Chapter 8

Peer effects in catastrophic risk insurance take-up

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8.1 INTRODUCTION

The economic consequences of catastrophic events have become more severe in recent years (Michel-Kerjan & Kunreuther, 2011). One major reason is accumulation of inhabitants and/or capital in many vulnerable areas; it is also understood that climate change leads to more variability and more extremity of weather events (Intergovernmental Panel on Climate Change [IPCC], 2012). Moreover, this tendency is expected to strengthen in the near future (Bevere *et al*. 2011).

To limit the negative consequences of catastrophic events, policies are required that reduce the vulnerability to catastrophic losses and redistribute or shift the exposure to risks to those who are willing and able to bear them. In many developing countries and in disaster-prone areas such as the Caribbean, insufficient supply of insurance (often due to missing or unaffordable reinsurance) is a major

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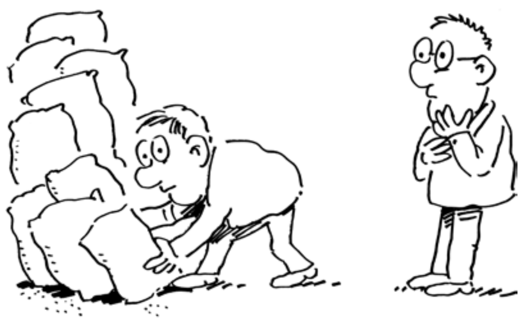
problem (Cavallo & Noy, 2009). Government-sponsored protection and insurance programs have already been installed in many countries. Examples include the National Flood Insurance Program in the USA, the Flood Re plan in the UK and the National Agricultural Insurance Scheme in India. They will increasingly become important if weather events become more severe in the near future.

While the supply of affordable insurance and protection products is crucial, it has been observed that there may also be important problems on the demand side for these products. As Michel-Kerjan and Kunreuther (2011) discuss, take-up of catastrophic insurance, for example flooding insurance, is surprisingly low. Inhabitants of vulnerable areas might be very hesitant to take up even subsidized insurance products. For example, only 40% of residents of the New Orleans parish had flood insurance when hurricane Katrina struck, despite support from the National Flood Insurance Program (Insurance Information Institute, 2005). This is a major puzzle in view of the assumption of predominant risk aversion (typically made at least in the context of Expected Utility Theory) which implies that taking up fair or subsidized insurance products should be very attractive. As discussed in other chapters of this volume, several potential explanations for this apparent puzzle have been suggested and tested. One important dimension of risk perception and insurance choice concerns the social effects caused by the observation of other decision-makers (Kunreuther *et al.* 2009). Most decisions under risks are not made in isolation. In the case of catastrophic risks, peer effects appear particularly important as, by the very nature of such threats, many people are simultaneously affected. Studying these effects might provide us with hints as to why some consumers may be reluctant to take up ever attractively priced insurance against natural disasters and what methods to encourage them to do so are likely to work. In this chapter we discuss possible mechanisms via which peer effects may operate, past empirical evidence trying to verify these mechanisms, and our own recent experimental study. In view of space constraints, only selected elements of the latter are covered, a more complete description (together with transcripts of stimuli used) is available in Krawczyk *et al.* (2017).

8.2 PEER EFFECTS: MECHANISMS



Several types of influences have been discussed under the umbrella term of ‘peer effects’ in insurance take-up choices and decision-making under risk in general, see Table 8.1. First, **observing others’ (not) incurring a loss** can affect one’s willingness to purchase insurance. It is largely an open question though, how own versus others’ experience is weighted. Second, an individual may be affected by **others’ insurance decisions**. Actually, several ways in which such peer effects may operate can be distinguished. Information about the underlying risky event or the insurance contract may be incomplete or it might take a non-trivial amount of time and effort to process it. In such cases, observing what others chose facing the same or a similar decision may provide a valuable hint as to what represents the optimal behavior. For example, if flood insurance is worth it for my neighbors, it may well be worth it for me; this can be called **rational social learning**. There is also ample evidence from sociology and social psychology that most people in most situations are to some degree prone to **conformity**, that is, following others’ choices just to be similar to them, to gain their acceptance and recognition (Cialdini & Goldstein, 2004) and not because these must necessarily be the best choices per se. The most basic form of conformity is **simple imitation**, whereby others’ decisions are followed blindly. While conformity can be modeled as others’ choices directly affecting utility associated with own choices, it can also be the case that others’ *outcomes* affect utility of own outcomes.

Table 8.1 Overview of peer effects.

Mechanism	Description
Learning from others’ losses	Using others’ experience to update own loss probability
Rational social learning	Using informed others’ choices as hints as to what is the optimal choice for self
Imitation	Simply following what others do
Social regret	Anticipating less regret when others are likely to be affected if I am and also neglected protection
Inequality aversion	Anticipating guilt or envy if disaster changes my income as compared to my reference group
Moral hazard	Anticipating better chances for government’s assistance when others affected as well

Two variants can be distinguished here. Under **social regret** (Cooper & Rege, 2011), also known as the ‘misery loves company’ effect, an unattractive outcome which could have been avoided is less aversive if others have made the same ‘mistake.’ Thus, in having one’s uninsured property destroyed by a hurricane, one may find some consolation that others had neglected protection as well. In a slightly different and much better known mechanism, **inequality aversion**

(Fehr & Schmidt, 1999), staying behind the reference groups is painful, no matter in what ways their financial situation was affected by their choices.

Finally, the extent of **moral hazard** in risk taking (the tendency to save on insurance in the hope that the government will help out in case a disaster strikes) may be affected by peers' situations as well. Indeed, the more people remain uninsured in the area, the greater their potential lobbying power with the government. Of course, correlation between peers' insurance choices could also be due to their similarity in (unobservable) dimensions, rather than any causal relationship. Note also that many of these mechanisms predict an overall effect of **awareness that others are threatened by the same risk** (even if no information on their decisions is conveyed) on willingness to purchase insurance. This is the third type of impact sometimes referred to as 'peer effects', one that is directly relevant for the problem of seemingly insufficient demand for catastrophic insurance. Notably, inequality aversion may lead to relatively low insurance take-up if others' risks are perfectly correlated with own risks, compared to when they are independent. Likewise, insufficient learning from others' experience and decisions is likely to lead to low catastrophic insurance take-up, as major disasters are, nearly by definition, rare, so that most people have not personally experienced one (yet). In other words, relying solely on own experience will lead people to base their decisions on small and potentially biased samples (Ert & Trautmann, 2014).

Overall, distinguishing between these various mechanisms (all of which lead to clusters of insured and uninsured households) is very difficult. However, it is not only of importance from the purely academic viewpoint of understanding human behavior in some specific circumstances; there may be significant differences in policy implications as well. For example, one may wonder what impact targeted subsidizing of insurance purchase will have on households that are *not* covered by such a campaign. Under rational learning, there will be little effect, because the targeted and not-targeted households will face different conditions; the fact that a person purchased some good or service when offered a discount that another person cannot enjoy does not make her think it is more attractive. By contrast, inequity aversion and moral hazard predict a strong effect, as one does not wish to be among the few with uncovered losses, while predictions based on social regret and conformity are probably intermediate, depending on the specific formulation of the concepts used. The picture is different if one considers providing potential insureds with additional information. It is expected to have a strong effect also on others if they are rational learners – it is especially valuable to follow a peer who has received specialized training and therefore made a truly informed decision.

8.3 PEER EFFECTS: EMPIRICAL STUDIES

Several empirical studies tried to distinguish between the mechanisms just discussed. It should be mentioned that due to insufficient literature focused on insurance, we also rely on studies investigating other types of decision-making under risk. As Richter *et al.* (2014) rightly point out, such inference must be made with caution, as there may be significant idiosyncrasies associated with decision-making in the insurance context – perception of risk and behavior under risk may differ from formally analogous situations in other domains (Slovic, 1987; Kusev *et al.* 2009).

Starting with effects of past losses (rather than decisions) it is often reported that catastrophes lead to increased perceived probability of a loss in the future (Cameron & Shah, 2015) and greater demand for insurance (Michel-Kerjan & Kousky, 2010). Using a particularly suitable data set and method, Gallagher (2014) found that local floods increased flood insurance take-up among American inhabitants of the Gulf of Mexico and Florida's Atlantic coast. A spike, followed by a slow decline (the effect being undistinguishable from zero after nine years) can be best captured by Bayesian learning with short memory – a model in which new information helps update prior beliefs but then fades away from the decision-maker's awareness. It should be noted that reliable long-run statistics available for these areas make actual information content of each specific event miniscule. Importantly, dwellers of unaffected areas also adjusted their behavior, suggesting that others' experience also plays a role (albeit a smaller one than own experience).

By contrast, Viscusi and Zeckhauser (2015), using field data on tap water contamination, showed that people may in fact nearly neglect potentially informative experiences of other people in their social environment. An important shortcoming of this analysis is that the authors do not observe whether people actually believe that the quality of their own tap water is correlated with the quality of the tap water of other people in their reference group. Indeed, water quality can strongly depend on local aspects such as the quality and material of the pipes etc.

In a laboratory experiment, Viscusi *et al.* (2011) directly addressed the issue of learning from others' choices (rather than experienced losses) by observing investment decisions made individually and in the group context. Even though subjects were provided with complete information about probabilities and outcomes, others' decisions made a difference in that choice behavior of subjects tended to drift towards the median choice.

A carefully designed laboratory study by Cooper and Rege (2011) allowed distinguishing between various mechanisms. These authors let their subjects choose between pairs of gambles represented as colored grids. One of the many tiny colored squares would be picked at random, with different colors standing for different amounts, so that their respective frequencies represented probabilities involved. Assessing the number of same-colored squares (and thus probabilities involved) was easy when they were clustered together ('simple' format – risk condition) but difficult or impossible when they were scrambled (ambiguity condition) or only some of them were visible at all ('blackout'). In any case, subjects only had eight seconds to choose between the gambles, which was clearly not enough to count the squares in the scrambled format. Each pair of gambles was shown three times, with two information conditions used: either only one's own past choice for this particular pair was shown (individual feedback) or also those of peers (social feedback). Social regret and inequality aversion thus predicted that there would be more regression towards others' choices in the social feedback, compared to individual feedback, largely regardless of presentation format. This was what Cooper and Rege actually observed. Moreover, rational social learning was expected to play a greater role in the scrambled format (others had different noisy signals of the underlying probability distributions, so their decisions represented a valuable hint) than in other formats (all subjects faced the same ambiguous situation), which was actually disconfirmed.

In a unique field experiment, Bursztyn *et al.* (2014) worked with a brokerage firm offering a new asset to their clients. In pairs of friends or relatives among them, one investor was approached and offered the asset. About half of them were interested, but were told that due to supply shortage, only half of those willing to purchase the asset would actually be able to do so. Subsequently, the other investor was informed or not informed (random treatment assignment) about the first investor's reaction and, in the former case, about the outcome (whether the first investor actually obtained the asset or not). This design allowed distinguishing between social learning and social regret/inequity aversion. Both channels were statistically and economically significant. Interestingly, social learning was positively correlated with first investor's advantage in financial sophistication over the second investor.

Lahno and Serra-Garcia (2015) focused on distinguishing between inequality aversion on the one hand and imitation and social regret on the other. They investigated choices between lotteries, conditional on peer's decision (choice treatment) or on an exogenous allocation the peer could not change (allocation treatment). Looking at individuals who changed their mind compared to purely individual decision, the authors concluded that the fraction of subjects following others nearly doubled in the choice treatment compared to the allocation treatment. This finding implies that both inequality aversion and conformism (or social regret) matter.

Friedl *et al.* (2014) did not study the impact of others' specific insurance decisions, but their simple design allowed investigating the impact of others being merely affected by the risk perfectly aligned with that of the decision-maker. In their short classroom experiment, Friedl and colleagues endowed their 149 participants with 10 euro each and told them they faced a 50% probability of losing this amount. This risk was either to be resolved independently for each participant, or jointly for all (a 'catastrophic' risk, affecting everyone). Each participant was then asked to indicate for amounts ranging from 4 euro to 6.25 euro whether she would be willing to purchase full insurance against the aforementioned risk for the amount in question. The main finding was that willingness to pay was greater for independent risks. A natural interpretation is that participants were affected by inequality aversion, so that uninsured losses were more acceptable when many other participants incurred losses as well (some of which were probably also uninsured).

Friedl *et al.* (2014) argued that the simultaneous over-insurance for high probability low cost risk and under-insurance of low probability high cost risks (as discussed in Browne *et al.* 2015) could be due to social comparison and correlated losses: typical low probability natural risks (such as floods) are highly correlated across people in a region or neighborhood while typical high probability risks (such as bike theft) are uncorrelated across people. In the former case, thus, social comparison does not lead to strong feelings of loss, because peers also lose, while in the latter case the loss is felt intensely.

While the idea that low insurance take-up fuels expectations of governmental help is appealing, the potential role of moral hazard in insurance demand is notoriously difficult to investigate. Grislain-Letrémy (2015) tried to test it using insurance data from France's overseas departments (Guadeloupe, Martinique, Reunion and French Guyana). They represent an interesting natural experiment of sorts, as French legislation results in good (and heavily subsidized) supply of natural disaster coverage which is much-needed in these hurricane/cyclone prone areas. Clearly, perceived probability of government's intervention is not observable and can only be tackled using a structural estimation based on strong parametric assumptions. Grislain-Letrémy found moral hazard to be one of two main obstacles on the demand side (the other being low quality of houses, making them ineligible for insurance).

Botzen and Van den Bergh (2012) investigated moral hazard more directly, albeit using declarative data. In a large survey of homeowners in the Dutch river delta, they elicited willingness to pay for flood insurance, manipulating inter alia the availability of public compensation for affected households. They observed a significant impact on reported willingness to pay for insurance. We are not aware of a study that directly tackles the link between the number of uninsured households and the probability of public intervention.

This review does not leave one with the impression that there are simple explanations for peer effects in insurance decisions. On the contrary, several mechanisms may be at work simultaneously and their relative strength depends in possibly subtle ways on the specific circumstances, institutional environment and study methodology.

8.4 EXPERIMENTAL STUDY: DESIGN AND METHODOLOGY

Our own empirical effort builds upon design choices and experiences of cited researchers, particularly Friedl *et al.* (2014). We wish to know to what extent findings from some specific empirical and experimental set-up will generalize across different situations. Studies based on field data as in Gallagher (2014), Viscusi and Zeckhauser (2015) and Browne *et al.* (2015) have high external validity for the type of catastrophic risk under investigation. However, they typically have less control over the underlying mechanisms. For example, it is not clear whether higher insurance take-up in neighboring counties after a flood is caused by higher demand by homeowners, or by stronger or more successful marketing effort from the side of the insurance companies. The strong influence of peers on insurance decisions found in some studies seems at odds with the neglect of peer information in the formation of expectations observed in others. It is clearly possible to have a utility function with social reference points while at the same time having beliefs that neglect others' information. However, from a psychological perspective it is at first sight surprising that social influences would be restricted to a certain dimension only. Exactly which mechanism is behind specific peer effects is typically difficult to identify.

In the context of small probability losses, we therefore investigate the robustness of these empirical patterns in a uniform catastrophic loss insurance setting: How do decision-makers process probabilistic information in low probability-loss settings? What is the effect of peer outcomes on beliefs and insurance take-up? How are these effects moderated by the correlation of potential losses among people?

Low probability events are especially difficult to study in the field. Moreover, it is not easy to identify causal effects of peers' behavior and experiences in non-experimental settings (see the discussion in Viscusi & Zeckhauser, 2015). As hinted before, because people may self-select into vulnerable areas (Page *et al.* 2014), and because this self-selection may interact with insurance choices, few conclusions regarding the effect of exogenous policy changes can be drawn. We therefore conducted a controlled laboratory experiment to identify the causal effects of social information and risk-correlation (across people) on risk perception and insurance take-up.

8.4.1 General set-up

In a two-stage set-up, participants first worked on a task unrelated to insurance and earned a substantial income of PLN 80 (~EUR 20). In the second stage they were exposed to an uncertain loss of this income and performed two tasks: they were asked to assess the uncertain probability of the loss and to make a decision on whether or not to purchase insurance against this potential loss.

To study how subjects update information and incorporate these updates in their decisions, the second-stage loss-exposure task consisted of 40 decision-making periods. Between these periods, the subjects received treatment-specific information. However, there were no dynamic changes in the subjects' financial status across periods: each period involved a new exposure to the loss of the original PLN 80 and a new insurance offer, irrespective of earlier losses or insurance costs. Exactly one of the 40 periods was selected at the end of the experiment to determine the monetary payments to a participant. This design thus assures that there are no interdependencies across periods, apart from the learning effects that are the focus of the current study.

In each period, subjects faced an uncertain chance of losing their PLN 80 endowment that had been randomly selected from the interval (5%, 25%). This probability was identical for all subjects in a group (defined in more detail below) and for all periods, but it differed across sessions. The true value of the probability was never disclosed to the subjects, but they were aware of the interval from which the probability was drawn. Keeping the probability constant over time allowed subjects to update their beliefs about the probability on the basis of past experiences and observations.

In each period subjects first made a prediction of the true underlying probability of the loss. These predictions were incentivized: If in this particular period the probability prediction task were selected for payment, they would receive PLN 80 minus a penalty for incorrect prediction. This penalty was calculated as the absolute difference between their guess and the true probability multiplied by four zloty. For example, if a subject predicted a probability of 15% given a true probability of 10%, she would receive $80 - 4 * |15 - 10| =$ PLN 60. Note that this is not a typical subjective probability elicitation task, which is often incentivized using so-called proper scoring rules. In this case, the correct probabilistic answer was well defined, so that we were able to simplify the rule. It was incentive-compatible (truthful reporting was optimal) for risk-neutral subjects, while (severe) risk aversion could draw subjects' guesses somewhat towards the middle of the interval (15%). Adjusting for risk aversion would require further complication.

The second task in each period was the insurance decision. Each subject was given six offers (prices) for a full-insurance contract against the loss. The prices were determined by adding a random noise in the range (–3, +3) to each of six deterministic values 6, 11, 16, 22, 31, and 41 (the procedure made sure that the six offers were always monotonically increasing after the random noise had been added). These prices were selected on the basis of pilot tests to cover the possible range of values that subjects may hold for insurance of a PLN 80 loss that occurs with a probability in the range (5%, 25%). This procedure appeared to be easier and less repetitive (due to the random variations in prices across periods) than a direct-matching elicitation of the certainty equivalent using a Becker-DeGroot-Marschak mechanism or auction format.

Prices were identical for all members in a group, but they were newly determined for each period. For each price, subjects had to indicate whether they wished to purchase insurance or not. If the respective period and task was selected for payment, one of these six decisions would subsequently be selected to determine insurance status and earnings. Subjects' willingness to pay for insurance (WTP) was defined as the midpoint between the highest price for which the person purchases insurance and the lowest price for which she prefers the sure loss. For example, for a subject who chooses insurance for prices 8, 11, and 16, but prefers to stay uninsured for prices 24, 33, and 43, we calculate a WTP of PLN 20. The structure of a typical round is illustrated in Figure 8.1.

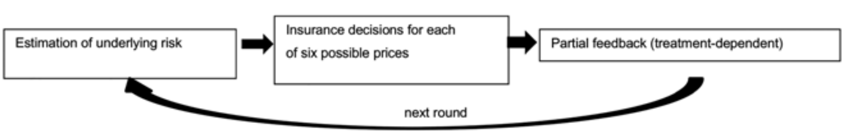


Figure 8.1 A typical round.

8.4.2 Treatments

Subjects participated in the second-stage task in groups of five. The identity of other group members was unknown to each subject. At the end of each period, subjects received feedback regarding their own and other group members' choices and outcomes, depending on three treatment conditions discussed below. Additionally, there were two treatment conditions that differed in the way the group members' losses were correlated. That is, the experiment implemented a 2 (risk correlation) × 3 (information condition) treatment design that we explain in more detail next.

In the *Uncorrelated Losses* condition each subject's loss was independently randomized in each period – the fact that any group member suffered a loss did not affect anybody else's chance. In the *Correlated Losses* condition, individual risks in each five-person group were highly correlated within each round – typically either none or several of the group members were affected Specifically, we independently drew two loss events from the underlying probability distribution of the loss. For example, with a 0.2 probability of a loss, two losses would be selected with probability 0.04, one loss and one no-loss with probability 0.16, and otherwise (with probability 0.64), two no-losses would be selected. Then each subject would be randomly assigned one of these two events. In other words, if two losses were selected in the first stage, all subjects would incur a loss, if there was one loss and one no-loss, each subject in this group faced the probability of losing of 0.5 and finally nobody would lose if two no-losses were selected in the first stage. As a result, loss experiences became correlated across subjects. For example, with the above underlying 0.2 probability of loss, the probability that none of the five subjects experienced a loss becomes $0.8^2 + 0.2 * 0.8 * 2 * 0.5^5 = 0.754$, compared to only 0.328 in the case of uncorrelated losses. Subjects were only given a verbal, informal description of their condition.

The three information conditions varied as follows. In the *Individual Information* condition at the end of each period each subject would learn: (i) which of the six insurance contract offers was drawn and could be relevant if this period was selected for monetary payment; (ii) as a consequence, whether she would be insured in this period or not, and (iii) whether she suffered a loss in this period or not. Note that an insured loss would still be accounted as a loss experience on the information screen because it provides relevant information on the uncertain loss probability, similar to the information content of covered losses outside the lab. In the *Social Information: Loss* condition, the subject would have all information as given in the Individual Information condition, and would additionally learn how many other members of the group experienced a loss event in this period. In the *Social Information: Loss and Choice* condition, the subject would have all information as given in the *Social Information: Loss* condition, and would additionally learn how many other members of the group were insured in this period. Again, insured losses were included as experienced losses because they are informative on the uncertain loss probability (which was identical for all members of the group in all treatments).

8.4.3 Lab details

Sessions were run in November and December 2013 in the Laboratory of Experimental Economics, University of Warsaw. Participants were recruited from the local subject pool. Because of the nature of the first stage of the experiment, it was emphasized in the invitations that the subjects had to be proficient in English and have some understanding of academic economics. However, the second stage of the experiment, as well as the instructions, were given in the Polish language. A session would have groups of four or five people each. In each session, all groups were either in the Correlated Losses or all in the Uncorrelated Losses condition. In contrast, the information treatment conditions were varied within a session. The experiment was computerized using z-tree software (Fischbacher, 2007). Sessions typically lasted almost two hours and subjects made nearly 70 PLN on average.

8.4.4 Predictions

Clearly, any other player's experience of losses was as valuable by way of information about the probability of the loss as own experience. As for the impact of others' choices, because subjects were provided with identical information and there was ample time to decide, there was little room for rational social learning. In other words, others' decisions were not necessary to make optimal choices. Likewise, moral hazard was excluded by design. Conformism/imitation was probably limited, as players were not told the exact maximum willing to pay chosen by others. By contrast, social regret and inequity aversion predicted that observing higher insurance take-up among others would also trigger higher willingness to pay for insurance. Moreover, insurance take-up will be higher under uncorrelated than correlated losses, as in the other case other players are also likely to end up with a loss if it happens to the decision-maker. As we had no treatment in which insurance take-up would be involuntary (as in Lahno & Serra-Garcia, 2015), we cannot make a clear distinction between these two mechanisms.

8.5 EXPERIMENTAL STUDY: RESULTS

We first compare willingness to purchase insurance across treatments. Table 8.2 reports WTPs derived from insurance take-up decisions as described before and averaged across all periods, by treatment. Additionally, we calculate a measure of risk aversion. To this end we subtract Subjective Expected Loss, SEL (possible loss, PLN 80, multiplied by participant's assessment of its probability elicited in a given period), from WTP. High values correspond to high risk aversion.

Unlike Friedl *et al.* (2014) we observe no effect of the correlation of risks: WTP (corrected for risk perception or not) is the same in the case of independent and correlated risks. This might be due to a lack of social comparisons of final outcomes made by the subjects; or it might be due to a low degree of social loss aversion despite salient social comparisons. Both the comparability with others and the degree of social loss feelings might be stronger in classroom settings with a low degree of anonymity as in the experiments of Friedl *et al.* (2014). Obviously we would expect our anonymous laboratory setting to be a boundary condition for these effects: outside the laboratory people do often observe the outcomes of their peers, and consider them important for their own well-being. In any case, it is clear that this social comparison effect strongly depends on the details of the situation, and generalizations should only be made with much care.

Table 8.2 Insurance take-up depending on treatment.

	Individual Info		Social Info: Loss		Social Info: Loss & Choice	
	WTP	**WTP-SEL**	**WTP**	**WTP-SEL**	**WTP**	**WTP-SEL**
Independent risks	24.69 ($N = 20$)	14.15 ($N = 20$)	23.23 ($N = 30$)	12.12 ($N = 30$)	22.27 ($N = 25$)	11.57 ($N = 25$)
Positively correlated risks	24.33 ($N = 24$)	13.84 ($N = 24$)	19.68 ($N = 35$)	9.98 ($N = 35$)	23.36 ($N = 40$)	12.85 ($N = 40$)
Mann-Whitney test	$p = 0.741$	$p = 0.479$	$p = 0.111$	$p = 0.211$	$p = 0.562$	$p = 0.618$

We now turn to investigating the impact of information available to a participant in a given period (rather than overall treatment effects). It was made clear to the participants that the unknown probability of the catastrophic loss was the same for all members of the 5-person group. Therefore observed losses of other group members should directly be used to update the estimate of the underlying probability. Importantly, equal weight should be attached to an update based on a subject's own experience and an update based on another person's experience.

To study the impact of observed losses on probability estimates and insurance choice (i.e., WTPs), we run fixed-effects panel regressions models (that is, we control for fixed individual-specific mean values). To check if the results are robust to the econometric specification, we take both actual values (levels) and changes (first differences) of the left-hand (dependent) variable and likewise for the main right-hand (explanatory) variables, that is, the historical frequency of losses. We restrict the analysis to the treatments with social information and uncorrelated risks. Table 8.3 reports regression coefficients for the probability estimates and Table 8.4 reports regression coefficients for insurance decisions (WTPs).

For the effect on probability estimates (Table 8.3) we find that the historical frequency of losses based on the outcomes of other participants has a stronger effect than a person's own frequency of past losses; this effect is significant only for levels and is non-significant for changes in the level (first differences). However, we should adjust for the fact that frequencies for the other people in the group are based on four observations for each single observation for the person's own experience.

When doing so, we find that own-experience based updates are stronger than those based on another person's experience. For changes in frequencies, the effect points in the same direction, but does not reach statistical significance.

Table 8.3 Own experiences versus others' experiences – effects of observed losses on probability estimates.

	Levels		**First Difference**	
	No Period Controls	**Controlling for Period**	**No Period Controls**	**Controlling for Period**
Historical frequency of own losses	0.052**	0.052**	0.028**	0.028**
Historical frequency of others' losses (4 people)	0.091**	0.091**	0.050**	0.050**
F-test testing own = 4 others[a]	(<) $p < 0.01$	(<) $p < 0.01$	(=) $p = 0.277$	(=) $p = 0.277$
F-test testing own = 1 other[a]	(>) $p < 0.01$	(>) $p < 0.01$	(=) $p = 0.129$	(=) $p = 0.129$

Notes: Entries are unstandardized regression coefficients. **Indicates 1% significance level. [a]Size of own effect versus effect of other indicted in parentheses.

When we look directly at the effect on *behavior*, that is, willingness to pay for insurance (Table 8.4), we find no significant differences between the effects of own experiences and the groups' experiences for either levels or differences. However, controlling again for the fact that others' experiences provide four times as many observations, we find significantly larger impact of the own experiences on insurance preferences (WTPs).

To summarize, we find evidence that people discount other people's information in their raw beliefs, and in their insurance choices. A change in historical loss frequency based on a decision-maker's own loss receives a much larger weight in her probability update and insurance decision than an equally large change based on experience of another individual. In the current settings this holds true despite both events being equally informative about the underlying event.

Finally, we look at the direct effect of others' *choices* on own insurance choices. As a simple test, we compare WTPs depending on the number of subjects in the peer group of five that were insured in the previous period. Table 8.5 shows that there is no effect (with all p values of non-parametric statistical tests exceeding 0.1). This holds true for the whole set of 40 periods, as well as for a restricted analysis based only on Period 2 (i.e., exactly one instance of learning). The same is true for the actual percentages of insured subjects given the different information sets. Clearly, other participants' observed past insurance decisions had no effect on subjects' insurance decisions.

Table 8.4 Own experiences versus others' experiences – effects of observed losses on insurance choices (WTPs).

	Levels		First Differences	
	No Period Controls	**Controlling for Period**	**No Period Controls**	**Controlling for Period**
Historical frequency of own losses	0.071**	0.073**	0.055**	0.054**
Historical frequency of others' losses (4 people)	0.081**	0.086**	0.017	0.017
F-test testing own = 4 others[a]	(=) $p = 0.765$	(=) $p = 0.678$	(=) $p = 0.398$	(=) $p = 0.391$
F-test testing own = 1 other[a]	(>) $p < 0.01$	(>) $p < 0.01$	(>) $p = 0.029$	(>) $p = 0.028$

Notes: Entries are unstandardized regression coefficients. **Indicates 1% significance level. [a]Size of own effect versus effect of other indicted in parentheses.

Table 8.5 Imitating others' insurance choices?

# Other Group Members Insured in Preceding Period	**All Periods**		**Only Period 2**	
	WTP (Mean)	**Insured (%)**	**WTP (Mean)**	**Insured (%)**
0	23.62	57.14	20.5	46.15
1	24.24	61.46	24.76	61.11
2	22.92	55.80	24.72	62.96
3	22.06	56.16	21.5	50.00
4	23.80	57.19	19.1	60.00

Notes: Based on data from Social Information: Loss and Choice condition only. Entries indicate behavior of those who observe others' decisions.

8.6 CONCLUSIONS

We identify a number of ways in which peers' experiences and decisions may affect insurance take-up. Some of these mechanisms may contribute to explaining and potentially solving the problem of insufficient demand for catastrophic insurance. This is particularly important with regard to flood insurance. Indeed, coverage is typically limited, even with governmental subsidies.

The importance of social comparison for economic decisions has been widely acknowledged in the field (e.g., World Bank, 2015), and recent research suggests that it may be central to insurance take-up as well. Specifically, catastrophic events, such as floods, are well covered by the media, which may strengthen peer effects. On the other hand, some authors have suggested that decision-makers put too little weight on other people's relevant information (Viscusi & Zeckhauser, 2015; see also Minson & Mueller (2012), for the case of groups exacerbating this effect). In our experiment we are able to study these effects in one uniform design, in a controlled laboratory environment that excludes alternative explanations which are typically possible in the case of field data. In particular, in our study the information regarding the other person is unambiguously relevant to the own insurance decision, and highly salient for the decision-maker.

We confirm previous findings that people discount experience of other decision-makers: upon observing a loss incurred by another person they update their beliefs and behavior but not quite as strongly as they would if they experienced it personally. Notably, this happens despite the fact that others' loss experiences are unambiguously equally informative in our setting as their own loss experience (which was not true in studies such as Viscusi & Zeckhauser (2015)). This means that, for example, potential buyers of houses in flood-prone areas will tend to underestimate the threat compared to the sellers.

The second key finding is that, unlike Friedl *et al.* (2014), we find no support for the hypotheses of social regret/inequity aversion. Indeed, our subjects do not seem to be directly affected by others' decisions. Moreover, their willingness to insure does not depend on whether risks are correlated or not.

Our data may provide policy implications concerning efforts to encourage vulnerable populaces to purchase catastrophic insurance and otherwise take sufficient measure to reduce and transfer the risks they are facing. In view of our findings of underestimation of risk, providing correct probability estimates of such events may be effective. The same can be said of disseminating information about catastrophic losses suffered by others. On the other hand, providing information about others' insurance decisions may have a very weak effect. Likewise, emphasizing possible losses relative to (insured) others may be less effective, in view of the negative findings of social regret/inequity aversion.

On a meta-level, our observations are in line with the broad picture found in existing literature: peer effects in insurance take-up do not seem to be very robust. On the contrary, they depend strongly on the respective setting. Clearly, future research combining theory, laboratory experiments, and field experiments will need to address interactions between features of the decision-making setting and particular channels through which peer effects in catastrophic insurance take up may operate. Until this is achieved, the benefit from these studies for policy-making will be limited.

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