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Philippe van de Calseyde

Implicit Cues in Social Interaction and Decision Making

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Cover design: me

Cover explanation: This dissertation is about how people make sense of their social situation by relying on various cues. Maybe you have tried to make sense of why there is a giraffe on the cover. There is no particular reason other than I love giraffes.

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Implicit cues in social interaction and decision making

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Chapter 1

General introduction

Our lives are characterized by a constant stream of decisions and many are made in the context of social interaction. For example, a person who has to decide whether to trust an investor with her money or a negotiator who must choose whether to concede to the demands of the other party. These interactions are important as they provide people with the opportunity to realize goals that cannot be attained individually. However, these interactions are often complex since the real intentions of those involved are usually unknown or intentionally hidden. For example, in negotiations, sellers normally conceal their lowest acceptable selling price, and similarly, buyers conceal the maximum price they are willing to pay. As a consequence, both parties need to rely on implicit cues to infer each other's price limits. The aim of this dissertation is twofold. I explore in a variety of social interactions (1) how people rely on various cues that people employ to make sense of their social situation and (2) how these cues influence subsequent decisions like whom to reward or punish, with whom to cooperate or with whom to negotiate.

The outline of the remainder of the introductory chapter is as follows. I first elaborate on defining the various strategies that people use in making sense of their social situation and how it affects decisions in a variety of areas. Next, I introduce the consequences of this process in the dissertation's areas of interest. That is, how are people guided by situational cues that affect decisions like whom to punish, with whom to cooperate or with whom to negotiate. Finally, a brief overview of each of the chapters is given.

Going beyond the information given

Social sense making refers to the process by which people provide meaning to their social experiences by 'going beyond the information given' (Bruner, 1957). Specifically, many of the things we want to know about others are not directly perceivable. For example, can we trust strangers who claim to be trustworthy or is a suspect indeed guilty? In order to understand these hidden qualities of others, people often rely (justly or unjustly) on perceivable cues that allow them to develop an understanding of the quality of interest.

An important perceivable cue that people use as source of information is the face of others and its expressions (Ekman, 1992, 1993). For instance, Darwin (1872,1962) already noted that people use facial cues, such as an adverting eye gaze, to infer a person's guilt. In a similar vein, recent research by Willis and Todorov (2006) revealed that people automatically attend to specific facial features in order to determine a stranger's trustworthiness. In one of their experiments, participants were exposed to unknown faces for 100 milliseconds and had to judge the trustworthiness of the person. The result indicated that these judgments correlated highly with judgments made in the absence of any time constraints (the question whether these judgments are indeed reliable is a different matter).

Research suggests that people not only attend to physical features but also make trait inferences by attending to the actions of others. Heider (1944, 1958) was probably the first to recognize that people extract information about other's dispositions from observed acts. In their seminal experiment, Heider and Simmel (1944) asked participants to watch a movie in which three geometrical figures were shown moving in various directions at various speeds. Participants were subsequently asked to "write down what happened in the

picture" (p. 245). Only one participant described the actual content of the scene (i.e., geometric figures moving around). All other participants described it in term of actions of animated characters, attributing motivational and emotional states to the figures in explaining the observed movements (e.g., the triangle is 'chasing' the circle because the triangle is angry). Overall, these results support the notion that people go beyond the information given by attending to cues that provide meaning to their social experiences. Next I discuss the implications of this process in various domains of interest.

Going beyond the information given in judicial decisions

One domain in which people are not expected to go beyond the information given is the area of legal decision making. Legal decision makers are expected to apply legal reasons to the facts of a case in a rational manner. However, to what extent are judges, juries and eyewitnesses susceptible to extraneous factors when reconstructing or evaluating legal cases?

Loftus and Palmer (1974) were among the first to show how random factors in courtroom procedures systematically influenced eyewitnesses testimonies. For instance, in one of their experiments, all participants saw the same film of a car accident and were subsequently asked how fast both cars were driving. Unbeknown to participants, the authors had several conditions in how they asked participants to estimate the driving speeds. In one version, participants had to estimate the driving speeds when they 'smashed' into each other while others had to estimate the speeds when they collided, bumped, hit, or contacted. The results showed that the driving speeds were estimated to be highest when participants answered the

'smashed' question while lowest when the question was framed as 'contacted'. Importantly, one week later, participants who answered the 'smashed' question were much more likely to remember, erroneously, that there was broken glass present in the accident. These results indicate that eyewitness testimonies may be guided, among other things, by (normatively) arbitrary cues like the wording of questions that significantly shaped their experiences and recollections of the incident. In Chapter 2 of this dissertation, I provide evidence for another arbitrary cue that legal decision makers use when evaluating the severity of moral transgressions. That is, Chapter 2 provides evidence that people impose milder punishments for identical crimes in which the victim happened to be insured as opposed to uninsured. However, before explaining Chapter 2 in detail, let us first explore how people develop an understanding of situations in the other areas of interest of this dissertation, namely strategic interdependent situations.

Going beyond the information given in interdependent situations

Interdependent situations are social interactions where individuals share and exchange resources in order to obtain mutually beneficial outcomes. A key component of these situations is that they are characterized by mixed motives. On one hand, parties have the incentive to collaborate to attain mutually beneficial goals. On the other hand, parties typically have the egocentric (often hidden) incentive to increase personal gain at the expense of one's interaction partner. As a consequence, people extensively rely on nonverbal cues of others in such situations to develop an understanding of the counterpart's intentions and motives. For example, in testing the consequences of 'reading' emotions in negotiations, Van

Kleef, DeDreu, and Manstead (2004) asked participants to continue negotiating with a seller who initially reacted either angry or happy to the participant's starting bid. The results showed that participants who were negotiating with an angry seller subsequently made the largest price concessions while those negotiating with happy sellers made the smallest ones. In explaining these findings, the authors indicated that a seller's emotional reaction to the initial bid implicitly revealed the seller's price limits that buyers took into account in determining subsequent bids.

Similar signaling effects were found by McCabe, Rigdon, and Smith (2003) in the domain of human cooperation (see also Camerer, 1988 for comparable signaling effects in gift giving). In explaining why people positively reciprocate trust, the authors found that trustees were more likely to honor trust when trust was voluntary (in the sense that the trustor also had the opportunity not to trust) as opposed to involuntary. In explaining this difference, the authors argued that a voluntary act of trust has the ability to signal that the trustor wants to cooperate and arrive at the mutually beneficial outcome. An involuntary act of trust is unable to signal such intentions since the trustor has no choice but to cooperate. Overall, these results again provide support for the notion that people 'go beyond the information given' by attending to cues that allow them to develop an understanding of the intentions and motives of others. In Chapters 3 and 4 of this dissertation I discuss similar findings in the domain of human cooperation and negotiations. In what follows, I present a brief overview of the empirical chapters in this dissertation.

Overview of the chapters

Each chapter contains between four and nine studies, twenty in total. The chapters and studies illustrate the diversity of circumstances in which people provide meaning to their social interactions by relying on cues that emerge in the situation. Each chapter represents an individual article that is either published or submitted for publication.

Chapter 2. In this chapter I tested empirically how legal decision makers determine the amount of punishment a perpetrator deserves by relying on cues like the insurance status of victims. The starting point was the observation that most legal systems are rooted in the premise that punishments should be proportional to the harm caused. The roots of this directive are closely associated with the tendency of people to respond to unkindness with a similar degree of unkindness (the eye-for-an-eye principle; see Carlsmith, Darley, & Robinson, 2002). Although this imperative makes sense, mechanisms like insurance have become wildly available that alleviate the harm of victims. What are the consequences of compensating victims by means of insurance in how people evaluate and punish immoral acts? This chapter provides the first empirical support that compensating harm may not only change the consequences for victims but also for perpetrators. In particular, using a variety of different transgressions and punishments, this chapter demonstrates that people impose milder punishments when the victim happened to be insured as opposed to uninsured.

Chapter 3. Whereas Chapter 2 explored the role of insurance in the legal domain, this chapter investigates its role in trust relationships. The chapter builds on the idea that trusting others carries some degree of risk and a common solution to mitigate problems of risk is to buy insurance.

Employing the trust game (e.g., Dasgupta, 1988; Berg, Dickhaut, & McCabe, 1995), I test the conjecture that trustees become more likely to act opportunistically when trustors choose to be insured against the risk of betrayal. The presumed safeguard against the risk of betrayal is thus expected to increase the probability of betrayal. Indeed, although insurance made betrayal less costly, the studies in this chapter demonstrated that it also makes it more likely under certain circumstances. In explaining this, this chapter provides support for the hypothesis that trustees become less cooperative because by *choosing* insurance, trustors implicitly signal that they expect the trustee to behave opportunistically, encouraging trustees not to cooperate.

Chapter 4. In the final empirical chapter, the role of another cue that frequently arises in interdependent situations is examined, namely the decision time of others. Specifically, I test the conjecture that people perceive the time that others need in reaching a decision as indicative of the doubt that the decision makers experienced. In nine experiments I show that these inferred perceptions of doubt have a variety of consequences like with whom people want to cooperate or negotiate.

Chapter 5. The overall empirical work presented in chapters 2-4 is examined in the final chapter. Specifically, Chapter 5 contains a summary of the findings and a conclusion in which the implications of this dissertation for theory and practice are discussed.

Chapter 2

The insured victim effect

Abstract: An insurance policy may not only affect the consequences for victims but also for perpetrators. In six experiments I find that people recommend milder punishments for perpetrators when the victim was insured, although people believe that a sentence should not depend on the victim's insurance status. The robustness of this effect is demonstrated by showing that recommendations can even be more lenient for crimes that are in fact more serious but in which the victim was insured. Moreover, even when harm was possible but did not materialize, people still prefer to punish crimes less severely when the (potential) victim was insured. The final two experiments suggest that the effect is associated with a change in (1) one's compassion for the victim and (2) the perceived severity of the transgression. Implications of this phenomenon are briefly discussed.

This chapter is based on: Van de Calseyde, P.P.F.M., Keren, G., & Zeelenberg, M. (2013c). The insured victim effect: When and why compensating harm decreases punishment recommendations. *Judgment and Decision Making*, *8*, 161-173.

The costly consequences of negative events may be compensated for by insurances. As such, an insurance policy lowers the risk of the insured. However, research has shown that controlling risk via insurances also comes at a cost. For example, when buying insurance, people tend to make decisions that are incompatible with rational choice theory (see Johnson, Hershey, Meszaros, & Kunreuther, 1993). In this chapter, I address a very different potential cost of insurance, namely, whether people would recommend lower punishments for crimes in which the victim was insured as opposed to uninsured. Interest in this issue stems from the fact that compensating harm may change the victim's outcome, yet it does not alter the severity of the crime. It only implies a transfer of the negative consequences from the victim to the insurance company. When victim compensation from insurance indeed improves the perpetrator's outcome, the insurance policy may prove to be a safeguard for the perpetrator as well.

In essence, an insurance policy is a safety mechanism by which a third party (the insurance company) undertakes to guarantee an insured party against losses that may be incurred by misfortunes. Insurance thus changes the severity of an unfortunate outcome by providing financial compensation in the event that a specific hazard occurs. A large stream of literature in both economics and psychology has focused on understanding the consequences of insurance from the perspective of the insured party (e.g., Holmstrom, 1979; Tykocinski, 2008; Hsee & Kunreuther, 2000; Johnson et al., 1993). For example, one of the unwanted side effects of insurance is the phenomenon of moral hazard. It refers to the increased risk taking by individuals for whom the consequences of risk are reduced which, in turn, increases the probability that misfortune will strike. Research revealed a positive correlation between accidents and car insurance

benefits and a positive correlation between health insurance and unhealthy lifestyles (e.g., Stanciole, 2007; Dave & Kaestner, 2009). In addition, Tykocinski (2008) found that reminding people of their insurance policy lowered their perceived likelihood that misfortune will befall them (but see Van Wolferen, Inbar & Zeelenberg, 2013). Together, these findings suggest that being insured may create an illusory sense of safety resulting in detrimental behavioral consequences.

I build upon this prior work and extend it in order to understand the *inter*personal consequences of insurance. Specifically, I investigate whether people would recommend different punishments for crimes in which the victim was insured as opposed to uninsured. To illustrate this point, imagine two thieves both of whom stole an identical digital camera from two different people. One of the victims happened to be insured against theft while the other was not. Two related questions can be raised concerning the punishment the thieves deserve: (1) should they be punished equally (the normative question) and (2) will they be punished equally (the descriptive question)? Building on prior work on how the outcome of victims may influence sentencing decisions (see Gino, Shu, & Bazerman, 2010; Gino, Moore, & Bazerman, 2009; Berg-Cross, 1975), I find that (1) although people believe that sentencing should not depend on whether a victim was insured or not, (2) people nonetheless recommend lower punishments for perpetrators when the victim was insured as opposed to uninsured.

Harm and punishment

Harm and punishment are intimately related. When a person is unjustly harmed, people experience a strong desire to punish the wrongdoer (Carlsmith, Darley, & Robinson, 2002) and more harm is typically accompanied with more

punishment. In fact, legal systems are often rooted in the premise that punishments should be proportional to the harm caused (e.g., an eye for an eye) and there is widespread consensus that this 'just deserts' principle is a justified moral rule (see Carlsmith et al., 2002). However, using the severity of harm as a guide may also prompt people to use the victim's outcome in a manner that can sometimes be normatively irrelevant (see Gino et al., 2009, 2010; Paharia, Kassam, Greene, & Bazerman, 2009; McCaffery, Kahneman, & Spitzer, 1995; Berg-Cross, 1975). For example, research by Cushman, Dreber, Wang, and Costa (2009) indicates that people punish behaviors that accidentally resulted in small detrimental consequences less harsh as compared to similar acts that by accident resulted in large consequences. In a similar vein, multiple studies by Gino and colleagues (2010) reveal that people are less likely to punish unfair behaviors when the unfairness *happened* to produce positive rather than negative consequences for a victim. Together, these studies suggest that people use the severity of a victim's outcome as a guide in evaluating and sentencing perpetrators. Since insurance positively changes the severity of the consequences for a victim, I hypothesized that, other things being equal, people will recommend lower punishments for crimes in which the victim was insured as opposed to uninsured.

Six experiments were conducted to examine this effect. Experiment 2.1 provides initial support for the hypothesized relationship between the extent to which a victim is insured or not and the corresponding severity of punishment. Experiment 2.2 aims to determine whether people knowingly punish perpetrators less severely when victims are insured, by asking participants to judge both the insured and uninsured conditions simultaneously. Experiment 2.3 extends these findings by looking at the role of foreknowledge of the

perpetrator. Experiment 2.4 tests whether people would also differentiate between insured and uninsured individuals when punishing a transgression that did not result in any harm. Experiment 2.5 aims to determine whether people would also punish perpetrators more mildly when the crime against the insured victim was in fact more severe. Finally, Experiment 2.6 examines the role of possible psychological processes that may explain why people are more lenient towards perpetrators when the victim happened to be insured as opposed to uninsured.

Experiment 2.1

Method

Twenty-nine students (6 male, 23 female) at Tilburg University participated in exchange for course credit (M_{age} = 19, SD = 1.40). All participants were asked to read a brief scenario concerning the theft of a camera and subsequently were asked to determine the severity of punishment the thief deserved. Participants were randomly assigned to the insurance or no insurance condition. The scenario in the insurance [no insurance] condition read as follows:

> Tom is an amateurphotographer who decides to purchase a new camera. In the store he is asked if he wants to insure the camera against theft. This type of insurance guarantees that Tom will receive a new camera in case the camera is stolen.
>
> Although Tom doubts whether to buy the insurance, he decides [not] to do so. In short, his camera is [not] insured against theft. A few days later when Tom is sitting on the terrace in the city and wants to leave, he discovers that his camera is stolen.

> Some time later Tom finds out that a person was arrested who confessed stealing and selling the camera. A common punishment for such an offence is imposing community service. Community service entails doing compulsory social work for several days (8 hours a day).

After reading the scenario, participants were asked to make a *punishment recommendation* by indicating the number of days (minimum 1 day, maximum of 20 days) they thought the perpetrator should fulfill community service.

Results and discussion

The results showed that participants recommended less severe punishments when the camera was insured (M = 9.20; SD = 5.98) as compared to when it was uninsured (M = 13.93; SD = 5.42), t (27) = 2.23, p = .04, d = 0.83. These findings confirm the initial prediction that people would punish the same transgression differently as a function of whether the victim was insured or not. These results can be interpreted in two ways. One possibility is that people recommend more severe punishments when stealing an uninsured possession. Alternatively, it may be the case that people recommend a milder punishment when the victim was insured. An additional study was designed to addresses this issue by including a baseline condition in the experimental design.

Additional study. Besides replicating the previous results, the purpose of this experiment was to test whether the uninsured condition would be punished more severely compared to a neutral baseline or whether the insured condition would be punished less severely compared with the same baseline. The procedure, scenario and dependent measure were identical as the one used in the previous

experiment except that a baseline condition was added which did not mention the possibility of buying insurance. Ninety-nine individuals at various locations in Tilburg (46 male, 53 female) volunteered to participate in this study (M_{age} = 25, *SD* = 8.44). Punishment recommendations again varied as a function of insurance, F (2, 96) = 5.30, p = .007, η^2 = .11 thus replicating the results of the first experiment. Planned comparisons showed that participants again imposed milder punishments on a perpetrator who stole an insured camera (M = 7.48; *SD* = 4.93) compared to a perpetrator who stole an uninsured camera (M = 12.00; *SD* = 6.30), t (64) = 3.24, p = .002, d = 0.80. Likewise, participants recommended a milder punishment on a perpetrator who stole an insured camera as compared to baseline participants (M = 10.97; *SD* = 6.37), t (64) = 2.49, p = .02, d = 0.61. Punishment recommendations did not vary between the uninsured and the baseline condition (t < 1, *ns.*). This pattern shows that compared to the neutral baseline, people recommend milder punishments when victims happen to be insured and not harsher punishments when victims are uninsured.

Experiment 2.2

The above results demonstrate that people differentiate between insured and uninsured victims when judging both cases separately. Would people still differentiate between both victims when evaluating the two cases jointly? Joint evaluations allow decision makers to *comparatively* evaluate multiple options while in separate evaluation only one of two options is evaluated. Previous research demonstrated preference reversals between what people choose in separate versus joint evaluation (Hsee, 1996; Hsee & Zhang, 2010). These reversals demonstrate that switching from one evaluation mode to the other changes the *relative* importance that people assign to a

given attribute (in our case, stealing an insured versus uninsured possession). Importantly, joint evaluations allow us to determine whether people believe they *should* differentiate between insured and uninsured victims when sentencing perpetrators (for similar reasoning, see Bazerman, Tenbrunsel, & Wade-Benzoni, 1998). The present experiment therefore manipulated evaluation mode by asking participants to assess the appropriate punishment for the theft of an insured as well as an uninsured camera. The purpose of this design was to test whether, under these comparative conditions, participants would still differentiate between the theft of an insured and an uninsured camera.

Method

Seventy-nine students (12 male, 67 female) at Tilburg University participated in exchange for course credit (M_{age} = 19, *SD* = 3.27). Participants were randomly assigned to one of three experimental conditions: *separate evaluation – insured victim* (SE-I), *separate evaluation – uninsured victim* (SE-U), or *joint evaluation* (JE). The same scenario as in the first experiment was used. Hence, the SE conditions constitute replication of Experiment 1. In the JE condition, participants were first asked to read a scenario in which a perpetrator stole an insured camera followed by a scenario in which a different perpetrator stole an uninsured camera. Both scenarios were presented on the same page. Thus, JE participants were implicitly encouraged to compare the two scenarios and were then asked to indicate the appropriate punishment for the described perpetrators. Order of presenting the two scenarios was counterbalanced which did not affect punishment recommendations.

Results and discussion

I first compared the punishment recommendations between the two separate evaluation conditions (SE-I and SE-U). Replicating the findings of Experiment 2.1, an independent t-test revealed that participants recommended less severe punishment for the perpetrator when the victim was insured (M = 9.52; SD = 6.27) than when the victim was uninsured (M = 12.85; SD = 5.39), t (51) = 2.07, p = .04, d = 0.57.[1] For the JE condition, however, a paired sample t-test revealed no significant difference, t (25) = -1,69, p =.10, d = 0.33, indicating that when the two cases were seen together, people punished equally stealing an insured (M = 10.73; SD = 6.37) or uninsured camera (M = 11.19; SD = 6.25). It is noteworthy to point out that *only* three participants in the JE condition provided different ratings for the two perpetrators. All other participants (89%; 23 out of 26) recommended an identical punishment for the two perpetrators. In sum, when placed in a comparative situation, the large majority of participants believe that a punishment *should not* be a function of the victim's insurance (or lack of it).

[1] The insured and uninsured conditions in Experiment 2.1 and the separate-conditions in Experiment 2.2 constitute exact replications. I therefore aggregated these observations to get a more exact estimate of the effect. The results indicate that perpetrators whose victims were insured received milder punishments than those whose victims were uninsured (M = 8.56; SD = 5.67 vs. M = 12.70; SD = 5.76), t (147) = 4.43, p < .001, d = 0.72. Interestingly, while only 8 out of 75 participants (10.7%) in the insured condition imposed a maximum sentence (20 days), significantly more imposed this sentence in the uninsured condition (28.4%; 21 out of 74), χ^2 (1, N = 149) = 7.46, p = .006, φ = .22. This pattern reversed for relatively low punishments. In the insured condition, 33 out of 75 participants (44%) imposed a punishment equal or less than 5 days while only 15% (11 out of 74) in the uninsured condition imposed these relatively low punishments, χ^2 (1, N = 149) = 15.19, p < .001, φ = .32.

Experiment 2.3

Experiment 2.2 demonstrates that the insured victim effect disappears when evaluating both cases jointly. Would people also recommend equal punishments for perpetrators who knowingly stole an insured or uninsured possession respectively? The principle of *mens rea* (Latin for 'guilty mind') states that 'the act does not make a person guilty unless the mind is guilty'. In essence, this principle states that the *relevance* of a victim's outcome in sentencing a perpetrator depends critically on the level of foreknowledge when causing harm. For example, involuntary manslaughter refers to the unlawful, but unforeseen, killing of a person while committing a crime (e.g., killing a pedestrian, without intent, when running a red light) and deserves less punishment than voluntary manslaughter (i.e., murder). In our case, the *mens rea* principle states that the relevance of insurance in sentencing may depend on whether a perpetrator knew in advance that one's victim was insured or uninsured. That is, knowingly stealing an uninsured (as opposed to insured) possession is more blameworthy while such a difference is unjustified when the perpetrator was unaware of the victim's insurance status. In testing the *mens rea* principle in the domain of insurance and punishment, the next experiment explicitly manipulated whether perpetrators knowingly or unknowingly stole an insured or uninsured possession.

Method

Eighty-one students (35 male, 46 female) at Fontys University of Applied Sciences participated in exchange for €4,- (M_{age} = 21, SD = 2.39). The current study was part of a set of unrelated studies. As in the joint evaluation condition in Experiment 2.2, participants were asked to read the two

scenarios concerning the theft of an insured or uninsured camera and subsequently were asked to assess the appropriate punishment each thief deserves. They were randomly assigned to one of two experimental conditions in which they were either informed that both perpetrators knew their victim's insurance status versus that both perpetrators were unaware of the insurance status of their victims. Order of presenting the two scenarios was counterbalanced; order did not affect punishment recommendations.

Results and discussion

I first compared participant's punishment recommendations for the two perpetrators who *unknowingly* stole an insured or uninsured camera. A paired sampled t-test revealed no difference in punishing both perpetrators (M = 11.52, SD = 6.17 versus M = 11.50, SD = 6.19), $t<1$, *ns.* However, the pattern changed when perpetrators *knowingly* stole an insured versus uninsured possession. Knowingly stealing an uninsured camera was judged as deserving a harsher punishment (M = 12.18, SD = 5.36) than knowingly stealing an insured camera (M = 11.35, SD = 5.46), t (38) = 2.36, p = .02, d = 0.38. Importantly, while almost no participant (5%; 2 out of 42) recommended different sentences when *unknowingly* stealing an uninsured or insured possession, significantly more (36%; 14 out of 39) participants imposed different punishments when *knowingly* stealing an insured or uninsured camera, χ^2 (1,81)= 12.21, $p < .001$, $\varphi = .15$.

In sum, people indicate that punishments *should not* be a function of a victim's insurance (or lack of it) when perpetrators *unknowingly* steal insured versus uninsured possessions. A large minority, however, does differentiate between perpetrators who *knowingly* steal insured versus uninsured possessions. These results confirm that more harm

for a victim indeed justifies more punishment, but only when a perpetrator *knowingly* steals an uninsured versus insured possession. This may suggest that the insured victim effect (see Experiment 2.1) can be accounted for by participants' implicit belief that perpetrators were aware of the insurance status of their victim. In a post experimental test, I therefore asked a different group of participants (N = 15) to read either the insured or uninsured scenario of our first experiment. Subsequently they were asked whether they thought that the perpetrator was aware of stealing an insured or uninsured camera respectively (yes vs. no). A large majority (93%) indicated that they thought the perpetrator was not aware of the (lack of) insurance, ruling out that a difference in foreknowledge drives the insured victim effect.

Experiment 2.4

Although the harm caused to an insured individual is less severe than the harm caused to an uninsured individual, harm is not always a necessary consequence. For example, a perpetrator might be caught in the act leaving the potential victim unharmed. Such an offense still deserves punishment. Would people again differentiate between insured and uninsured individuals when harm was potential but not realized? Insurance may not only change the severity of *actual* misfortunes, but may also influence thoughts about *what could have happened*. Research within the domain of counterfactual thinking indeed suggests that, when proposing a sentence for a wrongdoer, people are sensitive for *what could have happened* to a victim (see Miller & McFarland, 1986; Macrae, 1992). In a similar vein, I propose that being protected of harm by means of insurance alters *what could have happened* which in turn enhances the insured victim effect. Experiment 2.4 addresses

this issue by holding constant the outcome of a norm violation (i.e., there is a foul but no harm) while only varying a person's vulnerability to the consequences *if* harm had occurred (insured versus uninsured person). In addition, I employed a new vignette in order to determine whether the insured victim effect will replicate using a different norm violation and punishment type. Finally, because I employed a new vignette, I again varied the evaluation mode of participants (separate versus joint evaluation) in order to examine whether I would replicate that the effect disappears when evaluating both cases jointly.

Method

One hundred and nine students (41 male, 67 female, 1 unreported) at Fontys University of Applied Sciences participated in exchange for €4,- (M_{age} = 20, SD = 2.68). The current study was part of a set of unrelated studies. Participants were randomly assigned to one of three experimental conditions: *separate evaluation – insured employee* (SE-I), *separate evaluation – uninsured employee* (SE-U), or *joint evaluation* (JE). Participants in the separate evaluation conditions were asked to read one of the following two scenarios. The scenario in the insurance [no insurance] condition read as follows:

> Joris is a student who works for all kinds of companies on a daily basis via an employment agency. He is asked by the agency if he wants to buy disability insurance. This insurance guarantees that Joris, in case of work related injury, will receive specialised rehabilitation that will prevent a lasting disability. Although Joris doubts whether to buy the relatively expensive insurance, he

> decides [not] to do so. In short, Joris is [not] in the possession of disability insurance.
>
> Later that day Joris is employed as a window washer for a company called WASH & GLASS. After receiving a wooden ladder he is instructed to wash numerous windows at three meters height. While climbing up the ladder, it turns out that the fourth step is rotten and Joris falls down and lands on his knee. Joris does not get hurt.
>
> Sometime later Joris learns that the owner of WASH & GLASS confessed to the occupational safety authority of being negligent in the maintenance of the ladder. Meanwhile, the owner replaced the ladder. A common punishment for such an offense is to temporarily suspend the company's working permit. This means that during this period WASH & GLASS is not permitted to work and will lose revenue.

After reading the scenario, participants made their *punishment recommendation* by indicating the number of days (a minimum of 0 and a maximum of 20 days) the working permit of WASH & GLASS should be suspended. In the JE condition, participants were asked to read an insured and uninsured scenario (applying to different employees and different companies) presented on the same page. Order of presenting the two scenarios was again counterbalanced which did not have an effect on punishment recommendations.

Results and discussion

An unexpected gender difference was observed (male participants punished the business owner less severe than female participants; M =7.05; SD = 5.66 vs. M = 9.91, SD = 5.66),

t (107) = 2.55, *p* = .01, *d* = 0.50), and in the rest of the analyses I controlled for gender.[2] I first compared the punishments imposed by participants in the two separate evaluation conditions (SE-I and SE-U). An analysis of covariance (ANCOVA) revealed that participants recommended a lower punishment when the employee happened to be insured (*M* = 8.00; *SD* = 5.50) as opposed to uninsured (*M* = 10.37; *SD* = 5.53), *F* (1, 72) = 4.35, *p* = .04, η^2 = .05. The pattern in the JE condition was different. For the JE condition, a within-subject ANCOVA revealed that participants did not differentiate when punishing the two business owners. Specifically, participants did not impose a milder punishment for a business owner employing an insured employee (*M* = 7.70, *SD* = 6.10) than the one employing an uninsured employee (*M* = 7.60, *SD* = 6.15), *F* (1, 31) = 1.30, *p* = .26. Again a large majority (79%; 26 out of 33) of participants in joint evaluation reported an identical punishment recommendation for the two business owners. This pattern of results is very similar to those obtained in Experiment 2.2 demonstrating, once again, the impact of insurance (or lack of it) on punishment recommendations. Although participants believed that the victim's insurance status should not influence their punishment recommendation (as inferred from the joint-comparative condition), they nevertheless take it into account in their judgment when in a separate, non-comparative condition. Importantly, even in the absence of actual harm, people still recommend less severe punishments for transgressions in which the (potential) victim was insured as opposed to uninsured.

[2] I checked for gender differences in all other experiments but did not find any gender effect. An analysis without controlling for gender revealed no major differences.

Experiment 2.5

Experiments 2.2 and 2.4 demonstrated that the insured victim effect disappears when comparing both cases jointly. Does it imply that people are insensitive to the harm component when comparing both cases in joint evaluation? This would contradict the widespread belief that a punishment should be proportional to the harm caused. Experiment 2.5 tested the tenet that stealing a possession that is either insured or uninsured can be perceived as more or less harmful depending on how one assesses harm. For example, compare stealing an expensive but insured Mp3 player priced $299 with stealing a relatively inexpensive, uninsured Mp3 player priced $99. In terms of retail prices, the former theft causes more harm than the latter. However, in terms of harm to the victim, the second theft causes more harm because the Mp3 player was not insured. When are people guided by the insurance status of a victim and when by the actual value of the stolen possession in sentencing a perpetrator?

From a legal perspective, the actual value of a stolen possession should outweigh the victim's insurance status. After all, insurance only transfers the loss from the victim to the insurance company but it surely does not make the total consequences of the crime less severe. However, assessing the value of any object is inherently a comparative judgment and often hard to evaluate in isolation (see Hsee, 1996). The purpose of Experiment 2.5 was to test the proposition that people in joint versus separate evaluation use a legally relevant detail (i.e., the actual value of a stolen possession) differently when punishing a crime in which the value of the stolen possession is hard to evaluate. In order to make the value hard to evaluate, I stated the retail price in a currency of which the value was supposedly unfamiliar to our participants (Japanese

Yen). More specifically, participants were asked to impose an appropriate punishment for a perpetrator who either stole an insured camera (priced ¥260.000) or an uninsured camera (priced ¥21.000). Even though the actual price is what people should consider when evaluating how much punishment a perpetrator deserves, this attribute is hard to evaluate. Without a reference, most of our participants would not know whether a camera priced ¥260.000 is expensive or not. In contrast, the insurance status of a victim is relatively easy to evaluate. Most people would evaluate a victim's harm as little when insured while large when uninsured. It was therefore predicted that people in separate evaluation would punish stealing the insured, expensive camera less harsh while the pattern was expected to reverse in joint evaluation.

A second objective of this experiment was to examine a person's affective response to the insured victim. Prior research has indicated that less harm is associated with less compassion that in turn is associated with less severe punishment recommendations (e.g., Nadler & Rose, 2003). Given that insurance changes the severity of a victim's harm, I hypothesized that insured victims evoke less compassion than uninsured victims which, in turn, is expected to mediate the relationship between a victim's insurance status and punishment recommendations.

Method

One-hundred and one students (55 male, 38 female, 8 unreported) at various Dutch universities volunteered to participate (M_{age} = 22, SD = 4.67, 8 unreported). Participants were randomly assigned to one of three experimental conditions: *separate evaluation – insured & expensive camera* (SE-Iexp), *separate evaluation – uninsured & inexpensive camera* (SE-Uinexp), or *joint evaluation* (JE). Participants were asked to

read a scenario in which a perpetrator stole a digital camera. The scenario in the insured-expensive camera [uninsured-inexpensive camera] condition read as follows:

> Tom decides to buy a digital camera prized ¥260.000 [¥21.000] via the Japanse website of Amazon.com. Amazon also provides the possibility to insure the camera against theft. This type of insurance gurantees that Tom will receive a new camera in case the camera is stolen.
>
> Tom decides [not] to buy the insurance. In short, his camera is [not] insured against theft. A few days later when Tom is sitting on the terrace in the city and wants to leave, he discovers that his camera was stolen. Of course, he realizes that he insured [did not insure] the camera.
>
> Some time later Tom finds out that a person is arrested who confessed stealing and selling the camera. A common punishment for such an offence is imposing community service. Community service entails doing compulsory social work for several days (8 hours a day).

In the JE condition, participants were asked to read both the insured-expensive and the uninsured-inexpensive scenario (applying to different victims and perpetrators). Both scenarios were presented on the same page. Order of presenting the two scenarios was counterbalanced and did not affect any of the dependent variables.

After reading the scenarios, participants indicated their level of compassion with the victim via the following two questions, (1) how much compassion one felt for the victim (1 = *absolutely not*, to 7 = *absolutely*) and (2) how much harm the victim suffered (1 = *relatively little*, to 7 = *relatively much*).

These items were significantly correlated (r = .41, p < .001) and averaged into a compassion composite. Finally, participants indicated the number of days (a minimum of 1 day and a maximum of 20 days) they thought the perpetrator should fulfill community service. Participants in the two separate evaluation conditions were also asked if they knew the value of 1 Yen in Euros (1= *I don't have any idea* to 4 = *Yes, exactly*; for a similar procedure, see Hsee, 1996). This question was added in order to assess whether participants indeed thought the Japanese currency was hard to evaluate.

Results and discussion

Manipulation check. The mean evaluability score for a Japanese Yen was 1.52 (SD = .83) and differed significantly from the midpoint (2.5) of the scale t (65) = 9.67, p < .001, confirming that the value of the camera was a hard-to-evaluate attribute in this experiment.

Main findings. The main findings are summarized in Table 2.1. I first compared the two separate evaluation conditions (SE-Iexp and SE-Uinexp). A t-test revealed that insured victims indeed evoked less compassion than uninsured victims. In addition, replicating the insured victim effect, participants recommended a milder punishment for stealing an expensive but *insured* camera than an inexpensive but *uninsured* camera. For the JE condition, punishment recommendations reversed. Although the insured victim again evoked less compassion than uninsured victims, participants nevertheless punished stealing the insured but *expensive* camera more severe than the uninsured, *inexpensive* camera. These results confirm that people differentiate between a victim's harm and the actual value of the stolen possession when sentencing perpetrators. When the actual value is hard to evaluate independently, people are guided by the outcome of victims. When the value is

evaluated *comparatively,* however, the value of the possession becomes the primary factor underlying punishment recommendations and the victim's role is strongly attenuated.

Exploratory mediation analyses. A mediation analysis was applied only to the separate conditions because responses in the joint condition are not independent. I found support for a mediating role of compassion in the insured victim effect, using a series of regression analyses (cf. Baron & Kenny, 1987). Specifically, the relationship of the victim's insurance status and punishment recommendation (B = 0.26, t = 2.19, p = .032) became non-significant (B = 0.20, t = 1.64, p = .11) when compassion with the victim was added in the analysis (B = 0.22, t = 1.89, p = .07). When evaluating both cases separately, insured victims seem to evoke a less powerful emotional response that, in turn, affects punishment recommendations.

Table 2.1
Experiment 2.5: Effect of Possession Type and Evaluation Mode on Compassion with the Victim and Punishment Recommendation.

	Camera Victim				
	insured expensive	uninsured inexpensive			
	M (*SD*)	*M* (*SD*)	*t*	*p*	*d*
Separate Evaluation					
Compassion	3.43 (1.43)	4.22 (1.38)	2.37	.02	0.58
Punishment	9.38 (5.73)	12.68 (6.84)	2.19	.03	0.53
Joint Evaluation					
Compassion	3.58 (1.34)	4.47 (1.26)	2.55	.02	0.50
Punishment	12.03 (6.20)	10.60 (6.60)	2.92	.007	0.73

Note: The punishment decision was assessed on a scale ranging from a 1 day sentence to a 20 day sentence. To assess a participant's compassion with the victim, a 2-item 7-point scale was used, with higher score indicating more compassion.

Experiment 2.6

This experiment was designed to explore other potential mediators for the insured victim effect, in addition to the role of compassion. For example, compassion is related to anger and research shows that anger is related to punishment decisions (Kahneman, Schkade, & Sunstein, 1998; Nelissen & Zeelenberg, 2009; Sunstein, 2005). Moreover, transgressions that evoke more compassion are, in general, judged to be more serious or unethical and vice versa. Thus, although Experiment 2.5 showed that compassion may partly account for our findings, it remains unclear whether the effect is driven solely by compassion or whether it is driven by a change in anger or ethical judgment. In addition, the mediating role of compassion is not sufficient to account for all our findings, because the insured victim effect was observed even in the absence of actual harm (see Experiment 2.4). Given that harm and compassion are intimately related, the absence of harm in Experiment 2.4 would presumably have resulted in similar degrees of compassion in both experimental conditions. Yet, we still observed the insured victim effect. The objective of the final experiment was therefore to explore other potential explanations that could account for the insured victim effect namely, a change in anger, ethical judgment or compassion. I employed a new vignette and, as in previous experiments, evaluation mode (separate vs. joint) was manipulated.

Method

One hundred and thirteen students (14 male, 99 female) at Tilburg University participated in exchange for course credit (M_{age} = 20, SD = 3.83). The current study was part of a set of unrelated studies. Four participants were discarded for failing a

comprehension question.[3] Participants were randomly assigned to one of three experimental conditions: *separate evaluation – insured car owner* (SE-I), *separate evaluation – uninsured car owner* (SE-U), or *joint evaluation* (JE). Participants in the separate evaluation conditions were asked to read one of the following two scenarios. The scenario in the insurance [no insurance] condition read as follows:

> Tim recently received his driver's license. While leaving a parking space he hits another parked car. When stepping out, he notices that his car is undamaged. However, the other car is worse off. The side surface is dented and the paint is damaged. Tim looks around and realizes that no one has seen the accident. Tim doubts what to do. He decides to leave.
>
> Sometime later, Tim is arrested. A local resident saw the accident happening from her home. The police report showed that in the meantime the damaged car has already been repaired. The insurance company fully paid for the repair costs. The owner was well insured against such damages [The owner fully paid for the repair costs. The owner was not insured against such damages].

After reading the scenario, I asked a participant the following set of items:

Punishment recommendation. First, a participant indicated a *general* punishment recommendation by stating how much punishment Tim deserved (1 = *mild*, to 7 = *severe*). Next,

[3] Participants in the separate evaluation conditions were asked in a post experimental questionnaire to indicate whether the victim in their scenario was insured or uninsured. Four participants in the uninsured victim condition indicated erroneously that the victim in their scenario was insured. These four participants were discarded from the analysis. Excluding these participants did not change the pattern of results in any meaningful way.

participants made a *specific* punishment recommendation by indicating the number of days (a minimum of 1 day and a maximum of 20 days) they thought Tim should fulfill community service.

Compassion. To assess a participant's compassion with the victim, they were asked (1) how much compassion one felt for the victim (1 = *no compassion*, to 7 = *much compassion*) and (2) how much harm the victim suffered (1 = *no harm*, to 7 = *much harm*). The items were significantly correlated (r = .53, p < .001) and averaged into a compassion composite.

Ethical judgment of the situation. Participants were told that, "Some offenses are more severe than others. Please indicate the severity of Tim's offense" (1 = *not at all severe*, 7 = *very severe*). In addition, participants were told that, "Some offenses are more unethical than others. Please indicate how unethical Tim's offense is" (1 = *not at all unethical*, 7 = *very unethical*). These items were significantly correlated (r = .51, p < .001) and averaged into an ethical judgment composite.

Feelings of anger. Four items were employed to assess a participant's anger toward the situation (α = .95). Specifically, participants were asked to indicate how angry the transgression of Tim made them feel; how mad the transgression of Tim made them feel; how angry they were at Tim; and how mad they were at Tim (1= *not at all*, 7 = *very*).

Different punishment. Finally, participants in the separate evaluation conditions were asked whether they would punish the perpetrator differently had the victim been uninsured (in the insured victim condition) or insured (in the uninsured victim condition) by indicating: Yes, a more severe punishment; Yes, a less severe punishment; No, the same punishment. In the JE condition, participants were asked to read an insured and uninsured scenario (applying to different car owners and different perpetrators).

Results and discussion

Punishment recommendations. The main findings are summarized in Table 2.2. I first compared the two separate evaluation conditions and found that participants recommended milder punishments when the victim was insured. This was true for both the general- and specific punishment recommendation. However, when explicitly asked whether they would change their punishment had the victim been uninsured (in the insured condition) or insured (in the uninsured condition), a large majority of participants (81%; 65/80) indicated that they would not change their sentence.

This supports the idea that people believe that insurance should not affect punishments, yet they nonetheless impose milder punishments when victims are insured. This conclusion is even stronger supported by examining the recommendations of participants in the joint condition. When imposing a *specific* punishment (i.e., community service) for both cases jointly, people again did not differentiate between an insured or uninsured victim. In fact, a large majority (85%; 28 out of 33) imposed identical number of days of community service for both perpetrators, irrespective of the insurance status of their victims. For the *general* punishment scale, a paired-sample t-test indicated a significant statistical difference when participants compared both cases jointly (i.e., the insured case deserved a less severe punishment than the uninsured case). However, this difference was driven by a small minority of participants. While a large majority (76%; 24 out of 33) provided identical punishment responses for both cases, only 8 out of 33 participants (24%) indicated that both perpetrators should be punished differently.[4] Overall, these results confirm

[4] In testing whether the observed difference in the joint evaluation condition was statistically different from the difference in the separate evaluation condition, I ran a modified t-test. The results confirmed that the difference in the joint condition was smaller than the observed difference in the separate evaluation condition, t (106) = 2.51, $p < .01$ (one-tailed).

that the large majority of people believe that a punishment *should not* be a function of the victim's insurance, yet people nonetheless impose milder punishments when victims are insured when evaluating both cases separately.

Compassion. Analyzing first the two separate evaluation conditions, the findings of Experiment 2.5 were replicated showing that insured victims evoked less compassion than uninsured ones. A similar pattern, yet even stronger, was observed in the joint condition. Overall, people seem to feel less compassion for insured victims, irrespective of the evaluation mode of a person.

Ethical judgment. When comparing the separate evaluation conditions, the results indicate that a victim's insurance status changes how people evaluate the transgression. Participants judged the act to be less unethical when the victim was insured as opposed to uninsured. A similar pattern emerged when comparing the responses in the joint evaluation condition. A paired-sample t-test revealed that the transgression in the insured case was perceived to be less serious than the uninsured case. However, a large majority of participants in the joint condition (79%; 26 out of 33); provided identical ethicality ratings for both cases, while only 7 out of 33 participants (21%) rated both cases different.[5] Overall, these results indicate that the majority of participants seem to believe that the presence of an insured victim *should not* change the evaluation of the act, yet people nonetheless judge the act to be less severe when evaluating both cases separately. These results support the contention that a victim's outcome influences the evaluation of the act, even when the outcome is logically irrelevant. Evidently, when evaluating both

[5] In testing whether the observed difference in the joint evaluation condition was statistically different from the difference in the separate evaluation condition, I ran a modified t-test. The results confirmed that the difference in the joint condition was smaller than the observed difference in the separate evaluation condition, t (106) = 1.91, $p < .05$ (one-tailed).

cases separately, a transgression that resulted in less harm is perceived to be a smaller foul than an identical transgression that happened to result in more harm (see also Gino et al., 2009 for the outcome bias in ethical judgments).

Anger. When comparing the two separate evaluation conditions, I find no evidence for the idea that insured victims arouse less anger than uninsured victims. In joint evaluation however, an insured victim did evoke a less angry response as compared to when the victim was uninsured.

Exploratory mediation analyses. As in Experiment 2.5, the analysis was applied only in the separate conditions. As initial step, I first inspected the correlations between the potential mediators. Ethical judgments were significantly correlated with compassion with the victim (r = .41, p < .001) and the anger that the situation evoked (r = .54, p < .001) while anger, in turn, was significantly correlated with compassion with the victim (r = .38, p = .001). Overall, these results indicate that the potential mediators are strongly related, yet not fully correlated, suggesting that compassion, anger, and ethical judgment may differentially explain why people become more lenient towards perpetrators when victims are insured. To examine the mediating role of compassion, anger, and ethical judgment in explaining the insured victim effect, I ran a bootstrap analysis as recommended by Preacher and Hayes (2008) with 5000 bootstrapped samples. [6] The results suggest that the insured victim effect was mediated by the change in ethical judgment and not by the change in compassion or anger. Specifically, when I entered compassion, anger, and ethical judgment in the same bootstrapped model simultaneously, the ethical judgment was the only significant mediator, B = 0.27, Z = 2.04, p = .04, with a 95% confidence interval excluding zero (0.0577 to

[6] I report only the results of the mediation analyses on the *general* punishment recommendation since the results on the *specific* recommendation are identical in meaning.

0.6024). Compassion and anger did not show a significant pattern, compassion B = -0.03, Z = -.35, p = .73; anger B = 0.08, Z = 1.08, p = .28, with both a 95% confidence interval including zero. These results suggest that the presence of an insured victim seems to attenuate the perceived severity of the crime that, in turn, affects punishment recommendations.

Table 2.2
Experiment 2.6: Effect of Victim Type and Evaluation Mode on Punishment Recommendations, Compassion with the Victim, Ethicality Judgments and Feelings of Anger.

	Car of the Victim				
	insured *M* (*SD*)	uninsured *M* (*SD*)	*t*	*p*	*d*
Separate Evaluation					
Punishment General	3.55 (1.18)	4.53 (1.38)	3.72	.001	0.86
Punishment Specific	6.18 (5.73)	8.83 (6.84)	2.25	.03	0.52
Compassion	4.45 (1.07)	5.31 (1.19)	3.30	.002	0.75
Ethical Judgment	3.38 (1.19)	3.99 (0.86)	2.55	.02	0.60
Anger	3.21 (1.31)	3.78 (1.52)	1.76	.08	0.40
Joint Evaluation					
Punishment General	4.67 (0.78)	4.94 (0.90)	2.73	.01	0.49
Punishment Specific	7.06 (4.21)	7.67 (4.71)	1.50	.14	0.27
Compassion	3.97 (1.24)	5.91 (0.87)	8.28	.001	1.47
Ethical Judgment	4.00 (1.15)	4.15 (1.27)	2.73	.01	0.51
Anger	4.60 (1.42)	4.95 (1.48)	3.38	.001	0.65

Note: The punishment general measure is assessed on one-item 7-point scale, with higher scores indicating more punishment. The punishment specific score is assessed on a scale ranging from a 1 day sentence to a 20 day sentence. To assess compassion with the victim, a 2-item 7-point scale is used, with higher score indicating more compassion with the victim. The ethical judgment item is assessed on a 2-item 7-point scale, with higher scores indicating that the transgression is perceived as more unethical. Finally, to assess feelings of anger, a 4-item 7-point scale is used, with higher score indicating more feelings of anger.

General discussion

In this article I highlight a hidden cost of insurance: People recommend milder punishments for perpetrators when the victim happened to be insured, although people believe that a sentence should not depend on whether the victim was insured or not. The results of six experiments (using a variety of different transgressions and punishments) established the existence of an "insured victim" effect and suggest that people inadvertently differentiate between insured and uninsured victims when evaluating moral transgressions.

Experiment 2.1 demonstrated that people would punish identical transgressions less severely when victims are insured as opposed to uninsured. Experiment 2.2 found that the effect disappeared when participants had to determine jointly the sentence for both the insured and uninsured case. Experiment 2.3 extended these findings by ruling out that the effect is driven by the perpetrator's foreknowledge. Experiment 2.4 demonstrated that when harm was possible but not realized, people still punish crimes less severely when the (potential) victim was insured. Experiment 2.5 showed that punishment recommendations can even be more lenient for crimes that are in fact more serious but in which the victim was insured. Finally, in Experiment 2.6, I explored via correlational and mediational analyses, the extent to which a number of potential psychological mechanisms could account for the insured victim effect namely (1) how people evaluate the severity of the transgression, (2) one's compassion with the victim and (3) the anger that the situation evoked. The results suggest that the insured victim effect is associated with a change in how people evaluate the severity of the transgression and not by a change in compassion or anger.

The present research contributes to a recent stream of research investigating how legally irrelevant characteristics of victims enter judgments of ethical behavior For example, Nordgren and McDonnell (2001) recently showed that *increasing* the number of people victimized by a crime in turn *decreases* punishment recommendations. The authors explain their findings by the identifiable victim effect (e.g., Kogut & Ritov, 2005) following which unidentifiable victims evoke less sympathy and less severe punishments than identified victims (Kogut, 2011). I present evidence for a similar effect in which transferring losses to an unidentified entity (i.e., insurance company) results in less severe punishments.

This work also contributes to a stream of research highlighting the negative intra- and interpersonal consequences of safety mechanisms. For example, Walker (2007) has shown that when overtaking a cyclist, drivers are less cautious (i.e., get closer to the cyclist) when cyclists wear a helmet (analogous to insurance) than when they do not. Thus the safety measure may ironically attract hazard. The present studies supplement this line of research by showing that, other things being equal, insurance may lower the threshold for committing a crime due to possible reduced punishment.

The present results are seemingly incompatible with rationalist theories of moral judgment (Kohlberg, 1969) because of the punishment differences between the separate and joint evaluation modes. These reversals can be elucidated by further examining the vital difference between joint and separate evaluation mode. Specifically, in the joint mode, which is comparative in nature, it is evident that the severity of the crime is identical (e.g., the same camera was stolen in both cases). The fact that participants in this case impose exactly the same punishment implies that they consider the crime severity as the only relevant dimension (and hence believe that victim's

harm is irrelevant in this case). However, in the separate condition, in which there is no reference point for comparison, the insured victim evoked less compassion and participants evaluated the transgression to be less severe resulting in lower punishment recommendations. In other words, participants' norm (as inferred from the joint condition) is that compensating a victim's harm by means of insurance should not have an effect on the size of punishment. Yet, contrary to that belief, and supposedly being unaware of it, participants in the separate condition are swayed by the lack of suffering by the insured victim and, contrary to their standards, inflict a lower punishment in the insured case.

The results reported in the present article were all obtained from a population of laypersons. These findings should therefore be tested on other populations (especially professional judges and juries), although research indicates that professional judges or juries are no different than laypersons in being prone to biases (Vidmar, 2011; Rachlinski, Johnson, Wistrich, & Guthrie, 2009; Landsman & Rakos, 1994). Scenario studies obviously have their limitations, yet I maintain that for revealing punishment recommendations, this method is very useful. Note that legal cases are almost always presented in the form of scenarios to judges and juries. Notwithstanding, I do not undermine the importance of future research in addressing these issues by using more diverse samples and research methods.

On a final note, the foregoing results are important not only from a theoretical but also from an applied perspective. Legal systems are often rooted in the premise that punishments should be proportional to the harm caused. However, the harmfulness of an unethical act is evaluated differently when crimes are judged jointly or separately. In separate evaluation, people seem to focus on the consequences of the victims while

in joint evaluation people are primarily guided by the harmful consequences of a crime in absolute terms (independent of the consequences to victims). Hence, in separate evaluation, people may be vulnerable to the insured victim effect or other biases. It is important to realize that in real life situations, judges or jury members are usually in a separate rather than in a joint condition. Legal policy makers should be aware that people in separate evaluation are more easily swayed by legally irrelevant details (such as the insurance status of victims) when sentencing perpetrators.

Chapter 3

The hidden cost of insurance

Abstract: To trust is to risk and a common solution to mitigate risk is to buy insurance. Employing the trust game (e.g., Berg, Dickhaut, & McCabe, 1995), I demonstrate that buying insurance against the breach of trust may have a hidden cost: Trustees are more likely to act opportunistically and betray trust when the other party chooses to be insured against the risk of betrayal. Supposedly, trustees are less likely to cooperate when trustors decide to be insured because by choosing insurance, trustors implicitly signal that they expect the trustee to behave opportunistically, paradoxically encouraging trustees not to cooperate. These results shed new light on the potential weakness of financial safeguards that are intended to minimize the risky nature of trusting: The presumed safeguard against the risk of betrayal may, under certain circumstances, increase the probability of betrayal.

This chapter is based on: Van de Calseyde, P.P.F.M., Keren, G., & Zeelenberg, M. (2013b). The hidden cost of insurance on trust and reciprocity. *Manuscript under review.*

Social exchange is typically characterized by risk, especially when the different parties have to trust each other. In particular, there are many situations in which cooperation would be the most mutually beneficial outcome for both sides yet there are incentives for the trusted party to defect. For example, a buyer on eBay pays a seller in advance, only to receive the good in the future. The seller is often a stranger who has an incentive to behave opportunistically by sending nothing because not cooperating is rewarding. Standard economic theory assumes that agents are exclusively motivated by their own self-interest (e.g., Fehr & Fischbacher, 2002) and models of trust (in particular game theoretic ones) therefore suggest that individuals should be cautious (e.g., Rempel, Holmes, & Zanna, 1985).

A possible solution to mitigate problems of trust and the corresponding risk is to buy insurance that will compensate losses if trustees decide to act opportunistically. For example, contracts often specify whether one party decided to be insured in the event the exchange partner does not meet its obligations. Yet, how would an exchange partner respond when reading that the other party decided to obtain insurance? Following a signaling perspective on trust and reciprocity (McCabe, Rigdon, & Smith, 2003; Bacharach & Gambetta, 2001), I propose that protecting oneself against the possibility of betrayal is perceived as an act of distrust. By insuring themselves, trustors implicitly signal that they expect the other side to behave opportunistically. Importantly, the results suggest that trustees reciprocate these negative expectations by reducing their willingness to cooperate. The presumed remedy against the risk of betrayal may thus paradoxically increase the probability of betrayal. I report four experiments that investigate when and why an insured trustor affects a trustee's willingness to cooperate.

The paper is organized as follows. I start by examining the pertinent literature on how trustors' actions may conceal implicit signals that affect the willingness of trustees to cooperate. Second, I discuss how other control mechanisms intended to minimize risk decrease interpersonal cooperation in trust situations. Experimental tests of the conjectures are described in the third section. The last section provides a broader discussion of the results and their implications.

Trust in signs

Rousseau, Sitkin, Burt, and Camerer (1998) conceptualize trust as "a psychological state compromising the intention to accept vulnerability based upon positive expectations of the intentions or behavior of another" (p. 395). The "trust game" (Berg, Dickhaut, & McCabe, 1995; Dasgupta, 1988; Wrightsman, 1966) is a common experimental paradigm for studying two person interactions. A typical trust game, as depicted in Figure 3.1 in the appendix (p. 72), is an anonymous one-shot game in which people in the role of player 1 have to decide between two possible actions: if they do not trust the other person (and move left), the game ends, leaving both parties with a moderate reward. If they do trust (and move right), player 2 subsequently has to choose between two possible options: reciprocating ("honoring") trust by moving right, leaving both persons better off than when player 1 did not trust, or betraying trust (moving left), which maximizes personal gain for player 2 at the expense of player 1. Note that the depicted figure contains the words no trust, trust, betray trust, and honor trust, yet these were only included for clarification.

In other versions of the trust game, the decision of player 1 can be how much out of an endowment he or she sends, and similarly, for player 2 it can be a decision how much to send back. Importantly, the underlying dimensions of trusting and honoring remain the same in these slightly different games. Although not compatible with standard economic theory, research consistently finds that the majority of people trust, and correspondingly the majority in turn reciprocate trust, also in anonymous one-shot games (as opposed to repeated trust games) in which reputational concerns are not at stake (e.g., Berg et al., 1995).

A question that remains open concerns the conditions that may enhance and explain trustees' cooperative behavior leading them to reciprocate trust and sacrifice personal gain. A recent approach in explaining reciprocity emphasizes the role of signaling intentions and expectations in achieving a cooperative outcome (e.g., McCabe et al., 2003). This approach conceives the social interaction, as reflected in the trust game, as a conversational exchange. An act of trust (i.e., forgoing a sure though smaller outcome) is assumed to implicitly inform a trustee that the trustor wants to cooperate and arrive at a mutually beneficial outcome. Trustees are said to understand these concealed intentions and feel obliged to reciprocate. One consequence of this approach is that different trustees who face identical choice options may differ in their willingness to cooperate when the 'trusting act' signals different expectations. For example, McCabe et al. (2003) found that trustees were more likely to honor trust when trust was voluntary (in the sense that the trustor also had the opportunity not to trust) as opposed to involuntary. In explaining this difference, the authors argued that a *voluntary* act of trust has the ability to signal that a trustor wants to arrive at the mutually beneficial outcome. An *involuntary* act of trust is unable to signal such

intentions since the trustor had no choice but to cooperate. Thus, although trustees may face objectively identical choice options, they nevertheless may ascribe different intentions to the trustor depending on the signals conveyed by the trustee.

Research by Pillutla, Malhotra, and Murnigham (2003) provides further support for the idea that players interpret the behavior of other players as signals of their intentions. They found that reciprocation followed rather categorical perceptions of trust. In their experiment, trustors decided how much out of an endowment to send to the trustee. The amount sent tripled on its way and a trustee subsequently decided how much (if any) to return to the trustor. The results showed that sending anything less than the entire endowment was perceived as a sign of distrust. As a consequence, trustees reciprocated large acts of trust while small acts of trust made reciprocation unlikely. Taken together, these studies suggest that trustees are conditionally kind to trustors. That is, any sign of distrust may be sufficient to undermine a trustee's voluntary willingness to act in the interest of the trustor. In the next section I review relevant literature on how risk controlling devices that are indented to minimize risk may undermine cooperation by signaling distrust.

When controlling a trustee undermines cooperation by signaling distrust

Like insurance, legal control mechanisms (e.g., contracts or penalties) have become a common practice to minimize the risky nature of social exchange situations. Unlike insurance, however, the detrimental effects of these control mechanisms on trust and reciprocity have been well documented (e.g., Malhotra & Murnighan, 2002; Fehr & List, 2002; Fehr &

Rockenbach, 2004; Mulder, Van Dijk, De Cremer, & Wilke, 2006; Falk & Kosfeld, 2004).

When two parties lack a history of cooperation, a contract that specifies the agreement and the sanctions imposed on those who breach the agreement makes sense. Yet, specifying the agreement 'in writing' has an undesirable side effect: It signals a lack of trust. After all, if party A trusts party B, there is no need to constrain the actions of party B by threatening with sanctions. Indeed, a number of experiments indicate that threatening to penalize trustees when performances fall short may harm the relationship. For example, in testing the effects of imposing a fine on cooperation, Fehr and Rockenbach (2003) carried out a trust game in which a trustor decided how much out of an endowment to send to a trustee who, in turn, decided how much to return from a larger endowment. Yet, before the trustee decided how much to return, a trustor could choose to penalize the trustee if the return would not exceed a certain amount. Of course, the trustor could also refrain from imposing the fine. Since returning any amount is costly for a trustee, standard economic theory predicts that a trustee will return nothing. Threatening to fine trustees, by making it costly to defect, is therefore optimal. However, if there are trustees who are motivated to perform in the interest of the trustor, the threat may actually hinder cooperation. The results of Fehr and Rockenbach indeed showed that *not* imposing a fine rendered a much higher return as compared to when a fine was imposed. When a fine was imposed, trustees retuned on average just enough to avoid the fine, but not more, leaving a trustor relatively empty handed.

More support for the idea that control mechanisms may undermine cooperation by signaling distrust comes from a set of studies by Falk and Kosfeld (2006). Their studies examined whether demanding a minimum performance level of trustees

(e.g., labor contracts that specify a minimum output) would undermine a trustee's voluntary performance level. In a principal-agent game, agents were endowed with a given amount of money and had to decide how much (if any) to share with a principal. However, before an agent made his decision, principals could first choose to restrict (i.e., control) the choice set of an agent by demanding a minimum payoff. Although the minimum payoff was fixed and rather low, it would guarantee a principal with a certain outcome. Since sharing is costly to an agent, standard economic theory predicts that an agent would share nothing. Principals are therefore better off restricting the choice set of an agent rather than to trust the agent to perform in the interest of the principal. The results showed that trusting, rather than controlling an agent, rendered a higher return. In explaining these results, Falk and Kosfeld claimed and demonstrated that agents view demanding a minimum payoff as a signal of distrust, which in turn undermined voluntary contributions.

These results suggest that introducing control devices that are intended to minimize the risky nature of social interactions may, in fact, hinder cooperation by signaling distrust. I employ a similar reasoning in explaining why insurance may hinder cooperation by signaling distrust.

The insured trustor

In essence, an insurance policy is a safety instrument by which a third party (the insurance company) undertakes to compensate an insured party for losses that may be incurred by misfortunes (for a comprehensive review of behavioral economics research on insurance, see Kunreuther, Pauly, & McMorrow, 2013). Insurance thus changes the severity of an unfortunate outcome by changing the outcome of the insured

individual in the event that a specific hazard occurs. Importantly, while contracts and insurance policies are both designed to minimize risk, they do so in very different ways. Contracts and fines are designed to mandate cooperation by making it unattractive to behave opportunistically and not to cooperate. These mechanisms thus minimize risk by increasing the likelihood of cooperation because it becomes costly for the other party not to cooperate. An insurance policy, on the other hand, does not restrict the actions of interaction partners. Yet, it minimizes risk by transferring the risk of loss from an insured individual to the insurance company. How would trustees respond to trustors who chose to be insured against the consequences of a trustee's most opportunistic act?

Because defection is rewarding, trustees may act opportunistically leaving a trustor with less than what she would gain had she not chosen to trust. Trustors who anticipate a trustee to act selfish are therefore better off insuring themselves than not. However, following a signaling perspective, I conjecture that by insuring oneself, the trustor (implicitly) signals distrust, expecting the trustee to behave opportunistically. Given that trustees respond negatively to signs of distrust (e.g., Falk & Kosfeld, 2004; Fehr & Rockenbach, 2004), the insurance policy may do more harm than good. If there are trustees who are motivated to act in the interest of the trustor, the insurance policy may actually discourage cooperation. Thus, under certain circumstances, the presumed remedy against the risk of betrayal may in fact increase the probability of betrayal.

The following experiments were designed to test how trustees perceive and respond to trustors who are insured against the consequences of betrayal in one-shot trust games. Experiment 3.1 provides initial support for the hypothesized relationship between a trustor who is insured (as opposed to

uninsured) and the predicted decrease in trustees' readiness to cooperate. Experiment 3.2 extends these findings by ruling out that the observed decrease can be accounted for by a mere change in the payoff structure for a trustor who is insured as opposed to uninsured. Experiment 3.3 examined whether it is *being* insured that drives the effect (independent of whether the trustor chose to be insured or not) or whether *choosing* insurance over no insurance explains our findings. Evidently, cooperation decreased but only when trustors were insured by choice and not by chance. Finally, Experiment 3.4 extends these findings by showing that (1) trustors understand that choosing insurance hinders cooperation, yet (2) they nonetheless prefer to be insured as opposed to uninsured when presented with the choice.

Experiment 3.1

Participants in the role of player 2 were told that player 1 chose to be insured or not. Being insured in our experimental game meant that if player 2 would decide to defect, player 1 would only lose a moderate amount of money (instead of a larger sum when uninsured). Given that choosing to be insured implicitly signals distrust, I predicted a significant decrease in cooperation by player 2 when they learned that player 1 chose to be insured.

Method

One hundred and fifty seven students (41 male, 116 female) at Tilburg University participated in exchange for course credit or €8,- (M_{age} = 21, SD = 2.51). The current study was part of a set of unrelated studies and the study was run in two non-consecutive weeks.[1] Participants were randomly

[1] I initially planned to run the study in one week, yet I only ran (approximately) 80 participants. Because the results were at the margin of significance, I decided to run another week to

assigned to one of two conditions (Trustor: Insured vs. Uninsured). Upon arrival in the lab, participants were seated in separate cubicles, ostensibly to preserve anonymity. They received instructions on the computer screen explaining that they would make a number of financial choices together with another participant. Subsequently, using a chart as in Figure 3.2 (see appendix, p. 73), they received instructions regarding the rules of the two-person interaction. Specifically, they were told that player 1 first had to decide whether to choose left or right. If player 1 chose left, the game would end and each player would receive €5,-. If, however, player 1 chose right, player 2 was given the option to choose between moving left or right. Before player 2 could make his decision, player 1 still had the option to insure herself. Insurance implied that player 1 would receive €4,- if player 2 decided to defect. When uninsured, player 1 would receive €2,- if player 2 would defect. All other outcomes were kept identical. They were further explicitly informed that of all couples that participated in the experiment that week, 10 would be randomly chosen and paid according to the decisions they made. Participants read the instructions without any time pressure and were given the opportunity to ask clarification questions at any time.

Subsequently, participants learned that they were assigned the role of player 2 and their counterpart the role of player 1 (which, in reality, was a preprogrammed strategy), that they would interact via a network computer, and that all decisions would be displayed on the computer screen. In addition, they were explicitly informed that they would never meet or know the identity of their counterparts during or after the interaction. After these instructions, participants were asked to wait for player 1's decisions.

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increase power. Note, that in both Experiments 3.2 and 3.3 I again replicated the same pattern of results thus corroborating the results of the present experiment.

All participants saw that player 1 chose right (i.e., trust) thus leaving the final decision with the participant on how to split the larger sum. Yet, before they could make their decision, they were informed about player 1's insurance decision. Depending on condition, player 1 either decided to insure him/herself or not. After having seen player's 1 decision, participants indicated whether they would split the larger sum of money in an equal (i.e., honor trust) or unequal (betray trust) manner. Ten participants were randomly chosen by the end of the experiment and were paid according to their choice.

Results and discussion

As predicted, participants were less likely to cooperate when trustors chose to be insured as opposed to uninsured. Whereas a minority of participants (45% or 36 out of 80) decided to split the money equally when trustors were insured, significantly more participants favored this option (62% or 48 out of 77) when trustors were uninsured, χ^2 (1, N = 157) = 4.74, p = .03, φ = .17. These initial results support a signaling perspective on trust and reciprocity (e.g., McCabe et al., 2003). Choosing insurance over no insurance is conceived to signal distrust which, in turn, decreases the willingness of trustees to cooperate and act in a pro-social manner.

Note that by manipulating the insurance choice, I also slightly varied the payoffs for player 1. Specifically, although defection would endow the participant (in the role of player 2) in both conditions with €15,-, this option left player 1 with €4,- when insured and €2,- when uninsured. According to models of inequity aversion (e.g., Fehr & Schmidt, 1999; Loewenstein, Thompson, & Bazerman, 1989), the greater the relative difference between one's own outcome and the outcome of the other player, the more aversive the non-cooperative option becomes. An alternative explanation for the observed decrease

in cooperation could therefore be the reduced inequality rather than a change in signaling distrust when the trustor chose to be insured.

The following experiment addressed this issue by including baseline conditions that are equivalent in both experimental conditions in term of payoffs, without mentioning the possibility of insurance. Because 'outcome based models', like inequality aversion, exclusively focus on payoffs in explaining cooperation, these models would therefore predict no differences in cooperation rates between baseline- and experimental conditions. A signaling perspective, however, predicts an interaction in that cooperation will only decrease when the trustor chooses to be insured. A second objective of Experiment 3.2 was to assess whether the insurance decision was indeed perceived as an act of distrust. As mentioned in the introduction, I propose that the effect of insurance in decreasing cooperation is related to perceptions of distrust.

Experiment 3.2

Method

Two hundred and forty seven students (70 male, 177 female) at Tilburg University participated in exchange for course credit or €8,- (M_{age} = 20, SD = 3.12). The current experiment was part of a set of unrelated studies. The procedure was identical to Experiment 3.1, except that in the baseline conditions, the insurance option was deleted. Participants were randomly assigned to one of four conditions based on a 2 (Game Type: Insurance vs. Baseline) × 2 (Worst Possible Outcome for Trustor: €2,- vs. €4,-) between-subjects design. The game was explained to participants by exposing them to Figure 3.2 or 3.3 (see appendix, p 73 and p. 74), depending on condition.

Trust perceptions. After being informed about player 1's decisions, participants were asked four-questions concerning the other player's trust. Specifically, they were asked to indicate the extent (1 = *not at all*, 7 = *very*) they believed that (1) player 1 trusted the participant; (2) player 1 expects the participant to choose left (reversed coded); (3) the participant has the impression that player 1 does not trust (reversed coded); (4) player 1 doubts whether the participant's intentions are good (reversed coded). Responses were averaged into a 'trust perception' scale ($\alpha = .80$).

Cooperation. Participants were subsequently asked to choose how they wanted to split the larger sum of money (i.e., equal split versus unequal split). Ten (randomly chosen) participants were paid by the end of the experiment according to their choice.

Results and discussion

Trust perceptions. The results for the trust scale are shown in Table 3.1 (top row). A 2 (Game Type) x 2 (Worst Possible Outcome) ANOVA on the trust perceptions rating revealed a main effect for game type, $F(1, 243) = 35.62$, $p < .001$, $\eta^2 = .12$ and worst possible outcome, $F(1, 243) = 66.04$, $p < .001$, $\eta^2 = .21$. These main effects were qualified by a significant two-way interaction, $F(1, 243) = 55.60$, $p < .001$, $\eta^2 = .19$. Trustors who chose to be insured were seen as less trusting than those who chose to be uninsured, $t(118) = 9.33$, $p < .001$, $d = 1.70$. Comparing the two baseline conditions (where insurance was not an option), trustors were not seen as more or less trusting, $t < 1$. This pattern of results strongly suggest that the act of choosing insurance over no insurance signals distrust. Next I examine whether trustees reciprocate these expectations by decreasing one's willingness to cooperate.

Cooperation. The percentages of participants who reciprocated trust are portrayed in the second column of Table 3.1. Cooperation among participants again decreased when trustors chose to be insured (50% or 30 out of 60) as opposed to uninsured (74% or 46 out of 62), χ^2 (1, N = 120) = 10.47, p = .001, φ = .30. To test whether this decrease is influenced by the act of *choosing* insurance (and not by differential payoffs), a logistic regression analysis was performed, using a 2 (Game Type) x 2 (Worst Possible Outcome) design. There was a main effect for worst possible outcome, χ^2(1, N = 247) = 1.29, p = .002, φ = .07, but no main effect for game type, χ^2(1, N = 247) = .23, p = .59, φ = .03. Importantly, the predicted interaction between game type and worst possible outcome was observed, χ^2 (1, N = 247) = 3.83, p = .05, φ = .12. Results show that cooperation rates only decreased under the condition in which the trustors chose to be insured as compared to baseline participants, χ^2(1, N = 125) = 5.65, p = .02, φ = .21. Participants did not differ in their cooperation rates when trustors chose to be uninsured as compared to baseline participants, χ^2< 1. These results replicate the results of Experiment 3.1 and further corroborate the main hypothesis.

Earnings. When analyzing the earnings, I compared the experimental conditions with the corresponding baseline conditions because these conditions were identical in payoff structure. On average, participants earned more for themselves (Table 3.1, 3rd row) when the trustor decided to be insured as opposed to participants in the baseline insured condition, t (123) = 2.41, p = .02, d = 0.43. Participants earned neither more nor less when a trustor chose to be uninsured as compared to participants in the baseline uninsured condition, t < 1. Likewise, as portrayed in the final column of Table 3.1, being insured would have earned trustors significantly less as compared to trustors in the baseline insured condition.

Uninsured trustors would not have earned differently from trustors in the baseline, $t < 1$.[2] Although standard economic theory assumes that trustors are better off insuring themselves than not, our result suggest that choosing insurance may in fact leave the trustor relatively worse off. Overall, these results suggest that choosing insurance over no insurance hinders cooperation by decreasing a trustee's willingness to cooperate and act in the interest of the trustor. These results cannot be explained by theoretical accounts that focus exclusively on a comparison of outcomes in explaining trustees' decisions.

Mediation analysis. Earlier it was proposed that insurance leads to distrust which in turn reduces the likelihood for cooperation. To test this conjecture, I ran a bootstrap analysis (Preacher and Hayes, 2008) with 5000 bootstrapped samples. Consistent with our interpretation, when trust perceptions were entered in the bootstrapped model, it comprised a significant mediator of the effect of insurance on cooperation, $B = 0.35$, $Z = 2.48$, $p = .01$.

The results so far demonstrate that cooperation in the trust game is contingent on the signals of (dis)trust that trustees perceive in the choice to be insured. Yet, being insured may affect trustees in another distinct way: Because the trustor is insured, the trustee may be less reluctant not to cooperate realizing that the trustor will be compensated by the insurance. Following this account, trustees might decrease their willingness to cooperate because the trustor is insured, irrespective of whether he/she deliberately decides to be insured or not. Experiment 3.3 was designed to disentangle whether *being* insured per-se decreases cooperation (independent of the trustor's insurance decision), or whether the trustor's intentional choice for insurance determines the trustee's behavior. I address this issue by including two

[2] Note that the trustor in the experimental design was a preprogrammed player.

additional conditions to the experimental design in which the trustor is insured or not by chance (as opposed to by choice). As a final objective, I also measured whether choosing insurance over no insurance would influence interpersonal judgments (i.e., first impressions) besides interpersonal choices.

Table 3.1
Trust Perceptions, Percentage of Cooperative Choices, and the Payoffs of the Participant and Trustor in the Different Conditions, Experiment 3.2.

Condition	Insured	Uninsured	Baseline insured	Baseline uninsured
Trust	3.97 (1.32)	5.93 (0.95)	5.66 (0.82)	5.75 (0.80)
Cooperation	50% (30/60)	78% (47/60)	71% (46/65)	74% (46/62)
Payoff pp	12.50 (2.52)	11.08 (2.08)	11.46 (2.29)	11.29 (2.21)
Payoff trustor	7.00 (3.03)	8.27 (3.32)	8.25 (2.75)	8.06 (3.45)

Note: Trust perceptions are assessed on a 4-item 7-point scale, with higher scores indicating more perceived trust. Cooperation is the percentage (frequency within parentheses) of participants that honor the trust given by player 1. A higher payoff for the participant indicates a lower willingness to equally split the larger sum of money with the trustor (a preprogrammed player in our experimental design). Payoffs are in euros.

Experiment 3.3

Method

Two hundred seventy four students (66 male, 208 female; M_{age} = 20, SD = 2.68) at Tilburg University participated in exchange for course credit or €8, -. The current study was part of a set of unrelated studies. Participants were randomly assigned to one of four conditions based on a 2 (Trustor Insured vs. Uninsured) × 2 (Insurance by Choice vs. by Chance) between-subjects design. The procedure was identical to the

one employed in Experiment 3.1, except that, depending on condition, player 1 was insured or uninsured by deliberate choice or by a computer programmed random device. Participants were instructed about the nature of the two-person interaction by using a copy of Figure 3.2 or 3.4 (depending on condition; see appendix, p 73 and p. 75). After being informed about player 1's decisions, participants responded to the four-item trust scale from Experiment 3.2 (α = .87). Participants subsequently indicated how they wanted to split the money (i.e., equal split versus unequal split). Finally, participants were asked to indicate whether player 1 made a positive first impression (1 = *not at all*, 7 = *very*). Ten randomly chosen participants were paid by the end of the experiment according to their choice.

Results and discussion

Trust perceptions. The results for the trust scale are shown in Table 3.2 (top row). A 2 (Trustor Insured vs. Uninsured) x 2 (Insurance by Choice vs. by Chance) ANOVA on the trust perception rating revealed a main effect for whether the trustor was insured or not, $F(1,270) = 94.54$, $p < .001$, $\eta^2 = .26$ and whether the (un)insurance decision was by choice or chance, $F(1,270) = 52.85$, $p < .001$, $\eta^2 = .16$. Importantly, these main effects were qualified by a significant two-way interaction, $F(1,270) = 92.65$, $p < .001$, $\eta^2 = .26$. Replicating the findings of Experiment 3.2, trustors who were insured by choice were perceived as less trusting than trustors who were uninsured by choice, $t(146) = 12.56$, $p < .001$, $d = 2.06$. Trustors who were insured by chance were not seen as more or less trusting as compared to trustors who were uninsured by chance, $t < 1$. Once again, these results strongly suggest that it is the act of *choosing* insurance over no insurance that signals

distrust. Would trustees again reciprocate these negative expectations by decreasing their willingness to cooperate?

Cooperation. Replicating the results of Experiment 3.1 and 3.2, participants cooperated less when trustors chose to be insured as opposed to being uninsured, $\chi^2(1, N = 148) = 6.15$, $p = .01$, $\varphi = .20$. In testing whether this difference is influenced by the act of *choosing* insurance or *being* insured, a logistic regression was performed, using a 2 (Trustor Insured vs. Uninsured) x 2 (Insurance by Choice vs. by Chance) design. There was a main effect for whether a trustor was (un)insured by choice or chance, χ^2 (1, N = 274) = 1.02, $p = .005$, $\varphi = .06$, but no main effect for whether the trustor was insured or not, χ^2 (1, N = 274) = .30, $p = .44$, $\varphi = .03$. More importantly, the predicted interaction was obtained, χ^2 (1, N = 274) = 4.89, $p = .03$, $\varphi = .06$. Cooperation frequencies did not differ when trustors were uninsured by choice or by chance, $\chi^2 < 1$. However, as predicted, cooperation rates only decreased when trustors were insured by choice as compared to being insured by chance, $\chi^2(1, N = 136) = 7.94$, $p = .005$, $\varphi = .24$.

Earnings. On average, participants earned more for themselves (Table 3.2, 3rd row) when the trustor decided to be insured by choice as opposed to when the trustor was insured by chance, t (134) = 2.88, $p = .005$, $d = 0.48$. Participants earnings were the same regardless of whether a trustor was uninsured by choice or chance, $t < 1$. Likewise, as portrayed in the fourth column of Table 3.2, being insured by choice again left the trustor relatively empty handed as compared to trustors who were insured by chance. Trustors who were uninsured by choice would not have earned more or less compared to trustors who were uninsured by chance, $t < 1$. In sum, trustees do not maximize personal gain per-se when trustors are insured ruling out the explanation that the mere presence of an insured other would facilitate a non-cooperative

choice. Consistent with the signaling perspective, however, it is rather the act of choosing to insure oneself that is negatively reciprocated.

Mediation analysis. To examine whether I would replicate that the observed decrease in cooperation can be accounted for by the change in trust perceptions, I again ran a bootstrap analysis with 5000 bootstrapped samples. Replicating the findings of Experiment 2, the results suggest that the effect is again (partially at least) mediated by the change in perceived trust, $B = 0.23$, $Z = 1.78$, $p = .04$ (one-tailed).

Interpersonal judgment. Finally, the results for the 'first impression' scale are shown in the last row of Table 3.2. Trustors who were insured by choice made a less positive first impression than trustors who were insured by chance, $t\ (134) = 5.03$, $p < .001$, $d = 1.18$. Trustors who were uninsured by choice as compared to by chance, were not perceived more or less positively, $t < 1$. These results suggest that choosing insurance over no insurance not only hinders interpersonal cooperation, but also affects interpersonal evaluations.

Table 3.2
Trust Perceptions, Percentage of Cooperative Choices, the Payoffs of the Participant and Trustor, and Interpersonal Judgments, Experiment 3.3.

Condition	Insured (Choice)	Uninsured (Choice)	Insured (Chance)	Uninsured (Chance)
Trust	3.47 (1.31)	5.83 (0.95)	5.53 (0.79)	5.54 (0.84)
Cooperation	49% (37/73)	69% (52/75)	73% (46/63)	67% (42/63)
Payoff pp	12.53 (2.52)	11.53 (2.32)	11.35 (2.24)	11.67 (2.38)
Payoff trustor	6.96 (3.02)	7.55 (3.71)	8.38 (2.68)	7.67 (1.37)
Judgments	3.84 (1.50)	4.79 (1.73)	5.02 (1.18)	4.67 (1.37)

Note: Trust perceptions are assessed on a 4-item 7-point scale, with higher scores indicating more perceived trust. Cooperation is the percentage (frequency within parentheses) of players 2 that honor the trust given by player 1. A higher payoff for the participant (player 2) indicates a lower willingness to equally split the larger sum of money. Note that the trustor was a preprogrammed player in our experimental design. Interpersonal judgments are assessed on a one-item 7-point scale, with higher scores indicating a more positive first impression.

Experiment 3.4

The previous experiments demonstrate that trustees are less inclined to cooperate when trustors are insured by choice. The final experiment was designed to explore whether people in the role of trustor would (1) realize that that a trustee is more likely to act opportunistically when a trustor chooses to be insured, and (2) whether they would actually choose to be insured or not when given the choice.

Method

Eighty-two students (22 male, 60 female; M_{age} = 21, SD = 3.51) at Tilburg University participated in exchange for course credit or €8, -. The procedure was identical to the one employed in Experiment 3.1, except that participants were now asked to take the role of player 1 (trustor) instead of player 2.

They were instructed about the nature of the two-person interaction by using a copy of Figure 3.2 (see appendix, p. 73). After being instructed, participants were first asked to choose either left (i.e., trust) or right (distrust). Those who chose to trust were subsequently asked (1) whether they wanted to be insured or not and (2) to indicate their belief in how being insured may affect the choice of player 2. Specifically, they were asked: "Is player 2 more likely to choose left when you decide to be insured or uninsured?" Order of presenting the insurance choice and the belief question was counterbalanced. Participants subsequently learned that player 2 (a preprogrammed player) chose to equally split the larger sum. At the end of the experiment, 10 participants were randomly picked and paid according to their choice.

Results and discussion

Insurance beliefs. Of all participants, 27 (33%) chose not to trust, leaving a total of 55 participants (67%) for further analysis. Of these 55 participants, a large majority (82% or 45 out of 55) believed that player 2 is less likely to cooperate when choosing to be insured as opposed to uninsured, χ^2 (1, N = 55) = 22.27, $p < .001$, $\varphi = .64$. This belief was slightly stronger when it was assessed *after* having made one's insurance choice first (93% or 26 out of 28). When this belief was assessed *before* making the insurance choice, 70% (19 out of 27) thought that player 2 is less likely to cooperate when choosing to be insured, χ^2 (1, N = 55) = 4.67, $p < .03$, $\varphi = .29$.

Insurance decision. Of the 55 participants, 42 (76%) chose to be insured as opposed to uninsured, χ^2 (1, N = 55) = 15.29, $p < .001$, $\varphi = .53$. There was no significant order effect, χ^2 (1, N = 55) = 2.29, $p = .13$, $\varphi = .12$. Specifically, 19 participants chose to be insured (68%) when the insurance choice came first while

85% (23 out of 27) decided to be insured when the insurance choice came second.

In sum, although our previous results demonstrate that trustees respond negatively to trustors who are insured by choice, the results of this experiment suggest that (1) although a large majority of participants may realize that trustees are less likely to cooperate when choosing to be insured, (2) they nonetheless prefer to be insured against the risk of betrayal when given the opportunity. These results are compatible with the idea that acts of trust are primarily arise from egocentric reasoning while failing to take the trustee's incentives to defect into full consideration (Evan & Krueger, 2011; Malhotra, 2004; Snijders & Keren, 1999).

General discussion

To trust is to risk and a common solution to mitigate problems of risk is to buy insurance. In this article I highlight a hidden cost of insurance in situations requiring trust: Trustees are less likely to cooperate when a trusting party *chooses* to be insured against the risk of betrayal. Employing the trust game (e.g., Berg et al., 1995; Dasgupta, 1988), the results of four experiments established the existence of the effect and provided support for a signaling perspective on trust and reciprocity: By insuring oneself, a trustor implicitly signals that she expects the trustee to behave opportunistically. Trustees, in turn, reciprocate these negative expectations by decreasing their willingness to cooperate, leaving the insured trustor relatively empty handed. Experiment 3.1 showed that trustees indeed become less inclined to cooperate when a trustor chose to be insured against a trustee's most opportunistic act. Experiment 3.2 extended these findings by ruling out that the observed decrease in cooperation can be accounted for by a

mere change in the payoff structure for trustors who are insured. Moreover, Experiment 3.3 demonstrated that trustees only became less willing to cooperate when the trustor was insured by choice and not by chance. In other words, it is the deliberate choice of insurance (or refraining from it) that is interpreted by the trustee as a signal. Overall, these results are compatible with the idea that protecting oneself (by choice) against the possibility of betrayal is perceived as an act of distrust. Indeed, meditational analyses of both Experiment 3.2 and 3.3 further supports the assertion that choosing insurance provides a signal that the trustor expects the trustee to act opportunistically which, in turn, decreases a trustee's willingness to cooperate. Finally, Experiment 3.4 showed that (1) although trustors realize that choosing to be insured may decrease a trustee's willingness to cooperate (2) they nonetheless choose to be insured as opposed to uninsured when given the opportunity. In sum, although standard economic theory assumes that trustors are better off insuring themselves than not, our results suggest that choosing insurance over no insurance will eventually leave the insured trustor relatively worse off.

The present investigation contributes to a stream of research investigating how control mechanisms (e.g., contracts or sanctions) that are designed to control risk, interfere with developing a trusting relationship. For example, as discussed in the introduction, Falk and Kosfeld (2004) showed that the choice to control a trustee by demanding a minimum performance level (e.g., binding contracts in an employer-employee relationship) is perceived as a sign of distrust which, in turn, undermines a trustee's motivation to act in the interest of the trustor. In addition, Malhotra and Murnigham (2002) showed similar effects: Proposing a binding contract to mandate cooperation interfered with developing a mutually

beneficial relationship. The current findings represent an important extension of these results. First, although both contracts and insurance policies are designed to minimize risk, they do so in very different ways. Insurance policies minimize risk by transferring losses while contracts minimize risk by making it unattractive for the trustee not to cooperate. I thus provide evidence for the aversive consequences of safeguards on cooperation, yet with a very different safety mechanism. Second, I provide evidence that the hidden cost of insurance on cooperation is driven by the same process that can explain why people become reluctant to cooperate with a person who demands a contract. Choosing to be insured also seems to signal distrust, encouraging trustees not to cooperate.

Our work provides further insight into a stream of research focusing on understanding the intra- and interpersonal consequences of safety mechanisms (Van de Calseyde, Keren, & Zeelenberg, 2013c; Van Wolferen, Inbar, & Zeelenberg, 2013; Tykocinski, 2008, 2013). For example, Walker (2007) has shown that when overtaking a cyclist, drivers are less cautious when cyclists wear a helmet than when they do not. Thus the safety measure of wearing a helmet may ironically attract hazard. Similarly, the present studies demonstrate that being insured against the risk of betrayal may paradoxically increase the probability of betrayal.

Being insured may affect trustees in at least two distinct ways: (1) Because the trustor is insured, a trustee may be less reluctant not to cooperate realizing that the trustor will be compensated by the insurance, or (2) Choosing to be insured signals distrust, which trustees reciprocate by decreasing one's willingness to cooperate. The results of Experiment 3.3 are compatible with the second perspective because we only observed a decrease in cooperation rates when the trustor *chose* to be insured as opposed to when the trustor was insured

by a random device. The first perspective would have predicted no differences on trustees' unwillingness to cooperate.

In terms of theory, the standard economic model (based on value maximization) assumes that the utility of an action exclusively depends on its consequences. Only the intrinsic properties of the outcomes are assumed to drive behavior (McCabe et al., 2003). Yet, I provide evidence that people are also sensitive for the intentions behind an action. This idea is supported by a considerable body of evidence indicating that the attribution of intentions matters in explaining fair- and unfair behavior (e.g., Falk, Fehr, & Fischbacher, 1999; Kahneman, Knetsch, & Thaler, 1986; Malhotra & Murnighan, 2002; Falk & Kosfeld, 2006; McCabe et al., 2003). This is of great importance because it highlights that the principle of value maximization fails to explain why people engage in mutually beneficial, yet risky interactions.

On a final note, the foregoing results are important not only from a theoretical but also from a practical perspective. The use of insurance is both prevalent and pervasive. In many organizations, insurance has become a standardized solution to solve problems of risk. An insurance policy makes it possible for a risk-averse party to engage in a (potentially) beneficial relationship by minimizing the risky nature of the exchange. However, cooperation can also be established by relying on more efficient and less costly mechanisms like trust and reciprocity. Indeed, individuals in high-trust societies spend less to protect themselves from being exploited in transactions (Knack & Keefer, 1997). Yet, for trust and reciprocity to work, risk is needed since reciprocity feeds on the risk that the trustor is willing to take (e.g., McCabe et al., 2003). As a consequence, mechanisms like insurance that minimize the risky nature of trust taking may in fact hinder cooperation based on trust and reciprocity.

Appendix Chapter 3: Figures (3.1-3.4)

Figure 3.1. Simple trust game

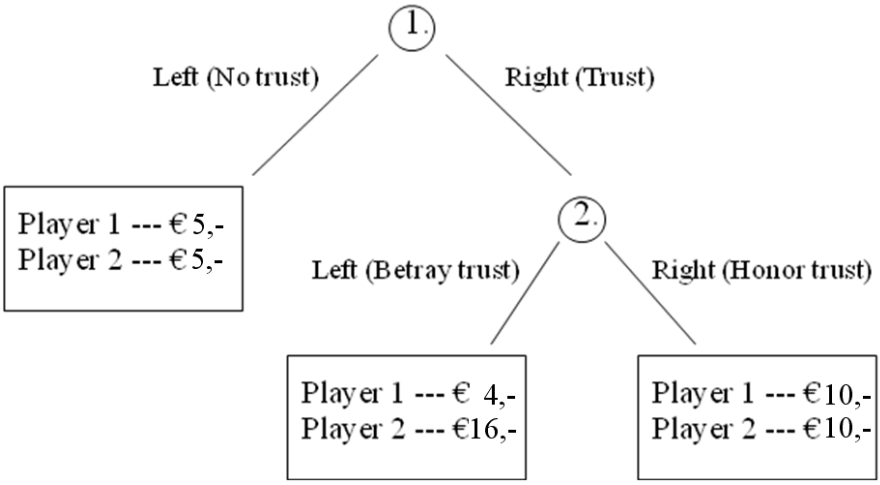


Figure 3.2. Modified trust game in which a trustor has the option to be insured (used in Experiment 3.1, 3.2, 3.3 and 3.4).

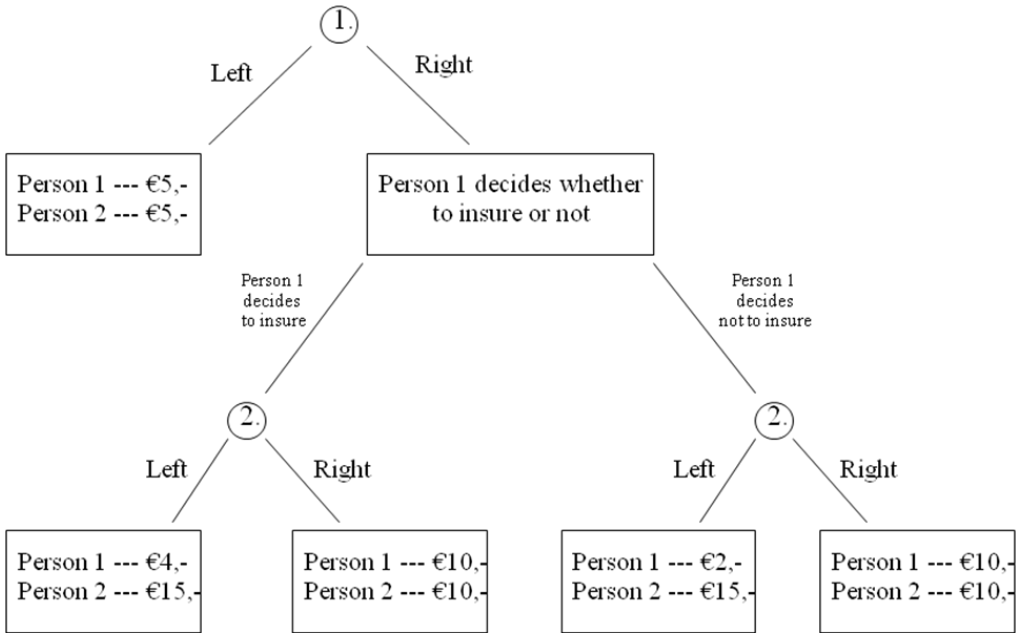


Figure 3.3. Simple trust game with €4,- or €2,- (depending on condition) as worst possible outcome for trustors serving as baseline conditions for the insured and uninsured trustor conditions (Experiment 3.2).

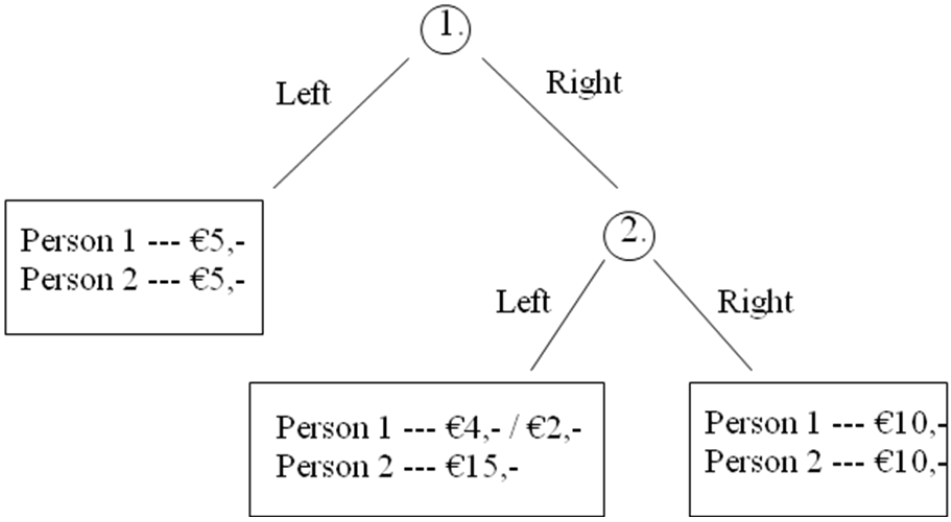
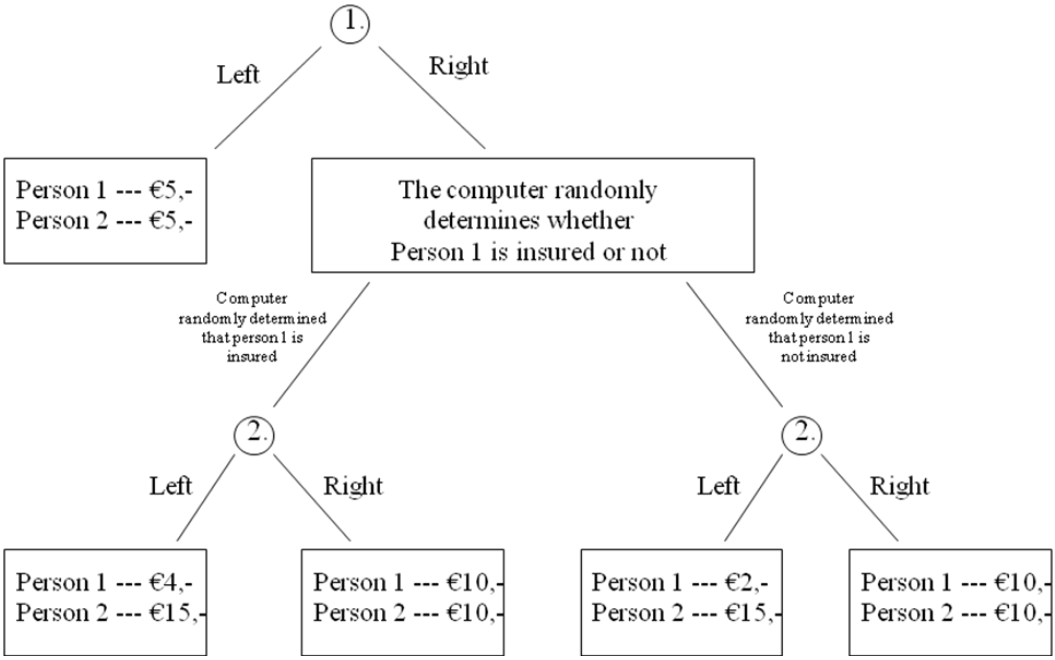


Figure 3.4. Modified trust game in which a random device determines whether a trustor is insured or not (Experiment 3.3).



Chapter 4

Decision time as information

Abstract: Any decision can be characterized by the time with which it is made and the decisions of others are no exception. Following a signaling perspective, I demonstrate in a variety of social interactions that people derive meaning from the time that others need in reaching a decision. Specifically, the findings reveal that the decision time of others is perceived as indicative of the degree of doubt that the decision makers experienced in reaching a decision. These perceptions of doubt, in turn, reliably affected people's preferences like with whom to collaborate and negotiate, even when the collaboration would yield a normatively inferior outcome. These results are incompatible with the assumption that the chosen alternative will be solely determined by the relevant outcomes. I portray a model that incorporates others' decision time as a component of the choice process. Implications for how choices are affected by both outcomes and signals are discussed.

This chapter is based on: Van de Calseyde, P.P.F.M., Keren, G., & Zeelenberg, M. (2013a). Decision time as information in judgment and choice. *Invited revision. Organizational Behavior and Human Decision Processes.*

Decisions are intimately related to various aspects of time (e.g., Ariely & Zakay, 2001). Decisions take time to make and implement, have consequences occurring at different moments in time, and often change over the course of time. Much research has therefore been concerned with understanding how time affects people's decisions.

It is important to distinguish between two different facets in which time and choices (or decisions more generally) interact. One concerns decision time, the time available or needed for making a decision, which may strongly effect the manner by which a decision is reached and the corresponding outcome (e.g., Benson & Beach, 1996; Ordóñez & Benson, 1997; Zakay, 1993). While time pressure may indeed impair the decision process, there is evidence suggesting that under time constraint, decision makers adjust by switching to simpler strategies (e.g., Payne, Bettman & Johnson, 1993; Weenig & Maarleveld, 2002). Another line of research in this context concerns evaluation in retrospect of decisions made in haste or under time pressure, indicating that people regret their choices more when having a limited amount of time to choose between a set of options (e.g., Inbar, Botti, & Hanko, 2011).

The second facet in which time and choices interact concerns decisions about time. The classical question in this context concerns intertemporal choice: Extensive research suggests that, other things being equal, people have a tendency to prefer immediate rewards over larger, delayed ones, affecting people's health and wealth (e.g., Ainslie, 1991; Keren & Roelofsma, 1995; Loewenstein, 1988; Thaler, 1981). Two major theoretical approaches to time preferences are discounted utility theory (Loewenstein, 1992) and construal level theory (Liberman & Trope, 1998).

The present research is related to the first facet namely decision time and investigates an aspect of time in choice that,

surprisingly, received little attention in the relevant literature. Most of the research of interest looked at decision time and its effects on the decision process from the decision maker's point of view. In the current article I take a different perspective and examine how the time that *others* need to reach decisions affects a person's choice in interdependent situations. Specifically, people are often sensitized to the time that others need in reaching a decision as it may provide important cues about the other, for instance the degree of doubt the person experienced while making the decision. I propose that in many social interactions, these perceptions of doubt may have strong implications for people's subsequent choices, sometimes even resulting in people choosing normatively inferior options. For example, imagine having had job interviews at company A and company B where both interviews went well. The position at company A offers an annual salary of $40.000, while that at company B offers $41.000. Suppose it is customary that a company decides whether to hire an applicant or not within five days following the interview. Company A immediately decides to offer you the job. Company B takes five days to decide to offer you the job. Which offer would you accept? Following the principle of value maximization, the answer should be simple: You take the job at company B, with the highest annual salary. Following a signaling perspective, however, I propose that different decision speeds are perceived as reflecting different degrees of doubt on the side of the companies. Specifically, company A's decision speed suggests solid confidence in hiring you while B's speed suggests hesitation. Given that people are sensitized to the degree of doubt that others express in interdependent situations (e.g., Falk & Kosfeld, 2006; Fehr & Rockenbach ,2003; Pillutla, Malhotra, & Murnigham, 2003), people are expected to be less

inclined to accept the position at company B and choose the position at company A despite its lower salary offer.

A key element in our reasoning is that decreasing the difference in decision times between parties should diminish the informational value of the signal. After all, similar decision times imply similar degrees of doubt. As a consequence, when both companies reach their hiring decision after say five days, a person is expected to maximize on outcomes and choose the better paying position at company B. Such a result would not only show that identical decisions by others (i.e., both decide to hire the applicant for the job) may shift preferences depending on how much time others needed in reaching this decision. It would also indicate that people perceive the decision times of others as a cue of the confidence or doubt the decision maker experienced while making the decision. This is important in interdependent decision situations as it shows that people are not only influenced by the outcomes of the interaction (i.e., decision outcome), but also by *how* they come about (i.e., the decision process).

Decision time as signal of doubt

Some prior evidence exists to support the intuition that people perceive the time that people need in reaching a decision as reflecting this person's doubt. For instance, studying individual choice, Van de Ven, Gilovich and Zeelenberg (2010) induced people to delay their decisions. They found that when people delay a choice, it leads them to feel doubt with respect to the normative option under consideration. As a consequence, following delay, people chose the normative option less often. Thus via self-perception, people infer extended decision time as a signal that they evidently doubt the option under consideration.

Critcher, Inbar and Pizarro (2013) examined similar processes in evaluating the decisions of others. They studied whether the time with which a person decided to act morally would influence people's evaluations of this person. In one of their experiments, participants were instructed to read a scenario in which two men, independently, found a cash filled wallet on a parking lot. The first person immediately decided to return the wallet to a nearby store while the second person similarly decided to return the wallet, yet after a delay. Participants were subsequently asked to evaluate the moral character of these men. Although both decided to return the wallet (i.e., the outcome is the same), the delayed choice person was perceived as less honest than the person who immediately returned it to the nearby store. In addition, the results showed that the effect was mediated by perceptions of doubt. Delaying one's decision to return the wallet was perceived by participants as an indication that the person doubted whether to keep it or not, which negatively affected the evaluations of his character.

More support for the idea that people perceive decision time of others as reflecting doubt comes from a set of studies by Galinsky, Seiden, Kim and Medvec (2002). In one of their studies, negotiators in the role of a buyer were significantly less satisfied with the outcome of the negotiation when their first offer was immediately accepted by the seller. This was even true when the objective outcome of those whose offer was immediately accepted was better than those whose offer was not immediately accepted. Supposedly, a seller who accepts the offer without hesitation implicitly reveals that his minimum reservation price was lower. Buyers who realize this, are likely to experience counterfactual thoughts about how they could have done better, lowering one's satisfaction with the negotiation outcome.

Overall, these results suggest that people are not only responsive to the decision outcome but also attend to *how* others reach their decisions. Evidently, people interpret the time others need to respond to a situation as the degree of doubt (or lack of it) the person experienced while making the decision. Whether decision time is indeed a reliable signal and whether inferences from decision time are valid are both normative questions that are beyond the scope of the present article. Similarly, I refrain from any statement regarding whether the agent is or is not aware of the signaling value of her decision time. In other words, the hypothesis is purely descriptive demonstrating a strong link between decision time and the corresponding inferences people derive from this type of information.

The present research

In the present article, I test in a series of interdependent situations (social interactions in which individuals share and exchange resources in order to obtain mutually beneficial outcomes) what information may be inferred from decision speeds of others and how people use it in choosing between multiple options. A similar experimental methodology is employed in all the following studies (except the last one, which is a field study). In each experiment, participants read a scenario (or are engaged in an interaction) with two other agents, A and B, who made identical decisions (e.g., both decided to hire the participant for a job), yet they reached their decisions at varying decision speeds (e.g., A decides immediately to hire the participant versus B who decides after five days to hire the participant). In addition, the outcome associated with agent B always dominated the corresponding outcome associated with agent A. It is hypothesized that

delayed decisions by others, as opposed to immediate decisions, are perceived as a sign that the actor is hesitant in reaching the decision. Because of the presumed doubts implied by delayed decisions, people are expected to be discouraged to choose the hesitant agent (B) as an interaction partner, even though such a choice is associated with a normatively superior outcome. The first study contains three initial experiments offering different demonstrations of how decision speed affects interpersonal choices.

Experiments 4.1, 4.2, and 4.3

Experiment 4.1

One hundred forty eight students at Fontys University of Applied Sciences in Tilburg participated in a set of unrelated studies in exchange for €5. Participants were allocated to one of two conditions and accordingly read one of two scenarios in which they received job offers from two companies. In one scenario (identical decision speeds condition) both offers were received after five days. In the other scenario (differential decision speeds condition), one offer came much faster (almost immediately) than the other (5 days delay). This latter condition read as follows:

> Imagine having job interviews at two different companies: Company A and Company B. The position at company A offers an annual salary of €40.000. The position at company B offers an annual salary of €41.000. The interview at both companies went equally well.
>
> It is customary that a company takes a hiring decision within a maximum of 5 days. Company A immediately

decides to offer you the job. Company B decides to offer you the job after 5 days.

In the identical decision speeds condition, both companies reached their hiring decision after 5 days. After reading the scenario, participants were asked to indicate the company they would choose to work for.

The results are shown in Table 4.1. While most participants preferred the higher paying position at company B when both companies decided after five days, most participants preferred the lower paying position at company A when this was offered immediately (in the differential decision speed condition, χ^2 (1, N = 148) = 52.25, p < .001, φ = .60. These initial findings support the main conjecture that people are sensitized to the time that others need in reaching a decision.

Table 4.1
Proportion (Actual Numbers in Parentheses) of Participants Choosing Company A (Annual salary €40.000) or Company B (Annual Salary €41.000) in the Different Decision Time Conditions, Experiments 4.1.

	Decision Time	
	A immediate B after 5 days	*A after 5 days B after 5 days*
Company A (€40.000)	67% (52)	10% (6)
Company B (€41.000)	33% (26)	90% (64)

Note: Preference for company A or company B is the percentage of participants (frequency within parentheses) who chose to accept the job offer at company A or B respectively.

Experiment 4.2

Experiment 4.2 constitutes a conceptual replication of Experiment 4.1 using a different scenario. In addition, note that the outcome variable in Experiment 4.1 (annual salary) can change over time. People may infer from an immediate hiring decision that their annual salary will rise faster in the near future. As such, people may strategically prefer the lower paying position in anticipation that a large salary raise will soon follow. Hence, Experiment 4.2 employs an attribute that is not dynamic (the size of a room in m^2) in order to test whether people would again be sensitive to the decision speeds of multiple parties.

Fifty-one Tilburg University students participated in exchange for course credit. The current study was part of a set of unrelated studies. Participants read a scenario about screening interviews at two dormitories (house A and house B, respectively). The scenario in in the differential speed condition read as follows:

> Imagine that you want to rent a room in a dormitory and you have an interview at two different houses, A and B. Each house has three residents who have to decide whether they want you as their new roommate. The room in house A has a surface of 25 m^2 while the room's surface in house B is 28 m^2. The rent for both rooms is identical.
>
> After an interview it is customary that the residents decide within 24 hours whether to accept you as their new roommate. The residents of house A immediately decide to offer you the room. The residents of house B decide after 24 hours to offer you the room.

In the identical decision speed condition, residents of both houses reached their decision after 24 hours. After reading the scenario, participants were asked to indicate which room they were going to rent.

The results are shown in Table 4.2. They show that most participants preferred to rent the more spacious room in house B when these were offered at the same time. However, when the residents of house A immediately reached their decision, the majority preferred to rent the less spacious room in house A. This choice reversal was significant, χ^2 (1, N = 51) = 17.07, p < .001, φ = .58. In sum, Experiments 4.1 and 4.2 both suggest that people are sensitive to (1) the difference in outcomes between the two choice options and, importantly, (2) the time that both agents needed to arrive at their decisions.

Table 4.2
Proportion (Actual Numbers in Parentheses) of Participants Choosing House A (Room Size 25 m^2) or House B (Room Size 28 m^2) in the Different Decision Time Conditions, Experiments 4.2.

	Decision Time	
	A immediate B after 24 hours	*A after 24 hours B after 24 hours*
Room A (25 m^2)	58% (15)	4% (1)
Room B (28 m^2)	42% (11)	96%(24)

Note: Preference for house A or House B is the percentage of participants (frequency within parentheses) who chose to rent the room in house A or B respectively.

Experiment 4.3

In our previous experiments, participants observed the decisions of two parties and were subsequently asked to make a choice that would affect their outcome (i.e., their annual salary or their room's surface). Experiment 4.3 was designed to extend these findings in situations in which the decisions of others would not affect the outcome of participants (i.e., the participant is a neutral bystander).

Fifty-eight members of the general public were recruited near the campus of Tilburg University and volunteered to participate. They were asked to imagine two homeowners (Bob and Tim) who intended to sell their house. The differential speed condition scenario, in which Bob was faster in accepting the offer, read as follows:

> Imagine two homeowners: Bob and Tim, who do not know each other. Both homeowners want to sell their house. Bob hopes to receive around €200 000 for his house and Tim also hopes to receive around €200 000 for his house. Both owners received an offer.
>
> After receiving an offer it is customary that a homeowner decides within 5 days to either decline or accept the offer. Bob received an offer of €193 000 and immediately decides to accept it. Tim received an offer of €196 000 and decides to accept the offer after 5 days.

In the identical decision speed condition both homeowners reached their decision after five days. After reading the scenario, participants were asked to indicate which seller (Bob or Time) was most *satisfied* with the sale of his house.

The results showed that the majority of participants (22 out of 28 or 79%) indicated that Tim would be most satisfied with the sell (i.e., the homeowner who accepted the highest offer) when both decided after five days to accept their respective offer. However, when Bob immediately accepted his relatively low offer, the majority of participants judged Bob (16 out of 30 or 53%) to be the most satisfied seller, despite the fact that he received much less for his house. This reversal is significant, χ^2 (1, N = 58) = 7.52, p = .005, φ = .36.

The perceived satisfaction of a seller is thus not only determined by the outcome of the sell, but also by the decision speed of the seller. Importantly, in line with the previous findings, people seem to derive information from the decision speeds of others, even when they are not actively involved in the situation. Overall, these three initial experiments provide preliminary evidence that people are not only responsive to a difference in outcomes but also *how* different parties arrived at their decisions. Moreover, as shown in Experiment 4.1 and 4.2, people seem to be even willing to sacrifice superior outcomes in favor of a party who brings an inferior outcome, yet who decided immediately.

Experiments 4.4 & 4.5

Experiment 4.1 and 4.2 demonstrate that people respond positively to those who decide first. These results corroborate the main hypothesis that people derive information from others' decision times. Yet, a somewhat different explanation could be that these choices are guided by a simpler decision rule namely "first come first served". Applying this rule to Experiment 4.1, people may have preferred 'quick' company A over 'delayed' company B because company A came first. This rule does not necessitate any complex attribution of intentions

(like I propose) in explaining why people prefer company A over B. The following two experiments (4.4 and 4.5) were designed to test these opposing explanations by including a condition in which target A is again faster than target B, yet only slightly faster. For example, imagine (Experiment 4.1) that company A decides after four days to offer the job to the participant and company B after five. Irrespective of the difference in decision speeds, a 'first come first served' perspective would predict that company A (who again reached their decision first) should be preferred over company B. Yet, a signalling perspective would predict differently since now the informational value of the difference in decision speeds has diminished. People are therefore predicted to prefer the higher paying position at company B. While testing between these two accounts, Experiments 4.4 and 4.5 also serve as replications for Experiments 4.1 and 4.2 respectively.

Experiment 4.4

Seventy-eight students at Tilburg University participated in exchange for course credit. The current study was part of a set of unrelated studies. The same experimental conditions as in Experiment 4.1 were used, except that an extra condition was added in which the difference in decision speed between both companies was small. Specifically, in this extra condition, participants were told that company A (with the lower annual salary) reached a decision after four days and company B (with the higher annual salary) reached their decision after five days.

The results are presented in Table 4.3. As can been seen, in the large difference condition (first column - immediate vs. 5 days), a large majority of participants preferred to accept the lower offer of company A (€40.000), yet this pattern is not maintained when the difference in decision speeds between companies is small or identical, as can be seen from the last two

columns. Compatible with a 'decision time as information' perspective (and not a 'first come first served' account), participants were more likely to accept the job at company A in the large difference (immediate/5 days) condition than in the 4/5 days condition, χ^2 (1, N = 52) = 7.88, p = .005, φ = .39. Following the same line of reasoning, participants were more likely to work for company A in the 4/5 days condition than the 5/5 condition, χ^2 (1, N = 52) = 4.13, p = .04, φ = .28.

This overall pattern between all conditions (i.e., decreasing the difference in decision speeds, in turn, increases the attractiveness of the normatively superior party who responded second) cannot be explained by a 'first come first served' account, but is supported by a 'decision time as information' perspective. Experiment 4.5 was designed to replicate this finding in a different choice context.

Table 4.3
Proportion (Actual Numbers in Parentheses) of Participants Choosing Company A (Annual Salary €40.000) or Company B (Annual Salary €41.000) in the Different Decision Time Conditions, Experiments 4.4.

	Decision Time		
	A immediate B after 5 days	*A after 4 days B after 5 days*	*A after 5 days B after 5 days*
A (€40.000)	61% (16)	23% (6)	4% (1)
B (€41.000)	39% (10)	77% (20)	96% (25)

Note: Preference for company A or company B is the percentage of participants (frequency within parentheses) who chose accept the job offer at company A or B respectively.

Experiment 4.5

One hundred and three students at Fontys University of Applied Sciences in Tilburg participated in a set of unrelated studies in in exchange for €5. The design was identical to the one used in the previous experiment employing the scenario of experiment 4.2. In the additional (intermediate) condition, participants were informed that the residents of house A (with the less spacious room) reached a decision after 22 hours and the residents of house B decided after 24 hours.

The results are presented in Table 4.4. As can been seen in the large difference condition (first column - immediate vs. 24 hours), a large majority of participants preferred to rent the smaller room in house A (25 m^2), yet this pattern is again not maintained when the difference in decision speeds becomes small or identical, as can be seen from the last two columns. As predicted by a 'decision time as information' perspective, participants were more likely to rent the smaller room in house A in the immediate/24 hours condition than the 22/24 hours condition, χ^2 (1, N = 68) = 11.69, p < .001, φ = .42. In addition, participants were more likely to rent a room in house A in the 22/24 hours condition than the 24/24 condition, χ^2 (1, N = 69) = 6.50, p = .01, φ = .31.

Both experiments (4.4 and 4.5) suggest that the dominating choice option (B) becomes less attractive as the decision speed of the 'inferior' option increases. These results are incompatible with a 'first come first served account' but are consistent with a 'decision time as information' perspective: The informational value of the difference in decision speeds increases as the difference between parties in reaching a decision increases.

Table 4.4
Proportion (Actual Numbers in Parentheses) of Participants Choosing House A (Room Size 25 m^2) or House B (Room Size 28 m^2) in the Different Decision Time Conditions, Experiments 4.5.

	Decision Time		
	A immediate B after 24 hours	*A after 22 hours B after 24 hours*	*A after 24 hours B after 24 hours*
A (25 m^2)	65% (22)	24% (8)	3% (1)
B (28 m^2)	35% (12)	76% (26)	97% (35)

Note: Preference for house A or House B is the percentage of participants (frequency within parentheses) who chose to rent the room in house A or B respectively.

Experiment 4.6

Experiment 4.6 provides two extensions: First, I replicate the findings in a real choice dilemma. Second, following a 'decision time as information' perspective, it was proposed that decision speed signals doubt such that longer decision times suggest more hesitation. I therefore measured perceived doubt and tested whether this mediated the effect of decision speed on choice preferences.

Method

A total of 149 Tilburg University students (44 male, 105 female) participated in exchange for a fixed amount of €8 (M_{age} = 21, *SD* = 2.23). Twenty-one participants were excluded from the analysis for participating in a previous, similar experiment thus leaving the final sample with 128 observations.[1] Including

[1] These participants had earlier participated in Experiment 4.8 reported later in this article in which they were fully debriefed and informed about the exact nature of the study.

these participants in the analyses did not change the results in any meaningful way.

Upon arrival in the lab, participants were seated in separate cubicles, ostensibly to ensure anonymity. They received instructions on the computer screen explaining that they were participating in an anonymous one-shot investment game. Participants learned that they were assigned the role of trustee while two other participants were assigned the role of investor (in reality, these investors were preprogrammed players). Participants were subsequently informed that both investors, A and B, received a random amount of money from the experimenter. Investor A received €4 while investor B received €4.50. Each investor had to decide, independently, whether to trust the participant by sending her the money or keeping the money for herself. When an investor decided to trust the money to the participant, the experimenter tripled the amount and gave it to the participant who subsequently had to decide how much (if any) to return. Thus, if investor A decided to send the €4, the participant would receive €12 to divide between her and investor A. Similarly, if investor B decided to send the €4.50, the participant would receive €13.50 to divide. Importantly, the instructions specified that a participant could divide money with only one of the investors. Hence, when *both* investors decided to invest, a participant first had to choose with whom she wanted to interact (i.e., dividing €12 with investor A or €13.50 with investor B). Participants had ample time to read the instructions and were given the opportunity to ask clarification questions at any time.

Following these instructions, participants were informed that both investors had sixty seconds to decide whether to trust their money to the participant or not. Two clocks (one for each investor) appeared on the center of the screen, counting from zero to sixty seconds. When one of the investors made her

choice, her corresponding clock stopped and her choice (give or keep) appeared underneath the clock. Meanwhile, the clock of the other investor kept counting until she would reach a decision (give or keep).

In this experiment, both investors decided to give the money to the participant, yet the time in reaching this decision was experimentally varied. In the *large difference* condition, investor A (sending €4) decided after 4 seconds to trust the money to the participant while investor B (sending €4.50) decided after 28 seconds. In *the small difference* condition, investor A decided after 25 seconds and investor B decided after 28 seconds.

Choice. After seeing that both investors decided to trust the money to the participant, a participant first had to choose with whom they wanted to interact (i.e., dividing €12 with investor A or €13.50 with investor B), which constituted the main dependent variable.

Perceptions of doubt. To assess participants' perceptions of doubt, participants indicated the extent to which (1) investor B had more doubts than investor A and, (2) whether investor B was less certain than investor A in deciding to hand the money to the participant (1= *not at all*, 7 = *very*). These items were averaged into a doubt perception composite (r = .87, p < .001).

Back-transfer. Finally, after answering these questions, participants were asked how much (if any) of the tripled amount they wanted to return to the investor. All participants were debriefed by the end of the experiment and ten, randomly chosen, participants received the money they did not return to the investor.

Results and Discussion

Choice. All results are shown in Table 4.5. When investor A was only slightly faster (after 25 seconds) than investor B in deciding to trust the money to the participant, the majority (46 out of 66 or 70%) chose to divide the money with investor B (i.e., the investor with whom a participant could divide the largest sum of money namely €13.50). However, when investor A was much faster than investor B in deciding to trust the money to the participant, the majority favored investor A (33 out of 62 or 53%). This choice reversal was significant, χ^2 (1, N = 128) = 6.92, p = .009, φ = .23. These findings again corroborate the main hypothesis that people are not solely driven by the intrinsic properties of outcomes. Note that participants played an anonymous one-shot game (as opposed to repeated games) in which strategic- and reputational concerns cannot account for these findings.

Perceptions of doubt. Although investor B always decided after 28 seconds to send the money to the participant, attributions of doubt depended on the speed in which investor A decided to hand his money. Specifically, investor B was perceived as more doubtful than A when investor A decided after 4 seconds (M = 5.81, SD = 0.99) as compared to when investor A decided after 25 seconds (M = 2.78, SD = 1.27), F (1, 126) = 226.56, p < .001, η^2 = .64. These results are again compatible with the conjecture that the decision times of others implicitly communicate an agent's doubt in sending the participant the money.

Mediation analysis. It was earlier proposed that the effect occurs via perceptions of doubt. To test this conjecture, I ran a bootstrap analysis (Preacher & Hayes, 2008) with 5000 bootstrapped samples. When perceptions of doubt were entered in the bootstrapped model, it comprised a significant mediator, B = - 0.76, Z = -3.90, p < .001, with a 95% confidence

interval excluding zero (1.3142 to 3.5448). Thus, the more doubt participants inferred from the investor's decision, the less likely this investor was chosen to share money with.

Back-transfer. Because the amount of money was less when dividing the sum with investor A (€12) than with investor B (€13.50), I first computed the back-transfer as a percentage of the tripled investment. The results indicated that the back-transfer depended both on the experimental condition and the chosen investor. Specifically, in the condition where investor A responded much quicker, participants selecting 'quick' investor A in this condition returned more (45% of €12) than participants selecting 'slow' investor B (28% of €13.50), Mann-Whitney test, z = -4.060, p < .001, r = .52. Yet, in the condition in which investor A was only slightly faster, participants selecting investor A returned 48% (of €12) and those selecting investor B returned 35% (of €13.50), which was not statistically different, Mann-Whitney test, z = -1.840, p = .07, r = .23.[2]

[2] Analyses on the average returns instead of the percentage of the tripled-investment yielded similar results.

Table 4.5
Choices to Interact with Investor A (dividing €12) or Investor B (dividing €13.50), Perceptions of Doubt in Investor B, and Back Transfer in the two Decision Time Conditions, Experiment 4.6.

	Speed in Trusting the Money to the Participant	
	A in 4 seconds B in 28 seconds	*A in 25 seconds B in 28 seconds*
Choice to divide €12 with A	53% (33/62)	30% (20/66)
Choice to divide €13.50 with B	47% (29/62)	70% (46/66)
Perceptions of doubt in B	5.81 (0.99)	2.78 (1.27)
Back transfer to A of €12	€5.81 (45%)	€5.75 (48%)
Back transfer to B of €13.50	€3.78 (28%)	€4.73 (35%)

Note: Preference for Investor A or B is the percentage of participants (frequency within parentheses) who chose to interact with Investor A or B respectively. Doubt perceptions are assessed on a 3-item 7-point scale, with higher scores indicating more perceived doubt in investor B.

Experiments 4.7 & 4.8

The prior results indicate that (1) people prefer to interact with parties who immediately respond to a situation while avoiding those who delayed their decision and (2) that this tendency is explained by the inferred doubt that the different decision speeds suggest. This may imply that there is an inherent positivity associated with immediate decisions while delayed responses are inherently negative. I propose that this is not always the case. Sometimes, the reverse may be true because people are often encouraged to interact with others who are in doubt while discouraged when other's actions reveal a lack of doubt. For example, in negotiations, an offer by a potential buyer is often declined by sellers. Would a buyer

prefer to continue negotiating with a seller who immediately declined the offer or would she prefer the seller who declined it after a delay? I conjecture that in these situations, people prefer to negotiate with the delayed seller because the delay communicates that the seller doubted to accept the offer and is open for further negotiations (while an immediate rejection may be perceived as final and inflexible). Thus, although our previous findings suggest that people are more prone to interact with parties who immediately decided to accept the participant as trustee or employee, the reverse is true in the case of negotiations when sellers decline offers. The following two experiments were designed to test this conjecture. Experiment 4.7 was a scenario-based experiment in which participants were negotiating with two different sellers who declined a participant's initial offer. Participants were subsequently asked with whom they wanted to continue negotiating. In Experiment 4.8, participants were again negotiating with two sellers, yet they made real choices instead of hypothetical ones.

Experiment 4.7

Sixty members of the general public were recruited near the Tilburg University campus and were asked to imagine that they were looking for a second hand refrigerator. They were told that there were two sellers (Tom and Fred). The scenario in which Tom immediately declined the offer read as follows:

> Imagine you are looking for a second hand refrigerator and there are two sellers, Tom and Fred, both of whom want to sell their refrigerator. Both refrigerators are identical in brand and type. Moreover, both refrigerators are in an identical condition. Tom's asking price is €90 while Fred's asking price is €100.

> You decide to call both sellers to make them an offer. You offer them both an openings bid of €70. Tom immediately declines your offer. Fred declines your offer after 30 minutes.

In the condition in which both sellers had identical decision speeds, both declined the offer after 30 minutes. After reading the scenario, participants were asked to indicate with whom they wanted to continue negotiating.

When both sellers declined the offer after 30 minutes, the majority of participants (22 out of 30 or 73%) preferred to continue negotiating with Tom whose initial asking price was lower than Fred's price. Thus, other things being equal, people prefer to continue negotiating with a seller whose initial asking price is lower as opposed to higher. However, when Tom immediately declined the offer while Fred declined the offer after 30 minutes, the majority of participants (16 out of 30 or 53%) preferred Fred as a negotiation partner, despite his initial higher asking price. This reversal was significant, χ^2 (1, N = 60) = 4.44, p = .04, φ = .27.

These results extend our findings in an important way. Our previous results suggest that people favor parties who immediately decided to accept the participant as employee, roommate, or trustee. The present findings demonstrate that the reverse is true when a request is denied (e.g., when an offer is declined). More generally, decision speed serves as an implicit signal of doubt and uncertainty. In the present study, the longer decision time of Fred (in the experimental condition) suggests that he may be uncertain and thus, supposedly open for further negotiations, encouraging people to choose him as negotiation partner (despite his higher initial asking price).

The goal of Experiment 4.8 was two folded: First, it was intended to replicate the findings of Experiment 4.7 in a real

choice setting. Second, it was designed to test whether, as in Experiment 4.6, the effect of speed on choice is mediated by perceptions of doubt. However, while more perceived doubt decreased the likelihood that a trustor was chosen in Experiment 4.6, I expected that a doubtful seller is *more likely* to be chosen as negotiation partner since the inferred doubt supposedly signals that the declined offer approached the seller's limit.

Experiment 4.8

Method

One hundred and one students (27 male) at Tilburg University participated in exchange for €8 (M_{age} = 21, SD = 2.48). The current study was part of a set of unrelated studies. Upon arrival in the lab, participants were seated in separate cubicles and received instructions on the computer screen explaining that they were participating in an anonymous one-shot negotiation game. Participants were told that they were assigned the role of buyer while two other participants were assigned the role of seller (in reality, the sellers were preprogrammed players). Subsequently they were told that both sellers, A and B, each received three new paper notebooks (for taking lecture notes) from the experimenter. The participant, in turn, received €10 from the experimenter with the goal of buying three paper notebooks from one of the two sellers. They were further explicitly informed that of all participants who participated that week (1) ten buyers would receive the paper notebooks plus the remaining amount after buying these notebooks and (2) ten sellers would receive the offer they accepted. Participants had ample time to read the instructions and were given the opportunity to ask clarification questions at any time.

After these instructions, both sellers started the negotiation by stating their asking prices. Specifically, participants saw on their computer screen that Seller A had an asking price of €8 for his paper notebooks while Seller B asked €9. After seeing these prices, a participant stated an opening offer (which was the same for both sellers). After stating their openings bid, participants learned that both sellers had a maximum of sixty seconds to either accept or decline the offer. Two clocks (one for each seller) subsequently appeared in the center of the screen, counting from zero to sixty seconds. When one of the sellers made his choice, her clock stopped counting and the choice (accept or decline) appeared underneath the clock. Meanwhile, the clock of the other seller kept counting until she reached a decision to accept or decline the offer.

In this experiment, both sellers declined the offer yet the decision speeds in reaching their decision was experimentally varied. In the *large difference condition*, Seller A (asking the lower price of €8) declined the offer after 3 seconds while seller B (asking the higher price of €9) declined it after 26 seconds. In the *small difference condition*, seller A and B declined the offer after 23 and 26 seconds, respectively. In the *identical condition*, both sellers declined the offer after 26 seconds.

Choice. After seeing that both sellers declined the offer, a participant indicated with whom they wanted to continue negotiating (i.e., Seller A or B), which constituted the main dependent variable.

Perceptions of doubt. Three items assessed a participant's perceptions of doubt. Specifically, participants had to indicate the extent that (1) seller B doubted more than seller A, (2) seller B thought that the offer was more acceptable than seller A, and (3) seller B was less certain than seller A in declining the offer. The three items were rates on a 1 to 7 scale (1= *not at all*,

7 = *very*) and were averaged into a doubt perception composite ($\alpha = .94$).

After answering these questions, a participant was asked to offer the seller a new bid (which was always accepted). All participants were debriefed at the end of the experiment and ten, randomly chosen, participants received the paper notebooks plus the money that they did not spent on buying these notebooks (€10 minus the accepted offer).

Results and Discussion

Choice. The results are presented in Table 4.6. When both sellers declined the offer after 26 seconds (last column), the majority of participants preferred to continue negotiating with seller A whose initial asking price was relatively low (€8). However, when seller A rejected the bid almost immediately (first column – 3 seconds vs. 26 seconds), this pattern reversed and a large majority of participants preferred to continue negotiating with seller B. This reversal is significant, χ^2 (1, N = 63) = 25.13, $p < .001$, $\varphi = .63$. In a similar vein, participants were more likely to continue negotiating with seller A in the 3/26 seconds condition as compared to the 23/26 seconds condition, χ^2 (1, N = 70) = 18.19, $p < .001$, $\varphi = .51$. Finally, comparing the choice frequencies of the 23/26 seconds condition with the 26/26 seconds condition, people seem to prefer to continue negotiating with A to a larger extent, but this increase was not significant, χ^2 (1, N = 63) = 1.65, $p = .19$, $\varphi = .16$.[3]

Overall, these findings again support the main hypothesis that people are not only driven by the intrinsic properties of outcomes (or initial asking prices) but also by the decision

[3] In Experiments 4.3 and 4.4 I did notice a difference between the 'small difference conditions' and the 'identical conditions'. Note that the trend in the current experiment is in the same direction and a possible reason that I did not replicate these results may be due to a lack of power.

speeds of relevant actors. Importantly, these results replicate the results of Experiment 4.7, yet in a situation in which participants made real choices as opposed to hypothetical decisions.

Perceptions of doubt. The results are presented in Table 4.6. Although seller B always declined the offer after 26 seconds, attributions of doubt in reaching this decision depended on the speed in which seller A declined the offer, F (2, 98) = 112.59, $p < .001$, $\eta^2 = .70$. Specifically, seller B was perceived to be more doubtful than A when seller A declined the offer after 3 seconds as compared to the condition in which seller A and B both declined the offer after 26 seconds, F (1, 61) = 325.87, $p < .001$, $\eta^2 = .84$. In addition, when seller A declined the offer after 23 seconds (second column), seller B was again perceived to be slightly more doubtful than A as compared to when seller A and B declined the offer after 26 seconds (third column), F (1, 68) = 57.20, $p < .001$, $\eta^2 = .46$. Finally, when comparing the 23/26 seconds condition with the 3/26 seconds condition, seller B was again perceived to be more doubtful, F (1, 67) = 50.30, $p < .001$, $\eta^2 = .43$. Overall, these results corroborate the main hypothesis that the decision speed of others implicitly reveals an agent's doubt in declining the offer.

Mediation analysis. To test the conjecture that decision speed is mediated by the inferred doubt that participants perceived in the actions of both sellers, I ran a bootstrap analysis (Preacher & Hayes, 2008) with 5000 bootstrapped samples. When perceptions of doubt were entered in the bootstrapped model, this was a significant mediator of the effect of decision speed on choice, $B = 0.71$, $Z = 3.17$, $p = .002$, with a 95% confidence interval excluding zero (-2.5903 to -0.4420). Thus, the more doubt participants inferred from the seller's decision to decline the initial offer, the more likely this seller was chosen as negotiation partner. These results

replicate the findings of Experiment 4.6 and support a 'decision time as information' perspective on the role of decision speeds in driving choice preferences. A relatively strong delay is evidently perceived as signal of doubt which people use in choosing among two choice alternatives. Note that unlike in Experiments 4.1 to 4.6, in the present experiment delayed decision times are actually preferred. In other words, immediate decisions may sometimes be conceived as encouraging and in other situations as discouraging. Regardless of how they are interpreted, our general proposal is that decision speed contains information that may affect the decision process.

Table 4.6
Preferences to Continue Negotiating with Seller A (asking price of €8) or Seller B (asking price of €9) and Perceptions of Doubt in Seller B in the Three Decision Time Conditions, Experiment 4.8.

	Speed in Declining the Offer		
	A in 3 sec *B in 26 sec*	*A in 23 sec* *B in 26 sec*	*A in 26 sec* *B in 26 sec*
Preference for A (€8)	28% (9)	79% (30)	90% (28)
Preference for B (€9)	72% (23)	21% (8)	10% (3)
Perceptions of doubt B	5.39 (1.00)	3.19 (1.36)	1.23 (0.81)

Note: Preference for seller A or B is the percentage of participants (frequency within parentheses) who chose to continue negotiating with seller A or B respectively. Doubt perceptions are assessed on a 3-item 7-point scale, with higher scores indicating more perceived doubt in seller B in declining the offer.

Experiment 4.9

Study 4.9 examined whether different decision speeds would also affect choice preferences in a field setting. For this purpose, I examined the behavior of game show contestants, using data from the television show *The Voice*. The Voice is a talent show that is broadcasted in more than 50 countries. Contestants are aspiring singers and there are four different stages of competition. The first stage, the blind auditions, provides a unique opportunity to analyze whether game show contestants are sensitized to the time that multiple parties need in reaching a decision.

The blind auditions constitute the first part of a show's season in which four coaches (usually well-known singers) listen to contestants in chairs facing backwards as to avoid seeing the contestant. Each contestant has 2 minutes of singing time to convince one or more coaches to accept him or her in the coach's team. When a coach is convinced by a contestant's talent, they press a button which rotates the chair, signifying that the coach wants to work with the contestant (when a chair rotates, the bottom illuminates saying 'I want you'). Importantly, when more than one coach presses their button during the song, the contestant eventually has to choose the coach he or she wants to work with. These features leaves the possibility that different coaches turn their chairs at different time points during a contestant's song (Experiment 4.6 essentially has the same design in which multiple trustors declared their trust in a participant at different time points). For example, imagine a contestant who enters the stage and starts singing. Coach A is convinced and turns his chair after 10 seconds. Coach B is also convinced, yet after 90 seconds. Both coach C and D are not convinced and decide not to turn their chairs. Who will the contestant choose to work with (Coach A

or B)? Note that this is an important decision for a contestant, since the coach is crucial in developing and mentoring the contestant with the goal of winning the talent show. Given that people are sensitive to the decision speed of others, I predicted that the contestant in the example is less likely to favor coach B over coach A. I tested this hypothesis by examining whether the observed frequency that a coach is chosen who turned last is different than the probability that a coach is expected to be chosen by chance.

Method

I used the video-sharing websites YouTube and Dailymotion as recourses to find blind auditions of The Voice. In total, I analyzed the blind auditions of 6 seasons in 4 different countries (The Voice U.S. 2011, 2012, The Voice Australia, 2012, The Voice UK, 2012, 2013 and The Voice of Holland, 2013. Unfortunately, not all auditions of the Voice of Holland 2013 were available. I therefore analyzed only those auditions that were available) in which there were 97 blind auditions in which at least two coaches turned their chair. These were the auditions of interest since now the contestant had to choose with whom they wanted to collaborate.[4]

For each contestant I recorded (1) how many coaches turned their chair, (2) when each coach turned his or her chair (i.e., was he first, second, third, or fourth in deciding to turn his or her chair), (3) the contestant's choice with whom he or she wanted to collaborate which constituted our main dependent

[4] Note that there were more auditions in these seasons, yet either no coach turned their chair or only one coached turned. When no coach turned, the contestant was eliminated. When only one coach turned his or her chair, the contestant was automatically assigned to this coach's team and thus had no choice with whom to collaborate. In addition, there were also auditions in which multiple coaches chose to turn their chair at exactly the same moment, making it impossible to determine who turned first or last.

variable, (4) the contestant's gender, and finally (5) the country and season in which the blind audition was aired.

Results and Discussion

There were three possible situations that a contestant could encounter: Either two, three, or four coaches turned their chairs, leaving a contestant with the choice between two, three, or four coaches respectively. According to our hypothesis, coaches who were last in deciding to turn their chair signaled the least degree of confidence in the contestant's ability as a singer. I thus predicted that these coaches were least likely to be chosen as mentor. As can be seen in Table 4.7, when contestants had the choice between two coaches, 18 out of 54 contestants (33%) chose a coach who turned last (i.e., second), 4 out of 14 (28%) chose a coach who turned third in the 'three-coach situation' while 4 out of 29 constants (14%) chose a coach who turned fourth in the 'four-coach' situation. The question is whether these observed frequencies are different from the frequency that a coach is expected to be chosen by chance. For the 'two-coach' situation, the expected frequency to be chosen by chance is 27 (54/2), in the 'three-coach' situation this is 4.66 (14/3) and when four coaches turned their chair this is 7.25 (29/4).

Next, I tested whether the observed frequencies that a coach was chosen who turned last differs from the frequency that can be expected when chosen by chance. In testing this, I first aggregated the data to increase power and computed a modified z-score that takes two properties of the aggregated data into account, namely (1) that the expected frequency that a coach is chosen by chance is different for the three possible situations and (2) that there were more auditions in which two coaches turned their chair while only a few in which three

coaches turned.[5] The result indicated that coaches who were last in turning their chair were less frequently chosen than expected by chance, Z = 2.75, p = .006 (two-tailed). Game show contestant in our sample were thus hesitant to choose a coach who was last in deciding that he or she wants to work with the contestant. These results extend the results of Experiment 4.6 in which people were similarly hesitant to cooperate with trustors who decided to trust the participant with a delay. In a similar vein, game contestant presumably also perceived doubt in a coach who turned his or her chairs last, decreasing a contestant's willingness to work with this coach. Obviously, field settings are noisy, yet given our previous experiments we feel confident to argue that at least part of a contestant's choice is determined by the coach's decision speed.

[5] $Z = \frac{\sum Observed\ frequency\ i - \sum Expected\ frequency\ i}{\sqrt{(54 * .5 * .5) + (14 * .33 * .66) + (29 * .25 * .75)}}$ were the observed frequency is the frequency that a coach is chosen who turned last for each possible situation. The expected frequency refers to the frequency that can be expected for each situation when a coach is chosen by chance. The denominator is the standard error of the mean.

Table 4.7
Number of Times (Percentages in Parentheses) That a Coach Who Was Chosen Turned First, Second, Third, or Fourth in the Three Possible Situations, Experiment 4.9.

Number of times that the coach who was chosen turned:	Number of Coaches Who Turned their Chair during the Audition		
	Two turned	Three turned	Four turned
First	36/54(67%)	5/14(36%)	11/29(38%)
Second	18/54(33%)	5/14(36%)	5/29(17%)
Third	-	4/14(28%)	9/29(31%)
Fourth	-	-	4/29(14%)

General Discussion

In a series experiments I examined what information the decision speeds of others reveal and how people use this in choosing between multiple parties. Specifically, delayed decisions by others, as opposed to immediate decisions, were consistently interpreted as a sign that the actor was hesitant in reaching the decision. Because of the doubt that delayed decisions implied, people were either discouraged (Experiment 4.1 to 4.6, and 4.9) or encouraged (Experiment 4.7 and 4.8) to choose the relatively slow party as interaction partner. For example, trustees were less likely to share money with a person who was relatively late in deciding to the trust this person (Experiment 4.6) while buyers were more likely to continue negotiating with a seller who declined the buyer's

offer after a delay as opposed to immediately (Experiment 4.7 and 4.8). These results support a 'decision time as information' proposition: People interpret the times that others need in reaching a decision as cues reflecting doubt in one's decision, thereby influencing subsequent choices and judgments. These signs of doubt even led people to favor choice options that were normatively inferior. When choosing between multiple parties, individuals are thus responsive not just to the outcomes that different parties may bring, but also to *how* these parties reached their decisions.

In fact, it is proposed that decision makers may, under certain circumstances, be caught in an internal conflict in which they have to resolve the tradeoff between choosing the maximal normative option and the potential uncertainty associated with it as derived from the cue concealed in the other party's decision speed. This tradeoff can be expressed by the following two parameters model:

$$\text{Choice} = F\,[(a * \text{Outcome}) + (b * \text{others decision speed}) + C]$$

where choice is a function of the optimal outcome and the doubts associated with the decision speed of the other agent. The weights associated with these two parameters are a and b respectively (C is a constant). The weight a would be mainly determined by the extent to which the best option dominates the other (or second best, in case of more than 2 options) alternative. The weight a should be larger the further the dominating alternative is from the other option. For instance, in Experiment 4.1, if the salaries offered by companies A and B would be €40.000 and €50.000 respectively, a would supposedly be approaching its maximal value and will not likely be compensated by decision speed. In other words, there

is probably a tipping point at which people become insensitive to the difference in decision speeds.

Several comments are in order. First, while the results support the conjecture that people perceive different response times in terms of doubt, I do not propose that this is the sole information contained in decision speed. Different response times may reveal more than a person's (lack of) doubt. For example, an immediate hiring decision may reveal (1) a lack of doubt in hiring the applicant which (2) communicates the company's enthusiasm about the person (Experiment 4.1). In a similar vein, a seller who immediately declines an offer may, in addition to a lack of doubt, signal one's anger or frustration in the low offer (Experiment 4.7 and 4.8). Thus, while response times are most likely always interpreted in term of doubt, context specific information may arise as a consequence of the inferred doubt.

A final comment concerns a methodological point. In all our experiments I employed what Hsee (1996) has termed *joint evaluation* in which all the options are presented and are thus comparable. Whether our conclusion equally apply to *separate evaluation* remains an open question. Clearly, if one gets an immediate job offer decision time is obviously short. However, whether an offer received after 2, 5 or 10 days is perceived long may depend on the context. As a consequence, people may be less sensitive to decision speed under separate evaluation conditions.

In theoretical terms, the decision time relation that is demonstrated in the present paper resembles a similar notion to what Hsee and Abelson (1991) termed "velocity relation". These authors propose that a person's satisfaction with an outcome depends on the velocity (i.e., rate) at which the outcome changes over time. For example, suppose that student's A standing in the class has been rising from the 30th

percentile to the 70th percentile over the past 6 weeks while that of student B rose from the 35th percentile to the 70th percentile over the past 3 weeks. Following Hsee and Abelson, student B's outcome satisfaction should be larger than that obtained by student A. Though B's standing has improved only by 35 points compared to A's improvement of 40 points, B's improvement velocity was much faster (3 weeks) than that of A (6 weeks). As in the model I proposed above, velocity of change can compensate for a somewhat lower outcome. As in our model, I speculate that once the difference in outcome exceeds a certain level, it cannot be compensated anymore by the speed or rate of change.

There are obviously fundamental differences between the velocity relation and the notion of decision speed examined in the present paper. First, the velocity relation describes the relationship between satisfaction and velocity while the relation that I demonstrate focuses on choice preferences. Second, the velocity relation describes how people respond to dynamic changes in outcomes. Thus, in the velocity relation, there is always a change in the state of the outcome. In the decision time relation that I described, outcomes do not change, but a party's decision to grant a person access to the outcome differs as a function of decision speed. Third, and perhaps most important, decision time in our case serves as a cue or a signal and as such thus influences choices, whereas velocity in Hsee and Abelson's model is actually part of the outcome. Notwithstanding these differences, the two models resemble in that both deal with a tradeoff associated with outcome vs. time considerations.

The present investigation contributes to a stream of research investigating how implicit signals that accompany actions affect behaviors and evaluations. For example, as discussed in the introduction, Critcher and colleagues (2013)

showed that people are sensitized to the decision speed of others when acting morally. A person who immediately decided to act morally was judged more positively than a person who similarly acted morally, yet after deliberating about it. In a similar vein, research by Galisnky and collegues (2002) showed that negotiators are less satisfied with the negotiated outcome when one's first offer is immediately accepted. The current findings represent an important extension of these results. First, the above mentioned studies tested the role of decision speeds on evaluations while I tested its effects on choice preferences. This is an important extension since evaluations and choice do not always align (e.g., Ajzen, 1991). Second, I show evidence that, given identical decision speed, people prefer the dominating choice option. However, when the dominating option was relatively late in reaching a decision, people prefer the normatively inferior alternative instead. People evidently derive (dis)utility from the decision speeds of others. Finally, I show evidence that the choice reversal cannot be accounted for by other mechanisms like a 'first come first served' rule. Instead, our results are supported by a 'decision speed as information' perspective.

On a final note, any decision making process can be characterized by the time with which a decision is made, yet only few have paid sufficient attention in illuminating its role in affecting choices. The present research is an initial step toward understanding this relationship by studying how outcomes and decision speeds interact in guiding choice preferences. More specifically, I examined the cues and signals afforded to decision makers by the decision speed of the other party. An open question for future research is the extent (and conditions) under which this signaling is a product of deliberate reasoning or whether it is more a result of automatic (unconscious) processing.

Chapter 5

Summary and Discussion

Social interactions are complex and people often need to 'go beyond the information given' in order to develop an understanding of the social situation. This process has important implications for people's experiences and subsequent decisions. The studies in this dissertation contribute to our understanding of how people go beyond the information given by describing how they rely on various cues that frequently arise in social interactions. The aim of the final chapter is to provide a summary of the chapters and discuss the theoretical implications of the research presented in this thesis.

Chapter 2: The insured victim effect

Summary

The first goal of the second chapter was to examine how people rely on characteristics of the victim when evaluating the severity of unlawful acts. Building on prior research indicating that people are guided by a 'more-harm-deserves-more-punishment' imperative, it was hypothesized that people would punish identical crimes more mildly when victims were insured as opposed to uninsured. Experiment 2.1 demonstrated that people indeed would punish identical crimes less severely when a victim happened to be insured. In Experiment 2.2 participants had to determine the sentence for the insured and uninsured case simultaneously and the results indicated that the effect disappeared under this comparative condition. These

results suggest that people believe they should not differentiate between insured and uninsured victims but nonetheless become milder for perpetrators whose victims were insured when evaluating the cases separately. Experiment 2.3 extended these findings by ruling out that the insured victim effect is driven by the perpetrator's foreknowledge while Experiment 2.4 indicated that transgressions do not have to result in harm for the insured victim effect to present itself. Specifically, when harm was possible but not realized, people still punished crimes less severely when the (potential) victim was insured. In addition, Experiment 2.5 showed that punishment recommendations can even become more lenient for crimes that are in fact more serious but in which the victim was insured. Finally, Experiment 2.6 indicated that the insured victim effect is associated with a change in how people evaluate the severity of the transgression. That is, transgressions in which the victim happened to be insured are perceived to be smaller fouls than identical transgressions in which victims were uninsured.

Discussion

For decades, economists and psychologists have studied the consequences of insurance from the perspective of the insured party (e.g., *intra*personal moral hazard). Their emphasis on why insured individuals are more prone to expose themselves to risk has provided key insights into a hidden cost of this safety mechanism. Chapter 2 adds to this research stream by highlighting the *inter*personal consequences of insurance and is intended as a first step in illuminating this relationship. Specifically, the findings in Chapter 2 add to a growing stream of literature in behavioral law, a perspective that challenges the rational actor assumption that is advocated by many legal scholars. This relatively new approach favors a

more accurate conception of (legal) decisions that is descriptive, rather than normative, stressing important 'bounds' on human decision making (e.g., Jolls, Sunstein, & Thaler, 1998). For example, normative theories of choice assume that people have stable and consistent preferences, regardless of how the preferences are elicited (Hsee, 1996). However, almost all experiments in Chapter 2 indicated that punishments reversed when people evaluated the insured and uninsured case either jointly or separately, challenging this fundamental premise.

Chapter 3: The hidden cost of insurance

Summary

Whereas the role of insurance is explored in the legal domain in Chapter 2, the third empirical chapter investigated its role in trust relationships. The studies in this chapter provide evidence for a hidden cost of insurance in situations requiring trust: Trustees are more likely to betray trustors when the trusting party *chooses* to be insured against the risk of betrayal. The presumed safeguard against the risk of betrayal thus paradoxically increased the probability of betrayal. Experiment 3.1 provided initial support for this idea. Experiment 3.2 extended these findings by ruling out that the observed decrease in cooperation can be accounted for by a mere change in the payoff structure for trustors who are insured. In a similar vein, Experiment 3.3 demonstrated that trustees only became less willing to cooperate when the trustor was insured by choice and not by chance. Finally, Experiment 3.4 indicated that trustors realize that choosing to be insured will decrease the trustees 'willingness to cooperate, yet they nonetheless chose to be insured when given the opportunity.

Discussion

Chapter 3 contributes to a stream of research investigating how risk management mechanisms (e.g., contracts and fines) interfere with developing relationships based on trust and this chapter represents an important extension to prior findings. Specifically, although both contracts and insurance policies are designed to minimize risk, they do so in very different ways. Contracts minimize risk by restricting the actions of trustees such that it becomes costly for a trustee not to cooperate. Contracts thus minimize risk by decreasing the probability that trustees betray trust. An insurance policy does not restrict the actions of trustees but minimize risk by mitigating the resulting costs if betrayal occurs. These different safety mechanisms thus decrease the risk of betrayal by each targeting different components of the risk equation (i.e., risk = probability of betrayal x the resulting cost when it happens). Chapter 3 provides a first demonstration of how lowering risk by mitigating the costs of betrayal hinders cooperation. Importantly, the results indicate that trustees become less cooperative because by *choosing* insurance, trustor's signal distrust, encouraging trustee's to defect. These results again support the idea that people 'go beyond the information given' and derive meaning from the acts of others.

Chapter 4: Decision time as information

Summary

The last empirical chapter investigated the role of another cue that frequently arises in interdependent situations, namely the decision time of others. The results of nine studies indicated that delayed decisions by others, as opposed to immediate decisions, were consistently interpreted as a sign that the actor was hesitant in reaching the decision. Because of

the doubt that delayed decisions implied, people were either discouraged (Experiment 4.1, 4.2, 4.3, 4.4, 4.5, 4.6 and 4.9) or encouraged (Experiment 4.7 and 4.8) to choose the relatively slow party as interaction partner. The results supported a 'decision time as information' hypothesis: People interpret the times that others need in reaching a decision as cues reflecting doubt in one's decision, thereby influencing subsequent choices and judgments. These signs of doubt even led people to favor choice options that were normatively inferior. When choosing between multiple parties, individuals are thus responsive not just to the outcomes that different parties may bring, but also to *how* these parties reach their decisions.

Discussion

Although decisions are intimately related to various aspects of time, most prior research looked at the role of time in choice from the decision maker's point of view (i.e., *intra*personal consequences of time in choice). Chapter 4 takes an *inter*personal perspective and is a first initial step in illuminating how the decision time of others affect interpersonal choices. This is important because any decision can be characterized by the time with which it is made and the decisions of others are no exception. The findings in Chapter 4 indicate that people perceive the decision speed of others as indicative of the degree of doubt the person experienced which, in turn, influenced a variety of social interactions and decisions.

Concluding remarks and future directions

Many characteristics of others with whom we are interacting with now, or plan to interact with in the future, are not directly perceivable. For instance, is the other person trustworthy or is one's opponent in a negotiation willing to

reach a compromise or not. These characteristics can be intentionally suppressed or simply hard to observe. Hence, we often need to rely on implicit cues to form an impression about the intentions, motives, or goals of others. Throughout this dissertation, I have shown how and when people are guided by such cues when choosing with whom to cooperate or negotiate or whom to punish. Although the cues examined in this dissertation are distinctively different from each other, they all possess a key feature: they transmit information. The findings presented in the empirical chapters clearly reveal that (1) a victim's harm is perceived as indicative of the seriousness of the crime, (2) a trustor's decision to protect oneself is perceived as an act of distrust, and (3) the decision time of others is taken as a sign of doubt. Whether these cues are indeed valid indicators of these constructs, or whether the inferences that recipients of these cues make are correct, constitute of course different questions. Future research is needed to address the conditions under which these cues can be considered as reliable sources of information. Chapter 2 may already have provided a first hint to answer this question by suggesting that a recipient's evaluation mode (separate or joint) may be an important factor when evaluating the validity of the transmitted cue (i.e., does it adequately represent the construct of interest) .

A related question is whether these signals are transmitted intentionally or unintentionally by the sender. Signals and cues are communication tools that are fundamental for understanding both human and other primates behavior. Indeed, there is a large literature on the role of signals and cues in animal behavior (e.g., Getty, 1998; Zahavi, 1975). Supposedly, the signaling and cueing in the animal domain is automatic rather than intentional. Where human behavior is concerned, the issue is more complicated since signaling can

take place with and without awareness. Presumably, the cues examined in the present thesis occurred mainly at the unconscious level. It seems likely that the signals sent out by people in everyday life can also serve a strategic role and hence may be sent out intentionally. Future research should provide more insight in the conditions under which cueing and signaling occurs with or without awareness.

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