix D.1): for example, one analysis in the appendix excludes participants for whom we imputed demographic data, and one in which we exclude participants who failed to answer two or more of five comprehension questions in the insurance experiment incorrectly. All robustness checks overall support the results discussed here. There are some differences, however. For example, CRRA is a significant predictor in the subsidized version but not in the actuarially fair version of the experiment, while WTR and LoC are significant predictors in the actuarially fair but not the subsidized version. Due to the generally low explanatory power from the small samples when we analyze the insurance choices separated by the experimental version, remaining measurement error and other unobservable factors, we can expect to find somewhat diverging results. Since all point estimates go into the same direction, we interpret the results as confirmatory of the overall results.

The differences in our results (main results vs. robustness analyses) and the fact that additional covariates affect the estimates on WTR and LoC are somewhat discouraging but support the recently revived discussion about the difficulty to measure risk attitudes appropriately so that the variables actually measure the construct they are supposed to, and predict outcomes as expected (Charness et al. 2020; Mata et al. 2018).

3.5.2 Risk attitudes and incentivized lottery choices

We additionally analyzed whether the self-reported willingness to take risks (WTR), locus of control (LoC), and the constant relative risk aversion (CRRA) parameter predict the risky lottery choices specifically. One may argue that the lottery choices and the insurance decisions both are merely different representations of gambles since both are risky financial decisions. Table 8 shows the results from logistic regressions of dependent dummy variables that indicate if a participant chose the safer pie chart out of two (always presented as an individual pair). The safe choice is either coded as the pie chart with the lower standard deviation (SD), the lower coefficient of variation (CV), or the higher expected value (EV). Similar to the previous section, to identify partial correlations and to rule out that our estimates are biased by other observable characteristics, in Table 8, we show our results after we sequentially included the big five personality traits (Panel B); a dummy that indicates if the participant is female (Panel C); and demographic characteristics as covariates (Panel D). In Table D-11 of the appendix we also show similar results for each of the risk measures (CRRA, WTR, LoC) individually without including any covariates. In these additional analyses, we found that locus of control does not significantly correlate with the lottery choices, although it does with the insurance choices. But a self-reported willingness to take risks still is a highly significant predictor of the safe option between a pair of lottery choices. In Table D-12, we furthermore show results from a robustness check using the number of times a participant chose the safe option for each of the 60 lottery pairs, supporting the results discussed here.

Table 8. Lottery choice results

Panel A	SD	CV	EV
CRRA	0.034*** (0.012)	-0.143*** (0.019)	-0.161*** (0.021)
WTR	-0.065*** (0.013)	0.092*** (0.020)	0.113*** (0.021)
LoC	0.020 (0.014)	0.011 (0.018)	-0.006 (0.018)
Pseudo R-squared	0.001	0.005	0.007
Panel B (+ big five personality)	SD	CV	EV
CRRA	0.036*** (0.012)	-0.144*** (0.019)	-0.162*** (0.021)
WTR	-0.063*** (0.015)	0.091*** (0.021)	0.111*** (0.022)
LoC	0.015 (0.015)	0.018 (0.020)	0.004 (0.020)
Pseudo R-squared	0.001	0.006	0.008
Panel C (+ Female dummy)	SD	CV	EV
CRRA	0.036*** (0.012)	-0.144*** (0.019)	-0.162*** (0.021)
WTR	-0.062*** (0.015)	0.091*** (0.021)	0.110*** (0.022)
LoC	0.014 (0.015)	0.019 (0.020)	0.005 (0.020)
Female	0.045* (0.026)	-0.064 (0.039)	-0.086** (0.041)
Pseudo R-squared	0.001	0.006	0.008
Panel D (+ demographics)	SD	CV	EV
CRRA	0.044*** (0.014)	-0.154*** (0.023)	-0.165*** (0.026)
WTR	-0.064*** (0.015)	0.094*** (0.021)	0.113*** (0.022)
LoC	0.020 (0.015)	0.021 (0.021)	0.002 (0.021)
Female	0.047* (0.025)	-0.071* (0.039)	-0.091** (0.041)
Pseudo R-squared	0.002	0.007	0.009

This table shows results from a logistic regression analysis of the lottery choices made by participants. The dependent variable takes the value 0 if the participant chose the risky choice, and 1 if the participant chose the safer choice out of two pie charts presented within a pair of pie charts. Panel A includes only the three measures of risk attitudes (CRRA< WTR, LoC). We then add covariates sequentially: Panel B includes the big five personality traits, panel C adds a female dummy, and panel D adds the same demographic controls we use in other regression models. 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.

3.5.3 Risk attitudes and socioeconomic real-world outcomes

To show that our measure of locus of control (LoC) captures an attitude related to other measures of LoC, we also investigated associations of LoC with real-life socioeconomic outcomes, and whether LoC predicts these beyond the other personality and risk attitude measures.

Table 9. Survey results

Panel A	Education	Income	Home ownership	Unemployment	Partnership
LoC	0.148** (0.052)	0.260*** (0.055)	0.055* (0.024)	-0.048** (0.015)	0.066** (0.025)
(Pseudo) R-squared	0.019	0.060	0.015	0.045	0.022
Panel B (+ risk attitudes)	Income	Education	Unemployment	Home ownership	Partnership
LoC	0.135** (0.052)	0.249*** (0.055)	0.055* (0.025)	-0.047** (0.015)	0.061* (0.024)
WTR	0.067 (0.052)	0.054 (0.050)	0.004 (0.025)	-0.003 (0.016)	0.018 (0.025)
CRRA	0.080 (0.054)	-0.098 (0.053)	-0.023 (0.025)	0.017 (0.017)	0.126*** (0.024)
(Pseudo) R-squared	0.024	0.067	0.017	0.050	0.065
Panel C (+ big five traits)	Education	Income	Home ownership	Unemployment	Partnership
LoC	0.098 (0.059)	0.213*** (0.059)	0.065* (0.027)	-0.059*** (0.017)	0.068* (0.027)
WTR	0.060 (0.052)	0.049 (0.053)	0.010 (0.025)	-0.004 (0.016)	0.018 (0.025)
CRRA	0.074 (0.054)	-0.094 (0.052)	-0.025 (0.025)	0.019 (0.016)	0.128*** (0.024)
(Pseudo) R-squared	0.036	0.073	0.034	0.074	0.089
Panel D (+ demographics)	Education	Income	Home ownership	Unemployment	Partnership
LoC	0.006 (0.055)	0.136* (0.057)	0.047 (0.026)	-0.042** (0.016)	0.035 (0.024)
WTR	-0.007 (0.051)	0.023 (0.050)	0.005 (0.024)	0.004 (0.014)	0.003 (0.023)
CRRA	0.123* (0.056)	-0.160** (0.056)	-0.050* (0.025)	0.029 (0.019)	0.169*** (0.023)
(Pseudo) R-squared	0.191	0.238	0.156	0.165	0.247
Unique people (N)	405	383	412	398	401

This table shows results from ordinary least squares (for the standardized and categorical dependent variables Income and Education) and Logit regressions (for the dummy dependent variables *Unemployed*, *Homeowner*, and *Partnership*). We regress the dependent variables on locus of control (LoC), the principal component of two willingness to take risks measures (WTR), a constant relative risk aversion parameter (CRRA), big five personality traits, and other demographic covariates. Robust standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.001, ** p-value < 0.01 and * p-value <0.05.

This time, some of the demographic information enter the models as *dependent* variables. We do not use any imputed demographic data as dependent variables, but we keep the imputed data as *independent* variables. To save some space, we do not specifically show how other demographic covariates correlate with each other in the following models. Our measure of locus of control has similar predictive validity as shown in previous research, which gives us some confidence that we have a similar measure of locus of control as the measures used in other studies. But our results are somewhat dependent on the covariates that we include in the regressions, as discussed next.

Locus of control predicts a higher education, higher income, a higher likelihood of being a homeowner, a lower likelihood of being unemployed, and a higher likelihood of being in a partnership before any other covariates are added (except a New Zealand dummy to account for cultural and socioeconomic differences between New Zealanders and Australians). Many of these associations remain when we add other risk attitudes (Panel B), but they disappear once big five personality traits are accounted for in the regressions (Panel C), even though few of the big five traits show a significant relationship with the dependent variables themselves: openness negatively correlates with homeownership and partnership status, while conscientiousness positively correlates with education, but there are no other significant associations. The associations are weaker again after additional demographic covariates are added to the model (Panel D). The relationship with locus of control only remains significant for income and unemployment. This finding might be driven by an overspecification of the models or some multicollinearity, as the rather small samples include many covariates of which many also correlate with each other. Most interestingly, we find statistically significant associations between the constant relative risk aversion (CRRA) parameter and some of the socioeconomic dependent variables, even after all other traits and demographic covariates are added in the models. The principal component of our willingness to take risks measures (WTR) never significantly predicts any of the outcome variables. In contrast to our experimental results, these results question the predictive validity of a self-reported measure of a willingness to take risks but give credibility to predictive validity of the expensive mathematical measure to derive a CRRA parameter. Perhaps this parameter indeed captures an underlying general risk attitude as our results would suggest.

3.6 Discussion

In this paper we have reviewed the recent literature that discusses several measures of risk attitudes, their reliability, convergent validity (correlation between different measures of risk attitudes), and predictive validity (correlations between risk measures and actual choices or socioeconomic outcomes). We have discussed and provided further evidence for the difficulty to measure risk attitudes reliably and argued that locus of control (LoC) might be a useful measure of risk attitudes that does not suffer from some of the issues that many other (especially behavioral) measures of risk attitudes suffer from: For example, their difficulty to understand for participants, costliness, and time requirement. Locus of control is related to risk attitudes in the way that locus of control measures to what extent people attribute their lives' trajectories and outcomes to luck, fate, or chance.

We discussed the literature that has shown that LoC is a significant predictor of socioeconomic outcomes and risky (financial) decisions inside and outside of the laboratory. These findings led us to conjecture that LoC might also be able to explain risky decisions in an experiment, for which we provide supportive evidence by showing two important findings: First, LoC significantly correlates with a self-reported willingness to take risks (WTR) measure which also significantly predicts the same socioeconomic outcomes and risky (financial) decisions that are predicted by LoC. Second, we show that LoC and WTR are both significantly associated with incentivized insurance decisions in our experiment. But these measures have no significant interaction effects, i.e., they do not affect each other directly when included in the regressions. Our research also has shown that LoC does not always predict risky choices and is far from being a perfect predictor. Importantly, we have seen that locus of control does not predict lottery choices while the self-reported willingness to take risks does, albeit as imperfectly as LoC predicts other risky decisions. Perhaps, LoC is a context-specific or framing-dependent predictor of risky decisions. Some studies have suggested that locus of control might be related to risk perceptions specifically (Caliendo and Hennecke 2020; Riechard and Peterson 1998), which is something we could not further investigate. Our findings suggest that this might be a mechanism through which locus of control affects decisions under risk.

3.6.1 Advantages of locus of control over behavioral measures of risk attitudes

Locus of control (LoC) has several advantages over behavioral risk elicitation methods: it is cheaper, simpler, less time-consuming, and therefore much easier to implement in large-scale survey studies, as we discuss in the following paragraphs.

First, since LoC is not incentivized, it might be a cheap method of obtaining a predictor of decisions with uncertain outcomes, similar to the validated method of a self-reported willingness to take risks (WTR). Another advantage shared with the WTR measure is that money is typically not even mentioned in the statements measuring a general LoC, so that LoC might even be a better predictor of risky decisions generally, not only in a financial context, which is less likely true for the commonly used behavioral measures of risk attitudes that are founded in financial decisions (e.g., lottery choices).

Second, the nature of LoC being a self-reported level of agreement with clearly defined and easily understandable statements (again like the WTR) makes LoC a simpler way to proxy risk attitudes than with behavioral risk elicitation methods. Respondents need *not* have a substantiated numerical understanding, whereas research that uses behavioral risk elicitation methods necessarily makes a strong assumption that everybody understands these choices (mathematically) and is able to answer them reliably (Dave et al. 2010).

Third, to ensure that people understand what they are answering or doing, behavioral methods generally are preceded by long instructions, which are followed by some tests and attention check questions. This approach takes a lot of time that often is not available in longitudinal studies. Locus of control does not need any of these lengthy instructions or tests and attention checks. Thus, LoC can be measured more quickly. All these arguments make LoC a more useful measure of risk attitudes in longitudinal surveys. We do not claim that locus of control is a substitute for other measures of risk attitudes, but that it rather is a complementary measure that helps us to better explain decisions involving risk overall, especially when combined with the measure of a self-reported willingness to take risks.

3.6.2 Limitations

There are few studies we are aware of that related locus of control to insurance decisions which have shown the opposite of what we found. Studies showed that those scoring higher on internal locus of control (i.e., lower on external LoC) are more likely to have disaster home insurance and to invest in disaster mitigating measures (Antwi-Boasiako 2017; Simpson-Housley and Bradshaw 1978). In another study of locus of control as a predictor of *personal* insurance decisions, Dudek (2021) shows that (internal) locus of control significantly predicts a higher likelihood of being an insurance policy holder. These studies show that locus of control predicts less risk taking. How, then, can we reconcile our findings with those studies?

First, our study has combined a behavioral with a survey measure in an incentivized experiment. The other studies only examined survey questions. The behavioral CRRA parameter might have picked up something that is not explained by LoC or WTR; excluding CRRA from the models does not change our results, however. This, we believe, is primarily because CRRA measures marginal utility of money and not a general risk attitude; and since there was no monetary risk-return trade-off in our experiment, CRRA should not explain insurance decisions in our setting anyway. Overall, CRRA does not explain insurance decisions, but recall that CRRA has some explanatory power in the subsidized insurance experiment. This is surprising and we have no explanation for this significant association.

Second, although our study aimed to investigate insurance decisions, it might be that more participants have deemed the decisions as purely financial decisions. This conjecture supports why LoC is negatively associated with the risky financial decisions in our study, similar to, e.g., Salamanca et al. (2020), who have shown that people scoring higher on internal locus of control are more likely to own equity investments. Internal locus of control thus leads to riskier decisions in our and their study. But real insurance decisions have an important non-financial component that might influence the decision whether to insure, which likely has no influence on insurance decisions in an experiment. People might not only buy insurance because they want to be financially secure, but insurance also buys freedom from worries about one's property and what to do if the property is damaged. Insurance decisions likely are not purely driven by financial incentives and considerations but also include factors such as the psychological stress resulting from the time needed to replace, repair, or deal with a loss of one's property. With the experiment being actuarially fair or subsidized, the insurance decision task has no proper risk-return trade-off; thus, in our experiment every participant who is risk averse as measured with a CRRA parameter should have bought insurance anyway. The fact that it does not explain the participant's choices and that some participants did not purchase insurance although they are classified as risk averse with the CRRA parameter also shows that the insurance choices are not purely driven by financial considerations. It could be more difficult to elicit real-world insurance behavior than many researchers might currently assume.

Third, and in support of the second point above, we often find that real-world insurance behavior is quite different from insurance decisions made in the lab. One such example is that in real-world data, people typically apply the availability heuristic, while researchers of insurance experiments are more likely to find that people apply a gambler's fallacy heuristic (Jaspersen and Aseervatham 2017), like we did in our study, or no heuristic at all. These are again contradicting findings to studies of real-world insurance decisions (Atreya, Ferreira, and Michel-Kerjan 2015; Ganderton et al. 2000; Kunreuther and Michel-Kerjan 2015; Yin et al. 2016). If insurance decision experiments in the lab are more likely to reflect gambling behavior or plain financially driven behavior, then we might need to improve the designs of insurance decision experiments. Most insurance experiments might be better at telling us something about risky financial decision-making more generally than about insurance decisions specifically.

Fourth, the obvious limitations of any social experiment are that the experiment might not reflect real-world behavior (a) because of the lack of the appropriate incentives (although we do incentivize decisions, the incentives still could have been to small); (b) because a financial endowment might be seen as a windfall payment that is seen as "house money", which has been shown to affect risk attitudes or choices that involve risk; and (c) because people might simply not understand the insurance decision task properly in the experiment or not pay the necessary attention, which is often raised as a concern especially in online experiments. This last point, however, seems to have been rebutted by recent studies that show a similar quality and effects across studies that compare online data to lab data (Buhrmester, Kwang and

Gosling, 2011; Palan and Schitter, 2018; Peer, Brandimarte, Samat and Acquisti, 2017), and in our own study the lab participants also did not make vastly different choices so that results do not differ significantly.

3.6.3 Implications and future research directions

Since a person's locus of control and (self-reported) willingness to take risks significantly correlate and both predict insurance choices – and other risk decisions and socioeconomic outcomes – we suspect that locus of control could be a more important variable in studies involving decisions under risk than previously assumed. Excluding locus of control from statistical models of decision-making could bias other estimates (omitted variables bias). Further research should investigate the predictive validity of other measures of risk-taking in conjunction with locus of control and both measures' correlation. Additionally, future research that examines insurance decisions in the lab should test whether different designs of insurance experiments can reflect real-world insurance decisions better, since it seems that many insurance decision experiments might elicit a behavior or mentality that is different from real-world insurance decisions.

3.6.4 Conclusions

Social scientists are interested in how to explain decisions involving risk properly. As we have shown, locus of control might be a valuable measure for such research. We have shown that locus of control significantly predicts risky incentivized financial decisions, does so more robustly than a behavioral measure of risk attitudes, and explains variation in the experiment's insurance choices beyond that explained by a self-reported willingness to take risks. We also have shown that locus of control in our data predicts other socioeconomic outcomes, as has been shown in previous studies. Our research shows that our measure of locus of control has a high alpha reliability coefficient, which, together with our other findings, afford our measure of LoC some internal validity and predictive validity of decisions involving risk. We thus provide empirical evidence that locus of control might be an important variable to take into consideration in future studies of decisions involving risk. But more research is needed to learn when locus of control predicts risky decisions and when it does not.

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4 Demand for multi-year catastrophe insurance contracts: Experimental evidence for mitigating the insurance gap

By Thomas Dudek, Eric R. Ulm and Ilan Noy.

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.

Keywords Individual decision-making · Choice under risk · Disaster insurance · Underinsurance · Status quo · Subsidies.

4.1 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 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 reflect 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.

4.1.1 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 like 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. [6]

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

[6] 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).

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 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).[7]

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 to their old no-insurance

[7] 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.

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.

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).

4.2 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.

4.2.1 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).

4.2.2 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).[8]

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).[9] 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).[10] 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.

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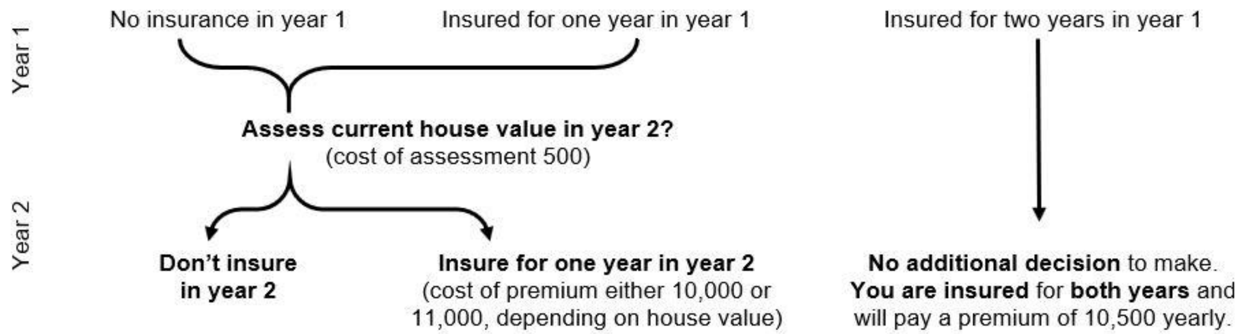
[8] We have not asked participants to perform a task before they receive their endowment, which might affect people's decisions in the experiment, as some might think that the endowment is purely "house money". The house money effect has been shown to influence risk taking (Cárdenas, De Roux, Jaramillo, & 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.

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

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

Figure 4 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.

Figure 4. 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 Figure 4, 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 must 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 5 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 10 describes the experiment's main variables.

Figure 5. Experimental process

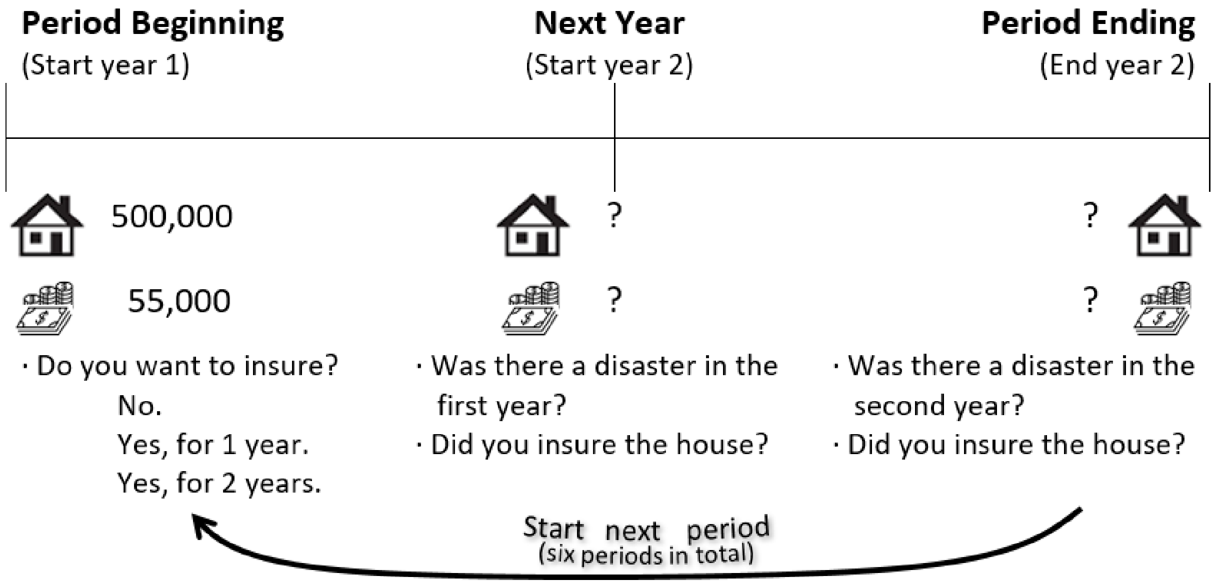


Table 10. Insurance decision task variables

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

4.2.3 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.[11] 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 studies that investigated long-term insurance decisions used a similar RPSM to incentivize their participants (Kunreuther & Michel-Kerjan, 2015; Papon, 2008).[12]

4.2.4 The participants and their decisions: Descriptive statistics

Table 11 and 12 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

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[11] 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.

[12] 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 paying one or paying all participants makes a large difference.

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.[13] We include dummy indicators for each missing demographic variable to account for this imputation.

Table 11. 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.

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

Table 12. 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.

4.3 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 E). We overall find no robust statistically significant relationship between the participants' insurance choices in the experiments and their demographic information with one exception: homeowners 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 C.1. 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 (E.4).

4.3.1 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.

4.3.1.1 Descriptive analyses of insurance choices

Figure 6. Insurance choices in both experimental versions over time

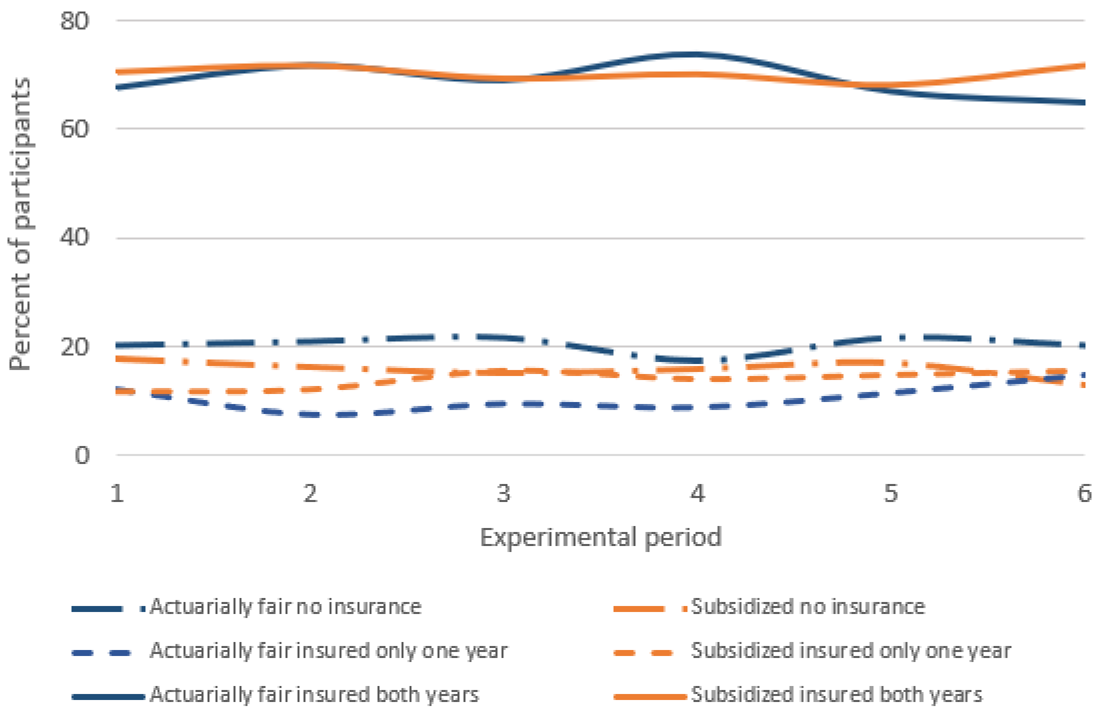

Figure 6 shows for each experimental version the percent of participants who did not insure at all within a period (dotted line), who insured only one of the two years within a period (dashed line), and who insured both years annually or biennially (solid line) within a period. Compared to the actuarially 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. Most of the participants insured both years either annually (between 22% and 30%) or biennially (between 35% and 48%) in both versions. Like 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.

4.3.1.2 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.[14] The multinomial logistic regressions include 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 E.4.1 and E.4.2). All regression coefficients are compared to the (omitted) choice of no insurance in both years. The coefficients in the table show relative risk ratios.

[14] 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.

Table 13. 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 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.[15]

4.3.2 Underinsurance 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.

4.3.2.1 Descriptive analyses

Table 14 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.

[15] 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 14. 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 $\leq$ 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.[16]

4.3.2.2 Regression analyses

A logistic regression analysis of the choice whether to assess the house price and whether 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.

[16] 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 15. 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)
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	n/a n/a	Chi^2 ≈1.45, p-value ≤ 0.693	Chi^2 ≈5.34 p-value ≤ 0.148	Chi^2 ≈0.99 p-value ≤ 0.806	Chi^2 ≈3.90 p-value ≤ 0.273	Chi^2 ≈0.99 p-value ≤ 0.804)	Chi^2 ≈4.71 p-value ≤ 0.194)
Chi-squared test for big five personality traits	n/a n/a	n/a n/a	n/a n/a	n/a n/a	Chi^2 ≈7.79 p-value ≤ 0.168	Chi^2 ≈4.23 p-value ≤ 0.521	Chi^2 ≈7.63 p-value ≤ 0.178	Chi^2 ≈3.97 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.

4.4 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, like 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 like 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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5 Discussion and conclusion

In the first chapter we studied the effect of one's sibling's sex on personality development. We combined data from 42,220 respondents of eight longitudinal surveys collected in seven countries. Our results show that women's and men's traits are not meaningfully affected by their sibling's sex. Contrary to our hypotheses, we cannot support that recent research findings showing siblings' sex affecting marriage decisions, educational attainment (Peter, Lundborg, Mikkelsen, and Webbink 2018; Vogl 2013), occupational choices and income (Brenoe 2021; Patacchini and Cools 2019; Rao and Chatterjee 2018) are driven by the effect of a sibling's sex on personality traits. Of course, it may be that the theories of social learning and sibling differentiation may apply to different families in varying degrees, which may result in effects that on average are indistinguishable from zero. Another explanation why we did not find siblings' sex effects on personality might also be that the influence of a sibling's sex on traits may have washed out over time and with additional environmental exposures. The increasing effect of environmental factors on traits over time and small effects of genes and family environments have been previously discussed in the personality development literature (Tucker-Drob and Briley 2014; Vukasović and Bratko 2015). Some examples that affect personality development are the school environment (Alison Booth, Cardona-Sosa, and Nolen 2014; Allison Booth and Nolen 2009), whether one experienced an earthquake, or lived through an economic crisis (Cameron and Shah 2015; Guiso, Sapienza, and Zingales 2018; Malmendier and Nagel 2011). As we have shown, in some surveys we did find some significant effects on some of the traits, but these are not systematic across the samples. Therefore, we do not believe that individual studies' findings provide an accurate explanation for the different results between our study and past literature on the topic. Given inconsistent methodologies and small sample sizes, it seems reasonable that at least some of the incoherence can be attributed to publication bias (Ioannidis, 2005), which can result in a "continuous stream of conflicting results" (Damian and Roberts, 2015b).

In the second chapter, we have reviewed the recent literature that discusses several measures of risk attitudes, their reliability, convergent validity, and predictive validity. Social scientists have been interested in how to explain decisions involving risk and how to measure risk attitudes properly for many decades but have not found a measure yet that captures these attitudes sufficiently. We have discussed and provided further evidence for the difficulty to measure risk attitudes reliably and argued that locus of control (LoC) might be a useful measure of risk attitudes that does not suffer from some of the issues that many other (especially behavioral) measures of risk attitudes suffer from: For example, they are costly, time consuming, and difficult for participants to understand. Locus of control is related to risk attitudes as it measures to what extent people attribute their lives' trajectories and outcomes to luck, fate, or chance. In this sense locus of control may be attributed to risk perceptions (as previously suggested in Caliendo and Hennecke 2020; Crisp and Barber 1995; Riechard and Peterson 1998). We provide supportive evidence for the importance of locus of control in explaining risky decisions by showing one important finding: LoC significantly correlates with a self-reported willingness to take risks (WTR) measure, and both significantly predict the same socioeconomic outcomes and risky (financial) decisions among the same study participants from New Zealand and Australia. Also noteworthy is that these measures have no significant interaction effects. These results lead us to suspect that locus of control could be a more important variable in studies involving decisions under risk than previously assumed. Excluding locus of control from statistical models of decision-making could bias other estimates. Further research should investigate the predictive validity of other measures of risk-taking in conjunction with locus of control and examine both measures' correlation and interaction.

Even though our research has shown that WTR and LoC might both be important, we also found that LoC does not always predict risky choices. In our study, for example, LoC does not predict lottery choices while WTR does. Perhaps, LoC is a context-specific or framing-dependent predictor of risky decisions. We also found that a constant relative risk aversion (CRRA) parameter derived from the lottery choices (which we used as dependent variables for the predictive power by LoC and WTR) is a significant predictor of some of the same socioeconomic variables that are associated with LoC and WTR, but CRRA does not predict insurance choices in the experiment. All these findings suggest that we need much better measures of risk attitudes if we want to measure and predict decision-making under risk. One potentially fruitful future investigation could be the link between locus of control and risk perceptions. Understanding risk perceptions as something separate from one's willingness to take risks might improve our understanding of decision-making under risk in general.

In the third chapter, we investigate insurance decisions in an incentivized insurance experiment. The study has three aims: First, we measure whether people prefer long-term insurance; second, we investigate house value changes as one potential reason for the emergence of underinsurance over time; and third, we examine the effect of subsidies on insurance take-up. Similar to previous studies, we find that people have a strong preference for long-term insurance. Of our 413 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. If only annual insurance contracts are offered, some people might not insure, as was argued by Jaffee, Kunreuther, and Michel-Kerjan (2010). Thus, offering different insurance contracts could cater to different preferences of those who choose insurance.

Another finding from this study is that people make very consistent choices and that a dummy indicating residential status (perhaps measuring cultural or social factors?) seem to matter more – or at least explain insurance decisions in our experiment better – than personal characteristics. Those who (do not) insure in the first year of the first period tend to (not) insure in following periods, i.e., people often make the same type of insurance choice repeatedly. Additionally, residents from New Zealand tend to prefer long-term insurance. We thus find that behavior is best explained by past choices and potentially social/cultural norms. Another finding in our study is that some people who are insured could nevertheless become underinsured over time if contracts do not automatically increase coverage over time. Many people are reluctant to pay for an updated coverage, even if they insure in following years. Automatic adjustments of the contract's coverage to, for example, a house reconstruction cost index increase or to new policy changes could be a solution. Some insurance companies offer such contracts, but not all do.

Finally, our study supports many previous studies who have found that subsidies are rather ineffective and expensive measures to increase insurance uptake. In our study, we found no statistically distinguishable difference between the insurance choices and the number of insured years whether we offered insurance at 40% of an actuarially fair price or not. Subsidized insurance might merely be a wealth transfer from some taxpayers to those few who choose subsidized insurance, without increasing insurance take-up overall.

In conclusion, if governments wish to increase insurance penetration, the most efficient strategy may be to increase the public's perception that being insured is the right thing to do, is what someone personally has done previously, and is what others are doing as well. 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. One way to increase insurance take-up could be by pointing out that many others are also insuring their property. Behaviorally informed insurance contracts could improve insurance take-up in the short and in the long term, while also mitigating underinsurance over time.

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Appendices

Appendix A. Extensive results to effect of siblings' sex on personality

A.1 Sample size by survey and personality trait

(1) Dataset	(2) Risk tolerance	(3) Trust	(4) Patience	(5) Open-ness	(6) Consci-entiousness	(7) Extra-version	(8) Agree-ableness	(9) Neuro-ticism	(10) Locus of Control	(11) Typical Female Psnlty.	(12) Distinct people
NLSY79	5,335	4,488		4,190	4,213	4,152	4,132	4,225	5,341	5,335	5,341
MCS	5,223	5,254	5,241							5,223	5,269
UKHLS	2,402	3,000		2,622	2,623	2,621	2,623	2,623		3,604	3,985
LISS	972	2,056		2,057	2,057	2,057	2,057	2,057		2,057	2,057
SOEP	5,853	5,109	3,473	4,046	4,069	4,072	4,070	4,072	3,687	5,936	6,123
HILDA	1,159	4,558								1,159	4,847
CFPS	3,617	2,848	3,976	3,294	3,294	3,294	3,294	3,294		3,730	4,047
IFLS	9,748	8,810		8,766	8,763	8,761	8,764	8,759	8,858	9,820	9,862
Total	34,309	36,123	12,690	24,975	25,019	24,957	24,940	25,030	17,886	36,864	41,531

The table shows the surveys and personality measures included in the estimation sample, as well as resulting sample sizes. Distinct people (column 12) refers to the total number of people per survey who were included in at least one analysis.

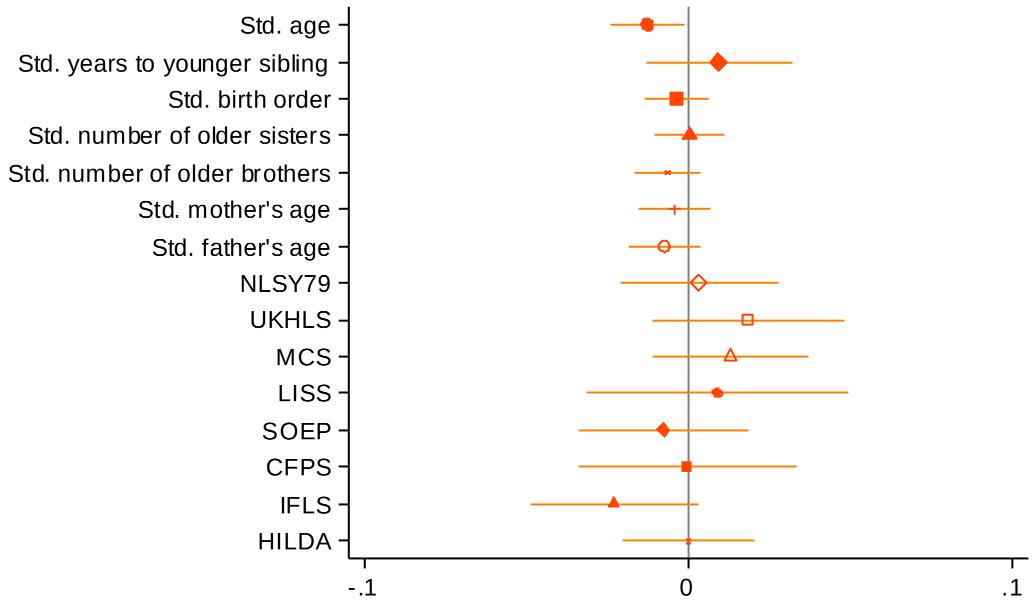
A.2 Balance Checks

Analyses of the effects of the sex of the next younger sibling rely on certain identifying assumptions. To estimate the effect of having a younger sister compared to a younger brother, we primarily rely on the assumption that the next younger sibling's sex is as good as random. We tested the plausibility of this assumption in three different ways: by testing balance on pre-determined characteristics (which should be good if younger sibling's sex was random); by looking for evidence of sex selective abortions (which could render younger sibling's sex non-random); and by testing whether we can predict the sex of the next younger sibling (which would be impossible if younger sibling's sex was random). We performed all three balance tests separately for women and men.

First, we investigated how people with a next younger sister differed from those with a next younger brother on seven pre-determined characteristics (i.e., characteristics that were determined before the next younger sibling was born): age, age difference to younger sibling, birth order, number of older sisters, number of older brothers, mother's age and father's age at birth. We estimated average differences on a given pre-determined characteristic between people with a next younger sister compared to a next younger brother with seven bivariate regressions of a younger sister dummy variable (1 if the next younger sibling is female, 0 otherwise) on that pre-determined characteristic. All regressions were estimated with observations at the individual level and with heteroscedasticity robust standard errors (which consider the non-normality of residuals resulting from linear regression with a binary outcome variable). For people whom we observe in multiple years, we used their average age in all survey waves they participated in. We standardized all variables by survey to have means of zero and standard deviations of one over all people in our estimation sample. As can be seen in Fig. A-1 (top seven demographic estimates), only one coefficient reached statistical significance for women, who are on average slightly younger if they have a younger sister. As shown in Fig. A-2, for men none of the predetermined characteristics reaches significance at $p<0.05$. All coefficients were tiny, supporting the notion that the sex of the next younger sibling is as good as random.

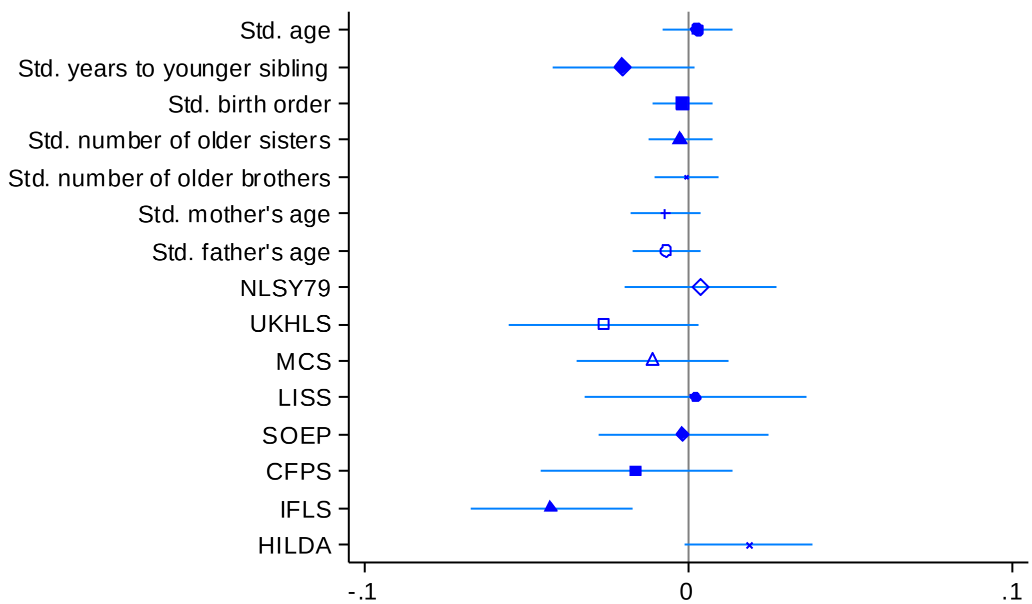
Second, we run 8 bivariate regressions in which we regressed a younger sister dummy variable on indicators for each survey, which allowed us to see whether the gender ratio in any survey is skewed, relative to the other surveys. Any such difference could result from sex selective abortions, from different survival rates, or other factors that influence the sex ratio of a population or of a survey. As can be seen in Fig. A-1 (bottom 8 survey estimates), among women, none of the survey dummies predicted the likelihood of having a younger sister significantly. For men, we find potential evidence for a gender skew in Indonesia (IFLS dummy). Men from Indonesia were 4.2 percentage points less likely to have a younger sister. Third, we tested if all our pre-determined characteristics and survey dummies jointly predicted whether the next younger sibling was female or male. For women, these variables were jointly insignificant ($F = 1.272$, $p = .205$). For men, these variables barely reached statistical significance ($F = 1.670$, $p = .045$). In both cases, the variance explained by the predictors (R^2) was a miniscule 0.1 percent, indicating that it was essentially impossible to predict the sex of the next younger sibling. Taken together, these three checks support the notion that the sex of the next younger sibling is as good as random.

Figure A-1. Balance check Women



Test of joint-significance:
All variables: F-stat = 1.272, Prob F>P = 0.205, R-squared = 0.001, N = 20142.
Family characteristics: F-stat = 1.491, Prob F>P = 0.145
Survey dummies: F-stat = 1.122, Prob F>P = 0.346

Figure A-2. Balance check Men



Test of joint-significance:
All variables: F-stat = 1.670, Prob F>P = 0.045, R-squared = 0.001, N = 21389.
Family characteristics: F-stat = 0.933, Prob F>P = 0.495
Survey dummies: F-stat = 2.006, Prob F>P = 0.050

A.3 Regression results for Figure 1

	(1) Risk Tolerance	(2) Trust	(3) Patience	(4) Openness to Exp.	(5) Conscien-tiousness	(6) Extra-version	(7) Agree-ableness	(8) Neuro-ticism	(9) Locus of Control	(10) TFP Index
Women										
Younger sister	-0.006	0.002	0.015	0.008	0.011	-0.002	-0.005	-0.027	0.003	0.005
SE	(0.013)	(0.014)	(0.024)	(0.020)	(0.017)	(0.019)	(0.018)	(0.019)	(0.017)	(0.011)
95% CI	[-0.032 - 0.020]	[-0.026 - 0.031]	[-0.032 - 0.062]	[-0.030 - 0.047]	[-0.023 - 0.045]	[-0.040 - 0.036]	[-0.040 - 0.029]	[-0.064 - 0.010]	[-0.032 - 0.037]	[-0.016 - 0.026]
Adj. R-squared	0.040	0.029	0.007	0.010	0.036	0.012	0.018	0.016	0.043	0.051
Observations	65,528	44,183	8,383	25,027	25,042	25,016	25,027	25,048	23,787	79,573
Distinct people	17419	18831	6349	12967	12985	12966	12962	12994	9157	18827
Men										
Younger sister	0.025	0.005	-0.026	-0.017	-0.006	-0.020	-0.018	-0.018	-0.027	-0.022*
SE	(0.016)	(0.015)	(0.024)	(0.019)	(0.019)	(0.020)	(0.019)	(0.019)	(0.018)	(0.012)
95% CI	[-0.006 - 0.056]	[-0.023 - 0.034]	[-0.073 - 0.020]	[-0.055 - 0.020]	[-0.043 - 0.030]	[-0.058 - 0.019]	[-0.055 - 0.019]	[-0.055 - 0.019]	[-0.063 - 0.008]	[-0.046 - 0.002]
Adj. R-squared	0.038	0.036	0.006	0.027	0.043	0.015	0.032	0.032	0.038	0.042
Observations	61,303	38,450	8,175	22,083	22,109	22,068	22,058	22,112	21,966	73,236
Distinct people	16890	17292	6341	12008	12034	11991	11978	12036	8729	18037

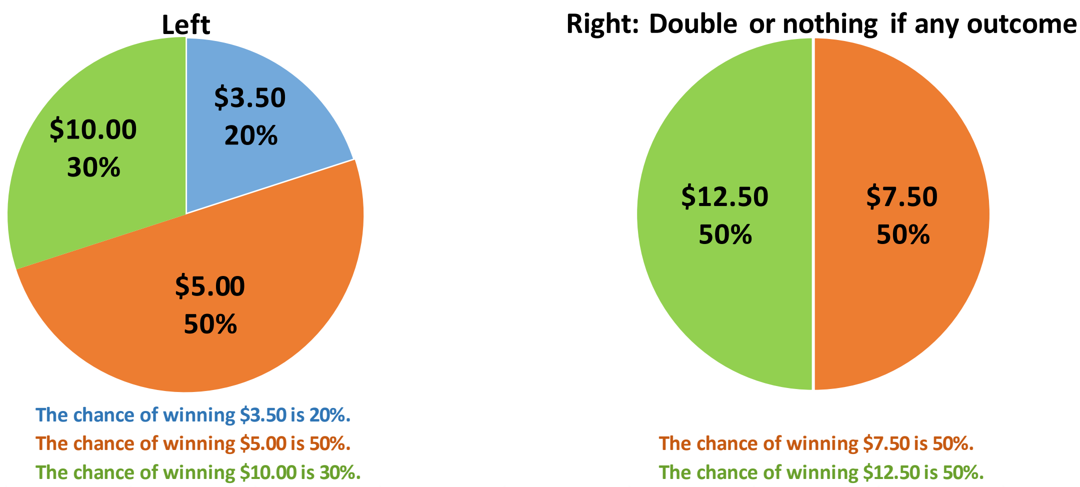
The table shows regression results from the central analyses reported in Fig. 3 (women, top panel) and Fig. 4 (men, bottom panel) for the effect of having a younger sister (as opposed to a younger brother) on personality. Each of the columns shows the results from a separate regression of the standardized trait shown in the header on a next younger sister dummy, including the controls described in the empirical strategy. Statistical significance is denoted by ** p <0.05, * p <0.1.

Appendix B. Measurement of risk attitudes and personality in the online experiment

B.1 Measurement of constant relative risk aversion (CRRA)

In the online experiment, we use a similar choice battery designed and used by Harrison and Ng (2016, 2018) to measure a constant relative risk aversion parameter. Participants make 60 binary choices between pie charts that offer varying payoffs at different degrees of risk. The illustration below shows one example of such pie charts.

Figure A-3. Binary lottery choice



B.2 Self-reported willingness to take risks (GR, FR, and WTR)

We use two Likert-scale questions as developed and validated by Dohmen et al. (2011) to measure general risk tolerance and risk tolerance in financial matters. These are the same measures of risk tolerance elicited in commonly used longitudinal surveys such as NLSY79 CH/YA, SOEP, and HILDA.[17]

We ask participants to:

> "Please indicate on a scale from 0 ("not at all willing to take risks") to 10 ("fully willing to take risks") how willing you are to take risks or if you rather try to avoid taking risks in general and in financial matters."

And participants rate themselves by clicking on one of the 11 possible choices, as shown here:

Figure A-4. Risk tolerance measurement on a self-reported Likert-scale.

In general, I am…

In financial matters, I am…

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[17] NLSY79 is the National Longitudinal Survey of Youth, SOEP is the German SOcioEconomic Panel, and HILDA is the Household, Income and Labour Dynamics in Australia survey.

not at all willing to take financial risks	0	○○○○○○○○○○○	10	fully willing to take financial risks

We construct a single measure of risk tolerance (WTR) extracting the first principal component of the two risk measures above. We derived WTR as one trait variable so that we would not have to include two variables that are highly correlated to avoid multicollinearity issues. Since the literature on risk attitudes does not seem to have one measure that appears to be a reliable and valid predictor of insurance decisions, we decided to extract a risk attitude factor from these two items. We did not do something similar for the other traits because these are reliable and valid measures that have been developed by psychologists over many decades of research and these traits are typically calculated as a simple averages, sometimes as a sum of the individual items—although, there is one notable example of a study that shows how using a factor analysis for LoC improves the measure of LoC in a German longitudinal survey (Piatek & Pinger 2016). The resulting factor loads strongly on both variables, explains about 79% of their common variance, and has an eigenvalue of 1.57. These properties make the measure reliable enough as a single measure of risk taking for our purposes.

B.3 Measurement of internal Locus of Control (LoC).

Locus of control (LoC) is interpreted as "a generalized attitude, belief, or expectancy regarding the nature of the causal relationship between one's own behavior and its consequences" (Rotter, 1966, p. 2). Those with an external locus of control generally attribute life's outcomes to external factors such as fate, chance, social or environmental circumstances; while those with an internal locus of control believe that much of what happens in life is because one's own actions.

Since there are many questions used in different ways to measure locus of control (Pearlin and Schooler, 1978; Rotter, 1990), we had some flexibility in choosing the statements that we perceived as measuring locus of control most accurately. These statements follow most closely those statements of the NLSY79 CH/YA, SOEP, and HILDA surveys. We present the statements in a random order to avoid potential order effects. All the statements are shown at the same time with the same design as the statements used for the self-reported willingness to take risks. Five of the ten LoC statements are reverse scored as indicated by an R in brackets behind the respective statements below. The participants are told that “[t]he following statements apply to different attitudes towards life and the future.” We then instruct participants to:

> “Please indicate, by selecting one of the circles for each statement, how much you agree or disagree with each of the statements. You can choose a range on a scale from 1 to 7 whether you (1) completely disagree or (7) completely agree with the statement. Choose what you think generally applies to you the best concerning your own life, not as you wish it to be in the future.”

The ten statements we used are:

1. How my life evolves mostly depends on me.
2. I can achieve about anything I really set my mind to.
3. I feel like I have the capacity to overcome the problems I face in my life.
4. Self-determination and hard work are the main ingredients to achieve what I want.
5. I am the main controlling influence on the direction of my life.
6. I often feel like I'm being pushed around in life. [R]
7. It's difficult to make plans in life because of its unpredictability. [R]
8. My success and achievements are often dependent on how lucky I am. [R]
9. I have little control over the outcomes of my life. [R]
10. The opportunities I have are mainly determined by other people or social circumstances. [R]

We construct the measure of locus of control by summing up the choices of a participant after reverse-scoring the five relevant statements. For example, the second statement to measure locus of control is reverse scored [R], so we subtract the participant’s response from 8 to reverse this measure to represent internal locus of control (instead of external locus of control). After we perform this reversal for each reverse-scored statement, all statements will be correctly scored so that they represent an internal locus of control (the higher the number on each sub-scale is, the more internal a person is). We then sum up the ten statements of internal locus of control and finally divide the sum by 70, so that we receive a measure between 0 and 1. Our locus of control measure has a very good internal reliability ratio (Ins_{fair} $\alpha = 0.87$; Ins_{subs} $\alpha = 0.90$).

B.4 Associations between measures of risk taking and locus of control

The following table shows associations from OLS regressions of each of the measures of risk attitudes individually (constant relative risk aversion, self-reported general risk tolerance, self-reported financial risk tolerance, and the principal component of the two self-reported risk tolerance measures) on locus of control – with and without the same demographic covariates that we use in all our analyses.

Table B-1. Associations between risk measures and locus of control.

	Panel A. Act. fair experiment		**Panel B. Subsidized experiments**		**Panel C. Whole sample**	
	(1)	(2)	(3)	(4)	(5)	(6)
CRRA	-0.071	-0.040	-0.013	0.061	-0.056*	-0.009
	(0.082)	(0.079)	(0.067)	(0.055)	(0.030)	(0.027)
RT	0.170**	0.139	0.184***	0.182***	0.174***	0.149***
	(0.079)	(0.086)	(0.061)	(0.069)	(0.028)	(0.030)
FRT	0.212**	0.182**	0.152**	0.117*	0.197***	0.165***
	(0.084)	(0.091)	(0.062)	(0.067)	(0.030)	(0.031)
WTR	0.211***	0.179**	0.174***	0.172**	0.202***	0.175***
	(0.080)	(0.085)	(0.061)	(0.068)	(0.029)	(0.030)
Obs.	149	149	264	264	413	413
Controls	No	Yes	No	Yes	No	Yes

CRRA=Constant relative risk aversion; RT=Risk tolerance; FRT=Financial risk tolerance; WTR=Principal component risk (derived from RT and FRT). Each column shows estimates from separate regressions of each measurement (CRRA, RT, FRT, or WTR) on locus of control individually. The regressions are shown without and with the standard set of demographic controls. Standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

B.5 Locus of control test-retest correlation matrices and alpha coefficients from longitudinal surveys

The tables show the survey years in which locus of control was measured and pairwise correlations for each year's measures and additionally show Cronbach's alphas for each year (in the bottom row denoted *Alpha*). Statistical significance is denoted by *** p<0.01, ** p<0.05, * p<0.1.

Table B-2. Test-retest correlations and Cronbach's alphas of locus of control in longitudinal survey data

National Longitudinal Survey of Youth 79 Children/Young Adults (NLSY79 CH/YA)

	1994	1996	1998	2000	2002	2004	2006	2008	2010	2012	2014	2016
1994	1.000											
1996	0.423***	1.000										
1998	0.311***	0.450***	1.000									
2000	0.287***	0.426***		1.000								
2002	0.304**	0.297***	0.312***		1.000							
2004	0.282***	0.352***	0.326***	0.333***	0.426***	1.000						
2006	0.322***	0.332***	0.353***	0.347***	0.376***		1.000					
2008	0.303***	0.356***	0.305***	0.323***	0.403***	0.422***	0.511***	1.000				
2010	0.262	0.337***	0.182*	0.307**	0.339***	0.442***	0.458***		1.000			
2012	0.235***	0.340***	0.334***	0.468***	0.308***	0.456***	0.541***	0.490***		1.000		
2014	0.354***	0.302***	0.285***	0.307***	0.396***	0.375***	0.314***	0.432***	0.552***		1.000	
2016			0.213***	0.315*	0.224***	0.354***	0.337***	0.379***	0.467***	0.603***	0.567***	1.000
Alpha	*.722*	*.701*	*.738*	*.767*	*.724*	*.742*	*.756*	*.764*	*.770*	*.800*	*.786*	*.784*

German socioeconomic Panel (SOEP)

	2005	2010	2015
2005	1.000		
2010	0.492***	1.000	
2015	0.456***	0.539***	1.000
Alpha	*.615*	*.626*	*.609*

Household, Income and Labour Dynamics (HILDA) Survey

Variables	2003	2004	2007	2011	2015
2003	1.000				
2004	0.616***	1.000			
2007	0.533***	0.555***	1.000		
2011	0.486***	0.500***	0.559***	1.000	
2015	0.457***	0.473***	0.516***	0.570***	1.000
Alpha	*.865*	*.871*	*.863*	*.869*	*.880*

B.6 Measurement of the Big Five personality traits (OCEAN).

To measure big five personality traits Openness to experiences, Conscientiousness, Extraversion, Agreeableness, and Neuroticism (OCEAN), we use five statements per trait, of which two statements are reverse scored as indicated by an *R* in brackets behind the respective statements below. We measure each trait on a 7-point Likert-scale and randomize the order of the statements to avoid order effects. The statements follow most closely those statements of the NLSY79 CH/YA, SOEP, and HILDA surveys. Below we explain each of the five personality traits more closely by applying the definitions and terminology as found in Borghans, Duckworth, Heckman, and Weel (2008, Table 1) and in Mueller and Plug (2006, Table 1) and as adapted from John and Srivastava (1999), Costa and McCrae (1992) and Hogan and Hogan (2007).

We instruct participants to

> "Please describe yourself as you generally are now, not as you wish to be in the future. Describe yourself as you honestly see yourself currently." And then participants are asked to:
> "Please indicate for each statement below on a scale from 1 to 7 whether it is (1) inaccurate, (4) neither accurate nor inaccurate, or rather (7) accurate as a description of you."

Openness vs. Closeness to Experiences. This trait measures "the degree to which a person needs intellectual stimulation, change, and variety". Another commonly used name for openness to experiences is also intellect. Adjectives that are commonly used in statements associated with openness to experience are simple vs. complicated, imaginative, intelligent, creative, artistic, aesthetic, unconventional.

- I am creative and imaginative.
- I quickly understand abstract and new ideas.
- I enjoy doing complex things where I need to think creatively.
- I do not care about artistic and creative things. [*R*]
- I prefer to stick to the simple and conventional things. [*R*]

Conscientiousness vs. Lack of Direction. This trait measures "the degree to which a person is willing to comply with conventional rules, norms, and standards." Adjectives that are commonly used in statements associated with conscientiousness are caring, orderly, frivolous vs. organized, thorough, precise, accurate, dutiful, competent, deliberate (not impulsive).

- I get chores done properly right away.

- I am orderly and organized.
- I make plans and prepare for the future.
- I often do things half-heartedly and therefore need to re-do them. [*R*]
- I do not care about precision/accuracy. [*R*]

Extraversion vs. Introversion. This trait measures "the degree to which a person needs attention and social interaction." Adjectives that are commonly used in statements associated with extraversion are quiet, reserved, shy vs. talkative, assertive, active, sociable.

- I am the life of the party.
- I talk to a lot of different people at social gatherings.
- I do not mind being the center of attention.
- I tend to keep in the background. [*R*]
- I do not talk a lot. [*R*]

Agreeableness vs. Antagonism. This trait measures "the degree to which a person needs pleasant and harmonious relations with others." Adjectives that are commonly used in statements associated with agreeableness are cold, unfriendly vs. sympathetic, kind, obedient or allegiant, trusting, altruistic, modest (not bragging).

- I sympathize with others' feelings.
- I make people feel at ease / People feel comfortable around me.
- I take time out for others.
- I do not take and follow orders or instructions easily. [*R*]
- I confront people with the things they do or say. [*R*]

Neuroticism vs. Emotional Stability. This trait measures "the degree to which a person experiences the world as threatening and beyond his/her control." Another commonly used name for neuroticism is emotional stability, which is supposed to be the opposite extreme of neuroticism. Adjectives that are commonly used in statements associated with neuroticism are tense, anxious, impulsive, vulnerable, nervous vs. stable, calm, contented.

- I have frequent mood swings.
- I get upset or anxious easily.

- I often worry about things.
- I generally am a happy person. [*R*]
- I am relaxed and calm most of the time. [*R*]

We construct each of the five personality traits first by summing up the choices of a participant after reverse-scoring the relevant statements for the respective trait (reverse-scored items measure the opposite end of the trait), and then I divide the total by five. For example, two of the statements to measure neuroticism are reverse scored [*R]* and therefore these statements measure emotional stability; so, I subtract the participant's response from 6 to reverse the statement's measure to represent neuroticism (instead of emotional stability). All statements are now correctly scored, so we sum up the five statements of neuroticism. Finally, we divide the sum by five to construct our measure of the respective trait.

Table B-3. Correlation matrix between main dependent, independent and demographic variables

Panel A. Act. fair experiment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) CRRA	1.000											
(2) WTR (eigenvalue=1.58)	-0.23 **	1.000										
(3) LoC (α=0.866)	-0.076	0.202 *	1.000									
(4) Female	0.364 ***	-0.200 *	-0.007	1.000								
(5) Age	0.004	-0.005	-0.005	0.036	1.000							
(6) Ethnicity: White	0.244 **	-0.292 ***	-0.07	0.052	0.22 **	1.000						
(7) Std. education	-0.033	0.162	0.199 *	-0.027	0.039	-0.188 **	1.000					
(8) Student	0.383 ***	-0.011	0.067	0.002	-0.255 ***	-0.003	-0.081	1.000				
(9) Unemployed	-0.055	-0.150	-0.222 **	0.03	0.157 *	0.055	-0.279 **	-0.124 ***	1.000			
(10) Partnership	0.225 **	-0.112	0.169 *	0.137	0.140	0.085	0.305 ***	-0.18 *	-0.137	1.000		
(11) Homeowner	-0.200 *	-0.011	0.128	0.001	0.393 ***	0.159 *	0.278 ***	-0.207 **	-0.145 *	0.316 ***	1.000	
(12) HH size > 2	-0.087	-0.072	0.028	0.066	0.079	0.062	-0.142	0.002	0.145	0.108	-0.042	1.000
(13) Insurance choice (0 - 2)	0.06	-0.184 **	-0.025	0.116	0.132 *	0.052	-0.034	-0.052	0.178 **	0.046	0.117	0.032
(14) Times insured (0 – 12)	0.072	-0.247 **	-0.03	0.167 *	0.147	0.116	-0.015	-0.044	0.134 *	0.11	0.144	0.047
Panel B. Subsidized experiment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) CRRA	1.000											
(2) WTR (eigenvalue=1.57)	0.046	1.000										
(3) LoC (α=0.902)	0.068	0.158 *	1.000									
(4) Female	0.090	-0.07	0.160 **	1.000								
(5) Age	-0.001	0.000	0.000	-0.075	1.000							
(6) Ethnicity: White	0.079	-0.086	-0.039	-0.063	0.164 **	1.000						
(7) Std. education	0.142 *	0.026	0.119	0.036	0.044	-0.173 **	1.000					
(8) Student	0.006	-0.098	-0.02	-0.002	-0.148 ***	-0.216 ***	-0.141 *	1.000				
(9) Unemployed	0.122 *	-0.03	-0.122	0.046	0.034	0.087	-0.15 **	-0.191 ***	1.000			
(10) Partnership	0.230 ***	0.108	0.120 *	0.004	0.174 **	0.127 *	0.208 ***	-0.215 ***	0.005	1.000		
(11) Homeowner	-0.012	0.04	0.103	0.005	0.224 ***	0.056	0.126 *	-0.179 **	0.068	0.167 **	1.000	
(12) HH size > 2	-0.149 *	-0.073	0.058	0.005	0.098	0.140 *	-0.037	0.045	0.117	0.090	0.055	1.000
(13) Insurance choice (0 - 2)	0.077	-0.051	-0.061	0.085	0.053	-0.014	0.041	-0.101 *	-0.001	0.081	0.090	-0.009
(14) Times insured (0 – 12)	0.126 *	-0.122 *	-0.135 *	0.106	0.078	-0.005	0.104	-0.111	0.011	0.103	0.100	-0.012

This table shows a correlation matrix for all the variables used in the regression models. The correlations are shown separately for the actuarially fair (N=149) and the subsidized experiment N=263). CRRA=Constant relative risk aversion; WTR=Principal component risk derived from two self-reported measures of willingness to take risks (eigenvalue≈1.6, explained variance≈0.79), LoC=Locus of control (α=Cronbach's alpha). Statistical significance is denoted by *** p-value < 0.001, ** p-value < 0.01 and * p-value <0.05.

Appendix C. Insurance decision experiment

C.1 Experiment variables

Table C-1. Description of the experiment's variables

Variable	Description
Times insured	This variable counts how many years a participant was insured in total out of the twelve years (max. two years in each of the six insurance periods)
Times long-term insurance	This variable counts how often a participant chose the biennial (long-term) contract.
Times insured annually	This variable counts how often the participant chose an annual contract.
Insurance choice (0 - 2)	This variable shows what insurance alternative a participant chose in a specific period: 0 = no insurance in both years, 0.5 = insured in one of the two years, 1 = insured annually in both years, 2 = insured with a biennial (long-term) contract.
Disaster pp	A dummy variable indicating if the participant experienced a disaster in the previous period.
Insured pp	A dummy variable indicating if the participant insured (at all) in the previous period.
Female	A dummy variable indicating if someone's biological sex is female.
Age	Standardized age derived from a categorical age variable: 1=age 18 to 22, 2= age 23 to 25, 3= age 26 to 30, 4= age 31 to 35, 5= age 36 to 40, 6= age 41 to 50, 7= age 51 to 60, 8= age 61 to 70, 9= age 70 or above. Age in years shows the middle value of these ranges. Age is top coded as 70 if the age is 70 or above.
Ethnicity: White	A dummy variable indicating if someone reported their ethnicity as White (as opposed to any other ethnicity).
Std. Education	Standardized education derived from a categorical education variable: 1=below high school (9 years), 2= high school equivalent (12 years), 3= trade, vocational, or equivalent technical training (14 years), 4= associate degree or equivalent (some college but below bachelor's, 15 years), 5= bachelor's degree or equivalent (16 years), 6= master's degree or equivalent (18 years), 7= professional degree (CFA, CPA, ACCA etc., 19 years), 8= doctorate degree (21 years).
Std. Income	Standardized income derived from a categorical income variable (in Australian or New Zealand dollars): 1= below $25,000, 2= $25,000-34,999; 3= $35,000-54,999; 4=55,000-69,999 $;5=70,000-84,999 $;6= 85,000-99,999$; 7= $100,000-129,999; 8= $130,000-160,000; 9= above $160,000
Student	A dummy variable indicating whether the participant's main occupation is student.
Unemployed	A dummy variable indicating whether the participant is unemployed or a homemaker.
Partnership	A dummy variable indicating whether the participant is in a partnership (married, legal de facto or other long-term romantic relationship)
Homeowner	A dummy variable indicating whether the participant is a homeowner.
HH size > 2	A dummy variable indicating whether the participant's household occupies more than two people.
CRRA	Constant relative risk aversion parameter derived from the 60 binary lottery choices.
Locus of control	Locus of control derived from ten self-reported Likert scale statements.
Risk tolerance (RT)	Risk tolerance derived from a single 11-point self-reported Likert scale statement.
Financial risk tolerance (FR)	Financial risk tolerance derived from a single 11-point self-reported Likert scale statement.
Principal component risk (WTR)	Risk tolerance derived as the first principal component from RT and FR.
New Zealand	A dummy variable indicating whether the participant is a New Zealand resident.

Online	A dummy variable indicating whether the data is from the subsidized online experiment. The actuarially fair experiment was only conducted online.

This table briefly describes all the variables used for the estimations and analyses of the experimental data.

Table C-2. The 2-periodic experiment's variable notation

The numbers here were used in the actuarially fair insurance experiment.

HV1 = 500,000	Home value in year 1.
HV2 = 500,000 or 550,000	Home value in year 2, depending on the house value/reconstruction cost increase after the first year.
p_{HV} = 50%	Probability that the house value increases by 10% after year 1.
E = 55,000	Endowment at the beginning of each period that participants can use to pay for insurance. Participants are told this is the beginning balance of their bank account.
C_1 = 25,000	Premium for annual insurance in year 1 of each period.
C_2 = 25,000 or 27,500	Premium for annual insurance in year 2 of each period, determined at the beginning of year 2, depending on the house value (reconstruction cost) increase after year 1 and the choice of the participant whether to assess the house value.
C_{LTI} = 26,250	Premium for the long-term insurance contract fixed for both years.
p_D = 10%	Probability of a disaster in any year. Fixed for each year and independent of the year or period.
A = 2,500	Cost of a house value/reconstruction cost assessment after year 1.
D = 250,000 or 275,000	Damage caused by the disaster in year 1 or year 2. The damage depends on the current house value (or reconstruction cost) and is always equal to 50% of the current house value.

C.2 Experimental design: Similarities and differences to related studies

Premiums. We do not investigate premium loadings or differences in choices depending on the number or types of offered insurance contracts, since Kunreuther and Michel-Kerjan (2015, henceforth: K and M-K) and Papon (2008) have already shown that people purchase less insurance if the price increases and they purchase more insurance merely as an effect of having more options to choose from.

Contract types. Long-term contracts in Papon's study covered four years, while long-term insurance covered two years in K and M-K's study. Therefore, the latter study is more comparable to ours.

Information updates. K and M-K were interested in people's choices after they received new information about the chance of a disaster occurring. We instead investigate the potential increase of the house value (or house reconstruction costs), which effectively (mathematically) leads to the same outcome: an increase of the insurance premium in the second year. So rather than exploring how people react to a rise in premiums due to increased risks, we can investigate how people react to a random increase of their house value (or the reconstruction of the damaged home).

Disaster damage and insurance coverage. As in K and M-K's (2015) experiment, the damage caused by a disaster in our experiment is equal to half of the current house value and this is fully covered by the insurance offered in our experiment, whereas in Papon's (2008) experiment, the participants could choose their insurance coverage as a percentage of their initial endowment (option were 0%, 50%, 10%, 90%, or 100% coverage). This feature again makes our study more comparable to K and M-K's.

Probability of a disaster. We inform participants that the chance of a disaster is 10% each year – the two other studies had a 4% chance of a disaster. We also tell participants that the occurrence of a disaster is independent of each year and period as it was in Papon's (2008), but not in K and M-K's (2015) study. Instead, K and M-K applied a predetermined sequence in which disasters were triggered in specific periods of the experiment, but participants were not told about this sequence. In our experiments, a disaster can occur in any year. Papon (2008) used dice to determine the disasters. We let the computer trigger these events randomly (instead of rolling die) since we conducted our experiments online, like K and M-K did. Participants are told that this is a random computer-generated process, and they receive information about whether a disaster happened after every year.

Personal characteristics. We elicit many more traits than previous studies that have investigated long-term insurance decisions in an experiment. This large number of traits helps us better to understand the individuals who purchase (certain) insurance and those who do not. But rather than after the insurance choices, we elicit these traits before the experiment to avoid potential reverse causality issues. Any measurement of a trait after the experiment might be affected by the experiment's decisions or outcomes and thus should be considered a bad control variable (Angrist and Pischke 2008 pp. 64-68). We also use multiple measures of risk attitudes to make sure we capture risk attitudes more thoroughly than most other experimental insurance studies. Kunreuther and Michel-Kerjan (2015) elicited their risk attitudes measure – the only trait they elicited – after the experiment, while Papon (2008) did not measure any traits at all. We also collected more demographic information than the other two studies. Table A-1 below briefly explains each of the covariates we used in our models as control variables.

C.3 Descriptive statistics and comparison to related studies

We collected data from a total of 413 participants in three experiments. In one experiment we offered insurance at an actuarially fair price (=149), conducted online on Prolific Academic, and in another experiment, we offered insurance at a subsidized price (40% of act. fair). The subsidized experiment also was conducted online on Prolific Academic (N=174) and additionally in the lab (N=89). We imputed missing demographic information for 28 participants (8 online, 20 in the lab) of 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 face-to-face lab data collection, because here we were not able to substitute any information with demographic data from any other source. We could substitute demographic information for the online data collection because Prolific makes such information available to researchers at no cost. We include missing dummy indicators for each demographic variable to account for this imputation. All results are shown including the missing demographics and missing dummy indicators (we do not mention this specifically in each of the tables' notes).

Table C-3. 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).

Panels A and B of Table B-4 show the percentage of participants who chose one of the five possible combinations of insurance in the actuarially fair and the subsidized version of the experiment. Panel C shows p-values from one-sided difference-in-means tests, assuming that people from the subsidized experiment would more likely and more often insure than those in the actuarially fair experiment.

Table C-4. Insurance decisions by choice alternative in percent (Ins_{fair} vs Ins_{sub}).

Panel A. Ins_{fair}	(1) Period 1	(2) Period 2	(3) Period 3	(4) Period 4	(5) Period 5	(6) Period 6	(7) Average	(8) K and M-K ≈
(0, 0)	20.1%	20.8	21.5	17.4	21.5	20.1	20.2	27.4%
(0, 1)	6.7	4.7	5.4	4.7	6.0	5.4	5.5	3.9
(1, 0)	5.4	2.7	4.0	4.0	5.4	9.4	5.1	4.4
Only one year	12.1	7.4	9.4	8.7	11.4	14.8	10.6	8.3
(1, 1)	21.5	26.8	28.9	31.5	27.5	29.5	27.6	6.5
(2, 2)	46.3	45.0	40.3	42.3	39.6	35.6	41.5	57.8
Both years	67.8	71.8	69.1	73.8	67.1	65.1	69.1%	64.3
Panel B. Ins_{sub}	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Average	K and M-K ≥
(0, 0)	17.8%	16.3	15.2	15.9	17.0	12.9	15.8	25.8
(0, 1)	7.6	5.7	7.6	8.7	9.1	8.7	7.9	5.0
(1, 0)	4.2	6.4	8.0	5.3	5.7	6.8	6.1	5.9
Only one year	11.7	12.1	15.5	14.0	14.8	15.5	14.0	10.9
(1, 1)	22.3	23.5	21.6	25.4	26.5	24.2	23.9	23.6
(2, 2)	48.1	48.1	47.7	44.7	41.7	47.3	46.3	39.7
Both years	70.5	71.6	69.3	70.1	68.2	71.6	70.2%	63.3
Panel C. p-values	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Average	
(0, 0)	0.560	0.251	0.104	0.686	0.268	0.051*	0.320	
(0, 1)	0.746	0.670	0.393	0.132	0.273	0.217	0.405	
(1, 0)	0.576	0.096*	0.122	0.563	0.894	0.348	0.433	
Only one year	0.919	0.131	0.078*	0.114	0.339	0.836	0.403	
(1, 1)	0.838	0.448	0.098*	0.179	0.826	0.241	0.438	
(2, 2)	0.726	0.540	0.144	0.636	0.682	0.020*	0.458	
Both years	0.573	0.962	0.968	0.419	0.824	0.171	0.653	

This table shows the percent of participants who chose either of the five possible insurance alternatives within each of the six periods. Panels A shows the data from the actuarially fair and Panel B the data from the subsidized version of the experiment. 0,0= no insurance in both years; 0,1= no insurance in year 1, insured in year 2; 1,0= insured in year 1, no insurance in year 2; 1,1= insured annually in both years; 2,2= insured with long-term insurance in both years. The row "Only one year" shows the sum of those who insured only one of the two years per period, and the row "Both years" shows the sum of those who insured both years either with the annual (1,1) or with the long-term contract (2,2). Columns (1) through (6) show our participants' choices for each period, column (7) shows the average choices across all periods and the final column shows the average choices made by participants in Kunreuther and Michel-Kerjan's (2015) study (≈from the actuarially fair and ≥ the 10% loaded treatment). Panel C shows one-sided p-values from difference-in-means t-tests, assuming that participants in the subsidized experiment should be more likely to insure. * Denotes statistical significance at conventional levels, please refer directly to the cells in the table as Panel C shows the p-values from the one-sided t-tests.

More participants of our experiment overall preferred two annual insurance contracts, while fewer preferred no insurance at all, and about the same share of participants chose to insure only one of the two years, compared to the studies of Papon (2008) and Kunreuther and Michel-Kerjan (2015). Since K and M-K's study is more comparable to our study, we show their results from the actuarially fair version of their experiment in column (8). We see that more people overall insured and especially many more insured both years annually in our experiments (compare columns (7) and (8)). These different findings could be a result of New Zealand and Australia being more prone to disasters than France, which is rarely affected by disasters, and these are mostly floods (Boccard 2018), and the United States, which is mostly affected in specific areas such as California, Texas and Florida (NOA NCEI 2021). Since we do not know anything about the origin of the participants from Papon (2008) and Kunreuther and Michel-Kerjan (2015), we cannot make any further comparisons.

Surprisingly, in our experiments participants made annual choices that are much more consistent with the choices made in K and M-K's treatment in which the long-term insurance contract was loaded while the annual contract was not. Recall that in our experiments either all or none of the choices were subsidized (priced at 40% of the actuarially fair insurance).

Table C-5. Descriptive statistics of insurance choices by period.

Panel A. Ins_{fair}	**Period 1**	**Period 2**	**Period 3**	**Period 4**	**Period 5**	**Period 6**
Insurance choice in year 1	1.195	1.195	1.134	1.201	1.121	1.101
Insurance choice in year 2	1.208	1.215	1.148	1.208	1.128	1.060
Insurance choice (0 - 2)	1.201	1.205	1.141	1.205	1.124	1.081
Panel B. Ins_{subs}	**Period 1**	**Period 2**	**Period 3**	**Period 4**	**Period 5**	**Period 6**
Insurance choice in year 1	1.227	1.261	1.250	1.201	1.155	1.258
Insurance choice in year 2	1.261	1.254	1.246	1.235	1.189	1.277
Insurance choice (0 - 2)	1.244	1.258	1.248	1.218	1.172	1.267

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) for each of the six insurance periods separately.

Table C-6. Correlation matrix.

Panel A. Act. fair experiment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) CRRA	1.000											
(2) WTR (eigenvalue=1.58)	-0.23 **	1.000										
(3) LoC (α=0.866)	-0.076	0.202 *	1.000									
(4) Female	0.364 ***	-0.2 *	-0.007	1.000								
(5) Age	0.004	-0.005	-0.005	0.036	1.000							
(6) Ethnicity: White	0.244 **	-0.292 ***	-0.07	0.052	0.22 **	1.000						
(7) Std. education	-0.033	0.162	0.199 *	-0.027	0.039	-0.188 **	1.000					
(8) Student	0.383 ***	-0.011	0.067	0.002	-0.255 ***	-0.003	-0.081	1.000				
(9) Unemployed	-0.055	-0.15	-0.222 **	0.03	0.157 *	0.055	-0.279 **	-0.124 ***	1.000			
(10) Partnership	0.225 **	-0.112	0.169 *	0.137	0.14	0.085	0.305 ***	-0.18 *	-0.137	1.000		
(11) Homeowner	-0.2 *	-0.011	0.128	0.001	0.393 ***	0.159 *	0.278 ***	-0.207 **	-0.145 *	0.316 ***	1.000	
(12) HH size > 2	-0.087	-0.072	0.028	0.066	0.079	0.062	-0.142	0.002	0.145	0.108	-0.042	1.000
(13) Insurance choice (0 - 2)	0.06	-0.184 **	-0.025	0.116	0.132 *	0.052	-0.034	-0.052	0.178 **	0.046	0.117	0.032
(14) Times insured (0 – 12)	0.072	-0.247 **	-0.03	0.167 *	0.147	0.116	-0.015	-0.044	0.134 *	0.11	0.144	0.047
Panel B. Subsidized experiment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) CRRA	1.000											
(2) WTR (eigenvalue=1.57)	0.046	1.000										
(3) LoC (α=0.902)	0.068	0.158 *	1.000									
(4) Female	0.09	-0.07	0.16 **	1.000								
(5) Age	-0.001	0	0	-0.075	1.000							
(6) Ethnicity: White	0.079	-0.086	-0.039	-0.063	0.164 **	1.000						
(7) Std. education	0.142 *	0.026	0.119	0.036	0.044	-0.173 **	1.000					
(8) Student	0.006	-0.098	-0.02	-0.002	-0.148 ***	-0.216 ***	-0.141 *	1.000				
(9) Unemployed	0.122 *	-0.03	-0.122	0.046	0.034	0.087	-0.15 **	-0.191 ***	1.000			
(10) Partnership	0.23 ***	0.108	0.12 *	0.004	0.174 **	0.127 *	0.208 ***	-0.215 ***	0.005	1.000		
(11) Homeowner	-0.012	0.04	0.103	0.005	0.224 ***	0.056	0.126 *	-0.179 **	0.068	0.167 **	1.000	
(12) HH size > 2	-0.149 *	-0.073	0.058	0.005	0.098	0.14 *	-0.037	0.045	0.117	0.09	0.055	1.000
(13) Insurance choice (0 - 2)	0.077	-0.051	-0.061	0.085	0.053	-0.014	0.041	-0.101 *	-0.001	0.081	0.09	-0.009
(14) Times insured (0 – 12)	0.126 *	-0.122 *	-0.135 *	0.106	0.078	-0.005	0.104	-0.111	0.011	0.103	0.1	-0.012

This table shows a correlation matrix for all the variables used in the regression models. The correlations are shown separately for the actuarially fair (N=149) and the subsidized experiment N=263). CRRA=Constant relative risk aversion; WTR=Principal component derived from two self-reported measures of willingness to take risks (eigenvalue≈1.6, explained variance≈0.79), LoC=Locus of control (α=Cronbach's alpha). Statistical significance is denoted by *** p-value < 0.001, ** p-value < 0.01 and * p-value <0.05.

Table C-7. Insurance decisions by choice alternative in percent for both experimental versions.

Panel A. Ins_{fair}	(1) Period 1	(2) Period 2	(3) Period 3	(4) Period 4	(5) Period 5	(6) Period 6	(7) Average	(8) K and M-J ≈
(0, 0)	20.0%	20.7	21.3	17.3	21.3	20.0	20.1	27.4%
(0, 1)	6.7	4.7	5.3	4.7	6.0	5.3	5.4	3.9
(1, 0)	5.3	2.7	4.0	4.0	5.3	9.3	5.1	4.4
Only one year	12.0	7.3	9.3	8.7	11.3	14.7	10.6	8.3
(1, 1)	22.0	27.3	29.3	32.0	28.0	30.0	28.1	6.5
(2, 2)	46.0	44.7	40.0	42.0	39.3	35.3	41.2	57.8
Both years	68.0	72.0	69.3	74.0	67.3	65.3	69.3%	64.3
Panel B. Ins_{sub}	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Average	K and M-J ≥
(0, 0)	17.8%	16.3	15.2	15.9	17.0	12.9	15.8	25.8
(0, 1)	7.6	5.7	7.6	8.7	9.1	8.7	7.9	5.0
(1, 0)	4.2	6.4	8.0	5.3	5.7	6.8	6.1	5.9
Only one year	11.7	12.1	15.5	14.0	14.8	15.5	14.0	10.9
(1, 1)	22.3	23.5	21.6	25.4	26.5	24.2	23.9	23.6
(2, 2)	47.7	48.1	47.7	44.7	41.7	47.3	46.2	39.7
Both years	70.1	71.6	69.3	70.1	68.2	71.6	70.1%	63.3

This table shows the percent of participants who chose either of the five possible insurance alternatives within each of the six periods. 0,0= no insurance in both years; 0,1= no insurance in year 1, insured in year 2; 1,0= insured in year 1, no insurance in year 2; 1,1= insured annually in both years; 2,2= insured with long-term insurance in both years. The row "Only one year" shows the sum of those who insured only one of the two years per period, and the row "Both years" shows the sum of those who insured both years either with the annual (1,1) or with the long-term contract (2,2). Columns (1) through (6) show our participants' choices for each period, column (7) shows the average choices across all periods and the final column shows the average choices made by participants in Kunreuther and Michel-Kerjan's (2015) experiment (≈from the actuarially fair, ≥ from the 10% loaded treatment) as a comparison to a study that most closely resembles our study's design.

Appendix D. Robustness analyses for Section 3

D.1 Multinomial logistic regressions

We show all estimates again after we sequentially include additional covariates as in the main results (Table 7). The only difference here is that we now only include participants whose demographics were complete. Our main variables of interest are the principal component of risk tolerance (WTR), the constant relative risk aversion (CRRA) parameter, and locus of control (LoC). We confirm the previous findings, with the only difference that by the exclusion of participants for whom one of the demographic data were missing, we do not find any significance on the CRRA parameter and the significance on LoC is more dependent on the inclusion of other covariates. For example, LoC is insignificant in most models but becomes significant once we include demographic controls. Interestingly, we often see in robustness analyses that the CRRA parameter is significant *before* we include demographic controls and that this significance disappears once demographics are accounted for.

Table D-1. Main results excluding participants with missing information.

Panel A (i.period)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
CRRA	0.197 (0.138)	0.164 (0.141)	0.179 (0.125)
WTR	-0.158 (0.124)	-0.349** (0.146)	-0.298** (0.128)
LoC	-0.105 (0.139)	-0.228 (0.153)	-0.166 (0.145)
Pseudo R-squared		0.012	
Panel B (+ subsidized)			
CRRA	0.166 (0.139)	0.154 (0.142)	0.158 (0.126)
WTR	-0.156 (0.123)	-0.346** (0.146)	-0.296** (0.128)
LoC	-0.112 (0.138)	-0.231 (0.154)	-0.171 (0.144)
Pseudo R-squared		0.016	
Panel C (+ New Zealand)			
CRRA	0.165 (0.140)	0.152 (0.142)	0.157 (0.127)
WTR	-0.159 (0.123)	-0.349** (0.145)	-0.302** (0.128)
LoC	-0.129 (0.141)	-0.260* (0.157)	-0.219 (0.147)
Pseudo R-squared		0.019	
Panel D (+ previous period dummies)			
CRRA	0.183 (0.139)	0.159 (0.127)	0.165 (0.112)
WTR	-0.147 (0.122)	-0.308** (0.131)	-0.263** (0.113)
LoC	-0.118 (0.127)	-0.225* (0.133)	-0.181 (0.122)
Pseudo R-squared		0.087	
Panel E (+ demographics)			
CRRA	0.148 (0.162)	0.080 (0.144)	0.121 (0.125)
WTR	-0.140 (0.124)	-0.329** (0.135)	-0.285** (0.118)
LoC	-0.158 (0.131)	-0.346** (0.142)	-0.258** (0.126)
Pseudo R-squared		0.129	
Panel F (+ big five personality)			
CRRA	0.119 (0.166)	0.062 (0.143)	0.119 (0.123)
WTR	-0.093 (0.130)	-0.271** (0.134)	-0.288** (0.120)
LoC	-0.096 (0.144)	-0.257* (0.152)	-0.255* (0.133)
Pseudo R-squared		0.117	

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). 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.

Table D-2. Main results excluding participants who failed 2 or more attention questions in the insurance decision task.

Panel A (i.period)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
CRRA	0.125 (0.143)	0.048 (0.157)	0.059 (0.133)
WTR	-0.217 (0.158)	-0.640*** (0.178)	-0.434*** (0.167)
LoC	-0.228 (0.211)	-0.405* (0.246)	-0.349 (0.236)
Pseudo R-squared		0.027	
Panel B (+ subsidized)			
CRRA	0.117 (0.146)	0.055 (0.157)	0.055 (0.135)
WTR	-0.216 (0.158)	-0.643*** (0.177)	-0.433*** (0.167)
LoC	-0.229 (0.208)	-0.405 (0.248)	-0.348 (0.234)
Pseudo R-squared		0.030	
Panel C (+ New Zealand)			
CRRA	0.122 (0.148)	0.068 (0.155)	0.078 (0.136)
WTR	-0.213 (0.157)	-0.635** (0.175)	-0.425** (0.165)
LoC	-0.248 (0.221)	-0.444* (0.261)	-0.419* (0.245)
Pseudo R-squared		0.034	
Panel D (+ previous period dummies)			
CRRA	0.150 (0.157)	0.096 (0.147)	0.107 (0.127)
WTR	-0.196 (0.160)	-0.573*** (0.161)	-0.361** (0.149)
LoC	-0.192 (0.192)	-0.343 (0.216)	-0.318 (0.202)
Pseudo R-squared		0.087	
Panel E (+ demographics)			
CRRA	0.061 (0.188)	-0.096 (0.199)	-0.094 (0.168)
WTR	-0.176 (0.159)	-0.544*** (0.165)	-0.390** (0.153)
LoC	-0.128 (0.185)	-0.374* (0.215)	-0.364* (0.203)
Pseudo R-squared		0.130	
Panel F (+ big five personality)			
CRRA	0.062 (0.189)	-0.109 (0.203)	-0.101 (0.172)
WTR	-0.179 (0.170)	-0.524*** (0.172)	-0.411** (0.163)
LoC	-0.086 (0.200)	-0.343 (0.217)	-0.403** (0.199)
Pseudo R-squared		0.135	

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). 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.

Table D-3. Main results for choices in the first period (period==1).

Panel A (i.period)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
CRRA	0.337* (0.189)	0.199 (0.165)	0.290** (0.145)
WTR	-0.432** (0.198)	-0.498*** (0.169)	-0.431*** (0.150)
LoC	-0.657*** (0.200)	-0.411** (0.177)	-0.380** (0.160)
Pseudo R-squared		0.030	
Panel B (+ subsidized)			
CRRA	0.333* (0.189)	0.193 (0.166)	0.284* (0.145)
WTR	-0.431** (0.198)	-0.498*** (0.169)	-0.430*** (0.150)
LoC	-0.658*** (0.200)	-0.413** (0.178)	-0.382** (0.160)
Pseudo R-squared		0.030	
Panel C (+ New Zealand)			
CRRA	0.340* (0.191)	0.207 (0.166)	0.298** (0.146)
WTR	-0.426** (0.196)	-0.499*** (0.169)	-0.431*** (0.150)
LoC	-0.656*** (0.204)	-0.464** (0.181)	-0.434*** (0.163)
Pseudo R-squared		0.037	
Panel D (+ previous period dummies) N/A	Skipped because we are only looking at the first period		
Panel E (+ demographics)			
CRRA	0.197 (0.242)	-0.043 (0.205)	0.118 (0.180)
WTR	-0.433** (0.213)	-0.517*** (0.184)	-0.466*** (0.162)
LoC	-0.728*** (0.233)	-0.585*** (0.199)	-0.479*** (0.178)
Pseudo R-squared		0.124	
Panel F (+ big five personality)			
CRRA	0.157 (0.250)	-0.070 (0.207)	0.096 (0.183)
WTR	-0.375* (0.222)	-0.502*** (0.190)	-0.461*** (0.169)
LoC	-0.645** (0.262)	-0.583*** (0.220)	-0.487** (0.198)
Pseudo R-squared		0.138	

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). 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.

Table D-4. Main results for choices after the first period (period>1)

Panel A (i.period)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
CRRA	0.129 (0.083)	0.203*** (0.071)	0.183*** (0.065)
WTR	-0.094 (0.085)	-0.311*** (0.072)	-0.232*** (0.066)
LoC	-0.094 (0.087)	-0.217*** (0.073)	-0.188*** (0.068)
Pseudo R-squared		0.011	
Panel B (+ subsidized)			
CRRA	0.110 (0.084)	0.197*** (0.071)	0.171*** (0.066)
WTR	-0.091 (0.085)	-0.309*** (0.072)	-0.230*** (0.066)
LoC	-0.100 (0.088)	-0.218*** (0.073)	-0.191*** (0.068)
Pseudo R-squared		0.015	
Panel C (+ New Zealand)			
CRRA	0.114 (0.084)	0.204*** (0.071)	0.180*** (0.066)
WTR	-0.092 (0.085)	-0.308*** (0.072)	-0.231*** (0.066)
LoC	-0.116 (0.089)	-0.243*** (0.075)	-0.230*** (0.069)
Pseudo R-squared		0.017	
Panel D (+ previous period dummies)			
CRRA	0.124 (0.086)	0.197** (0.078)	0.175** (0.072)
WTR	-0.070 (0.087)	-0.252*** (0.079)	-0.180** (0.073)
LoC	-0.100 (0.088)	-0.200** (0.079)	-0.185** (0.073)
Pseudo R-squared	0.093	0.093	0.093
Panel E (+ demographics)			
CRRA	0.028 (0.104)	0.093 (0.096)	0.113 (0.089)
WTR	-0.052 (0.092)	-0.249*** (0.083)	-0.211*** (0.078)
LoC	-0.073 (0.095)	-0.278*** (0.087)	-0.228*** (0.081)
Pseudo R-squared		0.123	
Panel F (+ big five personality)			
CRRA	0.014 (0.106)	0.081 (0.097)	0.112 (0.089)
WTR	-0.006 (0.098)	-0.164* (0.088)	-0.189** (0.082)
LoC	0.002 (0.104)	-0.153 (0.096)	-0.208** (0.089)
Pseudo R-squared		0.129	

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). 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.

D.2 Logistic regressions for each period, clustered on individual.

We ran Logit regressions on a dummy indicating if someone insured in full in a specific period (the dummy takes the value 0 if the period was not insured or only partially insured) on the usual independent variables (with and without demographics). Here, we show the results for the whole sample and only those whose demographic information was complete separately. The logit robustness analyses confirm that both WTR and LoC are significantly related to insurance choices.

Table D-5. Logit regression of dummy indicating if fully insured in period *i*.

	Whole sample, N=2,478				Excluding people with missing demographics, N=2,220			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CRRA	0.136	0.118*	0.083	0.085	0.088	0.077	0.039	0.046
	(0.084)	(0.069)	(0.086)	(0.087)	(0.098)	(0.080)	(0.094)	(0.096)
WTR	-0.227**	-0.195***	-0.222***	-0.213***	-0.252***	-0.211***	-0.238***	-0.241***
	(0.089)	(0.074)	(0.077)	(0.078)	(0.096)	(0.080)	(0.082)	(0.083)
LoC	-0.173*	-0.137*	-0.202**	-0.203**	-0.177*	-0.140	-0.209**	-0.209**
	(0.097)	(0.077)	(0.082)	(0.088)	(0.108)	(0.086)	(0.090)	(0.096)
Pseudo R-squared	0.022	0.150	0.182	0.183	0.022	0.151	0.175	0.177
Demographics	NO	NO	YES	YES	NO	NO	YES	YES
Big five traits	NO	NO	NO	YES	NO	NO	NO	YES

This table shows results from Logit regression analyses of an insurance choice dummy indicating if someone insured fully in a period. All regressions include dummies controlling for whether the data is from the subsidized or actuarially fair experiment, whether a participant is a New Zealand resident, and whether the participant experienced a disaster or insured in the previous period in full. We sequentially added demographics and the big five personality traits as shown in the table (YES/NO). 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.

D.3 OLS regressions on the number of years insured

An additional robustness analysis of the number of years insured regressed on the typical independent further lends support to the results in the main text.

Table D-6. OLS regression results of the number of years insured.

	Whole sample, N=2,478				Excluding people with missing demographics, N=2,220			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CRRA	0.316*	0.167	0.185	0.180	0.247	0.144	0.199	0.212
	(0.169)	(0.123)	(0.150)	(0.151)	(0.211)	(0.159)	(0.173)	(0.174)
WTR	-0.495***	-0.182	-0.229**	-0.166	-0.541***	-0.227*	-0.269**	-0.234*
	(0.188)	(0.115)	(0.114)	(0.119)	(0.201)	(0.121)	(0.120)	(0.126)
LoC	-0.399**	-0.243**	-0.308**	-0.229*	-0.374*	-0.193	-0.295**	-0.214
	(0.197)	(0.115)	(0.123)	(0.136)	(0.215)	(0.125)	(0.133)	(0.146)
Unique people	413	413	413	413	370	370	370	370
R-squared	0.049	0.617	0.651	0.655	0.047	0.606	0.635	0.639
Period 1	NO	YES	YES	YES	NO	YES	YES	YES
Demographics	NO	NO	YES	YES	NO	NO	YES	YES
Big five traits	NO	NO	NO	YES	NO	NO	NO	YES

This table shows results from Logit regression analyses of an insurance choice dummy indicating if someone insured fully in a period. All regressions include dummies controlling for whether the data is from the subsidized or actuarially fair experiment, and whether a participant is a New Zealand resident. Period 1 adds controls for the insurance choice and whether a participant experienced a disaster in the first period. 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.

D.4 Subsidized vs. actuarially fair experiment

Table D-7. Multinomial logistic results for the actuarially fair version.

Panel A (i.period)	**Alternative 0,1 or 1,0**	**Alternative 1,1**	**Alternative 2,2**
CRRA	-0.106 (0.209)	0.050 (0.217)	0.076 (0.192)
WTR	-0.311 (0.223)	-0.513** (0.244)	-0.557** (0.217)
LoC	-0.373 (0.253)	-0.009 (0.256)	-0.055 (0.245)
Pseudo R-squared		0.027	
Panel B (+ subsidized)	Omitted because we are only looking at the experimental versions separately		
Panel C (+ New Zealand)			
CRRA	-0.110 (0.203)	0.044 (0.220)	0.071 (0.198)
WTR	-0.284 (0.218)	-0.524** (0.247)	-0.575** (0.225)
LoC	-0.360 (0.253)	-0.020 (0.255)	-0.071 (0.243)
Pseudo R-squared		0.034	
Panel D (+ previous period dummies)			
CRRA	-0.098 (0.205)	0.053 (0.203)	0.078 (0.182)
WTR	-0.248 (0.213)	-0.437* (0.224)	-0.497** (0.199)
LoC	-0.370* (0.222)	-0.057 (0.215)	-0.109 (0.195)
Pseudo R-squared		0.120	
Panel E (+ demographics)			
CRRA	-0.236 (0.376)	-0.375 (0.335)	0.041 (0.265)
WTR	-0.180 (0.215)	-0.369 (0.234)	-0.420** (0.203)
LoC	-0.420* (0.237)	-0.162 (0.236)	-0.118 (0.204)
Pseudo R-squared		0.184	
Panel F (+ big five personality)			
CRRA	-0.317 (0.370)	-0.343 (0.342)	0.162 (0.275)
WTR	0.072 (0.278)	-0.127 (0.260)	-0.416* (0.228)
LoC	-0.036 (0.252)	0.172 (0.257)	0.049 (0.223)
Pseudo R-squared		0.221	

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). 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.

Table D-8. Multinomial logistic results for the subsidized version.

Panel A (i.period)	**Alternative 0,1 or 1,0**	**Alternative 1,1**	**Alternative 2,2**
CRRA	0.263* (0.143)	0.289** (0.139)	0.248* (0.128)
WTR	-0.053 (0.128)	-0.250 (0.158)	-0.108 (0.139)
LoC	-0.144 (0.133)	-0.394** (0.162)	-0.318** (0.152)
Pseudo R-squared		0.016	
Panel B (+ subsidized)		Omitted because we are only looking at the experimental versions separately	
Panel C (+ New Zealand)			
CRRA	0.284** (0.144)	0.311** (0.142)	0.273** (0.132)
WTR	-0.052 (0.129)	-0.249 (0.158)	-0.104 (0.137)
LoC	-0.196 (0.136)	-0.444*** (0.164)	-0.386** (0.153)
Pseudo R-squared		0.019	
Panel D (+ previous period dummies)			
CRRA	0.283* (0.147)	0.292** (0.129)	0.256** (0.116)
WTR	-0.067 (0.132)	-0.252* (0.145)	-0.106 (0.122)
LoC	-0.172 (0.131)	-0.377*** (0.143)	-0.316** (0.130)
Pseudo R-squared		0.080	
Panel E (+ demographics)			
CRRA	0.260 (0.191)	0.223 (0.189)	0.236 (0.161)
WTR	-0.061 (0.135)	-0.274* (0.147)	-0.158 (0.126)
LoC	-0.167 (0.139)	-0.490*** (0.152)	-0.401*** (0.137)
Pseudo R-squared		0.117	
Panel F (+ big five personality)			
CRRA	0.227 (0.193)	0.193 (0.190)	0.202 (0.158)
WTR	-0.012 (0.141)	-0.242 (0.151)	-0.162 (0.130)
LoC	-0.158 (0.164)	-0.484*** (0.166)	-0.458*** (0.148)
Pseudo R-squared		0.122	

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). 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.

D.5 Robustness analyses of safe lottery choices

Table D-9. Correlation matrix of 'safe' choices and risk attitudes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Safe Choice SD	1.000										
(2) Safe Choice CV	0.186 ***	1.000									
(3) Safe Choice EV	-0.044	0.772 ***	1.000								
(4) Constant relative risk aversion	**-0.031**	**0.025**	**0.002**	1.000							
(5) Willingness to take risks	**-0.063**	**0.033**	**0.063**	-0.067	1.000						
(6) Locus of control	**-0.04**	**-0.065**	**-0.037**	-0.015	0.17 ***	1.000					
(7) Female	0.047	0.027	0.022	0.183 ***	-0.122 *	0.099 *	1.000				
(8) Times insured (0-12)	-0.038	-0.026	-0.074	0.097 *	-0.164 ***	-0.081	0.129 **	1.000			
(9) Times safe choice EV (18-41)	-0.124 *	0.123 *	0.183 ***	**-0.392 *****	**0.276 *****	**0.048**	-0.104 *	-0.224 ***	1.000		
(10) Times safe choice CV (14-49)	-0.115 *	0.164 ***	0.197 ***	**-0.359 *****	**0.245 *****	**0.07**	-0.083	-0.208 ***	0.936 ***	1.000	
(11) Times safe choice SD (11-50)	0.175 ***	-0.013	-0.027	**0.124 ***	**-0.24 *****	**0.03**	0.087	0.114 *	-0.452 ***	-0.324 ***	1.000

This matrix shows pairwise correlations. Numbers printed in bold show the pairwise correlations between the risk attitude measures and the dependent variables that are used in the regressions shown in the main results and robustness analyses. Statistical significance denoted as *** p<0.001, ** p<0.01, *p<0.05.

Table D-10. Robustness analysis: Logistic regression of times chosen 'safe' option on each risk measure individually.

	SD	CV	EV
CRRA	0.037*** (0.013)	-0.147*** (0.019)	-0.165*** (0.021)
WTR	-0.063*** (0.014)	0.100*** (0.021)	0.118*** (0.022)
LoC	0.008 (0.015)	0.029 (0.019)	(0.016 (0.020)

This table shows coefficients individual logistic regression analyses of the lottery choices made by participants on either of the measures of risk attitudes as shown to the left of the table. These results are comparable to Panel A of Table 8. The dependent variable takes the value 0 if the participant chose the risky choice, and 1 if the participant chose the safer choice out of two pie charts presented within a pair of pie charts. 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.

Table D-11. Robustness analysis: OLS of times chosen 'safe' option.

Panel A	**Standard deviation**	**Coefficient of variance**	**Expected utility**
CRRA	0.511*** (0.185)	-2.120*** (0.277)	-2.370*** (0.300)
WTR	-0.976*** (0.200)	1.374*** (0.301)	1.683*** (0.314)
LoC	0.301 (0.212)	0.153 (0.271)	-0.098 (0.276)
R-squared	0.076	0.165	0.189
Panel B (+ big five personality)			
CRRA	0.533*** (0.187)	-2.127*** (0.277)	-2.383*** (0.299)
WTR	-0.941*** (0.222)	1.361*** (0.313)	1.655*** (0.329)
LoC	0.221 (0.231)	0.269 (0.299)	0.056 (0.296)
R-squared	0.093	0.176	0.201
Panel C (+ Female dummy)			
CRRA	0.533*** (0.186)	-2.127*** (0.276)	-2.384*** (0.299)
WTR	-0.932*** (0.225)	1.348*** (0.314)	1.639*** (0.330)
LoC	0.213 (0.229)	0.279 (0.298)	0.070 (0.294)
Female	0.668* (0.388)	-0.947 (0.586)	-1.272** (0.613)
R-squared	0.099	0.181	0.210
Panel D (+ demographics)			
CRRA	0.653*** (0.218)	-2.279*** (0.346)	-2.445*** (0.393)
WTR	-0.951*** (0.240)	1.398*** (0.328)	1.679*** (0.345)
LoC	0.294 (0.227)	0.305 (0.326)	0.030 (0.330)
Female	0.699* (0.390)	-1.053* (0.603)	-1.354** (0.634)
R-squared	0.134	0.202	0.227

This table shows results from an OLS regression analysis of the lottery choices made by participants. The dependent variable takes the value 0 if the participant chose the risky choice, and 1 if the participant chose the safer choice out of two pie charts presented within a pair of pie charts. Panel A includes only the three measures of risk attitudes (CRRA< WTR, LoC). We then add covariates sequentially: Panel B includes the big five personality traits, panel C adds a female dummy, and panel D adds the same demographic controls we use in other regression models. 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.

Appendix E. Robustness analyses of insurance choices for section 4

Table E-1. Extended results for Table 13.

	Alt (0,1) or (1,0)	Alt (1,1)	Alt (2,2)	Alt (0,1) or (1,0)	Alt (1,1)	Alt (2,2)	Alt (0,1) or (1,0)	Alt (1,1)	Alt (2,2)	Alt (0,1) or (1,0)	Alt (1,1)	Alt (2,2)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	-0.170	0.109	0.369*	-0.072	0.228	0.471**	-0.197	0.200	0.494**	-0.146	0.236	0.620***
New Zealand	(0.251)	(0.252)	(0.214)	(0.253)	(0.259)	(0.221)	(0.261)	(0.271)	(0.228)	(0.256)	(0.280)	(0.236)
Insured previous	0.901***	3.166***	2.802***	0.861***	3.104***	2.752***	0.865***	3.103***	2.731***	0.840***	2.981***	2.576***
period	(0.202)	(0.235)	(0.214)	(0.205)	(0.238)	(0.216)	(0.206)	(0.238)	(0.216)	(0.216)	(0.245)	(0.221)
Disaster previous	-0.123	-0.217	-0.160	-0.142	-0.241	-0.184	-0.129	-0.252	-0.165	-0.174	-0.239	-0.160
period	(0.204)	(0.211)	(0.194)	(0.207)	(0.212)	(0.195)	(0.208)	(0.212)	(0.194)	(0.212)	(0.218)	(0.199)
	-0.487*	-0.001	0.329	-0.545*	-0.060	0.275	-0.771**	-0.294	0.240	-0.973**	-0.606	-0.170
Online	(0.296)	(0.303)	(0.251)	(0.297)	(0.305)	(0.253)	(0.339)	(0.367)	(0.299)	(0.446)	(0.480)	(0.399)
	0.405	0.102	0.375*	0.344	0.033	0.312	0.293	-0.026	0.291	0.219	-0.087	0.244
Subsidized	(0.247)	(0.246)	(0.209)	(0.247)	(0.249)	(0.212)	(0.265)	(0.278)	(0.231)	(0.286)	(0.298)	(0.256)
Risk attitudes												
				0.178	0.187*	0.174*	0.163	0.175	0.169*	0.066	0.075	0.118
CRRA				(0.121)	(0.106)	(0.091)	(0.125)	(0.107)	(0.091)	(0.144)	(0.131)	(0.109)
				-0.136	-0.296**	-0.230**	-0.057	-0.208*	-0.210**	-0.044	-0.189	-0.230**
WTR				(0.112)	(0.116)	(0.098)	(0.116)	(0.118)	(0.102)	(0.119)	(0.121)	(0.103)
				-0.200*	-0.221*	-0.194*	-0.059	-0.116	-0.181*	-0.051	-0.174	-0.231**
LoC				(0.114)	(0.115)	(0.103)	(0.130)	(0.130)	(0.109)	(0.132)	(0.138)	(0.115)
Big five personality traits												
							-0.016	-0.095	0.005	0.081	-0.014	0.117
Openness							(0.115)	(0.127)	(0.106)	(0.123)	(0.137)	(0.116)
							-0.037	0.068	0.050	-0.062	-0.001	-0.001
Conscientiousness							(0.116)	(0.121)	(0.100)	(0.111)	(0.123)	(0.098)
							-0.146	-0.184	-0.029	-0.134	-0.208*	-0.031
Extraversion							(0.115)	(0.119)	(0.096)	(0.114)	(0.121)	(0.101)
							-0.109	-0.152	-0.107	-0.078	-0.162	-0.104
Agreeableness							(0.126)	(0.130)	(0.117)	(0.128)	(0.132)	(0.119)
							0.205*	0.154	-0.043	0.237*	0.186	-0.027
Neuroticism							(0.116)	(0.122)	(0.102)	(0.122)	(0.128)	(0.110)
Demographics												
										0.027	0.119	0.087
Income										(0.124)	(0.127)	(0.105)
										-0.143	-0.148	-0.240***
Income squared										(0.089)	(0.102)	(0.091)
										0.149	0.272	0.426**
Female										(0.200)	(0.217)	(0.182)
										-0.086	0.128	0.077
Age										(0.147)	(0.152)	(0.127)
										-0.073	-0.026	0.138
Age squared										(0.122)	(0.118)	(0.110)
										0.213	0.339	-0.013
Ethnicity white										(0.249)	(0.269)	(0.211)
										-0.107	0.128	-0.105
Education										(0.111)	(0.120)	(0.102)
Education										-0.074	-0.195**	-0.106*
squared										(0.071)	(0.083)	(0.062)
										-0.011	0.084	-0.131
Student										(0.364)	(0.360)	(0.300)
										0.545	0.264	0.438
Unemployed										(0.424)	(0.411)	(0.371)
										-0.006	0.232	0.227
Partnership										(0.262)	(0.268)	(0.235)
										0.463**	0.250	0.543***
Homeowner										(0.224)	(0.253)	(0.208)
										-0.228	-0.037	-0.149
HH size > 2										(0.230)	(0.229)	(0.202)
Observations	2,478	2,478	2,478	2,478	2,478	2,478	2,478	2,478	2,478	2,478	2,478	2,478
R-Squared	0.103	0.103	0.103	0.110	0.110	0.110	0.116	0.116	0.116	0.142	0.142	0.142
Additional R-squared	n/a	n/a	n/a	0.007	0.007	0.007	0.006	0.006	0.006	0.026	0.026	0.026
Chi2 Test Risk attitudes	NA	NA	NA	23.151	23.151	23.151	17.348	17.348	17.348	17.560	17.560	17.560
Chi2 p-value risk attitudes	NA	NA	NA	0.006	0.006	0.006	0.044	0.044	0.044	0.041	0.041	0.041
Chi2 Test Big 5 personality	NA	NA	NA	NA	NA	NA	17.011	17.011	17.011	19.897	19.897	19.897
Chi2 p-value Big 5 personality	NA	NA	NA	NA	NA	NA	0.318	0.318	0.318	0.176	0.176	0.176
Chi2 Test Demographics	NA	NA	NA	NA	NA	NA	NA	NA	NA	257.471	257.471	257.471
Chi2 p-value Demographics	NA	NA	NA	NA	NA	NA	NA	NA	NA	0.000	0.000	0.000

This table shows the full results that are abbreviated in Table 13.

All regressions in the following tables include the same control variables, unless noted otherwise. Risk attitudes are measured with three variables: a constant relative risk aversion (CRRA) parameter, a principal component of two willingness to take risks survey questions, and locus of control. The big five personality traits are openness to experiences, conscientiousness, extraversion, agreeableness, and neuroticism. Demographic controls include a categorical variable of income (standardized and squared), a female dummy, a categorical age variable (standardized and squared), a white ethnicity dummy, education in years (standardized and squared), a student and an unemployed dummy, a dummy indicating if someone is in a partnership, one dummy indicating whether someone is a homeowner, and one dummy indicating if someone lives in a household that is larger than 2 people. The main explanatory variables of interest are three dummy indicators, (1) one showing whether the participant is a New Zealand resident, (2) one dummy showing whether the participant experienced a disaster in the previous period and (1) one dummy showing whether the participant bought insurance in the previous period.

E.1 Excluding participants with missing information

Table E-2. Main results for the sample without missing demographics.

Both experiments combined, N=370 (x 6 periods = 2,220 obs.)			
	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.172 (0.262)	0.079 (0.274)	0.395* (0.239)
Insured previous period	1.125*** (0.198)	2.462*** (0.191)	2.457*** (0.194)
Disaster previous period	-0.235 (0.212)	-0.540*** (0.194)	-0.474*** (0.182)
Subsidized	0.402 (0.263)	0.042 (0.269)	0.348 (0.236)
Pseudo R-squared		0.084	
Risk attitudes incl. LoC		No	
Big five personality traits		No	
Demographics		No	
	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.096 (0.263)	0.214 (0.282)	0.502** (0.247)
Insured previous period	1.106*** (0.200)	2.418*** (0.194)	2.422*** (0.196)
Disaster previous period	-0.233 (0.214)	-0.534*** (0.194)	-0.472** (0.184)
Subsidized	0.346 (0.266)	-0.022 (0.273)	0.294 (0.238)
Pseudo R-squared		0.091 (Δ Pseudo R^2≈0.007)	
Risk attitudes incl. LoC		Chi^2-test≈17.68, p-value ≤ 0.039	
Big five personality traits		No	
Demographics		No	
	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.292 (0.273)	0.125 (0.296)	0.499* (0.257)
Insured previous period	1.118*** (0.203)	2.426*** (0.195)	2.413*** (0.197)
Disaster previous period	-0.216 (0.214)	-0.541*** (0.195)	-0.452** (0.184)
Subsidized	0.355 (0.279)	-0.076 (0.294)	0.312 (0.253)
Pseudo R-squared		0.098 (Δ Pseudo R^2≈0.007)	
Risk attitudes incl. LoC		Chi^2-test≈15.31, p-value ≤ 0.083	
Big five personality traits		Chi^2-test≈17.57, p-value ≤ 0.286	
Demographics		No	
	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.238 (0.259)	0.225 (0.298)	0.589** (0.258)
Insured previous period	1.116*** (0.206)	2.358*** (0.198)	2.345*** (0.199)
Disaster previous period	-0.240 (0.218)	-0.523*** (0.198)	-0.439** (0.188)
Subsidized	0.216 (0.290)	-0.093 (0.315)	0.268 (0.278)
Pseudo R-squared		0.120 (Δ Pseudo R^2≈ 0.022)	
Risk attitudes incl. LoC		Chi^2-test≈18.27, p-value ≤ 0.032	
Big five personality traits		Chi^2-test≈20.14, p-value ≤ 0.167	
Demographics		Chi^2-test≈61.17, p-value ≤ 0.013	

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 a self-reported risk tolerance, a constant relative risk aversion parameter, locus of control, and a set of demographic characteristics as explanatory variables: a categorical variable of income (standardized and squared), a female dummy, a categorical age variable (standardized and squared), a white ethnicity dummy, a categorical variable of years of education (standardized and squared), a student and unemployed dummy, a dummy indicating if someone is in a partnership, one dummy indicating whether someone is a home owner, one dummy indicating if someone lives 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. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.2 Logistic regressions of insured dummy for each period

We ran Logistic regressions of a dummy indicating if someone insured (at all) in a specific period on the usual independent variables (with and without demographics). The Logistic regressions confirm that both the principal component of our willingness to take risks measures (PCR) and locus of control (LoC) are related to insurance choices, whereas constant relative risk aversion (CRRA) is not. Our main results in section 3.1.2 indicate that New Zealanders are more likely to choose biennial insurance, which is no longer visible here. We again find that past choices are the best predictors of future choices.

Table E-3. Logistic robustness checks.

	Whole sample, N=2,478			**Excluding people with missing demographics, N=2,220**		
	(1)	(2)	(3)	(4)	(5)	(6)
New Zealand	0.275	0.380*	0.390*	0.229	0.332	0.374*
	(0.195)	(0.204)	(0.210)	(0.205)	(0.219)	(0.218)
Insurance choice in first period	1.667***	1.685***	1.699***	1.698***	1.730***	1.757***
	(0.121)	(0.123)	(0.127)	(0.130)	(0.132)	(0.134)
Disaster in first period	-0.310	-0.352	-0.302	-0.339	-0.349	-0.314
	(0.244)	(0.239)	(0.253)	(0.262)	(0.260)	(0.274)
subsidized	0.409*	0.334	0.202	0.435	0.310	0.369
	(0.243)	(0.283)	(0.362)	(0.268)	(0.315)	(0.396)
Pseudo R-squared	0.257	0.261	0.285	0.261	0.267	0.291
Risk attitudes	YES	YES	YES	YES	YES	YES
Big five personality	NO	YES	YES	NO	YES	YES
Demographic ctrls.	NO	NO	YES	NO	NO	YES

Logistic regressions of a dummy variable indicating if someone is fully insured (both years) in a specific period on a dummy indicating if the participant is a New Zealand resident, a categorical variable indicating what insurance choice a participant made in the first period (0=no insurance; 0.5=insured one of two years; 1=insured both years annually; 2=insured both years with biennial insurance), and a dummy indicating if the participant experienced a disaster in the first period. Risk attitudes include a CRRA parameter, a principal component of two willingness to take risks measures, and locus of control. Big five personality traits are openness to experiences, conscientiousness, extraversion, agreeableness, and neuroticism. Demographic controls include a categorical variable of income (standardized and squared), a female dummy, a categorical age variable (standardized and squared), a white ethnicity dummy, a categorical variable of years of education (standardized and squared), a student and unemployed dummy, a dummy indicating if someone is in a partnership, one dummy indicating whether someone is a homeowner, one dummy indicating if someone lives in a household that is larger than 2 people. Robust standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.3 OLS regressions of the number of times insured overall

We also ran ordinary least squares (OLS) regressions of a variable that measures how often a participant chose to insure overall out of 12 times. Recall that the experiment was done repeatedly for six periods, each accounting for two years, i.e., a total of 12 years. Like the Logistic regressions above, we do not find the coefficient on the New Zealand dummy to be significant anymore. It seems that New Zealanders prefer long-term insurance but did not insure more often than Australians overall. In the models here, our measures of risk attitudes are also not significant anymore, which indicates that model specifications may be important considerations in studies of insurance choices. Alternatively, it may be the case that risk attitudes only predict the choice in the first period, and all other periods' choices are primarily predicted by the first period's insurance choice. We test this in our next robustness checks.

Table E-4. OLS robustness checks.

	Whole sample, N=413			**Excluding people with missing demographics, N=370**		
	(1)	(2)	(3)	(4)	(5)	(6)
New Zealand	0.241	0.305	0.355	0.164	0.205	0.322
	(0.300)	(0.317)	(0.321)	(0.315)	(0.334)	(0.329)
Insurance choice in first period	3.197***	3.214***	3.164***	3.199***	3.228***	3.185***
	(0.171)	(0.172)	(0.182)	(0.185)	(0.186)	(0.187)
Disaster in first period	-0.475	-0.531	-0.469	-0.320	-0.349	-0.323
	(0.361)	(0.364)	(0.380)	(0.385)	(0.390)	(0.400)
subsidized	0.182	0.114	0.134	0.230	0.051	0.093
	(0.301)	(0.313)	(0.327)	(0.316)	(0.327)	(0.338)
R-squared	0.508	0.514	0.551	0.501	0.507	0.542
Risk attitudes	YES	YES	YES	YES	YES	YES
Big five personality	NO	YES	YES	NO	YES	YES
Demographic ctrls.	NO	NO	YES	NO	NO	YES

OLS regressions of a dummy variable measuring the number of years someone insured in total (out of 12 years) on a dummy indicating if the participant is a New Zealand resident, a categorical variable indicating what insurance choice a participant made in the first period (0=no insurance; 0.5=insured one of two years; 1=insured both years annually; 2=insured both years with biennial insurance), and a dummy indicating if the participant experienced a disaster in the first period. Risk attitudes include a CRRA parameter, a principal component of two willingness to take risks measures, and locus of control. Big five personality traits are openness to experiences, conscientiousness, extraversion, agreeableness, and neuroticism. Demographic controls include a categorical variable of income (standardized and squared), a female dummy, a categorical age variable (standardized and squared), a white ethnicity dummy, a categorical variable of years of education (standardized and squared), a student and unemployed dummy, a dummy indicating if someone is in a partnership, one dummy indicating whether someone is a homeowner, one dummy indicating if someone lives in a household that is larger than 2 people. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.4 Multinomial logistic regressions

E.4.1 Subsidized experiment

Table E-5. Multinomial logistic regression results (subsidized experiment).

Excluding people with missing demographics, N=236 (1,416 obs.)			
Panel A.	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	0.150 (0.327)	0.163 (0.352)	0.610** (0.307)
Insured previous period	0.764*** (0.241)	2.246*** (0.244)	2.313*** (0.245)
Disaster previous period	-0.326 (0.249)	-0.688*** (0.246)	-0.539** (0.225)
Pseudo R-squared		0.077	
Risk attitudes incl. LoC		No	
Big five personality traits		No	
Demographics		No	
Panel B. (+ risk attitudes)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	0.259 (0.329)	0.372 (0.363)	0.769** (0.315)
Insured previous period	0.742*** (0.244)	2.181*** (0.249)	2.273*** (0.247)
Disaster previous period	-0.335 (0.256)	-0.690*** (0.243)	-0.546** (0.221)
Pseudo R-squared		0.088	
Risk attitudes incl. LoC		Chi^2-test≈18.82, p-value ≤ 0.027	
Big five personality traits		No	
Demographics		No	
Panel C. (+ big five traits)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	0.290 (0.366)	0.486 (0.415)	1.109*** (0.363)
Insured previous period	0.736*** (0.243)	2.164*** (0.249)	2.234*** (0.246)
Disaster previous period	-0.339 (0.259)	-0.713*** (0.243)	-0.572** (0.222)
Pseudo R-squared		0.097	
Risk attitudes incl. LoC		Chi^2-test≈21.11, p-value ≤ 0.012	
Big five personality traits		Chi^2-test≈13.11, p-value ≤ 0.593	
Demographics		No	
Panel D. (+ demographics)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	0.279 (0.362)	0.451 (0.430)	1.074*** (0.378)
Insured previous period	0.729*** (0.251)	2.052*** (0.253)	2.138*** (0.251)
Disaster previous period	-0.359 (0.268)	-0.700*** (0.251)	-0.535** (0.230)
Pseudo R-squared		0.120	
Risk attitudes incl. LoC		Chi^2-test≈30.09, p-value ≤ 0.000	
Big five personality traits		Chi^2-test≈12.62, p-value ≤ 0. 632	
Demographics		Chi^2-test≈43.07, p-value ≤ 0.301	

This table shows results from a multinomial regression analysis of the insurance choices made by participants in the subsidized experiment. 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). Insured/disaster previous period are dummy indicators, each showing whether the participant experienced a disaster and whether the participant bought insurance in the previous period. New Zealand is a dummy indicating whether the participant is a New Zealand resident. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.4.2 Actuarially fair experiment

Table E-6. Multinomial logistic regression results (act. fair experiment).

Whole sample, N=149 (894 obs.)			
Panel A.	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.609 (0.416)	0.089 (0.380)	0.187 (0.332)
Insured previous period	1.285*** (0.338)	3.781*** (0.419)	3.065*** (0.368)
Disaster previous period	-0.009 (0.371)	-0.445 (0.370)	-0.382 (0.351)
Pseudo R-squared		0.126	
Risk attitudes incl. LoC		No	
Big five personality traits		No	
Demographics		No	
Panel B. (+ risk attitudes)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.554 (0.427)	0.145 (0.390)	0.278 (0.343)
Insured previous period	1.270*** (0.346)	3.712*** (0.424)	2.978*** (0.378)
Disaster previous period	0.083 (0.405)	-0.430 (0.388)	-0.372 (0.376)
Pseudo R-squared		0.140	
Risk attitudes incl. LoC		Chi^2-test≈21.70, p-value ≤ 0.010	
Big five personality traits		No	
Demographics		No	
Panel C. (+ big five traits)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.691* (0.411)	0.021 (0.391)	0.369 (0.349)
Insured previous period	1.263*** (0.359)	3.667*** (0.440)	2.777*** (0.388)
Disaster previous period	0.039 (0.419)	-0.495 (0.397)	-0.339 (0.386)
Pseudo R-squared		0.180	
Risk attitudes incl. LoC		Chi^2-test≈17.94, p-value ≤ 0.036	
Big five personality traits		Chi^2-test≈47.00, p-value ≤ 0.000	
Demographics		No	
Panel D. (+ demographics)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.435 (0.422)	0.241 (0.436)	0.648 (0.404)
Insured previous period	1.170*** (0.373)	3.442*** (0.447)	2.378*** (0.381)
Disaster previous period	0.043 (0.431)	-0.461 (0.411)	-0.418 (0.398)
Pseudo R-squared		0.232	
Risk attitudes incl. LoC		Chi^2-test≈14.98, p-value ≤ 0.092	
Big five personality traits		Chi^2-test≈45.08, p-value ≤ 0. 000	
Demographics		Chi^2-test≈329.11, p-value ≤ 0.000	

This table shows results from a multinomial regression analysis of the insurance choices made by participants in the actuarially fair experiment. 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). Insured/disaster previous period are dummy indicators, each showing whether the participant experienced a disaster and whether the participant bought insurance in the previous period. New Zealand is a dummy indicating whether the participant is a New Zealand resident. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.4.3 For period 1

Our robustness analysis above indicates that risk attitudes are not as important predictors of insurance choices as is predicted by economic theory. We noted, however, that some of the previous robustness analyses might hide the importance of risk attitudes. This could be similar to the loss of information from an aggregate measure (a dummy variable) that indicates if someone insured both years in a period loses some of the variation in insurance decisions, which lead to an insignificant coefficient on the New Zealand dummy in the previous analyses. We therefore regressed the insurance choices again using a multinomial logistic regression on the typical covariates but only for period 1 in the specification shown in Table A-12. Below, we ran the same models for periods 2 through 6. First, we see that risk attitudes are significant predictors of the insurance choices in period 1 again and we also see here that New Zealanders were more likely to insure with a biennial contract and slightly less likely to insure only one of the two years. These results indicate that New Zealanders were overall more likely to have both periods insured, even though the coefficients on Alternative 0,1/1,0 and Alternative 1,1 are not significant.

Table E-7. Multinomial logistic robustness checks (first period only).

Both experiments combined			
Panel A. (+ risk attitudes)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.444 (0.486)	0.406 (0.384)	0.667** (0.327)
Subsidized	-0.146 (0.438)	-0.225 (0.369)	-0.034 (0.318)
Pseudo R-squared		0.046	
Risk attitudes incl. LoC		Chi^2-test≈28.81, p-value ≤ 0.001	
Big five personality traits		No	
Demographics		No	
Panel B. (+ big five traits)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.608 (0.497)	0.386 (0.395)	0.640* (0.337)
Subsidized	0.087 (0.474)	-0.055 (0.413)	0.171 (0.358)
Pseudo R-squared		0.054 (Δ Pseudo R^2≈0.008)	
Risk attitudes incl. LoC		Chi^2-test≈22.65, p-value ≤ 0.007	
Big five personality traits		Chi^2-test≈7.73, p-value ≤ 0.934	
Demographics		No	
Panel C. (+ demographics)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.342 (0.541)	0.401 (0.437)	0.830** (0.371)
Subsidized	-0.255 (0.510)	-0.348 (0.464)	-0.088 (0.409)
Pseudo R-squared		0.143 (Δ Pseudo R^2≈ 0.089)	
Risk attitudes incl. LoC		Chi^2-test≈17.36, p-value ≤ 0.043	
Big five personality traits		Chi^2-test≈10.99, p-value ≤ 0.753	
Demographics		Chi^2-test≈2,592.14, p-value ≤ 0.000	

This table shows results from a multinomial regression analysis of the insurance choices made by all participants in both experiments in the first period. 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). *New Zealand* is a dummy indicating whether the participant is a New

Zealand resident. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

E.4.4 For period > 1

To find out how predictive risk attitudes are of the choices in periods 2 through 6, and whether they have any predictive capacity beyond the first period's choice, we ran multinomial logistic regressions of the insurance choices made in all other periods but the first on the typical set of covariates, as shown below. We find that a self-reported willingness to take risks still predicts choices, but overall, the significances become much weaker, locus of control is no longer significant. These results combined with the results above show that the first period's choice is the strongest predictor of insurance choices and that risk attitudes are determinants of the first period's choice. Hence, risk attitudes mediate the effect of the first period's choice on the following periods' choices.

Table E-8. Multinomial logistic robustness checks (2nd through 6th period).

Both experiments combined			
Panel A. (+ risk attitudes)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.028 (0.273)	0.246 (0.276)	0.469* (0.244)
Insured previous period	1.150*** (0.210)	2.642*** (0.205)	2.530*** (0.201)
Disaster previous period	-0.357* (0.213)	-0.683*** (0.199)	-0.544*** (0.183)
Subsidized	0.414 (0.268)	0.037 (0.265)	0.337 (0.232)
Pseudo R-squared		0.098	
Risk attitudes incl. LoC		Chi^2-test≈15.97, p-value ≤ 0.067	
Big five personality traits		No	
Demographics		No	
Panel B. (+ big five traits)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.142 (0.280)	0.223 (0.290)	0.500* (0.255)
Insured previous period	1.159*** (0.213)	2.651*** (0.206)	2.518*** (0.201)
Disaster previous period	-0.335 (0.213)	-0.685*** (0.200)	-0.529*** (0.182)
Subsidized	0.364 (0.286)	-0.010 (0.285)	0.347 (0.243)
Pseudo R-squared		0.104 (Δ Pseudo R^2≈0.006)	
Risk attitudes incl. LoC		Chi^2-test≈13.56, p-value ≤ 0.139	
Big five personality traits		Chi^2-test≈14.04, p-value ≤ 0.523	
Demographics		No	
Panel C. (+ demographics)	Alternative 0,1 or 1,0	Alternative 1,1	Alternative 2,2
New Zealand	-0.111 (0.276)	0.265 (0.297)	0.616** (0.261)
Insured previous period	1.168*** (0.219)	2.578*** (0.213)	2.422*** (0.208)
Disaster previous period	-0.395* (0.217)	-0.685*** (0.203)	-0.538*** (0.184)
Subsidized	0.328 (0.302)	-0.022 (0.304)	0.336 (0.264)
Pseudo R-squared		0.133 (Δ Pseudo R^2≈ 0.029)	
Risk attitudes incl. LoC		Chi^2-test≈14.66, p-value ≤ 0.101	
Big five personality traits		Chi^2-test≈14.68, p-value ≤ 0.475	
Demographics		Chi^2-test≈258.52, p-value ≤ 0.000	

This table shows results from a multinomial regression analysis of the insurance choices made by all participants in both experiments in all periods beyond the first period. 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). *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. Clustered standard errors are shown in parentheses and statistical significance is denoted by *** p-value < 0.01, ** p-value < 0.05 and * p-value <0.1.

Appendix F. Experimental instructions (Screenshots)

F.1 Participant info and consent

After you read the following statements, please click on the box with the blue text at the end of the consent statements if you wish to participate in our study and agree to the information and terms below.
If you don't agree with the terms laid out here and don't give your consent, you won't be able to further participate in this study. Your consent will be held on record for five years.

By clicking on the consent box below and participating in this study, **I confirm that**

- **I have read the information sheet fully understand and agree to it**;
- **I participate** in this study **voluntarily** at my own free will;
- **I am at least 18 years old**;
- **I understand that I can only ask questions by contacting the principal investigator before or after, but not during my participation**;
- **I understand** that **this study takes on average about 50 minutes** and **I have the time to answer** all of the questions **alone in one session, without any distractions or help from other people or reading material**;
- **while I'm participating alone, I also won't do anything else** (e.g., using the internet browser, apps or my phone) that may distract me or draw my attention away from the study.
  This includes activities such as **listening to music, watching videos, accessing social media websites or using my mobile phone for any other purpose.**
  (Please turn off and close all devices and applications that you don't need for this study now);
- **I understand** that **this study has received ethics approval and** that **this study is conducted in accordance with the ethics criteria** as laid out in the committee's ethics approval process;
- **all information I provide about myself that may identify me will be kept secret and strictly confidential** – only de-identified and aggregated data will be used in academic reports or presentations;
- **the researchers may use the de-identified information I provide for publications of research in academic journals and at academic conferences, presentations, and seminars.**
  The finalized and de-identified data will be saved on an accessible website for everyone, **once the report is published.**;
  Before publication, **the researchers will keep the data secured** on their institution's server to which only the researchers will have access;
- **I understand that I may stop my participation and withdraw from this study by contacting the researchers** at any point in time and **all information that I provided will then be destroyed**;
- **I will not discuss this study with anyone** and will treat the study's aims, goals, ideas, tasks shown on the screen, and all other text pertinent to this study confidential;
- **I will follow the instructions as shown on the screen** here and in the study's tasks;
- **I will provide answers to the best of my knowledge without any deception**, i.e. I will provide truthful and as accurate information as possible;

Please click on the box below to confirm you read and understood all the terms and instructions laid out above, and that you agree to follow these terms and instructions and consent to them entirely.

I read and understood all the terms and instructions laid out above.
I agree to follow these terms and instructions and I consent to them entirely.

To be able to continue, please enter your Prolific ID in the text field below.
Your Prolific ID has 24 alphanumeric characters, and may include both numbers and letters.

1sd3f13sd1f351we53131212

remaining characters 0

Begin the study.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

F.2 Instructions: lottery choices

Thank you very much for participating in this study. **Your answers are** very **important** for research and will be helpful to communicate improvements of regulations in property insurance.

In this first task, you will **choose between 60 pairs of payoffs** with varying payments and chances of winning, similar to the one shown below.
Each pair of payoffs is **shown individually**.
For each pair, you should **choose the one** (left or right) **you would prefer to receive for real.**
At the end of the study, **you will be paid according to one** of the 60 payoffs you chose.
You will virtually roll dice to find out according to which of the payoff pairs and payment areas you will be paid.

Therefore, you should think carefully about each choice so that you get paid for your preferred pie chart.
Here is an **example**:

The colored pie areas illustrate the chance with which your virtual roll of the dice can land on a specific number ranging clockwise from 1 to 100.

In this example:

The ***left* pie chart**

- **40% chance** in **blue** area: pays nothing **($0) if** the **number** you roll virtually **ranges from 1 to 40**,
- **40% chance** in **orange** area: pays five dollars **($5) if** the **number ranges from 41 to 80**; and
- **20% chance** in **green** area: pays ten dollars **($10) if** the number ranges from **81 to 100**.

The ***right* pie chart**

- **30% chance** in **blue** area: pays five dollars **($5) if** the **number** you roll virtually **ranges from 1 to 30**,
- **50% chance** in **orange** area: pays twelve-fifty **($12.50) if** the **number ranges from 31 to 80**; and
- **20% chance** in **green** area: pays ten dollars **($10) if** the number ranges from **81 to 100**.

Continue.

(Please click to see the next part. You may need to scroll all the way up to see the entire instructions.)

You could also get a **pair of payoffs** in which one or more of the pie areas will give you the **chance to receive Double or Nothing.**

Here is an **example** of a pair of payoffs with a Double or Nothing option:

Note, the *left* pie chart is exactly the same as the pie charts before, without a Double or Nothing option.
But the *right* pie chart in the example above may pay Double or Nothing.

In this example:

The ***left* pie chart**

- **20% chance** in **blue** area: pays three-fifty **($3.50) if** the **number** you roll virtually **ranges from 1 to 20**;
- **50% chance** in **orange** area: pays five dollars **($5) if** the **number ranges from 21 to 70**; and
- **30% chance** in **green** area pays ten dollars **($10) if** the number ranges from **71 to 100**.

The ***right* pie chart**

- **50% chance** in **blue** area: pays nothing **($0) if** the **number** you roll virtually **ranges from 1 to 50**;
- **50% chance** in **orange** area: pays ten dollars **($10) if** the **number ranges from 51 to 100** (before the flip of the coin).

If the number ranges from 51 to 100 (50% chance) and therefore lands on **orange**, you **additionally** get to virtually **flip a coin**.The virtual coin has a 50-50 chance to land on Heads or Tails. **If** it lands on **Heads**, you will be paid **double** ($20), and **if** it lands on **Tails** you get **nothing** ($0).

Therefore, a Double or Nothing option has two sequential random draws that determine your payment from this task (a first draw with two 10-sided dice and a second draw with a coin toss).
The payments shown for each pie area refer to the amounts *before* the virtual coin flip.

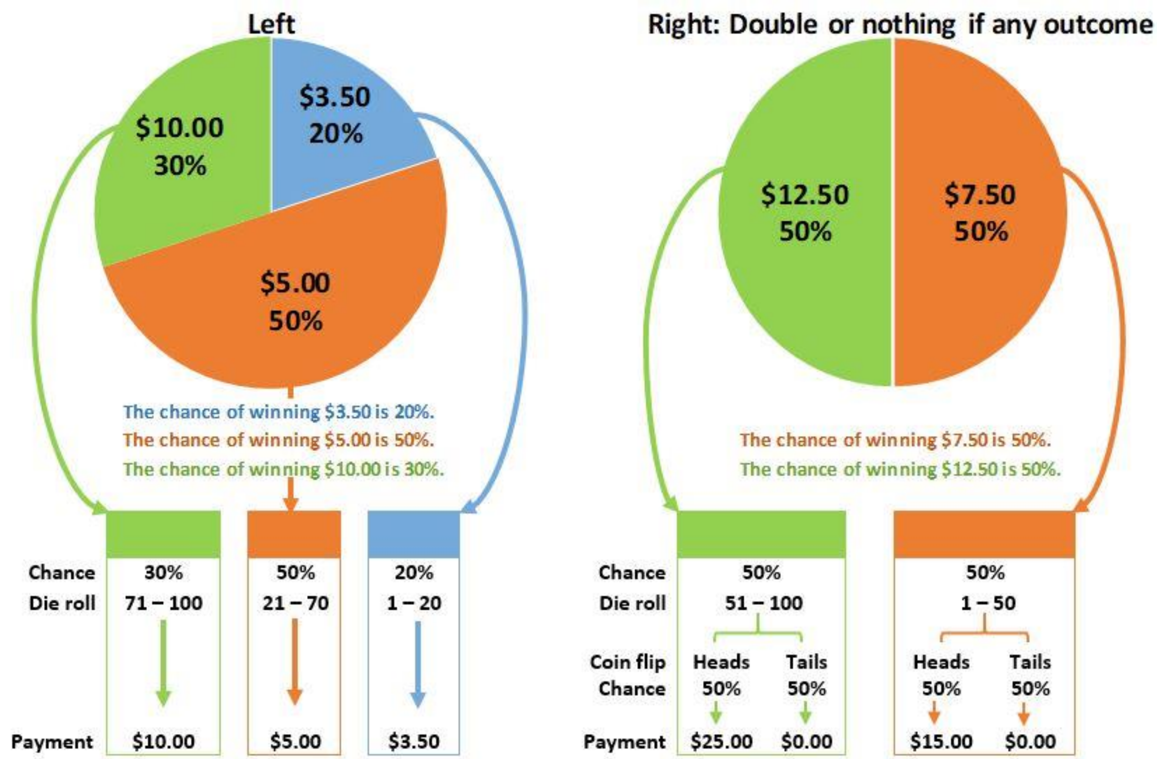
You might also see a **pie chart that pays double or nothing for any outcome**, as shown below.
In this case, if you chose the (here) right pie chart, you will get to **flip a coin for both the orange and green area**. So each of the payoffs shown can be either doubled or reduced to zero.

After you made the last choice, a continue button will appear that will let you proceed to the next part of the study.

Continue.

(Please click to see the next part. You may need to scroll all the way up to see the entire instructions.)

Here is a graphical illustration of possible outcomes shown for one of the previous payoff pairs.



To summarize, you will **make 60 choices** between two **payoff pairs** and **your payment is determined by four things**:

1. **The payoff pair** that is **selected using** the virtual **throw of dice**;
2. ***your choice* of the pie chart (left or right) for the selected payoff pair**;
3. **a number from 1 to 100 drawn by the roll of dice that determines the area** (e.g. blue, orange, or green) **of your preferred pie chart**; and
4. **the outcome of a coin toss if** the pie chart area is of the **"Double or Nothing"** type (the payment will be doubled if you flip Heads and zero if you flip Tails).

The **payoff pair** you choose **is a matter of personal preference**. Please **work alone without any distractions, and make your choices** by thinking **carefully** about each payoff pair. The payment for the pie chart chosen from this task will be added to your participation fee at the end of this study.

F.3 Measurement of risk attitudes

Please choose your preferred pie chart (left or right) of the displayed pair of risky payoffs.
Your choice should reflect according to which of the two pie charts you would prefer to be paid for real.
Decide carefully as *you will be paid according to one of your chosen pie charts* at the end of the study.

Additionally, please indicate on a scale from 0 (“not at all willing to take risks”) to 10 (“fully willing to take risks”) how willing you are to take risks or if you rather try to avoid taking risks in general and in financial matters.

In general, I am...

not at all willing to take risks **0** **10** fully willing to take risks

In financial matters, I am...

not at all willing to take financial risks **0** **10** fully willing to take financial risks

Finally, please indicate on a scale from 0 (“not at all patient”) to 10 (“very patient”) how patient you are generally. Would you describe yourself as an impatient or a patient person?

Generally, I am...

not at all patient **0** **10** very patient

Thanks for your participation so far. Please continue to the next part of this study.

Continue.

(When you pressed the button, please be patient. It may take a while for the system to load the next screen.)

F.4 Personality traits: big five

A few examples of the big five personality statements and selection buttons.

Please describe yourself as you generally are now, not as you wish to be in the future. **Describe yourself as you honestly see yourself currently.**
Please indicate for each statement below on a scale from 1 to 7 whether it is (1) inaccurate, (4) neither accurate nor inaccurate, or rather (7) accurate as description of you.

Note, the statement is printed in bold and the scale for each statement is right below.

I tend to keep in the background.

Strongly disagree **1** ○ ○ ○ ○ ○ ○ ○ **7** Strongly agree

I am creative and imaginative.

Strongly disagree **1** ○ ○ ○ ○ ○ ○ ○ **7** Strongly agree

I quickly understand abstract and new ideas.

Strongly disagree **1** ○ ○ ○ ○ ○ ○ ○ **7** Strongly agree

I sympathize with others' feelings.

Strongly disagree **1** ○ ○ ○ ○ ○ ○ ○ **7** Strongly agree

F.5 Personality: locus of control

A few examples of the locus of control statements and selection buttons.

The following statements apply to different attitudes towards life and the future. **Please indicate, by selecting one of the circles for each statement, how much you agree or disagree with each of the statements.** You can choose a range on a scale from 1 to 7 whether you (1) completely disagree or (7) completely agree with the statement. Choose what you think generally applies to you the best concerning your own life as it is, not as you wish it to be in the future.

Note, the statement is printed in bold and the scale for each statement is right below.

I feel like I have the capacity to overcome the problems I face in my life.

Strongly disagree **1** **7** Strongly agree

Self-determination and hard work are the main ingredients to achieve what I want.

Strongly disagree **1** **7** Strongly agree

I am the main controlling influence on the direction of my life.

Strongly disagree **1** **7** Strongly agree

I have little control over the outcomes of my life.

Strongly disagree **1** **7** Strongly agree

F.6 Instructions: insurance decisions

Please **put yourself into the position of a house owner** whose house is worth 500000 right now.
Your task is to decide whether **to insure that house or not**.
You can **choose** to **insure for one year**, **insure for two years**, or **not to insure** at all.
The **chance of a disaster** occurring **in any year** is **10%**. So, you should expect to experience a disaster in one out of ten years.
The **insurance** protects you from a disaster that would damage your house by half of its *current* value and **covers losses up to half of your *revealed* house value**.
At the end of the study, you will be paid according to the final wealth of one of the period's here, divided by 25000. For example, a final wealth of 250000 would result in a payment of $10.00 *for this task*.

You will make a total of 12 years' decisions, split into 6 periods.
Therefore, every period accounts for two years of insurance decisions.
After the first year of every period, **your house value may randomly go up by 10%** or stay the same.
If your **house value increases**, your **insurance premium may increase depending on your choices** in this task, as explained below.

Think of this task as a decision to protect your house from a natural disaster (earthquake, storm, or flood) that would damage your house by half of its current house value (e.g. 250000). This would be reimbursed if you decided to buy insurance. House prices often rise, and this can happen here, too. The more expensive a house is, the more expensive it is to insure the house. If you buy insurance and nothing happens (this has a 90% chance), you will not get the paid premium back.

Continue.

(Please click to see the next part. You may need to scroll up.)

The process of one period

Beginning of each period:
You **begin each period** with an **account balance of 55000**, which you can use to pay for the insurance of your **house** with an initial **value of 500000**.

Year 1 decisions:

In the first year, **insurance costs**
- a premium of **25000 for** an **annual** (one-year) contract; and
- a premium of **26250 per year for** a **biennial** (two-year) contract.

Therefore, your choices are

Insurance choices

Don't insure the house	**Insure** the house for **one year**	**Insure** for **two years**
(no premium cost)	(cost of premium 25,000)	(cost of premium 26,250 yearly)

<u>**Year 2 decisions:**</u>

A. House price assessment

After the first year, there is a **50% chance** that your **house value increases to 550000**.
However, you will not be informed automatically if the house value went up or stayed the same.

There are **two options** for you **to choose from, depending on your first year's insurance decision**:

• **Option 1: If** you **insured** for **two years**, **you will not** get the option to **have** your **house value assessed** after the first year. The **biennial insurance will cover any potential losses entirely in both years**.

• **Option 2: If** you **insured** the house for **one year *or* did not insure** the house, you can **decide to have** your **house value assessed** for a **cost** of **1250**. Your house value will then be adjusted to reflect the current true value.

The house value assessment is the same as paying an expert to evaluate your house in the current economic and political circumstances.

There are **three possible outcomes of** your **assessment** decision:

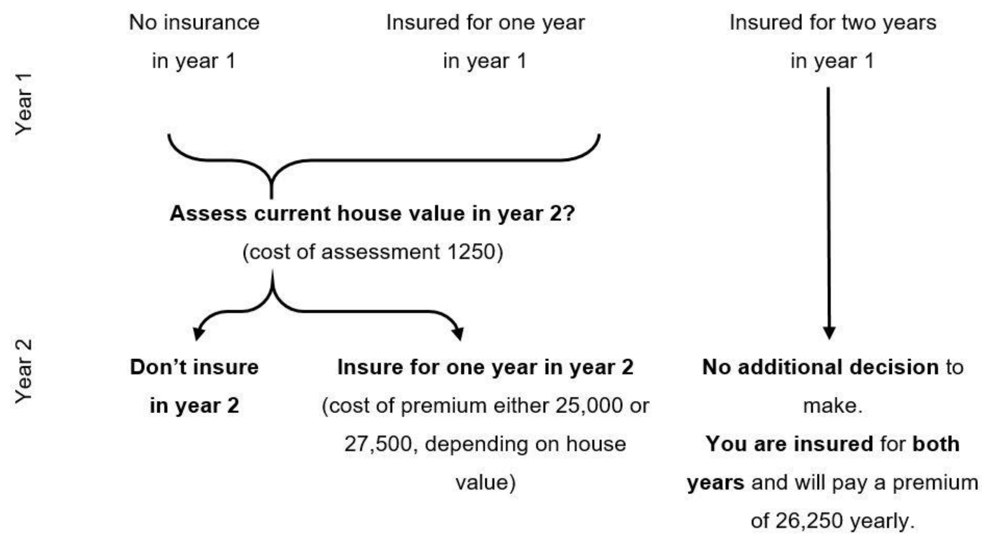
• **Outcome 1:** the **house value increased** (new home value = 550000), **insurance** for **one year** will **cost 27500** (an increase of 10%, equivalent to the house value increase) **for full coverage** (275000) in the second year.

• **Outcome 2:** the **house value** stayed the **same** (home value = 500000), **insurance** for **one year** will **cost 25000** (i.e., the same premium as in the first year) **for full coverage** (250000) in the second year.

• **Outcome 3:** you **did not assess** the **house's value**, **insurance** for **one year** will **cost 25000 for coverage** of half **of your initial house's value** (250000), irrespective of whether the house value actually went up or not.

B. Second year's insurance decision

If you **did not insure with a biennial contract**, you will **once again** get to **decide whether to insure the house** for the **second year**. Your **insurance premium** for another year of insurance **depends on** your decision **whether** to **assess the house** value or not **and** whether the **current house value** actually increased (premium of 27500) or stayed the same (premium of 25000).
Therefore, depending on your first year's choices, **your choices in year two** are:



Each period, **you start** once again **with** an **account balance** of **55000** and a **house value** of **500000 and** get to **make insurance decisions** again. **The periods are independent** of each other, so **your choices in one period do not affect the next period**.

In total, you will make these two-yearly decisions 6 times **(6 periods)**. Therefore, you can make **up to 12 yearly insurance decisions**.

At the end of this study, you will virtually **throw a die to find out according to which of the 6 periods you will be paid**. Since this depends on random chance, **you should treat each insurance period and decision as if it were the one according to which you will ultimately be paid out.**

Click on the button below to do one test-run. Your choices here have no effect on your payment or what happens in the actual insurance decisions task.

Continue.

(Please click to see the next part. You may need to scroll up.)

F.6.1 Example

Here is an **example** of what your decisions will look like on the screen. **Your choices *on this screen* do not affect any of your later outcomes. The buttons are mainly for illustrative purposes** so that you can familiarize yourself with them. The text here, however, appears as it will in the actual insurance choice task on the following screens.

First Year Insurance Decision

Your house is worth 500000.
Your account balance is 55000.

You can decide whether or not to insure your house against a potential disaster causing 250000 damage to the house.
Such a disaster occurs with a probability of 10%.

Do not insure.

Insure for one year at a premium of 25000.

Insure for two years at a yearly premium of 26250.

(Please select one of the options to continue.)

Assuming you bought insurance in the first year for 25000, the displayed messages could look as follows:

The first year was quiet, there was no disaster since the randomly drawn number was 46.
You purchased insurance for one year.

Your house value is 500000.
Your account balance is 30000.

Second Year Insurance Decision

At this time it's uncertain what the damage caused by a disaster in the second year would be.
Would you like to know whether your house's value increased last year or not?
If yes, you may pay 1250 from your account to get your house value assessed.

No, don't asses my house value.

Yes, assess my house value for 1250.

(Please select one of the options to continue.)

Second Year Insurance Decision

On the final screen, you will see your current account balance and house value again.
Here's an example, following the previous assumptions and additionally assuming that you decided to have the house value assessed and that it went up by 10%:

Your house value was assessed at 550000 last year.
Your account balance is 28750.

Once again, you can decide whether or not to insure your house against a potential disaster that occurs with a probability of 10%, as shown in the pie chart below.
Please choose from the two options carefully considering you will ultimately be paid according to your choices.

In the example here, the red portion represents the 10% chance that your house may be damaged by half of its current value. This means your house value decreases to 275000 in the second year. The green portion of the pie corresponds to the 90% chance that your house will not be damaged and its value remains at 550000. Of course, the house will only be damaged if it is not insured.

Purchase insurance for one year at a premium of 27500.

Do not insure.

(Please select one of the options to continue.)

To recap: The example assumed you bought insurance in the first year for 25000 and did not experience a disaster. Additionally, you chose to assess the current house value for a cost of 1250. In this case, the house value went up by 10%, hence it is valued at 550000. Since the house value went up, insurance for one year now costs 27500. Let's assume you also bought insurance in the second year for 27500, so that the current account balance is 1250.

With the numbers above, and assuming this example's period would be the one according to which you get paid, you would receive a payment of $ 22.00 *from this task* (rounded to the nearest ten cents). That is your final wealth from this period (551250) divided by 25000.

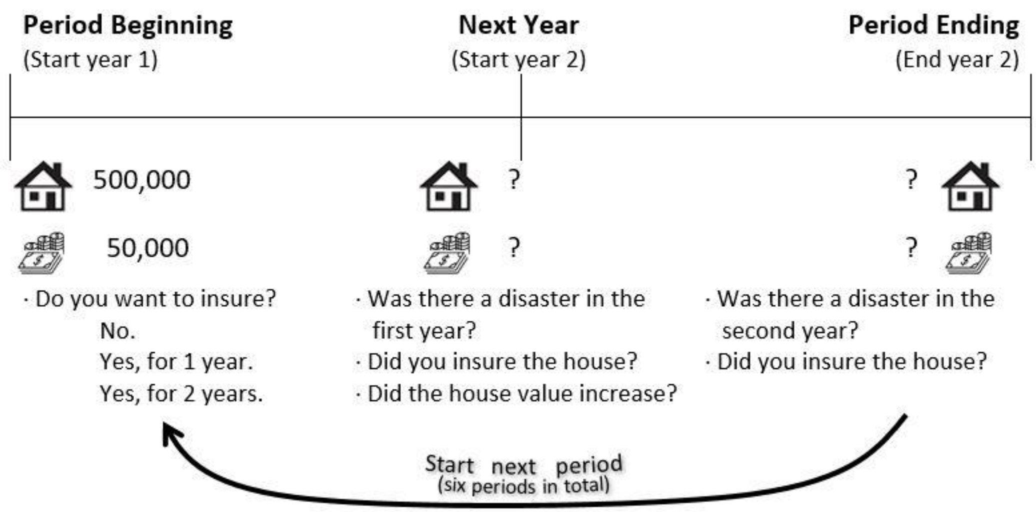
To summarize:

There are 6 periods and each period accounts for decisions of two years. Note that years and periods are all independent of each other. Therefore, you will make up to 12 insurance decisions in this task. At the start of every period, you receive an account balance of 55000 and have a house valued at 500000.

You can purchase insurance that covers a possible loss from a disaster and have three options to choose from:

- no insurance;
- insurance for one year for a premium of 25000, or
- insurance for two years for a premium of 26250 fixed for both years.

A disaster may occur each year with a probability of 10%, leading to a loss of half of your current house value.
This graph illustrates the insurance decisions' sequence.



You will be paid randomly according to the final wealth of one of the periods, depending on the occurrence of a disaster and the choices you made each period. Each period refers to decisions for two years. So you should **think carefully about** how much **the insurance** is worth to you each year and period.

Your payment from this task is determined by five things:

- the possibility of a disaster damaging your house;
- the decision whether you insured the house or not;
- the cost of the insurance;
- the possibility of an increase in the house value; and
- the randomly selected period for which you will be paid.

Your payment *from this task* is calculated as the final wealth at the end of the randomly selected period, divided by 25000.
For example, if you decided not to insure, your house's value increased by 10% after the first year, and you did not experience any disasters, your maximum payment *from this task* could be $24.20 (that is the sum of the house value of 550000 and your initial account balance of 55000, divided by 25000).
However, if you decided not to insure, your house's value did not increase after the first year, and there was a disaster in both years, you could end up being paid only $2.20 *for this task* (the initial account balance of 55000 divided by 25000).

Which **insurance decisions** you make is **a matter of personal preference**.
Please work alone silently without any distractions and think carefully about your insurance choices.
The payment from this task will be added to the participation fee and your payment from the risky payoff pair task at the end of the study.

F.7 Insurance decisions: first period – two annual contracts, no house assessment

Please **put yourself into the position of a house owner** whose house is worth 500000 right now.
Your task is to decide whether **to insure that house or not**.
You can **choose** to **insure for one year**, **insure for two years**, or **not to insure** at all.
The **insurance** protects you from disasters and **covers losses up to half of your revealed house value**.
The **chance of a disaster** occurring **in any year** is **10%**. So, you should expect to experience a disaster in one out of ten years. A **disaster damages** your **house by half of its current value** if the house is *un*insured.

You will make a total of 12 years' decisions, split into 6 periods.
Therefore, every period accounts for two years of insurance decisions.
After the first year of every period, **your house value may randomly go up by 10%** or stay the same.
If your **house value increases**, your **insurance premium may increase depending on your choices** in this task.

Think of this task as a real-life decision to protect your house from a natural disaster (earthquake, storm, or flood) that would cause damage to your house equal to half of your current house value and which would be covered entirely if you decided to buy insurance. House prices often rise, and this can happen here, too. The more expensive a house is, the more expensive it is to insure the house. If you bought insurance and nothing happened (90% chance), you won't get the premium paid back.

You are in year 1 of period 1 out of a total of 6 periods.
Your account balance is 55000.
Your house value is 500000.

First Year Insurance Decision

Please decide whether or not to insure your house against a potential disaster causing 250000 damage to the house. Such a disaster occurs with a probability of 10%, as shown in the graph below.

Please choose from the three options and consider carefully since you will be paid according to your choices.

Insure for two years for a yearly premium of 26250.

Insure for one year for a premium of 25000.

Do not insure.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

You are in year 1 of period 1 out of a total of 6 periods.
Your account balance is 30000.
Your house value is 500000.

You purchased insurance for one year.

Proceed to the next year's decisions.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

You are in year 2 of period 1 out of a total of 6 periods.
Your account balance is 30000.
Your house was valued 500000 last year.

You purchased insurance in the first year and there was no disaster (the randomly drawn number is 29).

At this time it's uncertain what the damage caused by a disaster in the second year would be.
Would you like to know whether the house value increased last year or not?
If you do, you may pay 1250 to get your house value assessed.

No, don't asses my house value.

Yes, assess my house value for 1250.

Continue.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

You are in year 2 of period 1 out of a total of 6 periods.
Your account balance is 30000.
Your house was valued 500000 last year.

Second Year Insurance Decision
Once again, you can decide whether or not to insure your house against a potential disaster causing damage to the house. Such a disaster occurs with a probability of 10% as shown below.

Please choose from the two options and consider carefully since you will be paid according to your choices.

Purchase insurance for one year at a premium of 25000.
Do not purchase any insurance.

F.8 Insurance decisions: second period – one biennial contract, no further choices

You are in year 1 of period 2 out of a total of 6 periods.
Your account balance is 55000.
Your house value is 500000.

You can purchase insurance to protect you from a disaster that has a 10% chance to happen.
The available options are:

- *no* insurance for the following year,
- insurance for the following *year* (annual contract), or
- insurance for the following *two years* (biennial contract).

After the first year, the house price value has a 50% chance to increase by 10%. This means your house value after this year may increase by 10% randomly. You won't be notified automatically whether the house value changed but you may pay 1250 for an assessment. If you learn your house value increased, the premium for a single year's insurance for the second year will also increase.

First Year Insurance Decision

Please decide whether or not to insure your house against a potential disaster causing 250000 damage to the house. Such a disaster occurs with a probability of 10%, as shown in the graph below.

Please choose from the three options and consider carefully since you will be paid according to your choices.

Insure for two years for a yearly premium of 26250.

Insure for one year for a premium of 25000.

Do not insure.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

You purchased insurance for two years.

Proceed to the next year's decisions.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)

You are in year 2 of period 2 out of a total of 6 periods.
Your account balance is 2500.
Your house was valued 500000 last year.

You purchased insurance in the first year and there was no disaster (the randomly drawn number is 68).

Second Year Insurance Decision

You purchased insurance for two years so you are protected from a potential disaster in the next year. Such a disaster occurs with a probability of 10%.
Since you already are insured, there are no decisions left for you to make at this point.

The second year was quiet, there was no disaster (the randomly drawn number is 19).

Proceed to the next period.

(When you pressed the button, please be patient. It will take a while for the system to load the next screen.)