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Judgment and Decision-Making Bias in Representative Negotiation Selection and Evaluation

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Judgment and Decision-Making Bias in Representative Negotiation

Selection and Evaluation

by

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Dedication

To Mom and Dad for your unconditional love and support. Thank you for encouraging me and all of my crazy ideas.

Judgment and Decision-Making Bias in Representative Negotiation Selection and Evaluation

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Representatives are frequently sought by groups and individuals to handle their negotiations. While much research has been dedicated to understanding representative negotiation processes and outcomes, to date there exists little research on how constituents choose and appraise their representatives in negotiation. One factor that makes this gap especially alarming is the extant research on other areas of representative negotiation, which regularly demonstrates that both constituents and agents are prone to biases, heuristic overuse, and constituent influence.

In our initial work, we found a systematic bias in constituent selection and evaluation processes of their representatives, such that highly optimistic representatives are evaluated better than more realistic ones, even when they fail to come through. Across six studies, this dissertation focuses on testing hypotheses regarding the cognitive mechanism which gives rise to this phenomenon, as well as further understanding downstream implications of the representative offer bias. Study 1 found that constituents are biased by their representatives' offers even when they did not choose them. Study 2 demonstrated that offers bias constituents when negotiations are framed around losses.

Studies 3A & B provided evidence that constituents will be persistent in rejecting offers in multi-shot negotiations. Studies 4 and 5 tested whether offers bias constituent risk perception relative to representative assessments and advice finding mixed evidence. Study 6 combined data from a number of state and individual difference measures collected from Studies 1-5 to test hypotheses related to power, control, bullshit receptivity, and optimism, finding generally weak or no support. Together, these findings demonstrate that when a representative reasonably overpromises, they are given more of a halo than they are horns - not to mention being more likely to receive the job to begin with. Beyond evaluation, representative offers appear to bias constituent behavior within the negotiation, though representatives can reorient their constituents with information potential outcomes.

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OVERVIEW AND BACKGROUND

Individuals and groups sometimes seek representatives when entering negotiations of disputes and deals. These representatives are often engaged for their positive attributes or abilities, such as idiosyncratic domain knowledge, general skill as a negotiator, or access to an influential network (Rubin & Sander, 1988). Yet another crucial reason people seek representation in negotiation is for presumed impartiality of the representative, as constituents may possess biases that negatively influence the process or outcome of a negotiation. A recent review identified 11 such biases constituents are subject to, including effects of anchoring, egocentricity, framing, and fixed pie beliefs (Caputo, 2013). Both lay perceptions and research thus suggest that a constituent's preconceptions or expectations for negotiations are not always suited to obtaining an optimal outcome on their own.

However, engaging a representative to negotiate one's behalf is not a silver bullet eliminating bias. Both experimental research and field studies have defined situations in which representatives may not be ideal for a negotiation. Representatives tend to assume that engaging with opposing parties competitively is the ideal default tactic in a negotiation (Benton & Druckman, 1973). This tendency can be especially damaging to a negotiation outcome where sustaining intergroup relations is valued by constituents, since competitive behavior leads to negative opposing party impressions regardless of the outcome (Saygi, Greer, van Kleef, & De Dreu, 2014). Although representatives shift away from this tendency when specifically instructed by constituents, representatives will continue to listen to a subset of the group that values competitiveness even if that fraction is a minority (Steinel, De Dreu, Ouwehand & Ramirez-Marin, 2009). I do not intend to focus on competitiveness in representative negotiation per se, I merely mean to illustrate that bias and influence exists *within* representative negotiation

processes. However, it remains unexplored whether the interaction of constituents and their representatives *before* a negotiation determines negotiation outcomes. While much research has sought to understand representative negotiation processes and outcomes, to date there exists little research on how constituents choose and appraise their representatives in negotiation. In this thesis, I outline a series of studies aimed to clarify and extend a bias in representative selection and evaluation that is found at one of the earliest parts of a negotiation – namely, the initial exposure of a representative to the constituent.

BIAS IN REPRESENTATIVE NEGOTIATION

Constituents enter into negotiations with expectations about the negotiation process (e.g. competitive or cooperative) as well as the outcome (e.g. compromise or integrative). The relative alignment of these interests between constituents and their representatives is especially important to representative selection since they don't necessarily behave in the best interest of their constituency (or even themselves). At times, representatives negotiate in a manner that benefits all parties mutually even when explicitly instructed not to, due to inherent prosocial motives (Aaldering et al., 2013). As a result, when constituents seek representation, the alignment between constituents and representatives will presumably impact constituent perceptions of representatives, influencing the constituent's decision to employ a given representative. However, this alignment may have effects beyond simply whether or not the representative gets chosen by constituents. Small pieces of information about others rapidly affect perceptions of one's personality traits and competence (Winter & Uleman, 1984; Uleman, Rim, Adil Saribay & Kressel, 2012). These perceptions, even unconsciously, have meaningful downstream effects, such as predicting important outcomes including criminal sentencing (Wilson & Rule, 2016) and

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election outcomes (Todorov, Mandisodza, Goren & Hall, 2005). It is reasonable, then, to suspect personality assessments can affect behavior between representatives and constituency in negotiation contexts. Similarly, constituent inferences can be critical to determining the outcome of the negotiation itself. Representative traits like narcissism and prosociality predict how representatives will behave and what the outcome of a negotiation will be (Aaldering, Greer, van Kleef & De Dreu, 2013; Panen, 2014). Whether constituents know this idiosyncrasy or not, constituents understand it is important that they use available information to predict how well-aligned they are with their representatives, or they wouldn't seek representation to begin with.

One way that constituents can gauge this alignment is through a representative's offer amount – how much potential representatives propose they will be able to earn for a constituent in a negotiation. This information could then color a constituent's perception of the representative and vice a versa. For example, emotional displays of constituencies lead representatives to adjust their initial offers - the more negative the display, the higher a representative will adjust their offer (Duijvestijn, 2016). Regardless of motive, representatives seem to understand that their offers influence the way their constituencies perceive them. In this thesis, I argue that representative offers color perceptions not only of their representative, but of the actual negotiation and negotiated outcomes. Before discussing the initial evidence supporting this hypothesis, I first discuss the two competing theories my colleagues and I tested regarding how a constituent may be influenced by representative offers: choosing the highest available offer, and expectation-similar offer. Next, I summarize the research which supported the high offer hypothesis. Finally, I describe a set of studies designed to further understand how high representative offers influence constituent judgment and decision-making in negotiations.

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AGREEMENT

The question lying at the crux of our previous research on representative offers is this: Will a constituent believe someone with an opinion different from their own can actually follow through? Given that constituents have their own expectations about the outcome of a negotiation will be, those who make offers which disagree with this expectation may be deemed untrustworthy, and avoided in favor of representatives proposing offers more in agreement with their expectation. By agreement, I refer to the similarity between what a constituent expects from the outcome of a negotiation and the amount a potential representative offers to earn for them. The notion of agreement between perspectives has been considered in marketing in agent selection (Gershoff, Mukherjee, Mukhopadhyay, 2001), in the social psychology of negotiation within the theory of naïve realism presented by Ross and Ward (1995), and elaborated upon by researchers like Gilovich (e.g. 2000, 2002a) and Pronin (e.g. 2007).

Naïve realism is a framework for cognitive biases related to how one's beliefs motivate their perceptions of the world. Essentially, naïve realism holds that people are motivated to believe they have privileged and unbiased access to the truth, and that other similarly unbiased and rational people will share these views. When others disagree, people engage in a search for what might lead others to hold these different viewpoints, while still maintaining that one's own view is the unbiased truth. What follows from this approach is that those with different views are immediately assumed to be incorrect, for reasons related either to that person's bias or incomplete access to relevant information. The assumption that different views are the subject of error rather than one's own bias or lack of information (or both) also has a tendency to exaggerate the perceived differences between people with opposing views. For example,

Robinson, Keltner, Ward & Ross (1995) found that people on pro-life and pro-choice sides of the abortion debate perceived those on the opposing side as being more prone to partisan ideology than themselves and those that share their perspective. Kruger and Gilovich (1999) found that enthusiasts of several hobbies expected enthusiasts of *other* hobbies to be more biased in their allocation of responsibility in other domains in life. They also viewed fellow enthusiasts as less biased than those of other hobbies.

This structure of opposing viewpoints is of particular relevance in negotiations, where the fundamental aim is to resolve issues of competing motivations. When each side views the other as biased, reaching integrative agreements can be especially difficult. Though it may seem possible to completely avoid bias by engaging a representative, the relationship between constituents and representatives is subject to a similar problem. For example, when the outcome or payment structure of constituents and representatives in a deal-making scenario are not aligned, this can lead to an asymmetry in preferences for the negotiation process. A representative paid hourly has less incentive to come to a swift cooperative settlement than a representative working for a flat rate (Png, 1983). Conversely, a representative that needs to maintain relationships with other negotiators in their network would be less inclined to engage with other negotiators in a fierce, competitive manner (Gilson & Mnookin, 1994). Assuming constituents intuit this information (albeit cynically), and given that differences in perspectives tend to exaggerate perceived differences, constituents may dislike any form of non-agreement in representative offers.

If constituents prefer representatives to agree with their expectations, we expected that any difference from a constituent's expectation – no matter the direction – would result in a more negative evaluation of a representative offer. Constituents would first assume the offer amount to

be incorrect or unjust, and infer that the representative is misinformed or biased. Under an agreement hypothesis, representative offers dissimilar to constituent expectations would be associated with lower likelihood of selection and reduced evaluations relative to representatives with expectation-similar offers.

HIGHEST AVAILABLE OFFERS

Another way a constituent can choose a representative is simply by choosing the highest available offer. To the extent that a constituent believes all available representatives can come through on their offers (thus, avoiding the problems introduced by naïve realism), choosing the highest offer is a reasonable approach in that it signals a representative's competence. But what factors of the constituent or negotiation situation might avoid inferences of incompetence resulting from dissimilar offers? Here I discuss two: Optimism and perceived risk attenuation. I would like to take a moment to note, however, that both of these explanations make the same predictions, and until this thesis we did not gain any insight into which of the two may be more (or less) in play.

Optimism. Folk psychology frequently splits people into binary "types" along a variety of dimensions. Among them are those who see the glass half empty (pessimists) and those who see it half full (optimists). Academic research on this dimension shows that those with optimistic vs. pessimistic expectations differ in the extent to which they hold positive vs. negative predictions about the future, as well as the confidence in their expectations (Carver, Scheier & Sergerstrom, 2010). Research on optimists' (sometimes overly) positive expectations shows a wide range of effects; they pertain mostly to oneself, and can influence the expected likelihood of negative

events, resilience when they actually occur, reduced response to painful stimuli, and generally lead to overall protective physical and mental health benefits (Ellicott, Hammen, Gitlin, Brown, & Jamison, 1990; Hanssen, Peters, Vlaeyen & Meevisen, 2012; Sharot, 2011). Optimistic beliefs about the future extend to financial and economic expectations as well, including underestimation of debt (Seaward & Kemp, 2000), overestimation of future income (Dawson & Henley, 2012; Arabsheibani, De Meza, Maloney & Pearson, 2000), and biased interpretations of investment portfolio feedback (Balasuriya, Muradoglu & Ayton, 2013). Research on economic optimism has demonstrated that extremely optimistic individuals are differentially biased in their self-control strategies, relative to less optimistic individuals (Puri & Robinson, 2007). Specifically, extremely optimistic expectations are associated with shorter planning horizons, intentions of saving less, and not endorsing the belief that saving is a prudent thing to do – all generally ill-advised financial behaviors.

Not surprisingly, being optimistic is associated with biased decision-making as well. Mood inductions of positive and negative affect (tied with optimism) are associated with optimistic and pessimistic expectations, respectively (Lewis, Dember, Scheft & Radenhausen, 1995; Lai et al., 2005). Affect is also linked with susceptibility to bias and heuristic use. Specifically, positive (negative) affect leads to more (less) reliance on heuristics (Alloy & Abramson, 1979; Bodenhausen et al., 1994; Park & Banaji, 1994) and overconfidence (Kramer, Newton & Pommerenke, 1993). Perhaps most germane to the current dissertation is a finding from Anderson and Galinsky (2006), in which power was manipulated as a means to influence optimism and risk-taking in a dyadic negotiation situation. The investigators predicted that increased power would lead to greater risk-taking in negotiations, mediated by optimism about the negotiation. Power was manipulated by giving one of the two members of the negotiation

dyad more resources than the other, and making those resources required for the opposing party to succeed. For example, in some negotiations, the objective of the low-power negotiator was to act as a recruiter filling a work position where there were no applicants other than the high power negotiator. The high power negotiator in these situations was told they had already received another job offer. In other negotiations, the resources were reversed - that is, the high power recruiter had many applicants to review, and the low power applicant did not have another offer. Optimism was measured by assessing how cooperative the opposing party was and whether they had everyone's best interest in mind during the negotiation. Finally, risk-taking was measured by assessing how much information participants were willing to divulge during the negotiation process. As predicted, high power negotiators took more risks during the negotiation, and this effect was mediated by the optimism of negotiators.

Risk Perception. Another way high offers may influence constituents to choose the respective representative is through reducing the extent to which a constituent feels their decision is risky. If constituents tend to give the representatives they encounter the benefit of the doubt in a competence assessment, high offers may reduce the extent to which a constituent believes high expectations are putting themselves at risk. Research suggests this may be a reasonable expectation for constituents in a number of scenarios. Overconfidence bias, introduced by Tversky and Kahnemann (1974) can occur because people fail to incorporate new information into their assessments, or because they are more easily able to find reasons to believe positive information (related to the availability heuristic). The illusion of control literature further supports this, in that overconfidence in one's abilities may extend to their control of representatives, leading them to misperceive how large a role chance, rather than skill alone,

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plays in outcomes (Langer, 1975). This may reduce the perceived risk associated with choosing a high offer. This process may lead constituents to underestimate inaccuracy, noise, or even deception surrounding representative offers, leaving them to opt for the highest available offer. This notion is further supported by people's failure to account for small numbers (also featured in Tversky & Kahnemann, 1974; Hogarth, 1980).

Bridging these literatures, we expected the offers representatives made for their potential constituents (relative to the constituent's expected amount) to affect constituent judgment and selection of their representatives. Further, we expected that representative offers would affect the negotiation itself and constituent decision-making style. Regardless of mechanism, under this high offer framework we hypothesized that constituents prefer the most positive representatives – the higher their offer, the better – and would be more willing to work with them as a result. Finally, we hypothesized that representative offers greater than a constituent's expectation would increase risky decision-making and representative evaluations, and vice versa for offers lower than expectations.

The highest available offer approach and agreement, thus, make competing predictions about the manner in which constituents select and evaluate their representatives. Under the highest offer hypothesis, the greater the representative offer, the better. Under the agreement hypothesis, constituents seek representatives in agreement over what should be expected from a negotiated outcome. I now turn to a series of experiments my colleagues and I ran during my graduate studies designed to investigate these hypotheses. All studies in the present manuscript follow the same core structure, as depicted in Figure 1. Participants were placed in ostensible or simulated negotiation situations as constituents seeking representation over a deal or dispute.

Constituents were either informed directly or asked to provide a numerical amount they expect to receive in the negotiation.[1] Constituents believe the expectation amount is conveyed to representatives seeking their work. Constituents get outcome offers from two representatives - what the representatives say they can earn in the negotiation for their constituent. They pick one of the representatives who then engages in an ostensible negotiation with an opposing party. Once the outcome of the negotiation is given to constituents, they provide outcome measures including representative evaluations, offer acceptance, and questions about potential future negotiations.

General Study Design Flow

Indicate expected amount to receive
per trial

Expectation –
Offer
Discrepancy

Choose a representative based on their offer

Outcome –
Offer
Discrepancy

Receive feedback about earned amount

Evaluate representative performance and decide
whether to continue with same representative

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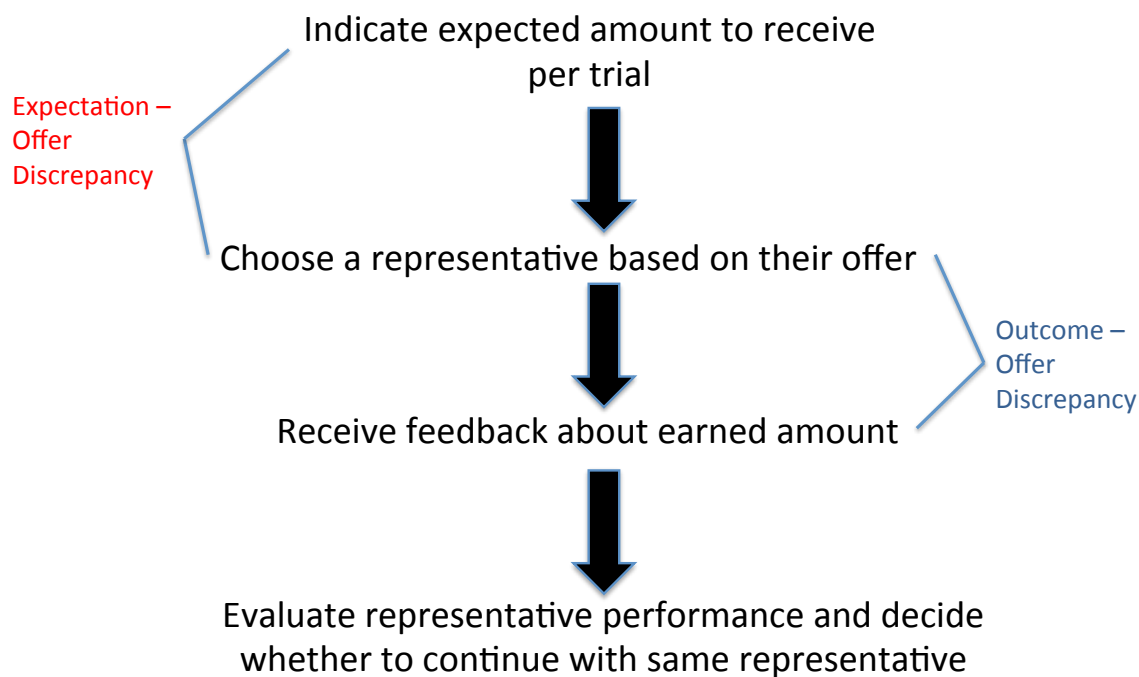
[1] All negotiations were over numerical amounts. In Background Studies 1, 3 and 4, they are monetary, while Background Study 2 negotiations were over point totals. All dissertation studies, however, are simulated monetary amounts.

Figure 1. The general design flow. In each study, participants were asked to indicate how much they expected to receive in upcoming negotiations, per trial. These expectations were ostensibly sent to representatives to evaluate and send their offers back, which participants would then choose from. The negotiation would ostensibly take place, and participants would receive feedback about how much the representative earned. Finally, participants would evaluate their representative's performance in that trial's negotiation, and choose to either continue working with the same representative or work with another representative in the next negotiation.

BACKGROUND STUDIES

Background Study 1[2]

The first study took place during an undergraduate social psychology lecture on social bias. At the beginning of the lecture, the experimenter asked if students with computers would be willing to volunteer for an experiment about bias in negotiations. The basic format of the study followed a variation of the dictator game (see Engel, 2011 for meta-review). The dictator game was used to acquire a wide distribution of subjective expectations based on participants lay beliefs of fairness (Krupka & Weber, 2008). Participants were told they would play two iterations of the game under two of three possible roles (proposer, client, or representative). They were told that they would be anonymously connected with two other classmates playing other roles in the negotiation. As in the classic design, participants were told that proposers would be allocated 10 imaginary dollars, which they were allowed to distribute as much or as little as they would like to those they are connected with as constituents. Although this role was explained to

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[2] For complete details on the methods and results of background studies, please see the Appendix section.

participants, no one was actually assigned to this position, and all participants fulfilled both the representative and constituent roles. This explanation made the design more believable. Further, though all participants played the representative role either before or after playing the constituent role, data acquired from this part of the task was not relevant to the current research question and will not be discussed. Though instructions were explained in terms of a series of interactions with classmates fulfilling other roles, we emphasize that there were no true interactions in the experiment. We hypothesized that participants would primarily wish to work with representatives that agreed with their expectations, and would evaluate their performance based on agreement. To be clear, we did not hypothesize that high offers would predict these measures.

Discussion

We found that constituents' initial expectations of what they would receive in a representative negotiation affected how constituents chose their representatives, evaluated their performance, and made decisions about working with them again. Specifically, constituents unanimously chose representatives that made offers matching constituent expectations whenever possible. Ultimately, this finding indicates participants were paying attention to instructions and taking the task seriously. More importantly, we found that once a representative was selected, the more positive chosen representative offers were to constituent expectations for negotiated outcomes, the more satisfied constituents were with their representative's performance and the more likely they were to choose to work with the same representative again in a subsequent negotiation, after controlling for the difference between offers and outcomes. There was mixed evidence as to the influence of similarity on these variables, where similarity predicted switching to different negotiators, but not satisfaction.

These data provide, to our knowledge, the first evidence that constituent expectations and representative offers in a negotiation scenario have downstream effects lasting well beyond the initial choice of who constituents choose to work on their behalf. Although when representatives fail to come through on an offer they can expect to receive a more negative evaluation, there appears to be something of a halo (horns) given to representatives that are positive (negative) relative to their constituents. It is worth noting that because constituent expectations were so much higher than actual outcomes in this task, the greater the agreement between constituent expectations and chosen offers, the more the representative failed to come through on their offer.

Because expectations were so high relative to outcomes, and offers were so low relative to expectations, this design did not fully test the effect of higher than expected offers or outcomes. Instead, these results are confined almost exclusively to being let down either by representative offers, results, or both. As a result, we were unable to distinguish between the highest available offer and similarity hypotheses. Study 2 aimed to explore higher than expected offers and outcomes on this phenomenon and clarify these hypotheses.

Background Study 2

The fundamental aim of Background Study 2 was to replicate the initial effects of Background Study 1 while addressing several limitations of the initial design. That is, we sought to dissociate disagreement between representative offers and constituent expectations, in any direction, from reactions to subjectively unfair low offers and outcomes. While the results of Background Study 1 offered promising insight into the evaluative process of constituents seeking representation, it remained unclear whether these effects would hold across longer-term constituent-representative relationships or whether constituents become savvy and learn to seek

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representation from a negotiator that actually follows through on their offer. Further, we were not able to clearly dissociate which hypothesis best explains the relationship between representative offers and constituent expectations – highest offer or similarity. In Background Study 2, participants engaged in a series of similar negotiations across 30 trials, while choosing between two experienced representatives - one accurate relative to their outcomes, and one overly positive with their offer (by varying degrees). We hypothesized that constituents would again choose the highest offers, and the same positive relationship would be found between offer amount and satisfaction. We further hypothesized that this effect would not change over time - indicating that constituents do not learn decouple offers from results.

Discussion

Background Study 2 distinguished between the competing hypotheses of high offers and agreement laid out in the background which we were not able to differentiate in Background Study 1. A highest available offer approach predicts that representatives that offer more will be evaluated better, while agreement predicts that offers similar to expectations will be evaluated better. In Background Study 2, we found evidence to support the highest offer hypothesis, as well as evidence that directly contradicts the agreement hypothesis. The higher representative offers were, the better participants evaluated their results and the more likely participants were to remain with them, after controlling for the amount that representatives earned. In contrast to the similarity hypothesis, absolute value differences did not lead to *worse* evaluations, but rather *better* evaluations (with weaker effects). Taken together, these results suggest that there is an asymmetry between offers under vs. over expectations, such that offers *unreasonably higher* than expectations confer more benefits than offers realistically lower than expectations. I make a

realistic/unrealistic distinction based on the fact that all negotiated point outcomes were designed to be roughly 10% lower than participant expectations.

Further, the advantages conferred to high offering representatives extend well into their relationship with constituents. Constituents exhibited biased behavior in an experimental setting with ostensibly experienced representatives for 30 total negotiations. Although participants became less and less satisfied with both representatives over time, this did not affect their judgments relative to individual representatives. Consumers choosing agents have been shown to choose inferior agents for recommendations based on irrelevant or non-optimal task information about the agent (Gershoff, Broniarczyk & West, 2001). In a similar vein, we show that constituents *continuously* choose and evaluate agents based on their offer amount. Having replicated and better establishing these effects relative to the highest offer hypothesis, we designed a series of studies to better understand potentially mechanisms and boundaries of the high offer representative bias.

Background Study 3

The primary aim of Background Study 3 was to pursue moderators and boundary conditions that clarify the effects observed in Background Studies 1 and 2. Specifically, we tested the moderating effect of opening offer, perceptions of control, and trait inferences based on offers. We also tested if the representative offer halo effect extended further in the negotiation to outcome satisfaction and rejection of opposing party offers and dispute resolution scenarios.

Opening Offer. An opening offer is the first price given by a representative to other negotiating parties at the beginning of a negotiation. Opening offers are important pieces of information,

aside from simply setting a reference point. They explain the majority of variance in a negotiated settlement (Van Poucke & Buelens, 2002), and higher opening offers frequently lead to higher settlements (Black & Diaz, 1996; Galinsky, Ku & Mussweiler, 2009; Weingart, Thompson, Bazerman, Carroll, 1995). Assuming constituents infer aggression or competency in representatives from an opening offer (and have previously inferred this from representative offers), then this opening offers may help explain the variance previously predicted by representative offers to their constituents. To test the influence of opening offer amounts, in Background Study 3A we showed participants both actual and opening representative offers while selecting representatives. We hypothesized that opening offers would partially, but not fully mediate the effect of offer amounts made to constituents.

Control. Perceived control is defined as the extent to which a constituent feels they had agency in determining outcomes in the negotiation process. Information passed both from constituent to representative and vice versa is regarded as the primary tool in representative negotiation control (Fassina, 2002). For example, a constituent can conceal their true reservation point such that their representative cannot anchor upon it, and thus more aggressively pursue the expected value (Lax & Sebenius, 1991). The only piece of information constituents can pass in the previously described experimental settings has been expectation amount. Since this is only one piece of information, the extent to which a representative validates a constituent's expectation could be perceived as having influence in the actual negotiation. Though illusions of control (Langer, 1975) can influence representative selection, they may be tempered by actual negotiation outcomes. In Background Study 3C, we asked participants how much control they felt they had over the negotiated outcome. If representative offers influence perceptions of control, we

expected that they would moderate the representative offer halo effect such that high offers are an indication of high control (and vice versa).

Outcome Satisfaction. So far the outcome variables explored relate specifically to the relationship between the constituent and their representative, and we have not investigated how constituents feel about the negotiated outcome itself. Negotiated outcomes are not always aligned with perceptions of satisfaction of the negotiators, relative to their expectations (Oliver, Balakrishnan & Barry, 1994; Thompson, Valley & Kramer, 1995). If offer amounts permeate perceptions of the actual negotiated deal, the representative halo may be an important feature beyond the constituent-representative dyad. To understand how constituent perceptions of overall outcome are influenced by representative offers, we asked participants in Studies 3C & D to indicate both representative as well as overall satisfaction after simulated negotiations.

Dispute Resolution. Another consistent feature of the previously discussed studies is that all negotiations took place in a deal-making context. In these situations, it is commonplace to be geared towards getting the best monetary amount possible. Disputes, on the other hand, typically involve complex relationships that need to be balanced with other features of the negotiation, at times including money (Ross & Ward, 1995). In Background Study 3D, we manipulated previous designs to be in a divorce dispute resolution scenario to better understand the boundaries of the representative offer halo. If high offer biases extend into situations where a cooperative relationship is explicitly preferred, we would expect that higher offers have less of an effect on representative selection and evaluation.

We tested these hypotheses in an online format drawing participants from the online web portal Amazon Mechanical Turk (MTurk). MTurk is a valid way to collect crowd-sourced experimental psychological data (Buhrmester, Kwang & Gosling, 2011), growing rapidly in popularity within the social sciences. Despite the convenience of this data-collection outlet, there are problems associated with participant engagement (see Mason & Suri, 2012; Paolacci & Chandler, 2014 for reviews) and an incentive structure that encourages participants to quickly (and perhaps inattentively) complete MTurk experiments and move on to the next assignment. This presented the ancillary aim of validating an experimental testing procedure in which MTurk would replicate and expand upon our previous results. Consequently, Studies 3 A-D all bore the same general structure as noted in Figure 1. Further, as MTurk workers have become increasingly suspicious of deceptive social scientific manipulations, we opted to test in this environment by asking participants to imagine, or mentally simulate, that they are in the described situation, rather than attempt to convince them of a true in-place negotiation. The primary goal of studies 3A and 3B was establishing that this paradigm was an effective way to test the previously observed effects of Studies 1 and 2 in an online mentally simulated context, as well as to test the effect of opening offer amounts as a mediator of the representative offer halo.

Background Studies 3A & B.

Discussion

The results of these initial MTurk investigation found a replication of both Background Studies 1 and 2 for both the effect of high representative offers against representative performance satisfaction as well as the willingness of a constituent to continue working with a representative in a new antique-selling negotiation. We did not, however, find any evidence that

opening bid amounts, as a marker of representative aggression, had any mediating power in those relationships.

We also sought to test whether the representative halo effect could be replicated in a scenario involving a real-estate agent selling a house, having replicated in an antique-selling scenario. By doing so, we would find support that MTurk is robust to the representative halo effect as a data-collection method in an online context.

These results once again support the notion that MTurk is a reliable way to collect data regarding the high representative offer bias. In Background Studies 3A and 3B, we fully replicated the effects observed in Background Studies 1 and 2. Specifically, the difference between representative offers and outcomes negatively predicts constituent satisfaction with representatives, while the difference between constituent expectations and representative offers positively predicts satisfaction. Further, offer-outcome difference negatively predicted willingness to continue working with a representative, while expectation-offer difference positively predicted willingness to continue with the representative. Background Study 3A was under the context of an antique-selling negotiation, while Background Study 3B was under the context of a house-selling negotiation. In neither study did we find evidence to support the idea that opening price amounts were related to the effects observed. We set out to explore opening price because we believed that, if it was going to be a mediating factor, it would be due to the fact that participants were making inferences about their representatives based on offer, outcome, and opening price amount. Since the opening price was not influential, we decided for Background Study 3C that we would look at inferences more directly. That is, we asked participants to make personality inferences about their representatives once the negotiation was finished to investigate any mediating effects of the high representative offer bias within the

inferences themselves. Further, we asked participants about their perception of control after

negotiations to see if representative offers influenced perception of control (and thereby

satisfaction and continuing likelihood).

Background Study 3C

We used the previous MTurk home-selling paradigm to expand upon previous our work

on the high representative offer bias in two ways. First, we sought to understand whether and

how constituents make trait inferences about their representatives based on expectations, offers,

and outcomes. We had participants complete the same task as laid out in Background Study 3B,

and then they completed a brief personality inventory about their representative. The inventory

we used was the Ten Item Personality Inventory, or TIPI. The TIPI is a validated measure that

briefly assesses respondent personality along the Five Factor Model of personality, or Big 5, with

slightly diminished psychometric properties relative to other, much longer assessment

inventories (Gosling, Rentfrow, & Swann, 2003). We hypothesized that if there were to be any

relationship amongst the halo effect and trait inferences, we would see a positive relationship

between high offers and conscientiousness. In addition to these inferences, we asked participants

how much control they felt they had in the negotiation once it was finished. We hypothesized

that the greater offer amounts were, the more control that the participants would feel they had in

a negotiation. Further, we believed that both control and conscientiousness inferences would at

least partially mediate the effects observed between expectation-offer differences with

satisfaction and continued work with a representative. Finally, we asked participants if they were

satisfied with the outcome of the negotiation, separately from their satisfaction with their

representative's performance, to see if the halo effect might extend into overall negotiation satisfaction.

Discussion

Using the method established in Background Study 3B, we replicated and expanded upon the representative halo effect. We found that high representative offers are positively related to satisfaction with negotiation outcomes generally, not simply representative performance. This effect was found after having controlled for representative satisfaction, indicating that this was not simply a response bias within the survey. Specifically, we show that high offers from representatives lead to more positive feelings about the negotiation, and low offers lead to more negative feelings. To our knowledge this is the first evidence to support the notion that a piece of information as simple as a representative offer can affect the way a constituent feels about a negotiated outcome, regardless of what happens during the negotiation process itself. By showing this, we have demonstrated that the representative offers have far reaching effects beyond just how a constituent engages with their representative in a negotiation.

Perceptions of control had minor relationships with representative offers and performance. Because of this, we do not find the evidence compelling that control has meaningful explanatory power in determining the mechanism underlying how representative offers influence constituent judgment and decision-making. Although perception of control is no doubt influential within a negotiation context, in regard to representative offer effects, control appears loose at best.

Finally, we found that the participants use information about representative offers and performance in a systematic way. Specifically, we found that representative performance is

related to how participants make inferences about four of the Big 5 personality traits (extraversion, agreeableness, conscientiousness, and openness), and high representative offers lead to higher ratings of conscientiousness. That these variables related to trait inferences differentially is not surprising, but is open to interpretation. It's possible that the effects across other Big 5 inferences were simply too underpowered, or that constituents specifically only make inferences of conscientiousness based on offer amounts. Regardless, the effect of conscientiousness inferences did not explain the effect of offer amount in predicting representative satisfaction, and is thus not an ideal explanatory variable in terms of how offers influence representative satisfaction.

Background Study 3D

The primary intention of Background Study 3D was to provide insight into boundary conditions of the representative offer bias in the realm of dispute resolution. Secondarily, we sought further evidence that the representative offer bias had more downstream implications within the negotiation process itself. Specifically, we tested whether offer amount influenced risk-taking in constituents. To accomplish these aims, we altered the format of the vignettes used thus far to be about a divorce scenario in which social as well as monetary outcomes were at stake. We expected that the social reputation component of the negotiation would lead constituents to be less focused on monetary outcomes, and representative offers would have less of an effect on constituent judgment and decision-making. As a result, we expected offer similarity to be preferred over the highest available offer. Once representatives reported back with a potential deal in the negotiation, constituents were asked whether they would accept the deal or reject it as a measure of risk-taking.

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Discussion

In all previous experimental designs, the primary objective for participants has been to maximize gains. With this design, we changed this context to contain social value as well, in order to provide a boundary condition to the high representative offer bias observed previously. Specifically, we instructed participants to try to maintain social ties with their soon-to-be ex-spouse in a divorce settlement context. Although it was not made explicitly clear, we implied that anything other than an even split of assets would put strain on the relationship, since both partners had discussed that they would like to split things evenly according to the vignette materials. Despite our predictions, we did not find support for the hypothesis that representative similarity would be preferred, providing representative offer effects a boundary by adding social capital to negotiation scenarios. High offers, and not similarity, predicted constituents' representative performance satisfaction and willingness to continue with a representative. Further, we found initial evidence that constituents are more likely to take risks in a negotiation process by rejecting proposed deals based on representative offers. Representative offer similarity did not predict risk-taking.

Thus, although we have not yet found a boundary condition for the previously observed effects, we have found that the effect is robust to certain social factors (i.e. divorce settlement). Further, we found further evidence that constituents are influenced by representative offers beyond the way they engage with their representative. Specifically, while in Background Study 3C we found that negotiated outcome satisfaction was influenced by representative offers, the present study found that risk-taking is also enhanced by the magnitude of representative offers.

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As such, we found that the high representative offer bias can have far-reaching impact within negotiation representative selection, negotiation process, and negotiated outcomes as a result.

Background Study 4

We aimed to replicate the finding from Background Study 3D indicating that risk is enhanced by high representative offers in a purely economically irrational context. That is, it is not the case that taking risks in the previous context (selling a house) is necessarily non-advantageous. In certain situations, pushing the opposing party somewhat harder to give may lead to greater outcomes in deal-making scenarios, and since we did not directly manipulate anything that would elucidate such a result, we did not draw the conclusion that constituent risk was necessarily a mistake. To make this claim, in this experiment we had students in an undergraduate psychology class complete a variant of the design used in Background Study 1. In this version, participants played a version of the dictator game in which actual monetary rewards were at stake, and rejecting proposer offers removed the possibility of receiving a reward. To reject any offer greater than 0 dollars in this situation is considered irrational under a purely economic model of decision-making - thereby making any effect of increased risk-taking an effect disadvantageous to a constituent. We also sought replication of personality inference relationships found in Background Study 3D.

Discussion

Thus, having again replicated the high representative offer bias effect, we found that representative offers also drive constituent decisions to reject offers in a dictator game context. As I previously stated, rejecting these offers is indicative of "economic irrationality," since any

offer proposers made offers a chance at receiving some money, while rejecting offers (regardless of how low) offers no chance. This means that constituents are making these decisions in light of other information or feelings. For example, Yamagishi et al. (2012) showed that rejecting offers that are viewed as unfair is driven in part by a desire to avoid feeling inferior or accepting a lower status. Finally, we were unable to replicate any personality inference findings from Background Study 3. It is possible that, since constituents knew each other from class, they had a personality schema in mind for their classmates which drove inferences beyond any effect that the task had.

Background Summary

Across four studies we found evidence of a pervasive phenomenon throughout the representative negotiation process related to the offers that representatives make to potential constituents. First, constituents tend to opt towards the highest offer available. This is not surprising, particularly when the offers in the experiments have been designed to exist within the realm of reasonable possibility. Somewhat alarming, though, is the effect these offers appear to have on constituents after selecting them. Specifically, higher offers lead to better representative evaluations, likelihood of continued professional relationship, satisfaction with negotiated outcomes, and risk taking within negotiations.

In Background Study 1, we found that representative offers were positively related to performance satisfaction and continued representative relationships within a dictator game context. Students in an undergraduate social psychology class were led to believe they were negotiating with their classmates. Since outcomes were negatively biased relative to expectations, it was unclear if a similarity or highest available account best explained the

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observed pattern. Background Study 2 addressed this concern in a laboratory experimental design by directly manipulating whether offer amounts were above or below expectations, finding full replication of Background Study 1 and that an highest available offer account was the better explanation for results. We told participants that their representatives were trained, making expertise judgments a less-likely explanatory factor. Further, we found that participants did not change their behavior with time, they simply got less satisfied with representatives overall.

Background Study 3 moved the negotiation scenario online to MTurk workers who participated in simulated negotiation scenarios. We found that constituents' satisfaction with negotiated outcomes was enhanced by high representative offers, and that this was not explained by perceptions of control within the negotiation process. Higher offers were associated with trait inferences of conscientiousness, but they did not mediate the observed phenomena. Further, we found that constituents made more risky decisions, and that dispute resolution settings did not provide a boundary to the effects of high representative offers. Background Study 4 replicated the offer rejection effect within a classroom context similar to Background Study 1. Results from personality inferences failed to replicate those in Background Study 3, casting doubt on the relative contribution of Big 5 trait inferences influencing constituent behavior.

Overview of Dissertation Studies and Hypotheses

The goal of this dissertation is to provide theoretical shape to the patterns observed in previous studies of biased constituent judgment and decision-making in representative negotiation. That is, now that the high representative offer bias has been observed in a variety of

contexts and we have demonstrated that it has meaningful effects aside from the relationship between constituents and representatives, I aimed to better understand the mechanism that gives rise to it (Studies 1, 4, 5, 6), its boundaries (Studies 1, 2, 5, 6), and how it further influences constituent behavior (Studies 3, 4, 5).

The samples discussed within this dissertation were all collected within the Mechanical Turk research platform, as data from Studies 3A-D demonstrated that participants are able to simulate negotiation scenarios in such a way that similar effects were observed as those in-person and laboratory designs (i.e. Background Studies 1, 2, and 4). In general, each study was collected as its own ~100 person sample, except for Study 6. That is, for Studies 1-5, each sample was collected including a question(s) or manipulation that was not shared in other studies. For the final study, however, I observed the relationships between several individual difference measures collected across all of these samples.

Study 1: Regret and Representative Assignment

In study 1, we attempted to understand more of the underlying mechanism for the previously observed representative offer phenomena with a simple manipulation. To date, in all the studies we ran observing the influence of representative offers, we gave participants multiple offers (i.e. representatives) to choose from before the negotiation begins. Effectively, this is the only true opportunity for a choice that participants have in the experiments which (they believe) will have an impact upon the outcome of the negotiation process and outcomes. Presumably, this makes representative choices a highly salient feature of the task. In line with regret theory research, it is possible that participants point to external sources (e.g. opposing parties) rather

than internal or affiliative sources (e.g. the self or representative) of explanation when evaluating negotiation outcomes.

Regret. When people make decisions under uncertainty, they are often motivated to maximize the utility of their choice as well as minimize the potential regret they may experience by learning about the outcomes (Bell, 1982). Once decision makers learn their choice led to an unsatisfactory outcome, they may believe (or know, depending upon the paradigm) that one of the unchosen alternative options was the better choice. These feelings can be amplified by *anticipated* regret prior to choice, because people are more risk-seeking when they do not believe they will learn the outcomes of unchosen alternatives (Zeelenberg, 1999). This may explain constituent behavior in my previous experiments, since constituents presumably believe they will not learn how an unchosen representative would have performed, leading them to choose the higher (presumably riskier) offer. Regret is also enhanced under explicit choice paradigms, compared to inaction. For example, Kahneman and Tversky (1982) found that people judged events as more regretful when they deliberately made a choice which led to the event, rather than passively letting an event occur through inaction.

Regret has been researched in the negotiation realm as well. When negotiators believed they would not receive feedback about unchosen risky alternatives, they were more risk averse in their negotiation strategies (Larrick & Boles, 1995). Thus, it is possible the biases we have observed so far related to representative offers are attempts to manage feelings of regret that constituents feel when their representative fails to come through by overvaluing their choice (relative to a more realistic, perhaps more skilled alternative). Since the amount of regret experienced in choice is positively related to the number of alternatives the choice involved (Sagi

& Friedland, 2007), when constituents do not have an alternative choice to compare their outcome to, they should not experience regret. Further, assuming that the highest offer available in a two-option choice represents a riskier choice (especially since expectations tend to start off lower in our manipulations), regret may be enhanced by perceiving the alternative choice as a safer, better option. Under our usual two representative choice task, the act of choosing a representative who underperforms may enhance a rebound response relative to passive inaction (e.g. simply being given a representative). To explore this scenario in this study, I did not provide constituents representative options to choose from. Instead, they were told that they already had a representative as well as what that representative offered to earn for them in the negotiation.

Under this framework, I hypothesized that satisfaction, re-hiring and risk-taking tendencies we found based on initial offer amounts so far have been the result of post-decision regret. Once participants chose their representative (and especially once the outcomes of the negotiation have been revealed), participants have been engaging in post-hoc justification strategies to align with their choice of representative, leading to greater satisfaction and likelihood of continuing to work with the representative. As a result, if constituents are not given a choice - if they simple are provided with a representative offer over which they have no control - regret-reducing processes will not be engaged. I predicted that participants without representative options would use offer-outcome differences alone would be used as the basis for which representatives would be evaluated. This highlights the two-step procedure of representative offer amounts. First, higher offer amounts make representatives more likely to be selected, as previously demonstrated. Second, the act of selection itself drives the evaluations in tandem with offer amounts, perhaps explaining downstream offer results.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal, and 97 accurately completed the attention check. Each participant was paid $.50 for their participation in the experiment. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The study followed the same general experimental design as that laid out in Figure 1, except participants did not choose their representatives. Specifically, participants went through the procedure as a person who is selling their house, trying to earn as much as possible in the sale. Participants were told they expected to receive $250,000 based on their research and preparing the house for sale. Unlike the flow in Figure 1, however, participants did not choose between two representatives and their offers. Instead, where the typical task structure would introduce participants to representative options, they were simply told that they had found a representative who planned to earn them some amount of money (their offer amount). Participants learned their representative negotiated a potential deal, and what the monetary outcome of the negotiation was. As in previous designs, we asked participants how satisfied they were with their representative's performance and the negotiated outcome, whether they would accept the potential offer, and whether would like to continue working with their representative. Finally, participants completed an additional survey battery of questionnaires which will not be discussed in this study, including measures of power, bullshit receptivity, likelihood of engaging in risky acts, locus of control, optimism, and affect.

Results

The analysis plan for this study was the same as those run previously, however, there was no chosen offer used relative to a separate alternative offer, simply the given representative's offer. It is critical to note that in this, and all studies within this dissertation, offer amounts are entered into models as predictors rather than offer-expectation differences. The reason behind this simplification is that expectations do not vary across participants (they are all $250,000), and thus no calculation of expectation difference was required.

I first tested whether the previously observed effects of representative offers with satisfaction and continuing to work with representatives held when no representative was chosen using standard OLS regression (see Tables 5, 6 and 7). Counter to my predictions, satisfaction remained positively associated with offer amounts ($b = 3.449$, $t(94) = 7.082$, $p < .001$) after controlling for offer-outcome differences ($b = 4.206$, $t(94) = 11.561$, $p < .001$). Next, I tested the same predictors on whether participants were willing to continue working with their representative using binary logistic regression, which also replicated previous results. That is, higher offer amounts were associated with greater likelihood of continuing with a representative ($b = 3.127$, $z = 3.112$, p $< .01$) after controlling for offer-outcome differences ($b = 4.365$, $z = 4.889$, p $< .001$). Finally, I tested whether offers were related to participants' willingness to reject the proposed offers from opposing parties (again, a binary outcome), finding a similar positive relationship between offers and rejections ($b = 4.630$, $z = 4.001$, $p < .001$) after controlling for offer-outcome differences ($b = 5.663$, $z = 5.431$, $p < .001$).

Table 5. Regression Models Estimating Effects of Expectations, Negotiated Outcomes, and Loss of Deal on Representative

Performance Satisfaction Within Studies 1-5.

Study	Variables	Model 1			Model 2			Model Comparison
		B	SE	t	B	SE	t	χ²
Study 1	Offer	3.449	0.487	7.082***				
	Offer - Outcome	4.206	0.363	11.561***				
Study 2	Offer	3.111	0.592	5.253***				
	Offer - Outcome	3.783	0.387	9.77***				
Study 3a	Offer	2.49	0.628	3.965***	1.598	0.524	3.046**	43.229***
	Offer - Outcome	3.779	0.41	9.204***	2.617	0.37	7.07***	
	No Deal				-1.869	0.259	-7.198***	
Study 3b	Offer	2.952	0.684	4.313***	1.265	0.551	2.296*	
	Offer - Outcome	3.688	0.471	7.824***	1.701	0.423	4.018***	56.826***
	No Deal				-2.563	0.298	-8.594***	
Study 4	Offer	3.271	0.759	4.308***	2.579	0.603	4.274***	49.153***
	Offer - Outcome	3.31	0.463	7.139***	2.356	0.384	6.129***	
	No Deal				-2.227	0.285	-7.807***	
Study 5	Offer	0.202	0.78	0.26	0.572	0.621	0.922	46.5***
	Offer - Outcome	8.297	0.52	1.596	0.546	0.413	1.321	
	No Deal				-2.226	0.294	-7.555***	

*p < .05, **p<.01, ***p<.001. Monetary values were divided by 10,000 for more easily interpretable beta estimates.

Table 6. Regression Models Estimating Effects of Expectations, Negotiated Outcomes, and Loss of Deal on Continuing with Representatives in Studies1-5.

Study	Variables	Model1			Model 2			Model Comparison
		B	SE	t	B	SE	t	χ^2
Study 1	Offer	3.127	1.005	3.112**				
	Offer - Outcome	4.365	0.893	4.889***				
Study 2	Offer	4.765	1.277	3.732***				
	Offer - Outcome	5.421	1.079	5.022***				
Study 3	Offer	3.211	1.292	2.485*	2.544	1.497	1.7	9.49**
	Offer - Outcome	4.282	0.925	4.628***	3.385	1.005	3.369***	
	No Deal				2.385	0.649	3.674***	
Study 3a	Offer	1.996	0.97	2.06*	0.233	1.285	0.182	38.636***
	Offer - Outcome	3.116	0.746	4.171***	1.02	1.09	0.275	
	No Deal				4.442	1.09	4.052***	
Study 4	Offer	4.083	1.417	2.882**	4.471	1.778	2.514*	26.447***
	Offer - Outcome	4.484	0.938	4.561***	4.61	1.206	3.824***	
	No Deal				3.093	0.693	4.459***	
Study 5	Offer	0.543	0.919	0.548	1.4	1.11	1.261	36.381***
	Offer - Outcome	0.804	0.612	1.314	5.98	0.704	0.849	
	No Deal				3.272	0.691	4.734***	

*p < .05, **p<.01, ***p<.001. Monetary values were divided by 10,000 for more easily interpretable beta estimates.

Table 7. Regression Models Estimating Effects of Expectations, Negotiated Outcomes, and Risk Conditions on Opposing Offer Rejections in Studies1-5.

Study	Variables	Model		
		B	**SE**	**z**
Study 1	Offer	4.63	1.157	4.001***
	Offer - Outcome	5.663	1.043	5.431***
Study 2	Offer	5.032	1.443	3.488***
	Offer - Outcome	6.613	1.417	4.667***

		Model 1			Model 2			Model Comparison
		B	**SE**	**t**	**B**	**SE**	**t**	χ^2
Study 3a	Offer	1.162	0.529	2.195*				
	Offer - Outcome	1.841	0.346	5.320***				
Study 3b	Offer	1.814	0.484	3.746***				
	Offer - Outcome	2.136	0.334	6.406***				
Study 4	Offer	0.787	0.535	1.473	3.234	0.757	4.272***	1.778
	Offer - Outcome	1.582	0.326	4.843***	3.262	0.463	7.039***	
	Risk Condition				-4.113	0.313	-1.312	
Study 5	Offer	0.49	0.586	0.837	0.486	0.576	0.843	4.112*
	Offer - Outcome	0.505	0.39	1.296	0.633	0.389	1.627	
	Risk Condition				0.558	0.278	2.007*	

*$p < .05$, **$p<.01$, ***$p<.001$. Models for Studies 1 and 2 were binomial regressions for acceptance or rejected outcomes, which Studies 3-5 were run on continuous rejection number outcomes. Monetary values were divided by 10,000 for more easily interpretable beta estimates.

Discussion

In this study, participants did not have a choice of which agent was representing them in a simulated home-selling negotiation setting. Instead, they were told that they had a representative from the outset, who had made them an offer (or proposal, in this case) regarding their house. In line with research on regret, I predicted that the effects we have observed regarding constituent outcomes (including representative satisfaction and behavioral outcomes) in negotiation would not be present in this sample, as the choice of one's representative is the determining factor in high offer biases. This prediction appears to be unsubstantiated by the data. In all, we found full replication of the previous datasets. Representative offers were associated with satisfaction, such that as offer amounts increased, satisfaction increased as well. Constituents were also more likely to continue working with their representatives in the future as their offers increased. Finally, constituents were more likely to reject offers from other parties the higher their representatives initial offers were.

My search for a boundary condition on representative offer effects did not yield results within this study, given the complete replication of previous designs. It is curious that there is no effect of choice whatsoever, and that effects were similar (or larger) than those previous, given the extant research on post-decision regret. Indeed, these results strongly suggest that such an effect is not in play when constituents do not make their own representative choices. While this does not illuminate constraints on offer effects, it provides substantial help describing the previously observed phenomena. Whereas it was not clear if there were separate effects of choosing higher offers and evaluating negotiation processes and outcomes, these results highlight their distinction. Thus we have learned two pieces of information from this study: experienced regret from representative choice does not drive the previously observed effects, and

representative offers first influence choice, and separately drive constituent judgment and decision-making.

Study 2: Testing Representative Offer Biases in Loss-Framed Negotiations

Across numerous contexts, my colleagues and I found that initial exposure to representatives through their offer amounts starts a chain reaction which reverberates through several parts of the negotiation setting. However, an important feature of these studies thus far has been that every format has been framed around how much participants will gain from the negotiation beyond a 0 starting point, even in the divorce dispute resolution situation. Thus, a question remains: Do high representative offers have the same effects when the negotiation is framed around losses? This question is spawned from the literature on prospect theory generally, and loss aversion specifically as initially put forth by Tversky and Kahneman (1979; 1991). Research from prospect theory demonstrates that an asymmetric "S-function" defines how people generally evaluate risky scenarios. There are three primary characteristics that define the function.

Reference dependence: People evaluate a prospective value change in terms of whether it is a loss or gain relative to a reference point.

Diminishing sensitivity: The incremental change associated with increasing gains and losses diminishes, such that people are more sensitive the closer they are to the reference point.

Loss aversion: People are asymmetrically sensitive to losses, such that the function is steeper for losses than gains relative to the reference point.

Loss aversion is a pervasive behavioral phenomenon observed in a variety of choice scenarios, such as brand preference, (Hardie, Johnson & Fader, 1993), life satisfaction (Vendrik & Woltjer, 2007), and most germane to the present study, job candidate selection (Highhouse & Johnson, 1996). Because the reference point in all previous designs has been well above zero – a "gains" frame – we have not observed whether loss frames affect constituents' judgments of representative offers. While it is not clear whether expectations serve as the primary reference point, I find this unlikely as a gain is still a gain (albeit perhaps lower than one may expect). Further, because it is unlikely that diminishing sensitivity would hold relative to an expected gain (compared to a zero point), it is likely that we have only examined one side of how this model might predict constituent behavior relative to representative offers during a negotiation. As a result, I believe this function provides several predictions about the way constituents will behave in selecting and engaging with representatives in negotiation. First, I predicted that constituents would be susceptible to high (in this case, less negative) offers from potential representatives, completely in line with previous results. As a secondary hypothesis, I predicted that constituents would be more sensitive to high offers from representatives negotiating over what will inevitably result in constituent losses. That is, the effect observed for representative and outcome satisfaction, likelihood to continue with a representative, and offer rejection will be larger than gain-framed equivalent stimuli.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal, all of whom completed the attention check. Each participant was paid $.50 for their participation in the

experiment. Informed consent was obtained in accordance with procedures approved by the UT

Austin Human Subjects and Institutional Review Board.

Procedure. There were two conditions in this experiment, a deal making (n = 50) and a dispute

resolution condition (n = 50). Once again, the study followed the general experimental design

depicted in Figure 1, which varied slightly by condition only in terms of the vignettes used. In

each condition, it was emphasized to participants they would be losing money with minor

differences in the story surrounding the loss.

The dispute resolution condition resembled the vignette in previous Background Study

3C, however, it had to be tweaked to accommodate the task's emphasis on losses. In this version,

participants were told to imagine that they were in a failing relationship in which both partners

sought to split up amicably. However, the participant's partner was incapable of currently

providing for themselves and would be seeking monetary and liquid assets from participants. I

told participants they have roughly $500,000 in assets, and that they expected to lose half

($250,000) in the proceedings. Participants learned they were choosing between two lawyers

who aim to keep their losses to a minimum in the process. Each representative made offers in

terms of how much the participants would be losing in the deal, and offers ranged from $200,000

to $300,000 in $10,000 increments.

In the deal-making scenario, participants were informed they would be imagining a

scenario of relocating and buying a home, rather than selling one. In this situation, participants

were told to imagine they had found a neighborhood that they like in their new area, and that

based on their research on a number of houses they were interested in, they expected to pay

$250,000. Participants in this condition also chose between two potential representatives who

had made offers regarding how much they would be able to negotiate for the price of a house in the participant's neighborhood. These offers were in the exact range as those in the dispute resolution scenario. After each vignette, I told participants what their representative had negotiated a deal for in the respective conditions. All outcomes were in the same range as offers ($200,000 - $300,000). I asked participants how satisfied they were with their representative's performance and the outcome of the negotiation, whether they would accept the deal, and whether they would continue working with the same representative in the future. In addition, they were asked the survey battery discussed in Study 6.

Results

There were no differences between vignette type in this task for any model, so we collapsed analyses across vignettes. Testing the primary aim of this hypothesis required simply running the replication models run in previous designs from data gathered in Background Study 1. The first regression model used the predictors of offer amount and offer-outcome difference as the predictors. As predicted, offer amount was positively predictive of satisfaction ($b = 3.111$, $t(97) = 5.253$, $p < .001$). Next, the test of offers on continuing to work with representatives revealed a positive relationship as well ($b = 4.675$, $z = 3.732$, p $< .001$), and finally the same test on rejecting also revealed a similar positive relationship ($b = 3.674$, $z = 2.042$, $p < .05$). All of these models controlled for offer-outcome differences (see Tables 5, 6 and 7).

I tested the second part of this hypothesis by collapsing data which used similar vignettes across loss and gain frame into one dataset. 50 participants from the house selling and divorce settlement conditions of Background Study 3 were randomly sampled and collapsed into one 200 person dataset. To test whether the slope of the above reported effects was greater for the loss

condition than gains, the linear model tested included offers, offer-outcome differences, and a

dummy categorical interaction term indicating whether the task was gains- or loss-framed. This

model did not reveal an interaction effect for either offers (b = -.338, $t(191)$ = -.442, p = .658) or

gains (b = .423, $t(191)$ = .792, p = .429). I next tested continuing with representatives using

offers (b = 1.638, $t(191)$ = 1.008, p = .313) and offer-outcome difference (b = -1.05, $t(191)$ = -

.754, p = .451) interactions with the loss frame term, again revealing no relationship. Finally, I

tested the same predictor set against rejecting offers. This time, the model revealed positive

relationships for offers (b = 4.354, z = 2.581, p < .01) or offer-outcome differences (b = 6.669, z

= 4.280, p < .001), such that the slope for the effects of offers and offer-outcome differences is

steeper in the loss condition than in the gain condition.

Discussion

This study aimed to demonstrate whether representative offers had similar (or greater)

effects upon constituent judgment and decision-making when their representatives were

negotiating over a loss-framed task, rather than gains. The results find promising support of both

hypotheses. First, I found full support for the initial hypothesis that constituents evaluate their

representatives and reject offers in light of the offers that representatives make initially. Both

variables, as in all the background studies, were positively associated with satisfaction,

continuing with representatives, and rejection of offers. That is, as offers and outcomes

increased, so too did these outcome measures. This demonstrates that for loss-framed

negotiations, the effects of offer amount remain. Secondly, I found some support for the second

hypothesis, that losses affect constituent behavior more than gains. However, this effect was only

obtained when constituents were deciding to accept or reject offers. There were no differences

between loss and gain-framed tasks on representative satisfaction or continuing to work with the representative. As such, it seems that losses only influence the manner people pursue their goal within the task – their representative seems to be unaffected by the fact that a loss is on the line.

It is unclear whether the results for satisfaction and continuing with representatives are an indication that constituents did not adequately mentally simulate the negotiation as conferring a loss (given that they did not truly lose anything), or whether losses are represented mentally in such a way that it does not affect one's view of representatives. Given that we have replicated results under a gain frame without there being true gains involved in the task, the latter explanation seems more appealing.

Study 3A: Representative Offer Bias and Risk-Taking In Multi-Shot Representative Negotiations

In several studies, both within the background and dissertation projects, we found evidence that constituents take greater risks during negotiation processes (i.e. rejecting offers) when their representatives make high offers. Specifically, Background Study 3D found that constituents whose representatives were highly positive in offer over a divorce settlement outcome were more likely to reject offers. Background Study 4 found that constituents were more likely to reject offers even when all offers were in a constituent's best interest to accept. Within the present work, both Studies 1 and 2 found similar effects of constituents' deal rejections being positively related to initial representative offer amounts. Together these results suggest that high offers correspond to greater risk-taking. However, because these were simply one-shot negotiation scenarios, these results are only generalizable to situations in which a deal seemingly must be agreed upon immediately with the opposing party. Though constituents may

believe the negotiation has chance to continue, they have never been specifically instructed so. As a result, the results do not map well to negotiation scenarios in which representatives return to the bargaining table to find an alternative solution that constituents may accept, which are far more common in the real world than one-shot negotiations. Further, previous studies do not relate to risk specifically, per se. That is, a situation involving risk is generally one where a person's potential decision is tied to exposure to a threat. In these situations, since the negotiation is over and done with after rejecting an offer, these are simply costly decisions, where the risk premium is incalculable. A design more sensitive to risk exposure must allow for the negotiation to have potential to continue after an offer is rejected, while increasing the probability that the opposing party is no longer willing to come to the bargaining table.

The experimental structure I used to investigate this was designed to resemble the Balloon Analogue Risk Task (BART) in a representative negotiation setting (Lejuez et al., 2002). In the traditional BART task, participants complete the measure using a computer. They are presented with balloons on screen, each worth a certain monetary amount that they can cash in by clicking a button. However, there is another button allowing them to "pump" the balloon up, which increases its size. Pumping the balloon increases its size and monetary value. Participants are free to pump the balloon as many times as they would like before they choose to stop and cash in the balloon. However, every time participants pump a balloon, they run the risk of "over-inflating" it, which causes the balloon to pop. Different balloons pop at different sizes, so participants are completely unaware of precisely when a pop will occur. Critically, a popped balloon has no monetary value in the task, making every pump a risky decision. Because participants are told at the beginning of the task what their finite number of possible balloons is, it is in the best interest of participants to find an optimal number of pumps that increases overall

monetary value without popping so many balloons that they earn less money than making no

pumps at all. The BART has been used to explore individual differences in risk-preference

(Lejuez et al., 2002), elicit preferences for sensation-seeking and impulsivity (Lauriola et al.,

2014), and differentiate between drug users and non-users (Lejuez et al., 2005). In experimental

manipulations, increased balloon pumps were found in conditions involving sleep deprivation

(Acheson, Richards & de Wit, 2007; Killgore, 2007), acute stress (Lighthall, Mather & Gorlick,

2009), and alcohol use (Lejuez et al., 2007).

In the present study, I constructed a variant of the typical MTurk negotiation design in

which the task is analogically similar to the BART in order to gauge constituent risk-taking in

representative negotiation scenarios. I predicted that choosing high representative offers would

be positively related to risk-taking in bargaining negotiation situations. Specifically, the more

positive a representative offer is, the more likely a constituent will be to reject opposing party

offers, leading to more entirely failed negotiations. Further, when a deal is lost and opposing

parties no longer wish to negotiation, I predicted this would negatively influence satisfaction and

willingness to continue with a representative.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal, all of

whom completed the attention check. Each participant was paid $.50 for their participation in the

experiment. As bonuses were involved in this task, many participants (n = 56) were given

additional money based on their performance ($.10 - $.28, M = $.16). Informed consent was

obtained in accordance with procedures approved by the UT Austin Human Subjects and

Institutional Review Board.

Procedure. Participants completed a procedure identical to the house-selling design used in Study 3C, with additional features after the vignette. Participants were selling a home hoping for $250,000, and seeking a representative to negotiate as much as possible. They chose between two based on their offer amounts. Specifically, additional dependent measures were added to create a more ecologically valid negotiation situation in which participants were able to renegotiate with opposing parties by rejecting offers in the hopes that a better deal may be achieved by their representative in an ongoing negotiation. As stated above, this was designed ultimately to be similar to the BART task, in that every rejected offer would result in a chance of greater payout, and a chance that the negotiation would fail completely.

Once participants chose a representative, they were informed that their representative had eventually negotiated a deal with an interested party for an amount ranging from $200,000 to $300,000 in $10,000 increments. They were told how much this amount equated to in MTurk bonus amount, which was the same across all participants regardless of the actual outcome amount. In this version, participants did not have to immediately accept this deal. Instead, they were able to reject the offer and continue the negotiation. It was emphasized that although the negotiated amount may increase, it is also possible that the interested party would no longer be interested in negotiating a deal, and there would be no sale (and thus, no MTurk bonus amount). All participants were given the opportunity to reject their given deals 3 times. Each of the first two rejected offers resulted in increasing outcome amounts and bonus amounts. After the third rejection, participants found out that the opposing party was no longer interested and the deal had fallen through, resulting in no bonus. Although true BART tasks involve a variable number of "pumps" (in this case, rejected offers), since this was a one-shot negotiation, there was no need

to vary the number of rejected offers since participants would not be able to "learn" the optimal number of rejections. As such, a general risk tolerance was assessed within the one-shot negotiation. Once participants completed the BART phase of the task, they were asked how satisfied they were with their representative's performance and the outcome of the negotiation, whether they would accept the deal, and whether they would continue working with the same representative in the future. Finally, they completed a subset of the survey battery for Study 6.

Results

Overall, 64 participants rejected opposing offers overall, and 44 participants rejected enough opposing offers for the deal to be lost completely. Once again, I first tested whether offers and offer-outcome differences yielded similar effects to those of the previous studies (see Tables 5, 6 and 7). Offers positively predicted satisfaction with representative performance ($b =$ 2.490, $t(97) = 3.965$, $p < .001$) as well as likelihood of continuing with representatives ($b =$ 3.211, $z = 2.485$, $p < .05$). The size of the effect of continuing with a representative was notably smaller than in previous studies, due largely to the increase in standard error of the model term. Next I tested whether these same outcome measure would be better explained by models that included whether or not the deal fell through. As predicted, lost deals predicted satisfaction with representatives ($b = -1.869$, $t(96) = -7.198$, $p < .001$) and willingness to continue working with a representative ($b = 2.385$, $z = 3.674$, $p < .001$) (see tables 5, 6 and 7 for full model results). Further, each of these models better predicted the simpler models ($\chi^2 = 43.229$, p<.001, and $\chi^2 = 26.447$, p < .001, respectively).

Next, I tested whether the number of times participants rejected offers from opposing parties was related to initial exposure to chosen representative offers, once again using OLS

regression and the sum of rejections as a continuous dependent variable. In line with the previous results for one-shot rejections, both offer amount ($b = 1.162$, $t(96) = 2.195$, $p < .05$) and offer-outcome differences ($b = 1.841$, $t(96) = 5.320$, $p < .001$) predicted the number of times participants rejected offers from opposing parties. Finally, I tested whether the objective monetary outcomes was reduced as chosen offer amounts increased. This model was a binary logistic regression in which bonuses were entered as the dependent measure (any bonus = 1), with offers and outcome differences as the dependent measures. This model found that both offers ($b = -1.941$, $z = -2.056$, $p < .05$) and outcome differences ($b = -2.468$, $z = -3.937$, $p < .05$) negatively predicted receiving a monetary bonus from the task.

Discussion

The present study aimed to give constituents an opportunity to enter a more realistic representative negotiation simulation by providing the opportunity to reject offers made by opposing parties. Once participants had chosen their representatives based on their offers, they were able to reject up to three offers in the hopes of gaining more money in the negotiation. Up until the third rejection, they could accept the offer for a monetary reward. I again found that the representatives who constituents chose were evaluated based on how high their initial offer amounts were. This was true for both performance satisfaction and likelihood of being rehired. Further, I found that the largest predictor of these measures was whether a deal fell through from too many rejections – however, the effect of offers still held, albeit to a lesser extent. The number of deals from opposing parties that constituents rejected was also positively associated with how large representatives' initial offers were. Finally, constituents were less likely to receive actual objective monetary bonuses from the experiment as chosen representative offer amounts

increased. Taken together, this presents an alarming context for the effects of representative offers on constituent judgment.

Previously, I had only demonstrated that once a deal was completed, assessments were made based on the value gained in the negotiation and representatives' initial offers, and it remained unclear whether other markers of representative performance would negate these effects. This is the first study to provide evidence that, although completely losing a deal is detrimental to representative performance reviews, constituents remain biased by their representatives' initial offer amounts (should they choose to continue working with them). Ironically, losing a deal is more likely when the offers representatives give are high. It remains possible that, although one's assessment may drop temporarily, it rebounds once a deal is finally secured with a future party. However, in make-or-break deal-making scenarios, this may never be an option, in which case the present finding suggest that representative offers have far-reaching negative impact upon constituencies, biasing them to take too many risks in the hopes of earning a (perhaps impossibly) high amount.

Study 3B: Replication of Study 3A

Study 3A found that the effects of representative offers on constituent behavior extended into multi-step negotiations. However, it is notable that effect sizes of offer amounts across models were smaller once other factors were introduced (e.g. loss of deal, multi-step negotiations) in that study. The effect of offers on risk-taking was especially low (see table X), which was somewhat alarming given its focus in the study. Ioannidis (2005) has suggested that the practices used for statistical analysis and peer-review lead to the widespread publication of type 1, false positive effects. Further, the staggering lack of reproducibility found for small

effects tested in the replication project (Aarts et al., 2014) is cause for alarm when reporting

small effects after large manipulation changes. This report also found that replication results are

best predicted by size of original effects, rather than factors of the research teams or project.

Given the importance of Study 3A's offer rejection results to this dissertation, I conducted Study

3B to ensure the reported effects had replicable results.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal. Two

failed to correctly complete the attention check and their data was subsequently discarded. Each

participant was paid $1.00 for their participation in the experiment. A total of 47 bonuses were

were given to participants based on their performance ($.10 - $.28, M = $.17). Informed consent

was obtained in accordance with procedures approved by the UT Austin Human Subjects and

Institutional Review Board.

Procedure. The procedure used was completely identical to that in Study 3A. Please see Study

3A methods for details.

Results

For overall study results, please see Tables 5, 6 and 7. The analysis of particular interest

was the model predicting the number of rejections using offers and offer-outcome differences as

predictor independent variables. This model revealed positive effects of both offers ($b = 1.814$,

$t(95) = 3.746$, $p < .001$) as well as outcomes ($b = 2.136$, $t(95) = 6.406$, $p < .001$). It is hard to say

much about changes in effects from only two samples, however, I believe it is of note to mention

that this model's effect size is larger than the one previously tested ($r^2 = .304$, compared to .24). The logistic regression model testing bonus likelihood replicated the negative effects of offer amount ($b = -3.201, z = -3.158, p < .01$) and outcome differences ($b = -3.891, z = -4.812, p < .001$). Thus, based on these samples, the effects appear to hold within this negotiation context.

Discussion

This study did not aim to extend previous results across new contexts or apply boundaries, it was only intended to assess the reproducibility of Study 3A's primary result. That is, Study 3A found that representative offers are positively related to a constituents propensity to reject opposing party offers within a task environment designed to be similar to the BART task. It was successful in this aim. Specifically, the effects of this model were repeated, and even larger, than the identical model's results in Study 3A. In line with Greenwald, Gonzalez, Harris & Guthrie (1996), errors associated with null hypothesis statistical testing are best avoided when reported findings are simultaneously important to present theory, and when statistical testing used avoids use of p = .05 for isolated findings. As a result, I feel it is prudent to assume confidence in the effect under the task conditions specified.

Study 4: Testing Risk Perception and Representative Offer Bias With Representative Risk Assessments

Studies 3a and b demonstrated that the downsides of high representative offers may be more far-reaching than previously thought. Should the effect of high offers simply lead constituents to be over-satisfied with their underperforming representatives, this wouldn't be a

huge problem for constituencies. After all, a happy client is a happy client (even if the client must be happy with less than they wanted). However, I found that high initial offers from representatives affect constituent judgment such that they become willing to take greater risks than if their representative's offers were lower, perhaps leading to an opposing party's unwillingness to continue negotiation with the constituent and representative. The present study aimed to continue assessing these affects, while giving representative's more of a voice in the decision-making process of their constituents. In the previous studies, it seemed (from the constituents' end) that the only responsibility of their representatives was to do the in-face negotiation with opposing parties, acting as a "black box" intermediary for their constituencies. While part of a representative's job is certainly to conduct the actual negotiations for their constituencies, another facet of their job is to provide their expert advice for their constituencies.

This study gave representatives a more robust role during the course of the BART multi-trial task used in Studies 3a & b. Specifically, they provided their constituents with their belief in the probability of success probability of success was in seeking more from opposing parties in either high risk or low risk conditions. Probability of success is not "advice" per se, however, it is useful information about the negotiation coming from a presumable expert. This additional manipulation had two fundamental purposes. First, continued feedback from representatives during negotiation more closely resembles an actual negotiation process. Second, this manipulation helps tease out the mechanism underlying how risk-taking may be affected by representative offer amounts. The phenomenon of risk is cognitively construed. That is, for every scenario there is an objective amount of risk based on information from previous events, and a perceived risk represented cognitively by decision-makers. Although generally people seem to know when many scenarios are objectively riskier than others (Lichtenstein et al., 1978;

Wintefeldt, John, & Borcherding, 1981), these estimates are considerably more accurate when they have actual experience in the situation (Thompson & Mingjay, 1991). One of the main reasons constituents seek representatives in negotiation is their lack of experience (Rubin & Sander, 1988), leading to inaccurate risk estimates in negotiation. Further, and most importantly, a number of biases and heuristics have been identified as affecting risk perceptions, including representativeness, availability, and anchoring (Tversky & Kahneman, 1973 & 1974).

If high offers bias constituents to reduce their perceived risk of the situation, then those in a high risk scenario should be affected by offer amounts in a positive linear fashion. That is, I hypothesized that those in the low risk condition would accept more offers than those in the high risk condition. I further predicted that representative offer amounts would interact with objective risk percentage such that those in the high risk condition would have a stronger linear relationship between offers and rejections.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal. All 100 correctly completed the attention check. Each participant was paid $1.00 for their participation in the experiment. A total of 70 bonuses were were given to participants based on their performance ($.10 - $.28, M = $.19). Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The procedure used was an extension of that used in Studies 3a & b, and was nearly identical except for one additional piece of information. Participants were instructed to mentally simulate the same house-selling vignette, and chose their representatives based on their offer

amounts. However, once the BART negotiation task was under way and an interested party made participants an offer, their representative told them they could keep the offer for a specified monetary amount (as well as its real life bonus amount), participants were told that their representative gave them a percentile risk assessment of the situation. These estimates related to the probability that the opposing party would refuse to continue negotiating after participants had rejected a deal. There were two conditions for these percentage estimates, a "high" risk (n = 50), and "low" risk (n = 50) condition. In the high risk condition, risk estimates started at 60% probability of losing the deal, and grew 10% with each rejection up to 3, when the opposing party would refuse to continue negotiating. In the low risk condition, these estimates started at 10%, and also grew by 10% with each rejection. Thus, the difference in recommendations between conditions was always 50%. If participants accepted a deal or they rejected three potential deals (in which case the opposing party refused to continue), the negotiation was over. Once participants completed the negotiation, they again answered how satisfied they were with their representative and whether they would continue working with them. Finally, they completed the battery reported in study 6.

Results

Overall, 68 participants rejected opposing offers overall, and 55 participants rejected enough opposing offers for the deal to be lost completely. There were no differences between risk conditions of rejection amounts ($t(97.519) = .082$, $p = .934$) or lost deals ($t(97.98) = .302$, $p = .762$).

I first tested the replication of initial offer results from previous studies regarding satisfaction with representatives and continuing to work with representatives (for full model

results see Tables 5, 6 and 7). These models found similar positive relationship effects to those observed before for offers with satisfaction ($b = 3.271$, $t(97) = 4.308$, $p < .001$) and continuing with representatives ($b = 4.083$, $z = 2.882$, $p < .01$). Once again, however, the models that best predicted both satisfaction as well as continuing to work with representatives included the additional predictor variable of losing the deal, which greatly negatively impacted these measures ($b = -2.563$, $t(94) =$, $p < .001$, and $b = 2.385$, $z = 3.674$, $p < .001$, respectively). Next, I tested the replication model of offer amount with the number of rejected opposing offers participants made, which did not reproduce the effect ($b = -.490$, $t(95) = -.837$, $p = .404$). Finally, I tested the hypothesized interaction model in which offers and offer-outcome differences interacted with the risk condition factor. This model suggested I failed to find an interaction with offers ($b = 1.502$, $t(94) = 1.392$, $p = .167$) or offer-outcome differences ($b = -.620$, $t(95) = -.931$, $p = .354$).

Discussion

This study aimed to identify whether offer amounts were associated with changes in risk perception by providing constituents risk estimate percentages from their representatives (either high or low) during the same BART style negotiation task used in Studies 3a & B. I hypothesized that the effect of offer amounts would be larger in the high risk group, as risk estimates in the low condition would require less construal adjustment in order to be deemed "safe" to reject. I did not find evidence for this hypothesis.

There were no differences between groups regarding the number of rejections or the number of lost deals. This is especially of note as a manipulation check, since these differences are essentially prerequisite to infer offer amounts having an impact upon risk perceptions.

Further, the number of lost deals in this study (30) was considerably lower than the number of

rejections in either Study 3A (44) or 3b (53). As such, I see at least two potential hypotheses.

First, if constituent perceptions about base rates of losing deals has been anywhere under ~20%,

the percentages used in this study simply overshot that amount. Second, assuming that a dual-

process system (e.g. Chaiken, 1987) has been involved in previous studies, having provided

concrete risk percentages may have driven participants into a more "type 1" style of thinking.

This style of thinking recruits more cognitive prospection, resulting in thinking more about

potential negative outcomes (van Gelder, De Vries, Van Der Pligt, 2009), which would

dramatically reduce rejections.

Regardless, this study replicated the previous results indicating that offers increase

representative satisfaction and continuing to work with representatives. Additionally, losing a

deal was still greatly predictive of these measures as well, indicating that participants were

indeed using information about the task to make their assessments. Taken together, this indicates

that under certain conditions, risk-taking is affected by representative offers. However, when

representative provide specific types of feedback (i.e. risk assessments), constituents are unfazed

by offer amounts and use another method to determine their willingness to reject offers. Once the

negotiation is complete, however, constituencies remain quite pleased with representatives who

made high offers to them initially. However, this is not the only (or even the most likely) type of

information that representatives would give to their constituencies during a negotiation, though.

In many cases, although representatives will be internally making assessments of risk privately,

they will choose to provide their constituencies binary recommendations about their next move

in the negotiation. This was explored as a better way to provide constituents insight from

representatives during the negotiation process in Study 5.

Study 5: Testing Risk Perception and Representative Offer Bias With Representative

Recommendations

Expert offers (Loschelder, Friese, Schaerer & Galinsky, 2016) and recommendations (Josh & O'Connor, 1999) are highly salient and influential pieces of information during negotiation processes. We have explored how offers influence constituencies numerous times within the context of representative negotiation across this dissertation, but we have not yet assessed the actual advice that representatives give to their constituencies. Although Study 4 aimed to assess how risk perception was affected by offer amounts in terms of actual raw percentiles, it is not evident that constituents used that information in a linear fashion as I had expected. In the present study, representatives gave constituents binary recommendations about whether they should reject a proposed offer from opposing parties. In this way, I could assess whether or not participants were more or less willing to take the advice of their representative based on their initial offer amounts. Indeed, if the dual-process hypothesis proposed in Study 4's discussion holds water, then I would expect constituents to take fewer risks whenever they are given a hard fact about the state of the task (especially when money is on the line). In general, it is thought that people think of risk with a "fuzzy" representation, in which they try to boil down the decision to a simpler gist (Reyna, 2004). This is especially poignant given the decision-makers tendency to be especially bad at dealing with actual percentiles, frequently overestimate small risks and underestimating large ones (Gilovich et al., 2002b).

In order to keep in line with this empirical background, I changed the manipulation used in Study 4 to a similar "gist." Constituents were either given recommendations from their

representative to reject or accept offers from the opposing party. If risk perceptions are altered by initial offers from representatives, in this study I expected constituents to be less likely to take the recommendation of their representative when the recommendation was to accept the offer, rather than reject and seek more. Further, this manipulation is more ecologically valid, in that representatives are more likely to provide recommendations than they are to simply give their constituencies a risk assessment in the form of a percentage.

Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal. Two failed to correctly complete the attention check and their data was subsequently discarded. Each participant was paid $1.50 for their participation in the experiment. A total of 45 bonuses were were given to participants based on their performance ($.10 - $.28, M = $.21). Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The task used was identical to that used in Study 5 (see above), save for one change. During the BART negotiation task, when participants were choosing whether to accept or reject opposing offers, their representatives gave them a recommendation of how to act. There were two conditions for this recommendation type, accept (n = 49) and reject (n = 49). In the accept condition, participants were told their representative recommends accepting the deal before the opposing party walks away when making their decisions. Those in the rejection condition were told their representative believes they can earn more and recommends rejecting. After three

rejections (or accepting the deal) the negotiation was over, and participants completed the

satisfaction and continuing outcome questions, as well as the Study 6 battery.

Results

In total, 68 participants rejected offers, and 55 of those continued rejecting until they lost

the deal completely. There was a trend indicating that the high risk group rejected more offers

than the low risk group ($t(94.773) = 1.723, p = .087$), and a much less convincing trend for

rejecting three offers and losing the deal completely ($t(95.874) = 1.425, p = .157$).

The models testing replication of representative offers influence upon satisfaction and

continuing, for the first time, failed to find any effects (see Tables 5, 6, and 7). Specifically,

representative offers did not predict constituents' satisfaction with representatives ($b = -.202$,

$t(95) = -.260, p = .795$), and interestingly, neither did outcome-offer differences ($b = 8.297, t(95)$

$= 1.596, p = .113$), although there was a slight positive trend. Once again, losing the deal

completely was a large negative predictor of satisfaction ($b = -2.226, t(94) = -7.555, p < .001$)

and continuing with representatives ($b = 3.272, z = 4.734, p < .001$). Next, I tested the models

predicting the number of rejections that participants made. I did not find an effect of offer

amount ($b = .486, t(94) = .843, p = .401$) or offer-outcome difference ($b = .633, t(94) = 1.627, p$

$= .107$), though again there was a trend. Finally, the model including these variables as well as

the risk condition factor, yielded a significant result of condition ($b = .587, t(94) = 2.007, p < .05$,

though the effect was nearly marginal, which was surprising given my expectation.

Discussion

In this study, constituents were given offers as well as recommendations for deciding whether to accept opposing party offers within a house-selling negotiation process. These recommendations were either pro- or anti- rejection, though otherwise the study was identical to the Study 4. I aimed to better understand whether risk perception is altered by representative offer amounts such that constituents take greater risks during negotiation processes. This hypothesis was not supported. In fact, this study is the first in which I failed to replicate even the effects of representative satisfaction and continuing with representatives. To be specific, initial offers and offer-outcome differences did not positively (or negatively) predict representative satisfaction or willingness to continue working with representatives. When having lost a deal was entered into these models, this factor was negatively predictive of dependent measures, indicating that constituents used information about the negotiation setting to make their inferences, but not offers and outcome differences. Further, in models predicting opposing offer rejections, the representative offers constituents chose and the differences with outcomes were also not predictive. However, there was a small effect of recommendations, in that constituents were more likely to reject an offer if their representative told them they should try to earn more from the other party.

There are a number of potential reasons why these results were observed. The first, which I would be remiss not to mention, is power. Although there was a trend of offer-outcome difference, it did not reach significance. It's possible that, as more features of the negotiation environment are introduced and become more realistic, the effects of the previously observed phenomena get weaker, and we would need to collect more data to observe them. Given offer-outcome differences strength of effect in previous studies, we may not expect to see an effect of offer amounts until these differences are sufficiently powered.

Another explanation is related to the advice-taking literature. Clients tend to use advice from consultants based on the quality of advice and trustworthiness of the person providing it (Jungermann, 1999). Though we did not measure these constructs, these results demonstrate that it would be useful to see whether the effects observed presently are related to changes in perceived quality and trustworthiness based upon offer amounts. Further, when decision-makers take advice, there tends to be an offloading, or sharing, of responsibility amongst advice giver and taker (Harvey & Fischer, 1997). It's possible that our previous findings have been the result of individual responsibility of the representative. When responsibility is shared through advice-giving, constituents are no longer biased by their representatives' offers. Using advice tends to be cognitively demanding for advice-takers (Harvey, Harries & Fischer, 2000), so the extent to which deliberative vs. automatic processing is involved in offer biases may further explain these findings.

These results demonstrate a boundary condition for offer amount effects. Given a well-controlled negotiation situation in which representatives have little power save for organizing potential offers for their constituents to handle on their own, constituent judgment and decision-making may be affected by their representative's offers. However, when there are enough features for constituents to pay attention to *after* offers have been accepted, this information is used instead to evaluate representatives as well as how to decide on opposing offers. One note that is somewhat odd is that the effect of recommendations was somewhat small, even when other factors were controlled for. It's promising that there was an effect, however, that I could find no better explanation for offer rejections or representative evaluations is somewhat alarming.

More than anything, I took this to indicate that there exist a variety of other factors at play in these scenarios which may relate to constituent behavior, such as constituent state features (e.g. receptivity, feelings of power), or individual difference trait features (e.g. locus of control, optimism). Because it seems clear that participants were indeed paying attention to the task, and taking evaluations seriously, I chose to investigate such variables across all present studies collected. This is the focus of the final study of my dissertation.

Study 6: State and Trait Assessments and the Influence of Representative Offers

In Study 5 I demonstrated that constituents use many features of the negotiation scenarios used, and I have explored only a subset of them in the present work. This study aimed to explore more driving factors within representative negotiations by taking another perspective. Specifically, rather than the typical situational approach used in the preceding experiments, this study collects a series of questionnaire batteries across Studies 1-6 in order to understand if there are any stable psychological traits which influence susceptibility to high offer biases, or if there are core psychological tendencies which are influenced by representative offers. For the sake of the reader's time, I will provide a brief executive summary of results: the only planned analysis that revealed predicted effects was the Bullshit Receptivity Scale. Locus of control, risk, power, optimism, and affect measures did not have the predicted outcomes.

Locus of Control. How much objective control one has over one's life is distinct from how much control one perceives oneself to possess. This concept was introduced decades ago by Rotter (1954) as the locus of control, a personality trait that is stable across the lifetime. Those with an internal locus of control believe they have a high degree of agency of their behavior and

outcomes. It is their own thoughts and actions which dictate the course of their life. At the other

end of this spectrum, those with an external locus of control feel that external forces beyond their

control influence their lives. They are essentially a passenger in a car, and events in life happen

to them. This construct has associations with many practical psychological factors. For example,

those with an internal locus of control tend to be politically conservative and be higher in

socioeconomic status, while those who have an external locus of control are liberal and are lower

in socioeconomic status (Cohen, Vigoda & Samorly, 2001; Gootnik, 1974). Weiss and Larson

(1990) found that having an internal locus of control was associated with more healthy life

behaviors. In an organizational setting, those with internal loci of control were shown to take

more action towards changing jobs (Martin, Veer & Pervan 2007). In a representative

negotiation setting (especially those that we have explored so far) one might expect those with an

external locus of control to be more comfortable with their representative regardless of outcome,

as the outcome of negotiations is seemingly predetermined. However, those with an internal

locus of control might be uncomfortable giving up the reins to another, and be especially hard on

them when they fail to come through. In light of this research, I collected this scale predicting

that high locus of control scores (being more external) would positively predict satisfaction and

continuing with representatives.

Risk-Taking. For some time, experimental investigations of risky decision-making focused

primarily on situational designs, as researchers found that groups had more consistency than

individuals in their risk propensity and perceptions (Sitkin & Pablo, 1992). Although risk was

frequently conceptualized as something similar to a stable trait (Pratt, 1964; Weber & Hsee,

1998), it was difficult to find a meaningful assessment of risk that encapsulated how an

individual would behave across different types of risky-situations. A potential explanation for variations across situations was that risk perception was variable across different domains (Bromiley & Curley, 1992; Weber & Milliman, 1997; Xie & Wang, 2003). Stemming from the results supporting this, Weber, Blais and Betz (2002) introduced the domain-specific risk attitude scale (DOSPERT), a scale that separated risk-taking across several domains, with the intention of conceptualizing of individual risk-perception as having individual reliability at the domain level. Indeed, individual responses on the DOSPERT have been associated with both experimental and real-world risk-taking (Blais & Weber, 2006; Hanoch, Johnson & Wilke, 2006; Zuniga & Bouzas, 2005). The domains included on the scale cover ethical (e.g. having a romantic affair), financial (e.g. investing in a business venture), health and safety (e.g. engaging in unprotected sex), social (e.g. disagreeing with an authority figure), and recreational domains (e.g. skydiving). I collected this scale across Studies 1-5. I hypothesized that constituents who scored highly on specific subdomains of the DOSPERT would show greater risk taking in representative selection, evaluation, and negotiation behavior. Specifically, in purely deal-making scenarios, risk will be positively associated with the financial subscale, while in dispute scenarios, risk will be positively associated with financial, social, and ethical subscales.

Pseudo-profound Bullshit Receptivity. It is unclear if constituents handle high representative offers as an indication that they are lacking critical information that is necessary to understand the negotiation setting. High offers may be an indication that the representative is aware of something participants are not. To this end, it is possible that those who seek and endorse higher offers may be "reading too far" into the offer value. I used a measure of "pseudo-profound bullshit" receptivity to investigate this. Bullshit is defined as a statement that implies, but does

not contain, adequate meaning or truth (Frankfurt & Bischoff, 2005). For example, the sentence "Unparalleled transforms meaning beauty hidden abstract" is completely incomprehensible due to its lack of syntactic structure, while "Hidden meaning transforms unparalleled abstract beauty" is meaningless while upholding English grammatical and syntactic rules. Importantly, since the sentence follows these rules, it implies a meaning, where there is none. The propensity to infer this meaning without actually understanding or grasping the sentence's content is referred to as "bullshit receptivity." Previous research on pseudo-profound bullshit demonstrates that people who are willing to accept pseudo-profound bullshit statements as actually profound are less reflective, lower in verbal and fluid intelligence, and more susceptible to believing paranormal, religious, and conspiratorial explanations of events (Pennycook et al., 2015). Thus, the bullshit receptivity scale (BSR) provides a means to assess the extent that constituents that accept overly positive offers by inferring representatives have meaningful information about the negotiation, where none truly exists. I hypothesized that the biasing effects high representative offers on participant behavior were moderated by trait pseudo-profound bullshit receptivity. Specifically, participants who score highly on the BSR are more likely to choose overly positive representative offers, and these scores will partially mediate the propensity to engage and highly rate high-offering representatives.

Power and Optimism. Optimism has received much attention in this document, given that it has empirical and theoretical relationships with the observed phenomena. However, there are yet other constructs suggested from previous experimental investigations related to optimism. Specifically, the construct of power has a long history of behavioral relationships within negotiation settings. Some investigators approach power from the position of a structural view,

in which one has clear ability to influence another (Bacharach & Lawler, 1981), however, my focus is on perceived power. That is, the subjective belief that one may influence another. Perceived power, be it fictitious or real, influences a wide range of behaviors within negotiation contexts, including aspirations, demands, concessions, and information processing style (De Dreu, 1995; Pinkley, Neale & Bennet, 1994; De Dreu & Van Kleef, 2003; see Kim, Pinkley & Fragale, 2005 for review). Perceived power has both state and trait like qualities – while some individuals have higher or lower feelings of perceived power than others (Ng, 1980), it can also be directly manipulated (Anderson, John & Keltner, 2005). The sense of power scale, developed by Anderson and colleagues is able to capture both state and trait power, such that an individual's assessment of a current dynamic (i.e. negotiation situation) or general perceptions of relationships in their lives can be assessed, and these assessments correlate with actual power hierarchies in their lives. I tested both in relationship to high representative offers and constituent behavior. I predicted that state and trait perceived power is positively associated with representative evaluations and continued relationships.

Power is a complex construct that shares variance with a number of other potentially mediating factors I controlled for. Those who are higher in power pay more attention to information about rewards, and experience greater positive affect (Anderson & Berdahl, 2002; Langner & Keltner, 2008; Smith & Bargh, 2008). Importantly, low power is also associated with lower affect – offering a possible explanation for the effects of high and low offers relative to expectations. Given this, I controlled for these effects using the PANAS scale in planned models.

Finally, the relationship between power and optimism is important to clarify. Anderson and Galinsky (2005) showed that those put in high power (relative to low power) positions within a negotiation took greater risks during a negotiation process. Critically, this effect was

mediated by how optimistic constituents were when approaching the negotiation. While risk was

defined in this situation as the amount of information divulged to the opposing negotiator, the

relationship between general risk and power is not entirely novel aside from this study (Lerner &

Keltner, 2001). In light of this literature, I predicted that power increases risk-taking in

negotiation, and is mediated by optimism within the context of representative offers.

Table 8. Collection of scales across datasets. An X denotes that the scale was collected, a blank

denotes it was not.

	N	DOSPERT	BSR	Locus of Control	Power	PANAS	LOT-R
Study 1	97	X	X	X	X	X	X
Study 2	100	X	X	X	X	X	X
Study 3A	100	X		X	X	X	X
Study 3B	98	X					
Study 4	100	X		X			
Study 5	98	X		X	X	X	X

An X denotes that the scale was collected, a blank denotes it was not.

Methods

The data collected for this study was run across the previous five studies. However not all

studies included all questionnaires. Table 8 shows a breakdown of scales collected in each study.

In almost every study, all participants first completed the negotiation task prior to taking scales

in order to ensure that participants were entirely focused on the negotiation task first, and would

not try to ascertain the purpose of the task based on the questions they previously answered. The

only exception to this is the power scale. In studies 3a and 3b, participants either completed the

power scale before or after the negotiation task to test whether offer amounts actually changed

perceptions of power within the negotiation scenario. To assess risk-taking, the DOSPERT scale

was collected (Weber, Blais & Betz, 2002), all subscales were summed and calculated

separately. Locus of control, power, positive and negative affect, and bullshit receptivity scores

were calculated as the sum of all collected (and reverse scored) items.

Results

Across all studies, 445 participants chose the highest offer available out of a possible 497

choices. The first model I ran tested if choosing high representative offers was positively related

to risk-taking in bargaining negotiation situations. A regression analysis was conducted

predicting rejection of offers with offer amount and offer-outcome difference as predictors. This

analysis did not reveal the predicted results, as there were no relationships between risk taking

for financial ($b = -7.07$, $t(393) = -.828$, $p = .408$) or social ($b = -4.684$, $t(393) = -.455$, $p = .65$)

situations and offer rejections. Next, I tested if constituents who score highly on financial

subscales show biased evaluations of representatives and continuing with representatives.

Neither the model predicting satisfaction ($b = -9.118$, $t(590) = -1.034$, $p = .302$) nor continuing

with representatives ($b = .011$, $z = .806$, $p = .42$) showed any relationship controlling for offers

and outcome differences.

Next, I tested if the effects high representative offers have on participant behavior are

moderated by trait bullshit receptivity, again using linear regression while controlling for offers

and outcomes. I found a positive linear relationship with the scale and satisfaction, such that

participants who score highly on the BSR are more satisfied with their representative's

performance ($b = 3.380$, $t(193) = 3.321$, $p < .01$). However, this effect actually enhanced the

influence of offers (b = 3.332, t(193) = 9.096, p < .001), rather than controlling for it. BSR scores also positively predicted continuing with representatives (b = .059, z = 2.629, p < .01) after controlling for offers (b = 4.234, z =5.06, p < .001) and outcome differences (b = 5.039, z =6.954, p < .001). Finally, BSR scores were not correlated with choosing the highest offer (r(489) = .05, p = .594). Thus, this is an additional feature to the offer bias framework.

Scores on the state Sense of Power scale were negatively correlated with choosing highest offer amounts (r(389) = -.078, p = 081), in the opposite direction of what was predicted. In the model testing the replication of Anderson & Galinsky (2005), I found no predicted results. Specifically, neither State of Power (b = .012, t(290) = 1.04, p = .295), Life Orientation Test (b = .024, t(290) = 1.406, p = .161), positive (b = .016, t(290) = 1.113, p = .266) nor negative affect (b = -.005, t(290) = -.337, p = .736) had relationships with satisfaction (or continuing with representatives). Optimism on its own did not predict satisfaction (b = .007, t(392) = .621, p = .535) or offer rejections (b = -.012, t(195) = -.821, p = .412). Duration of time on task did not predict satisfaction (b = .0001, t(592) = .565, p = .572) or continuing with representatives (b = .0001, z =.741, p = .459).

Discussion

The person by situation debate has been raging for some time, seemingly going back and forth depending on the study. Back in the beginning of the debate, Walter Mischel, a proponent of the situationist side of the argument, has said that only intelligence should be considered a trait that can generalize to behavioral tendencies (1968). If anything, this study can be considered a victory for the situationists. While the present collected data did not test intelligence, it did test a number of other measures. Our results indicate that perhaps Dr. Mischel should have to waited

for the Bullshit Receptivity Scale to speak so boldly. I am being tongue-in-cheek, however this sentiment captures the essence of results of the present study. A number of measures were collected across the previous 5 studies. These measures included the DOSPERT assessing risk-taking, the LOT-R assessing optimism, the BSR for assessing pseudo-profound belief endorsement. Of all the predictions made (in light of previous research), the only one to yield predicted results - indeed, any relationship at all - was the tendency for constituents who endorsed many pseudo-profound statements to be more satisfied and willing to continue with representatives. However, this prediction was not entirely accurate. The effect of the BSR enhanced, rather than mediated, the effects of offer amounts. As a result, I believe that what this scale measured is something relative to the entire experimental procedure. That is, people who participated in this experiment that believed there was something deeper happening within the experimental manipulation were more likely to endorse the pseudo-profound statements of the scale.

Risk-taking measures did not reveal the predicted results with actual negotiation risk-taking. Specifically, financial and social risk-taking measures showed no effects within deal-making and conflict resolution (respectively) negotiation satisfaction, representative relationship and actual risk-taking in the form of offer rejections. It remains possible that the task was underpowered, however, the sample size being as large as it was, I tend to believe that what has driven the previous effects is the situational arrangement of the task, rather than personal proclivities towards risk.

The locus of control measure also failed to indicate that more external loci of control were associated with representative offer effects. Rather, it seems that both external and internal loci prefer high offers from representatives, and the downstream effects remain the same. As a

result, this individual difference factor does not seem to be playing a role in the how constituents engage and evaluate their representatives.

Finally, power, affect, and optimism showed no effects of satisfaction, continuing with representatives, or offer rejections. This was a complicated model that attempted to replicate the results of a different negotiation setting by Anderson and Galinsky (2005). Given the previous study's direct manipulation to these effects, it is possible that there were more controls within the study to measure these effects, while the studies within this dissertation were not able to be sensitive to these measures. There is one critical piece of information that was gained from this analysis, however. Specifically, the LOT-R optimism scale did not show any relationships with outcome variables of interest, including choosing high offers, representative satisfaction and continued relationship, or rejection of offers. While this scale may not be the perfect assessment of optimism for these purposes (it assesses many types of optimism outside of financial gain), it does further suggest that optimism is not an adequate explanation for dispositional tendencies within previously observed effects. Further, since so many people (90% in this dissertation) have the same general tendency to choose high offers, it appears that something else is at play, giving credence to the high offer hypothesis discussed in the background section of this work.

GENERAL DISCUSSION

The present dissertation has substantially added to understanding of how representative offers influence constituent judgment and decision-making. Across 4 studies we replicated and extended the effects observed in the background. Study 1 demonstrated that the effects of representative offers are not the result of post-decision regret by removing the opportunity to choose a representative. Instead, constituents were forced into who their representative was in

the proceeding negotiation. We found offers predicted several outcomes without this choice, indicating that offer amount simultaneously influences representative choice, and then separately influences downstream judgments.

Study 2 showed that representative negotiations over loss do not reduce or negate the influence of representative offers upon their constituency. In fact, I found that offers in a loss context actually enhance the degree to which these effects are instantiated. One line of research is coherent with our finding that satisfaction is not influenced by a loss frame. Kermer, Driver-Lin, Wilson, and Gilbert (2010) found evidence that loss aversion as it appears in decision tasks is the result of poor affective forecasting. According to these authors, people suspect that a loss will affect them more negatively than a gain would affect them positively, resulting in the multitude of results replicating Kahneman and Tversky's classic result (1991). If these results are related, one might see satisfaction and willingness to continue as assessments of the present – how one feels right now. Insofar as loss aversion does not differentially affect hedonic outcomes, but rather forecasted outcomes, then satisfaction and willingness to continue with a representative should be unfazed. However, the forecasted feelings from rejecting an offer (theoretically to potentially gain more in the future) may continue to be influenced by the loss frame. This potential gain may be overweighed in the process of forecasting. Regardless of the mechanism underlying these results, the Study 2 suggests that high (i.e. more positive) representative offers increase constituent loss aversion, leading them to reject more offers when a negotiation will result in a constituent loss.

Studies 3a and 3b provided important practical applications for downstream influences of representative offers upon constituencies. Specifically, in both studies I found that high offers led constituents to take riskier decisions by rejecting more offers from their opponents. This also led

to many instances in which constituents rejected so many opposing offers that deals were lost completely. Although lost deals were not mapped to real world lost negotiations per se, the number of times a party will come back to the negotiating table is presumably finite and small, leading me to believe that scenarios such as this are likely. Thus, this study provided evidence that a high representative offer has the possibility of leading to a better deal through rejecting offers, but it also has potential to be destructive to completing a deal.

Study 4 aimed to test whether the mechanism driving high offers to increase risk-taking was reduced risk perception. Representatives provided either low or high percentage chances of the deal falling through to constituencies while they decided whether or not to reject deals. There were no differences between groups in any analysis, however, the original effects related to satisfaction, continuing with representatives, and rejecting opposing offers replicated. Of all the studies run in this dissertation, by far the fewest number of offers were rejected, suggesting that the percentile information provided by representatives (regardless of the magnitude) drives constituents away from rejecting offers generally, perhaps so much so that there is no way to observe potential effects of offer amounts.

In study 5, I was still interested in offer amounts influencing risk perception in a different, more realistic fashion that would still allow for observable effects of representative offers. Representatives recommended constituents to either accept or reject offers from opposing offers. Though it was not the predicted pattern of results, this manipulation provided an elusive boundary condition for the effect of representative offers. That is, though constituents generally listened to recommendations from their representatives, their decisions and evaluations were not based on offer amounts in this study.

This research adds to the body of research indicating bias is all too common in influencing the decision-making processes, especially within negotiations. When strong biases are in effect, people on both sides of a negotiation are less likely to accept otherwise reasonable and fair offers, because they are perceived as unfair (Babcock, Lowenstein, Issacharoff & Camerer, 1995; Gelfand et al., 2002; Kriss, Loewenstein, Wang & Weber, 2011). While I did not find evidence of reduced risk perception within these negotiations, I did find evidence of increased risk-taking and positive representative evaluations based on high representative offers. Along the same lines as Thompson and Loewenstein (1992), these findings may be derived from confirmation bias, whereby negotiators and constituents selectively encode and interpret the information relevant to a negotiation such that new information aligns with their perspective. In the representative negotiations described here, these effects may reflect a constituency's perception of what constitutes a fair or reasonable offer may be reinforced by their representative's offer when it is greater than their own, thereby biasing them away from a distributive, fair, and potentially satisfactory solution for all parties.

Further, although high goals can enhance objective outcomes, they lead negotiators to resist new information found in the course of a negotiation, and focus on the distributive, rather than integrative solutions to a negotiation (Polzer & Neale, 1995). Thus, relationships with an opposing party may be strained after a negotiation, which may damage the potential for beneficial outcomes in future negotiations. In a similar vein, negotiators who focus on extreme goals also create more impasses in negotiations (White & Neale, 1994). Interestingly, Galinsky, Mussweiler & Medvec (2002) found that although focusing on high goals led to higher objective outcomes, the negotiators who achieved them were less satisfied with the outcomes. It is important to note that these studies were of direct, rather than representative negotiations.

Regardless, the downstream effects of representative-constituent agreement appear to be substantive, and potentially detrimental to all parties involved in a negotiation. Further research into the mechanism and subsequent effects on negotiation behavior are necessary to fully understand this phenomenon.

When taken altogether, there may a silver lining surrounding the aggregate results. While we found that high offers increased risk-taking (which led to lost deals), once further interaction regarding the actual risks involved took place between constituents and their representatives, increased risk-taking was ameliorated. This was especially true when representatives gave their probability assessments. However, across a number of contexts, constituents' satisfaction with their representatives' performance satisfaction and outcomes (though extremely well-correlated), were frequently high as well. In light of this, representative it's possible that, in the right hands, high representative offers are mostly positive tactic. While high offers bias constituents' representative choices, they may lead to positive relationships between representatives and constituencies. This may develop into a good rapport between parties with well-managed information flow during the process that may dangerously bias risk-taking. For example, assuming representatives who offer to obtain high outcomes on behalf of their constituency actually plan on following through (i.e. they believe their offer is reasonable), a body of research suggests that this may actually enhance outcomes (Black & Diaz, 1996; Galinsky, Ku & Mussweiler, 2009; Weingart, Thompson, Bazerman, Carroll, 1995). Further, since constituents seem to be actually satisfied with their negotiated outcomes and their representatives performances indicates that even if a constituent is getting a worse deal than they otherwise could have with a lower offer, at least they seem to be unaware of it.

Future Directions

This research provides a wide range of potential future directions. First, it remains unclear whether risk perception is actually reduced by high offers in certain negotiation contexts, or if other mechanisms are in play. Studies 4 and 5 attempted to investigate this factor, but the manipulation itself tended to obliterate the actual effect of risk-taking observed in previous studies. A dual systems approach (see Chaiken & Trope, 1999) may explain these results, in that percentiles and recommendations may have made the decision-making process more cognitively involved for participants, which shifted them away from biased decision-making. Regardless, it would be beneficial to find a manipulation such that risk-taking remains, and risk-perception can still be observed. An alternative mechanism that should be explored is whether an anchoring and adjustment approach explains these phenomena. That is, the representative's offer may serve as an anchor from which constituents use to frame their outcomes achieved. Outcome anchors have been demonstrated to play a role in both experimental negotiations (Ritov, 1996; Schaerer, Swaab & Galinsky, 2015; Whyte & Sebenius, 1997) and actual negotiations (Wilson, 2012). Thus, it would not be surprising if constituents' expectations are abandoned as anchors in favor of representatives' offers.

Further, high offers from representatives may only be influential for non-experts. A recent study by Loschelder, Friese, Schaerer & Galinsky (2016) showed that non-experts in one-on-one (non-representative) negotiations were easily influenced by highly precise offers from opposing parties, believing them to be an indication of competence. As the expertise of the offer recipient increased, their view of highly precise offers began to decrease, since experts knew that such high precision is unreasonable within a negotiation. If similar effects exist for positivity, increasing the skill of a constituent may provide further boundary for observed phenomena.

There are a number of reasons why representatives are sought aside from their expertise, so this remains an important question regarding the generalization of these effects to the real world. It is still unclear if, once a new party proposes an offer (as one would expect in certain negotiation settings like selling a home), whether these effects would hold or be diminished.

Finally, the generalizability of these results is perhaps a crucial future direction from an experimental and applied perspective. While I have only investigated how representative offers can affect their constituency within negotiations, there are a number of analogous scenarios in which people look to experts for expected outcomes and make decisions in light of them. If these results can be replicated in a real-world dataset, then it would become a priority to further assess how to de-bias decision-makers on all sides of the decision-making process, negotiation or otherwise.

Conclusion

Across 6 studies I found that representative offers bias constituent decision-making, but that this bias does not always hold. Critically, it seems that the requirements for such positive outcomes require representatives (rather than constituents) to have more control of negotiations, but not complete. Establishing contact between representatives and constituent, especially providing advice, reduced the number of biased outcomes discussed above. These results build upon Fassina's (2002) theory of constituent and representative interaction, which outlines procedures to choose between the relative allocation of power in direct and representative negotiation. This approach uses seven criteria for choosing the proper amount of authority (if any) to be assigned to constituents and representatives in negotiation: dependence on expertise, important of opposing party relationships, emotional influence, time constraints, number of

possible agreements, reputation fit, and outcome accountability. The present results suggest that an additional factor may be important to designation of agency in negotiation: susceptibility to bias in decision-making processes. Depending on the amount of information transfer between constituents and representatives (e.g. recommendations and advice) constituents may become more or less likely to be biased by their representatives' offers. A thoughtful and insightful representative can help steer their constituency away from this bias given the right information, even if they do give unreasonably high offers just to attain a client. If representatives have less control, however, it may be best to provide potential constituents with a reasonable offer to avoid biasing client behavior to drive other parties away from the bargaining table.

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Appendix A: Background Study Methods and Results

Background Study 1 Methods

Participants. Sixty-three students at the University of Texas at Austin completed the experiment as volunteers in a social psychology class. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. Participants were introduced to the experimenter as a guest lecturer in a social psychology class. The experimenter informed volunteers that they were going to be taking part in an online representative negotiation exercise with their classmates. The task was presented to participants through the Qualtrics online survey website, and explained as a variant of the classic Dictator Game.

Those in the constituent role were told their goal in the task was to acquire as much money from the proposer they were connected with as possible, by choosing the best representative to work on their behalf. They were first asked for a monetary amount (integers from 0-10) they expected to receive from the proposer, and that this information would be sent to two classmates playing the representative role. These representatives would see the constituent's expectation and send back an offer amount that they believed they could convince the proposer to allocate to the constituent. The constituent would then be shown these two offers, and select one of the two representatives to send a message to the proposer in an attempt to persuade the proposer to send the constituent as much as possible. constituents were informed how much the proposer allocated to them and asked to provide feedback of how satisfied they were with their representative's performance in this trial on a 7 point Likert scale (1 = "Very Dissatisfied", 7 = "Very Satisfied"). After providing performance feedback, they were asked to choose whether

they would like to continue working with the same representative they chose again in the next trial (trial 1), or if they would continue working with the same representative if there was another trial (trial 2).

Design and Stimuli. When participants first began the constituent role, they were randomly assigned to one of two conditions: an agreement condition and a non-agreement condition. In the agreement condition, participants would be shown two offers from representatives, one of which matched the expected amount they had indicated they would like to receive from the proposer. The second offer randomly differed from the expected amount, and was in the range of 1-5 dollars. In the non-agreement condition, both representative's offers randomly differed from participant expectations, and were both in the range from 1-5 dollars. The need for these conditions arose from our assumption that a disproportionate number of participants would choose offers that matched their expectations, leaving inadequate data to observe how differences from expectations affect judgments. On the first trial, all participants were told they received $2 as the outcome from the proposer, and on the second trial, all participants received $1 as the outcome from the proposer.

Study 1 Results

As predicted, participants provided a wide range of expectations in terms of proposer allocations, ranging from $2 - $6 (M = 4.41, SD = .99). Although we expected dispersion of expectations, they were unexpectedly high, with 10 participants expecting to receive more than half of the money allocated to the proposer. Expecting anything beyond an even split is surprising, because the proposer is explained as having no obligation to allocate anything to

clients. Participants all received $2 on the first trial and $1 on the second, creating a range of discrepancies between what constituents expected and received.

All participants, regardless of the condition they were assigned to, chose the highest offer available to them, as the highest offer hypothesis would predict. There was no difference between groups in expectation amounts, $t(61) = .175$, $p = .861$. Generally, participants were dissatisfied with their representatives ($M = 1.82$, $SD = 1.53$), and they chose to work with new representatives on 75% of trials. This likely occurred because expectations were generally much higher than outcomes.

To test the hypothesis that the high representative offers influenced constituent satisfaction with representative performance, three mixed linear models were run predicting constituent satisfaction (See Table 1). The first model included only the difference between chosen offers and outcomes as a predictor, while the second model included both the difference between chosen offers and outcomes, as well as the difference between expectations and offers chosen as predictors. The final model used the absolute value difference from expectation and chosen offer (instead of the raw difference). Each model also included a nested ID variable to control for within-subject variability across the two trials.

In the first model, as predicted, offer-outcome differences positively predicted constituent satisfaction, $b = .423$, $t(62) = 4.75$ $p < .001$. Because chosen offers tended to be higher than outcomes, this positive relationship indicates that the closer this discrepancy got to 0 (meeting or surpassing the offered amount), the more satisfied constituents were. The second model testing the additional contribution of relatively positive offers between expectations and chosen offers revealed a positive effect of expectation-offer positivity, $b = .421$, $t(61) = 2.82$, $p < .01$, such that the greater offer relative to expectation, the higher constituent satisfaction ratings. Offer-outcome

discrepancy again positively predicted satisfaction, $b = .756$, $t(61) = 5.23$, $p < .001$. The third

model testing similarity of offers to expectations did not find an effect of absolute offer

differences, $b = .128$, $t(61) = .916$, $p = .363$, but found an effect of outcome-offer difference, $b =$

$.484$, $t(61) = 4.44$, $p < .001$. Only the model testing the highest offer hypothesis fit better than the

model with only outcome-offer difference, $\chi^2(1) = 7.86$, $p < .01$. Thus, representatives who are

positive about outcomes going into a negotiation are positively evaluated relative to less positive

representatives - regardless of the whether they come through on that outcome. Both models

were run using the lme function in the nlme library of R.

To test the hypothesis that agreement between expectations and representative offers

influenced constituents' decisions to subsequently work with a new representative, three mixed

binary logistic regressions were run predicting constituent decisions to work with a new

representative. The first model included only offer-outcome difference as a predictor, the second

model additionally included expectation-offer differences, and the third included the absolute

expectation-offer similarity. Once again, each model included a nested ID variable to control for

within-subject variability (see Table 2). The first model revealed an effect of offer-outcome

difference on choosing a new representative, $b = -.493$, $z = -2.15$, $p < .05$, such that the lower the

difference, the less likely a constituent was to choose a new representative. The second model

revealed both an effect of expectation-offer difference, $b = -1.13$, $z = -2.57$, $p < .05$, as well as

offer-outcome difference, $b = -1.32$, $z = -2.96$, $p < .01$. This model indicates that both the extent

to which a representative comes through on an offer as well as how positive that offer is predicts

whether constituents choose to work with a new representative. The third model found an effect

of both absolute expectation-offer differences, $b = -1.35$, $z = -2.92$, $p < .01$, as well as offer-

outcome differences, $b = -1.14$, $z = -3.18$, $p < .01$. Both models testing highest available offers

and similarity fit better than the model with offer-outcome difference alone, $\chi^2(1) = 10.11$, p <

.01 and $\chi^2(1) = 14.3$, p < .01, respectively. Binary logistic regressions were conducted using the

glmer function in the lme4 library of R.[3]

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[3] To ensure playing the representative role before the constituent role did not influence subsequent constituent behavior, analyses were also conducted including task order, negotiator performance, and a subjective measure of self-efficacy as predictors. These predictors added no significant explanatory value to the models and were removed.

Table 1:   Hierarchical Regression Models Estimating Effect of Expectations and Negotiated Outcomes on Representative

Performance Satisfaction in Study 1 (n = 63)

Variables	Model 1			Model 2			Model 3		
	B	SE	t	B	SE	t	B	SE	t
Outcome - Offer	0.423	0.088	4.75***	0.756	0.144	5.23****	.484	.109	4.44***
Offer - Expectation				.421	.149	2.82***			
| Offer - Expectation |							-.128	.140	-.916
(Intercept)	2.65	.226	11.69***	3.49	.366	9.53***	2.87	.325	8.82***
AIC		433.56			427.70			434.72	

*p < .05, **p<.01, ***p<.001

Table 2: Hierarchical Logistic Regression Models Estimating Effect of Expectations and Negotiated Outcomes on Choosing a

New Representative in Study 1 (n = 63)

Variables	Model 1			Model 2			Model 3		
	B	SE	z	B	SE	z	B	SE	z
Outcome - Offer	0.493	0.229	2.152*	-1.321	0.446	-2.96**	-1.145	.395	-3.186
Offer - Expectation				-1.133	.441	2.569*			
| Offer - Expectation |							1.35	.461	2.924**
(Intercept)	.76	.575	1.323	-1.355	0.859	-1.577	-1.559	0.789	-1.976**
AIC		135.5			127.4			123.2	

*p < .05, **p<.01, ***p<.001

Background Study 2 Methods

Participants. Fifty-two students at the University of Texas at Austin completed the experiment for course credit. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. Participants were randomly assigned to either the highly positive or equal-expectation condition before arrival. Upon entering the laboratory, participants were told they were going to be participating in a campus-wide study on negotiation practices. They were told that students in the MBA program were taking a class on representation and negotiation and currently practicing the negotiation techniques they have learned. Participants were told that the business school was studying which practices in representative negotiation were the most effective, and that the psychology department was involved in researching how constituents in representative negotiation relationships come to their evaluations of representatives.

The negotiation scenario itself was adapted from Thompson and DeHarpport (1998). Participants were instructed to imagine planning a vacation with a friend in which vacation factors that the two parties did not agree upon were being negotiated, but not by the participants. They would be choosing representatives from the business school to do the negotiating for them in a computerized task. Participants were given a sheet assigning arbitrary point values as preferences to the five vacation factors to be negotiated, and were told their goal was to obtain as many points as possible in negotiating vacation plans. Participants believed they would be connected with another opponent-representative pair, also aiming to get as many points as possible. The representatives would engage in a text-based chat to determine an appropriate agreement. In order to obtain the most points, participants were told to choose the best representatives to work on their behalf. Participants were assigned two representatives (given the

ambiguously gendered names "Dylan" and "Adrian") to choose from in 30 total negotiations and could work with either as much or as little as they liked. To further enhance the believability of the design, the instructions included a statement indicating that there were two conditions to the study; in one condition constituents could see the representatives chat, thus watching the negotiation take place, and in another condition the negotiation would not be visible. No chat was ever visible. To incentivize task engagement, participants were told that they could enter a $50 raffle if they reach a point criterion. Preference totals were designed to allow the raffle criterion to be challenging but realistic. After the explanation of the task design, participants were asked to review their point preference sheet and to write down the number of points they expected to receive on average per negotiation so the experimenter could send this information to their representatives to view before engaging in any negotiations. After the experimenter left the room to set up the task, participants were informed the MBA students were ready to begin, and welcomed into a separate room to begin the computer task.

Design and Stimuli. Once again, all stimuli in the task were designed for each participant based on the amount they indicated they expected to receive. In the beginning of the task, participants were told they were connected with two representatives and that each had made an offer about how much they believe they can earn for the participant on average from trial-to-trial. In the equal-expectation condition, one of these offers was the participant's expected point amount, while the other pessimistically offered 90% of this amount (each rounded to the nearest value divisible by 5). In the positive condition, one offer was the participant's expected point amount, while the other was 110% of this amount.

Participants chose from and evaluated the performance of their representatives on each of 30 trials. After choosing their representative, they were ostensibly connected with an opponent to

negotiate with, after which the number of points their representative "earned" was shown to

participants. Earned points were designed to be approximately 90% of participant expectations

(SD = 9%) regardless of condition or which representative was selected. Thus, by design, the

only representative that would be relatively accurate was the pessimistic representative. After

viewing the outcome of each negotiation, participants indicated how satisfied they were with

their representative's performance in this negotiation with a mouse on a 100 point sliding scale at

the bottom of the computer screen. After these evaluations, they would choose which of the same

two representatives to work with on the following trial. After all trials were completed,

participants were debriefed, questioned as to whether they were aware of the deceptive nature of

the task, and thanked for their participation.

Study 2 Results

Despite being given the same point reference sheet, participants indicated a variety of

trial-to-trial point expectations, ranging from 200-1120 ($M = 754.32$, $SD = 207.18$). Though

some of these expectations could certainly be said to be more realistic than others, they were all

technically possible. Participants also used the values of the 100-point satisfaction sliding scale

effectively, generally tending to give slightly positive evaluations of performance on the whole

($M = 57.07$, $SD = 26.8$). It is likely that the slightly positive tendency in participant evaluations

was driven to some extent by the task instructions insinuating that the grades of representatives

would be affected by evaluations. Indeed, several participants remarked that this was a somewhat

stressful situation to be put in, however this did not stop them from making evaluations relative

to the performance of their representative. Participants elected to work with the representative

that had the highest offer more than the accurate representative that made the lower offer on first

trial $\chi^2(1) = 15.38$, p < .001. However, overall, there was a solid spread of decisions of whom to work with, and no one representative was chosen significantly more frequently than the other despite a trend of higher offers χ^2 (1) = 2.16, p =.14.

We first sought to replicate and further elucidate the pattern observed in Study 1 across all trials, to observe if the effects of agreement and outcome difference hold across an extended series of negotiations with representatives. Thus, we first ran three models testing the effects of offer-outcome, offer-expectation, and absolute expectation-offer differences on representative performance satisfaction, with participant ID entered as a nested variable (see Table 3). Point units were rescaled by dividing by 100 for more interpretable coefficients. The first model tested only the effect of offer-outcome difference, $b = .180$, t(1,505) = 34.77, p < .01, finding again that the more a came through on their offer, the better their evaluation was. The second model found an effect of both offer-outcome difference, $b = .232$, t(1,504) = 45.25, p < .01, and expectation-offer difference, $b = .239$, t(1,504) = 21.51, p < .01, such that participants were more satisfied with high-offering representatives. The third model testing offer-outcome difference and absolute expectation-offer differences found a similar effect of offer-outcome difference, , $b = .180$, t(1,504) = 35.01, p < .01, and an effect of absolute offer-expectation difference, $b = .040$, t(1,504) = 3.5, p < .01, such that as differences increased in any direction, representative satisfaction increased.

Next, we aimed to further understand the relationship between expectations, offers, outcomes, and continuing to work with a representative after a negotiation (See Table 4). We used the same set of independent variables above to predict using the same representative on the upcoming trial (switching to the other representative). In the first model, offer-outcome difference predicted switching representatives, $b = -.101$, z = -11.5, p < .001, such that the

greater the outcome relative to offer amount, the less likely participants were to switch. The second model found the effect of offer-outcome difference, $b = -.154$, $z = -13.7$, $p < .001$, and an effect of offer-expectation difference, $b = -.174$, $z = -9.12$, $p < .001$, indicating that the participants were less likely to switch from more high-offering representatives. Finally, the third model found the similar effect of offer-outcome difference, $b = -.010$, $z = -11.39$, $p < .01$, and an effect of expectation-offer difference, $b = .028$, $z = 1.8$, $p = .07$, such that any difference from expectation, regardless of direction, increased participants' likelihood of staying with their current representative.

Lastly, we tested whether participants changed their judgments about their representatives using a model testing the effect of time on representative satisfaction judgments. Satisfaction for representative performance went down as the task went on ($b = -.19$, $t(1,453)= -2.63$, $p < .01$) but this did not vary by representative.

Table 3:   Hierarchical Regression Models Estimating Effect of Expectations and Negotiated Outcomes on Representative

Performance Satisfaction in Study 2 (n = 52)

Variables	Model 1			Model 2			Model 3		
	B	SE	t	B	SE	T	B	SE	t
Outcome - Offer	0.180	0.005	34.77***	0.231	0.005	45.25***	0.180	0.005	35.01***
Offer - Expectation				0.239	0.011	21.51***			
| Offer - Expectation |							0.041	0.01	3.54**
(Intercept)	74.2	2.09	35.4***	77.3	2.11	36.51***	72.6	2.10	34.55***
AIC		-856.47			-1,258.46			-866.95	

*p < .05, **p<.01, ***p<.001

Table 4:   Hierarchical Logistic Regression Models Estimating Effect of Expectations and Negotiated Outcomes on Choosing a

New Representative in Study 2 (n = 52)

Variables	Model 1			Model 2			Model 3		
	B	SE	z	B	SE	z	B	SE	z
Outcome - Offer	-0.101	.008	-11.5	-0.154	.011	-13.7***	-0.101	.008	-11.3***
Offer - Expectation				-0.174	0.019	-9.12***			
| Offer - Expectation |							-.028	0.016	-1.79
(Intercept)	-20.5	1.82	-11.3	-25.3		-1.57***	-19.39	1.88	-10.2***
AIC		1,664.1			1,524.3			1,616.5	

*p < .05, **p<.01, ***p<.001

Background Study 3A Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal, however only 96 participants completed the study in entirety. Of these, all 96 accurately completed the attention check. Each participant was paid $.50 for their participation in the experiment. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board. Power analysis of effect sizes in Studies 1 and 2 indicated that this was an appropriate N, conservatively assuming that effects of inattentiveness would add noise to responses.

Procedure. We informed participants initially that they were participating in a simulated negotiation scenario about inheriting an antique which they were trying to sell at an antique convention. We explicitly told participants that no real negotiation was taking place, and asked them to try to immerse themselves in the situation and try to imagine themselves engaging in the described interactions.

The situation was described as inheriting a grandfather clock which they were bringing to a professional antique convention, which they did not feel comfortable doing themselves as the grandfather clock was appraised at a high price. As a result, they were seeking a representative to sell the antique on their behalf. We told participants that the clock was appraised at $25,000, and that this is what they expected to receive in the auction. Participants were then told that they were deciding between two potential representatives in the sale of the clock, who had open price strategies as well as an amount they believed they could earn for the clock. Each of these values (opening price and offer amount) were randomly distributed in increments of $1000 ranging

from $20,000 - $30,000. Once participants chose their representative, they were told the

representative earned them an amount for the vase in the same range.

After the vignette portion of the task, we asked participants how they were satisfied their

representative's performance (1-7 Likert scale), whether they would continue to work with this

representative in the future (yes/no), and which representative they selected as an attention

check. All participants correctly answered this question and were entered into the analysis.

Participants were then debriefed, paid, and thanked for their participation.

Results

First, we tested models to replicate the effects observed in Studies 1 and 2. Specifically,

we tested whether the differences between expectations and chosen offer as well as offer amount

and outcome predicted representative satisfaction and the likelihood of continuing with that

representative. Using multivariate OLS regression, we first a model with chosen offer-outcome

difference as well as expectation-offer difference as predictors of outcome satisfaction. This

model found relationships of both offer-outcome difference[4] ($b = -3.19$, $t(93) = -9.82$, $p < .001$)

and expectation-offer difference ($b = 2.69$, $t(93) = -5.81$, $p < .001$) with representative

satisfaction. Again, the closer an offer was to an outcome, and the more positive an offer was

both positively predicted satisfaction with the representative's performance. Next we tested the

same predictor variables against continuing with the representative in hypothetical future

negotiations in a binary logistic regression, also finding relationships with both offer-outcome

difference ($b = 6.04$, $z = 4.386$, $p < .001$) and expectation-offer difference ($b = 6.74$, $z =$

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[4] Since the values in this experiment were in such large values, model parameter beta amounts are relatively small. To make them more easily interpretable for the reader, we divided all monetary amounts by 10,000. This process does not influence t or p values.

3.655, $p < .001$). Similarly, outcomes closer to offers and positive offers relative to expectations made participants more likely to continue working with their representative. Next, we added a predictor to each of these models testing the effect of opening offer, to observe if this variable mediated the effect of expectation offer differences. We did not find support for this in either the model predicting satisfaction ($b = -.009$, $t = -.258$, $p = .797$) or continuing with the representative ($b = .026$, $z = .241$, $p = .809$).

Background Study 3B Methods

Subjects. 100 participants were recruited through the Amazon Mechanical Turk web portal, and all 100 accurately completed the attention check. Each participant was paid $.50 for their participation in the experiment. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The procedure for Background Study 3B was almost entirely identical to Study 3A, except for parts of the vignette structure. Participants were told that they were home-owners who were looking to sell their current house, and they were currently searching for a real-estate agent to work on their behalf in the sale. Participants were told that they believed that their house was worth $250,000 (ten times the amount of the clock in Study 3A), and were deciding between two potential representatives, again based on their offer amount and their opening price. These values, as well as actual outcome amounts, were randomly distributed in increments of $10,000 ranging from $200,000 - $300,000. After participants chose their representative, they were told that the representative had negotiated a deal with an interested buy for them and what the amount of the deal was.

Once again, participants indicated how they were satisfied their representative's performance (1-7 Likert scale), whether they would continue to work with this representative in the future (yes/no), and which representative they selected as an attention check. Again, all participants correctly answered this question and were entered into the analysis. Participants were then debriefed, paid, and thanked for their participation.

Results

We found complete replication of the Study 3A's results using multivariate OLS regression and binary logistic regression. Both offer-outcome and expectation-offer differences predicted satisfaction with the chosen representative ($b = -.375$, $t(96) = 10.87$, $p < .001$ and $b = .179$, $t(96) = 4.83$, $p < .001$, respectively), as well as the likelihood to continue with the chosen representative ($b = -.846$, $z = -4.596$, $p < .001$ and $b = .338$, $t(96) = 2.84$, $p < .01$, respectively). In models testing the effect of opening offer amount, we again found no relationship with either representative satisfaction ($b = .067$, $t(96) = 1.82$, $p = .07$), or continuing with the representative ($b = -.078$, $z = -.732$, $p = .46$).

Study 3C Methods

Subjects. 105 participants were recruited through the Amazon Mechanical Turk web portal, and 103 accurately completed the attention check. Of those 103, three did not fully complete the survey, for a total of 100 participants. Each participant was paid $.50 for their participation in the experiment. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. We used the same general design that we used in Background Study 3B with additional dependent variables. Participants were read the same vignette about imagining they

were in the position of a person selling their house and seeking a real estate agent to work on

their behalf. Participants were told they had an expected sale value of $250,000, and received

offers from two representatives randomly ranging from $200-$300,000 in increments of $10,000.

After choosing a representative, participants were told their representative earned an amount that

also randomly ranged within this same range.

After being informed of the outcome amount, participants were given a series of

measures about their perception of the negotiation as well as their representative. We asked

measures of satisfaction with representative performance (Likert 1-7), whether they would

continue to work with this representative in the future (yes/no), and which representative they

selected as an attention check. Additionally, we asked how satisfied participants were with the

outcome of the negotiation, separate from their satisfaction with their representative, as well as

how much control they felt they had in the negotiation (both using 1-7 Likert response scales).

Finally, we asked participants to complete a personality questionnaire, the TIPI, about their

representative. Once participants completed these measures, they were debriefed, paid, and

thanked for their participation.

Results

We first tested models to ensure we replicated our previous results. In predicting

representative satisfaction, we confirmed replication of both effects of outcome-offer difference

$(b = -.470, t(97) = -12.28, p < .001)^5$ as well as expectation-offer difference $(b = .385, t(97) =$

$6.86, p < .001)$. We also replicated the effects of outcome-offer difference $(b = .705, z = 5.03, p <$

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[5] We divided predictors by 10,000 again for easier beta interpretation.

.001) and expectation offer difference ($b = -.490$, $z = -3.28$, $p < .01$) in a model predicting willingness to continue working with a representative.

Next, we tested whether outcome-difference and expectation-outcome difference predicted overall outcome satisfaction. To be conservative in this model, we also included representative satisfaction as an additional control predictor, as representative and overall satisfaction were highly positively correlated ($r(98) = .97$, $p < .01$). In addition to finding an effect of representative satisfaction ($b = .916$, $t(96) = 27.12$, $p < .001$), both outcome-offer difference ($b = -.045$, $t(96) = -2.18$, $p < .05$), as well as expectation-offer difference ($b = .049$, $t(96) = 2.16$, $p < .05$) predicted outcome satisfaction. In a model predicting participant perception of control, we found a slight trend of expectation-offer difference ($b = .052$, $t(97) = 1.6$, $p = .1$), and a small effect of outcome-offer difference ($b = -.048$, $t(97) = -2.23$, $p < .05$).

Next, we explored the effects of outcomes and offers on Big 5 personality trait inferences. We tested 5 models with each of the Big 5 as outcome variables. Each model included both outcome-offer differences as well as expectation-offer differences. Outcome-offer differences predicted four of the Big 5 trait inferences -- extraversion negatively ($b = -.038$, $t(97) = -2.21$, $p < .05$), agreeableness positively ($b = .023$, $t(97) = 2.48$, $p < .05$), openness negatively ($b = -.049$, $t(97) = -2.3$, $p < .05$), and conscientiousness negatively ($b = -.161$, $t(97) = -6.1$, $p < .001$). Across all five models, expectation-offer differences only predicted one trait -- conscientiousness positively ($b = .079$, $t(97) = 2.05$, $p < .05$). Using the results from the model predicting conscientiousness, we next tested whether conscientiousness mediated the effect of expectation-offer differences on representative satisfaction and willingness to continue working with a representative. In the model predicting representative satisfaction, we found large effects of each variable -- outcome-offer difference ($b = -.382$, $t(97) = -9.1$, $p < .001$), expectation-offer

difference (b = .341, $t(97)$ = 6.4, $p < .001$), and conscientiousness inferences (b = .544, $t(97)$ = -2.3, $p < .001$). Thus, conscientiousness inferences do not fully explain the effects of offer amounts in representative judgments or decision-making.

Study 3D Methods

Subjects. 104 participants were recruited through the Amazon Mechanical Turk web portal, and 102 accurately completed the attention check. Of those 102, 3 did not fully complete the survey, for a total of 99 participants. Each participant was paid $.50 for their participation in the experiment. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The study followed the same general experimental design as that laid out in Figure 1, with a contextual adjustment such that the negotiation was no longer a purely deal-making scenario. Specifically, participants were told to imagine they were ending an unsuccessful marriage under amicable circumstances. To emphasize the amicable nature of the divorce, participants were told that a continued non-romantic social relationship was preferred by both parties. After establishing this, participants were told that of the total amount of assets was $230,000, and that each partner had decided to split the assets evenly. We told participants that this meant they expected to receive $115,000 at the end of the divorce settlement.

As in previous studies, we provided participants two potential representatives to choose from along with offer amounts that they proposed to get in the settlement. Once participants chose one of the representatives, they were told that both parties representatives had gone through the legal legwork to come to a deal, and what the proposed monetary value of the asset split was between parties. Participants then evaluated how satisfied they were with their

representative's performance (Likert 1-7), and whether they were willing to accept the deal. Finally, we asked participants who did not accept the deal if they would like to continue working with their representative or work with someone new. In total, only 27 participants rejected the proposed deal, resulting in a potentially underpowered subset of participants.

Results

First, we tested whether highest offer was a better predictor of representative performance, as it has been in our previous studies. We ran two models to test this using OLS regression with representative satisfaction as the dependent variable. The first model included offer-outcome differences and expectation-offer differences as predictors, while the second included offer-outcome differences and absolute expectation-offer differences. The first model found effects of both offer-outcome ($b = -.767$, $t(96) = -8.68$, $p < .001$)[6] and expectation-offer differences ($b = .509$, $t(96) = 3.73$, $p < .001$). The second model found an effect of offer-outcome differences ($b = -.567$, $t(96) = 7.37$, $p < .001$), but not absolute expectation-offer differences ($b = .078$, $t(96) = .452$, $p = .652$). As such, we did not find evidence of a boundary condition to representative satisfaction ratings within a dispute resolution context.

Next, we tested whether participants were more likely to reject proposed deals from their representatives based on positive offers or dissimilar offers. Each model was a binary logistic regression using participant responses to accept or reject the proposed deal. Again, the first model included offer-outcome differences and expectation-offer differences, while the second model included offer-outcome differences and absolute expectation-offer differences. The first model found an effect of both offer-outcome difference ($b = -.948$, $t(96) = -4.4$, $p < .001$) and

---

[6] We divided monetary amounts by 10,000 for more interpretable beta estimates.

expectation offer-difference ($b = .551$, $t(96) = 2.0$, $p < .05$), while the second found only an effect of offer-outcome difference ($b = -.786$, $t(96) = -4.35$, $p < .001$, expectation-offer difference non-significant relationship: $b = .368$, $t(96) = 1.1$, $p = .272$). These results indicate that positivity in offer, and not similarity, influence risk-taking in negotiations.

Finally, we tested whether high offers or similarity better explained participants' willingness to continue working with a representative. As mentioned above, since this was framed as a divorce, asking whether participants would be willing to work with a representative in future divorces seemed somewhat odd and overly artificial, so we only asked participants that had rejected the proposed deal. There were a total of 27 subjects who fit this criteria. Each of two OLS regression models testing willingness to continue with representatives included offer offer-outcome differences. The first also included expectation-offer differences, and the second included the absolute value of these differences. The first model found effects of both offer-outcome differences ($b = -.531$, $t(24) = -3.96$, $p < .001$) and expectation-offer differences ($b = .503$, $t(24) = 2.66$, $p < .05$), while the second only found an effect of offer-outcome differences ($b = -.335$, $t(24) = -2.5$, $p < .05$, expectation offer effect non-significant: $b = .161$, $t(24) = .63$, $p = .528$). Once again, this demonstrates that the high offer account better explains behavior than similarity in this study.

Background Study 4 Methods

Participants. Seventy-two students at the University of Texas at Austin completed the experiment as volunteers in a social psychology class. Informed consent was obtained in accordance with procedures approved by the UT Austin Human Subjects and Institutional Review Board.

Procedure. The procedure was based on Study 1, and had a few critical differences. Again, once the experimenter was introduced as a guest lecturer, they welcomed the present students to volunteer to complete a study on representative negotiation. All 72 students had laptops and were able to complete the online task. The task was described as the previous variant of the dictator game in which the roles were proposer, negotiator, and client. Once again, no participants completed the proposer role, but instead completed one round as both negotiator and client. This time, the experimenter explained that the behavior of participants might play out at the end of the experiment. One negotiation would be randomly selected by the experimenter, and whatever the participant in the client role chose to do for that negotiation (accept/reject proposer offer) would be used to determine how much money that participant would be given in actual cash.

No real negotiations took place, and there was no actual interaction between participants. In the client role, participants were asked how much of the proposer's $10 allotment they would like to receive. Participants were told this expectation amount was sent to participants in class in the negotiation role, who would send back offers. Participants were then shown two offers from these ostensible negotiators. Since we were able to get a wide range of expectation amounts in Study 1, we did not tailor offers to expectation amounts. Instead, offer amounts were randomized between $1-10. After participants selected one of the offers, they waited while the negotiation "took place" and then received feedback about what the proposer was willing to split with the participant. Participants were then allowed to choose whether they accept or reject this offer. It was emphasized to participants that if they chose to reject the offer, they would receive no monetary amount if their trial was chosen at the end of the experiment. Participants then evaluated their representative's performance and satisfaction with the outcome of the negotiation (both Likert 1-7), and completed a brief personality assessment of the representative (the TIPI).

This concluded the client portion of the task. At the end of class, a participant was chosen to receive their payout from the task.

Results

Expectations were once again fairly high relative to what one might expect in a dictator game allocation, ranging from $0 - 9 (M = 5.7, SD = 1.8). After debriefing the class and inquiring about this, several students replied that they felt they knew each other, and expected that they would act benevolently. Still, this does not explain expecting more than half of the allocation, however. Participants were more satisfied with their representatives in this class study (M = 3.45, SD = 1.64), likely driven by outcomes being much higher due to randomization between 1-10. We replicated previous results that representative performance was predicted by differences between outcomes and offers (b = .634, $t(69)$ = 9.47, $p < .001$) as well as differences between expectations and outcomes (b = -.384, $t(69)$ = -5.92, $p < .001$). Next, we replicated the finding from Study 3C indicating outcome satisfaction was predicted by offer-outcome differences (b = .193, $t(68)$ = 2.01, $p < .05$) and expectation-outcome differences (b = -.167, $t(68)$ = -2.21, $p < .05$). Again, in this model we controlled for representative performance satisfaction (.289, $t(68)$ = 2.53, $p < .05$), since the correlation between the variables was high (r = .582, p < .001). Further, we found rejecting a proposer offer was predicted by both offer-outcome difference (b = .945, $t(69)$ = 3.96, $p < .001$) and expectation-offer differences (b = .719, $t(69)$ = 3.64, $p < .001$), such that the less a representative followed through on their offer, and the lower an offer was, the more likely participants were to reject the proposer's offer. Finally, we tested whether offer and outcome differences drove TIPI Big 5 personality inferences, and found no relationships across all measures (all p values > .05).

Appendix B: Mechanical Turk Analyses

Mechanical Turk is a now well-established means to acquire fast, inexpensive experimental data in a variety of contexts (Mason & Suri, 2012). That said, it is important to ensure that data quality is monitored across all studies to ensure that I avoid the problems associated with crowdsourced data beyond inclusion of an attention check (Paolacci & Chandler, 2014). I ran several analyses towards this endeavor.

Time of Day/Day of Week. Across all dissertation studies, regression analyses were run replicating the effects of offer amounts upon satisfaction and willingness to continue with a representative which control for the time of day split into categorical daytime, evening, and late night hours, as well as categorically split across weekday and weekend days. MTurker "types" are not randomly distributed across these times, and the reported effects were not driven by a subset of workers based on time. I also tested time and date as numerical continuous variables, again finding no relationship with outcomes across all studies.

Engagement. MTurkers at times engage in activities other than the task at hand while completing surveys (Neka, Cacioppo, Norman & Cacioppo, 2016), which frequently increases the amount of time on the survey. I ran the same regression models in Time of Day, this time controlling for the amount of time spent on the survey. Once again there were no effects of duration on task.

Appendix C: Measures

Life Orientation Test – Revised (LOT-R)

Please be as honest and accurate as you can throughout. Try not to let your response to one statement influence your responses to other statements. There are no "correct" or "incorrect" answers. Answer according to your own feelings, rather than how you think "most people" would answer.

A = I agree a lot

B = I agree a little

C = I neither agree nor disagree

D = I DISagree a little

E = I DISagree a lot

1. In uncertain times, I usually expect the best.

[2. It's easy for me to relax.]

3. If something can go wrong for me, it will.

4. I'm always optimistic about my future.

[5. I enjoy my friends a lot.]

[6. It's important for me to keep busy.]

7. I hardly ever expect things to go my way.

[8. I don't get upset too easily.]

9. I rarely count on good things happening to me.

10. Overall, I expect more good things to happen to me than bad.

Note:

Items 2, 5, 6, and 8 are fillers.   Responses to "scored" items are to be coded so that high values

imply optimism. Researchers who are interested in testing the potential difference between

affirmation of optimism and disaffirmation of pessimism should compute separate subtotals of

the relevant items.

The DOSPERT Scale (from Blais, & Weber, 2006)

To generate a short version of the scale with items that would be interpretable by a wider range of respondents in different cultures, the 40 items of the original scale (Weber, Blais, & Betz, 2002) were revised and eight new items were added. The response scale was modified slightly by increasing the number of scale points from 5 to 7 and by labeling all of them (i.e., instead of just the two endpoints) in an effort to increase the psychometric quality of the scale (Visser, Krosnick, & Lavrakas, 2000). The new set of 48 items was administered to a group of 372 North Americans, and this group was randomly split into two sub-groups. Data from one sub-group were analyzed in an exploratory manner and resulted in a 30-item model that was tested through confirmatory factor analyses using the other sub-group (Blais, & Weber, 2005).

The *risk-taking* responses of the 30-item version of the DOSPERT Scale evaluate behavioral intentions -or the likelihood with which respondents might engage in risky activities/behaviors- originating from five domains of life (i.e., ethical, financial, health/safety, social, and recreational risks), using a 7-point rating scale ranging from 1 (*Extremely Unlikely*) to 7 (*Extremely Likely*).[7] Sample items include "Having an affair with a married man/woman" (*Ethical*), "Investing 10% of your annual income in a new business venture" (*Financial*), "Engaging in unprotected sex" (*Health/Safety*), "Disagreeing with an authority figure on a major issue" (*Social*), and "Taking a weekend sky-diving class"

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[7] The six financial items can be split into three gambling and three investment items for further decomposition of the construct. Conversely, all 30 items can be added up, yielding an overall scale score, for a broader assessment of the risk-taking constructs. These models were also tested through confirmatory factor analyses (Blais, & Weber, 2005, 2006).

(*Recreational*). Item ratings are added across all items of a given subscale to obtain subscale

scores. Higher scores indicate greater risk taking in the domain of the subscale.

The *risk-perception* responses evaluate the respondents' gut level assessment of how

risky each activity/behavior is, using a 7-point rating scale ranging from 1 (*Not at all*) to 7

(*Extremely Risky*). Item ratings are added across all items of a given subscale to obtain subscale

scores, with higher scores suggesting perceptions of greater risk in the domain of the subscale.

The internal consistency reliability estimates associated with the original 48-item English

risk-taking scores ranged from .70 to .84 (mean α = .78), and those associated with the risk-

perception scores, from .70 to .81 (mean α = .77), as reported by Weber, et al. (2002). The

authors also found moderate test-retest reliability estimates (albeit for an earlier version of the

instrument) and provided evidence for the factorial and convergent/discriminant validity of the

scores with respect to constructs such as sensation seeking, dispositional risk taking, intolerance

for ambiguity, and social desirability. Construct validity was also assessed via correlations with

the results of a risky gambling task as well as with tests of gender differences.

Domain-Specific Risk-Taking (Adult) Scale – Risk Taking

For each of the following statements, please indicate the **likelihood** that you would engage in the

described activity or behavior if you were to find yourself in that situation. Provide a rating from

Extremely Unlikely to *Extremely Likely*, using the following scale:

_____-

_____

1	2	3	4	5	6	7
Extremely	Moderately	Somewhat	Not Sure	Somewhat		
Moderately	Extremely					
Unlikely	Unlikely		Unlikely		Likely	
Likely	Likely					

1. Admitting that your tastes are different from those of a friend. (S)

2. Going camping in the wilderness. (R)

3. Betting a day's income at the horse races. (F/G)

4. Investing 10% of your annual income in a moderate growth diversified fund. (F/I)

5. Drinking heavily at a social function. (H/S)

6. Taking some questionable deductions on your income tax return. (E)

7. Disagreeing with an authority figure on a major issue. (S)

8. Betting a day's income at a high-stake poker game. (F/G)

9. Having an affair with a married man/woman. (E)

10. Passing off somebody else's work as your own. (E)

11. Going down a ski run that is beyond your ability. (R)

12. Investing 5% of your annual income in a very speculative stock. (F/I)

13. Going whitewater rafting at high water in the spring. (R)

14. Betting a day's income on the outcome of a sporting event (F/G)

15. Engaging in unprotected sex. (H/S)

16. Revealing a friend's secret to someone else. (E)

17. Driving a car without wearing a seat belt. (H/S)

18. Investing 10% of your annual income in a new business venture. (F/I)

19. Taking a skydiving class. (R)

20. Riding a motorcycle without a helmet. (H/S)

21. Choosing a career that you truly enjoy over a more secure one. (S)

22. Speaking your mind about an unpopular issue in a meeting at work. (S)

23. Sunbathing without sunscreen. (H/S)

24. Bungee jumping off a tall bridge. (R)

25. Piloting a small plane. (R)

26. Walking home alone at night in an unsafe area of town. (H/S)

27. Moving to a city far away from your extended family. (S)

28. Starting a new career in your mid-thirties. (S)

29. Leaving your young children alone at home while running an errand. (E)

30. Not returning a wallet you found that contains $200. (E)

Note. E = Ethical, F = Financial, H/S = Health/Safety, R = Recreational, and S = Social.

Domain-Specific Risk-Taking (Adult) Scale – Risk Perceptions

People often see some risk in situations that contain uncertainty about what the outcome or

consequences will be and for which there is the possibility of negative consequences. However,

riskiness is a very personal and intuitive notion, and we are interested in **your gut level**

assessment of how risky each situation or behavior is.

For each of the following statements, please indicate **how risky you perceive** each situation.

Provide a rating from *Not at all Risky* to *Extremely Risky*, using the following scale:

_____-

_____

1	2	3	4	5	6	7
Not at all	Slightly	Somewhat	Moderately	Risky	Very	Extremely
Risky	Risky	Risky	Risky			
Risky	Risky					

Domain-Specific Risk-Taking (Adult) Scale – Expected Benefits

For each of the following statements, please indicate **the benefits** you would obtain from each

situation. Provide a rating from **1 to 7**, using the following scale:

_____-

_____

| 1 | 2 | 3 | 4 | 5 | 6 | 7 |

No benefits   Moderate

Great

  At all   Benefits

     Benefits

Reference:

Blais, A-R. and E. U. Weber. 2006. "A Domain-specific Risk-taking (DOSPERT) Scale for

Adult Populations." *Judgment and Decision Making, 1*, 33-47.

Bullshit Receptivity Scale.

Instructions: We are interested in how people experience the profound. Below are a series of statements taken from relevant websites. Please read each statement and take a moment to think about what it might mean. Then please rate how "profound" you think it is. Profound means "of deep meaning; of great and broadly inclusive significance."

Please rate profoundness on the following 5-point scale: 1= Not at all profound, 2 = somewhat profound, 3 = fairly profound, 4 = definitely profound, 5 = very profound.

1. Hidden meaning transforms unparalleled abstract beauty.

2. Good health imparts reality to subtle creativity.

3. Wholeness quiets infinite phenomena.

4. The future explains irrational facts.

5. Imagination is inside exponential space time events.

6. We are in the midst of a self-aware blossoming of being that will align us with the nexus itself.

7. Consciousness consists of frequencies of quantum energy. "Quantum" means an unveiling of the unrestricted.

8. Consciousness is the growth of coherence, and of us.

9. We are in the midst of a high-frequency blossoming of interconnectedness that will give us access to the quantum soup itself

10. Today, science tells us that the essence of nature is joy.

Subjective Sense of Power Scale (Anderson et al., 2005)

In rating each of the items below, please use the following scale:

1 Disagree strongly

2 Disagree

3 Disagree a little

4 Neither agree nor disagree

5 Agree a little

6 Agree

7 Agree strongly

1. I can get him/her/them to listen to what I say.

2. My wishes do not carry much weight. (r)

3. I can get him/her/them to do what I want.

4. Even if I voice them, my views have little sway. (r)

5. I think I have a great deal of power.

6. My ideas and opinions are often ignored. (r)

7. Even when I try, I am not able to get my way. (r)

8. If I want to, I get to make the decisions.

PANAS Questionnaire

This scale consists of a number of words that describe different feelings and emotions. Read each item and then list the number from the scale below next to each word. Indicate to what extent you feel this way right now, that is, at the present moment OR indicate the extent you have felt this way over the past week

1 Very Slightly or Not at All

2 A Little

3 Moderately

4 Quite a Bit

5 Extremely

1. Interested

2. Distressed

3. Excited

4. Upset

5. Strong

6. Guilty

7. Scared

8. Hostile

9. Enthusiastic

10. Proud

11. Irritable

12. Alert

13. Ashamed

14. Inspired

15. Nervous

16. Determined

17. Attentive

18. Jittery

19. Active

20. Afraid

Scoring Instructions: Positive Affect Score: Add the scores on items 1, 3, 5, 9, 10, 12, 14, 16, 17, and 19. Scores can range from 10 – 50, with higher scores representing higher levels of positive affect. Mean Scores: Momentary 29.7 (SD 7.9); Weekly 33.3 (SD 7.2) Negative Affect Score: Add the scores on items 2, 4, 6, 7, 8, 11, 13, 15, 18, and 20. Scores can range from 10 – 50, with lower scores representing lower levels of negative affect. Mean Score: Momentary 14.8 (SD 5.4); Weekly 17.4 (SD 6.2)