

Giraffes, Religion and Conflict: Essays in Behavioral Decision Making

by

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A.B. Psychology, Brown University, 2002

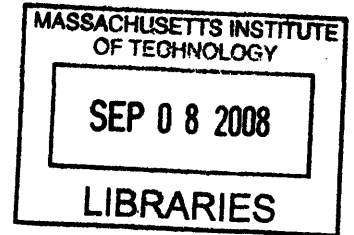
Submitted to the Alfred P. Sloan School of Management
in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy in Management

at the

Massachusetts Institute of Technology

September 2008



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ABSTRACT

Essay 1: Objective Scale Anchoring in Sequential Judgments

We explore the scope and boundary conditions for anchoring when respondents render sequential judgments in the absence of an explicit comparative standard. We show that previous judgments can anchor subsequent ones, even when these are not explicitly compared. We then demonstrate that the effect is caused by the reinterpretation of objective response scales. Much like the use of the label ‘very heavy’ will depend on its context, we show that the use of labels such as ‘1000 pounds’ will also depend on context. Even though the pounds scale is objectively defined, psychologically it still must be interpreted, and is subject to similar effects as those that influence subjective scales.

Essay 2: The Impact of Regular Activities on Well-being

Because of people’s tendency to adapt, few events in life have a lasting impact on subjective well-being. We suggest that while major events may not provide lasting increases in well-being, certain seemingly minor events – such as attending religious services or exercising – may do so by providing small but frequent boosts. In two studies we demonstrate the existence of such boosts to well-being, and provide evidence that these boosts may be cumulative. We suggest that shifting focus from the impact of major life changes on well-being to the impact of seemingly minor repeated behaviors is crucial for understanding how best to improve well-being.

Essay 3: The Doomsday Device: A Mechanism for Avoiding Conflict

Arms races are a common occurrence in the business world. In the current paper, we propose that such arms races can be avoided by the use of commitment to an extreme response, and we test this idea experimentally. Our findings suggest that actual commitment is an effective but underused tool for avoiding the escalation of conflict. We also find that while cheap talk is not effective in the short run, it is useful for establishing a reputation for future interactions.

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Acknowledgments

It is difficult to write a proper acknowledgments section, since no matter how long I make it, I will not be able to include the vast number of people who have helped me get to this point. So I apologize in advance to all the people who in their own way deserve to be included but are not.

First of all I would like to thank my committee. I could not have asked for a better group of advisors, who have encouraged and challenged me for the past four years. From the day I got here, Dan has been watching out for me and going beyond what could be reasonably expected from an advisor to help me succeed. He constantly reminds me that research can be both fun and significant. Shane has been like a second advisor to me since I started working with him. His thirst for understanding phenomena down to their deepest elements has changed the way I conduct my research. Mike has been a pleasure to collaborate with since I met him. I always look forward to working with Mike both because of his friendly nature, and because all of our projects together are always fun, interesting, and make good cocktail party conversation.

I would also like to thank all of the faculty in the Marketing department at MIT: Michael Braun, John Hauser, John Little, Drazen Prelec, Duncan Simester, Catherine Tucker, Glen Urban, Birger Wernerfelt, and Juanjuan Zhang. Each in their own way has vastly changed the way I think about my research.

I would not have made it this far without my fellow graduate student and post-docs: Anat, Boris, Cristina, Daria, Gui, Han, Kristina, Leonard, Matt, Nathan, Nina, Ray, Rebecca, Rene, and Zoe. Their friendship has kept me sane over the course of the program.

Lastly, I would like to thank all of my family and friends, who have been along with me for the entire ride. I am especially indebted to my parents Simon and Lynne Mochon, who have been my advisors for much longer than my committee. Finally, I would like to thank my girlfriend Adriana Costa for all of her support during my graduate degree and for putting up with my spending an inordinate amount of time talking about giraffes, religion and conflict.

Essay 1

Objective Scale Anchoring in Sequential Judgments

ABSTRACT

We explore the scope and boundary conditions for anchoring when respondents render sequential judgments in the absence of an explicit comparative standard. We show that previous judgments can anchor subsequent ones, even when these are not explicitly compared. We then demonstrate that the effect is caused by the reinterpretation of objective response scales. Much like the use of the label ‘very heavy’ will depend on its context, we show that the use of labels such as ‘1000 pounds’ will also depend on context. Even though the pounds scale is objectively defined, psychologically it still must be interpreted, and is subject to similar effects as those that influence subjective scales.

Many studies have shown that judgments can be affected by responses to previous questions. This usually occurs because the preceding judgments provide information that helps interpret the subsequent ones. Sometimes these provide the context for interpreting inherently ambiguous verbal or numeric quantifiers in which judgments are rendered (Lynch, Chakravarti, and Mitra 1991; Parducci 1965; Upshaw 1978). For instance, a 40 gram weight may be called 'heavy' after hefting a 20 gram weight or 'light' after hefting an 80 gram weight (e.g. Brown 1953; Sherif, Taub, and Hovland 1958). Similarly, a person may be rated a "5" in the context of average looking photographs, but just a "4" if the previously rated photos contained fashion models (Wedell, Parducci, and Geiselman 1987).

In other situations, prior questions help interpret otherwise ambiguous questions. (Schwarz 1999; Schwarz, Strack, and Mai 1991; Strack and Martin 1987). For instance, attitudes towards a fictitious "educational contribution" depend on whether previous questions were about the tuition system in the U.S. or about grants that are given to students in Sweden (Strack, Schwarz, and Wanke 1991). Finally, previous questions sometimes provide an appropriate standard of comparison which is used when constructing the subsequent judgment (Helson 1964; Herr, Sherman, and Fazio 1983; Mussweiler 2003; Stapel and Koomen 2001). For example, judgments of the abilities of people are affected by those previously rated, since the information used in previous judgments is used while constructing subsequent ones (Damisch, Mussweiler, and Plessner 2006; Murphy et al. 1985).

These results suggest that such sequential effects can be avoided when previous judgments contain no relevant information. In particular, if the question leaves no room for interpretation, the response scale is objective and externally anchored, and previous judgments do not provide a relevant standard of comparison, then previous judgments should not affect subsequent ones.

However, sometimes prior questions affect answers to subsequent ones even when the preceding judgment provides no information. For example, respondents judge the Mississippi River to be 222 miles shorter (856 vs. 1013) if the respondents are first asked whether its length exceeds 70 miles than if first asked whether it is 2000 miles long (Jacowitz and Kahneman 1995). This "anchoring" effect (Chapman and Johnson 1999; Strack and Mussweiler 1997; Tversky and Kahneman 1974) can occur even when the comparative standards are manifestly uninformative. For example, when Ariely, Prelec & Loewenstein (2003) had respondents compare their willingness to pay for a cordless keyboard against the last two digits of their social security number before stating their maximum willingness to pay, they found a correlation of 0.52 between the stated willingness to pay and the last digits of their social security number (see also, Chapman and Johnson 1999; Tversky and Kahneman 1974)¹

While these anchoring effects are highly reliable, they have been shown within a very narrow paradigm. Except by design, the specific situation in which an enforced comparison to a provided standard precedes a point estimate will arise rarely. More commonly, preceding responses will serve as *potential* comparative standards for

¹ Typically, these comparative standards are provided. In other cases they are spontaneously recruited as approximate, but incorrect responses. For example, Epley and Gilovich (2001) propose that judgments of the freezing point of vodka are based on the consideration of the freezing point of water (32°).

subsequent judgments without being denoted or intended as such. Furthermore, while some have found that judgments can be anchored on numbers not explicitly presented as comparative standards (Critcher and Gilovich 2008; Mussweiler and Englich 2005; Wilson et al. 1996), the effects tend to be smaller and less robust. For example, when 35 four-digit numbers were copied for an ostensibly unrelated purpose, the numbers later influenced respondents estimates of the number of their peers who would eventually succumb to cancer (Wilson et al. 1996). However, this effect did not obtain when the preceding numbers were copied “only” seven times, and Brewer & Chapman (2002) failed to replicate it using the “full strength” manipulation.²

In the current article, we examine anchoring effects in a situation that arises naturally, but has not been examined: sequential judgment. For example, will estimates of a giraffe's weight (in pounds) be greater or smaller if respondents first estimate the weight of a raccoon? Through a series of studies we show, as with other studies on anchoring, even when the question and response scale are unambiguous, and the anchor (the weight of a raccoon) is judged as irrelevant, judgments tend to be assimilated towards the anchor.

However, we propose that the underlying mechanism for these effects differs from that of standard anchoring. In contrast with the standard anchoring paradigm, where the effects are caused by the selective accessibility of anchor consistent information (Chapman and Johnson 1999; Mussweiler and Strack 1999; Strack and Mussweiler 1997), we propose that the mechanism that causes anchoring in sequential

² Oppenheimer, Brewer, and Leboeuf (2008) propose that the anchoring stimulus need not even be numeric – respondents who drew short lines judged the Mississippi River to be shorter and Honolulu to be cooler than those who drew long lines.

judgment is actually more similar to that of sequential effects when judgments lack an "objective" response scale. We propose that respondents' use of "objective" responses such as '1000 pounds' will be affected by context in much the same way that their use of 'very heavy' will be affected by context. Despite having an external unambiguous definition, objective scales must still have an internal psychological representation, and this representation can be affected by the information that preceding judgments make salient.

In the first set of studies presented we demonstrate this novel anchoring effect, and explore its boundary conditions. We then show that unlike anchoring in the standard paradigm, anchoring in sequential judgments is not due anchor consistent information being recruited, rather it is caused by a change in the use of the objective response scale.

STUDY 1

Method

Promising a chance to win Amazon gift certificates, we recruited 565 participants from an online survey site.³ Each respondent made four "*target*" judgments: (1) the weight of an average adult giraffe (in pounds), (2) the weight of a bowling ball sized sphere of lead (in pounds), (3) the population of Japan (in millions), and (4) the manufacturing cost of a fifty inch flatscreen LCD television (in dollars). Each of the four target judgments appeared on a different page and the order was randomized between subjects.

³ All participants were recruited from eLab – an online survey site hosted by Yale University. All who participated more than once were excluded from the analysis. We also excluded two extreme outliers whose estimates were more than 7 Inter quartile ranges from the median.

Each participant encountered four different conditions, one for each domain: the target judgment was either presented by itself (*None*), preceded by a “small” stimulus (*Small*), a “medium” stimulus (*Medium*), or both (*Both*). Thus, each participant made eight judgments – the target judgment in all four domains, along with preceding judgments about the small stimulus in the *Small* and *Both* conditions, and the medium stimulus in the *Medium* and *Both* conditions. For each participant, we randomized which of the four conditions was assigned to each of the four domains.

••• Table 1 •••

Results

As is seen in Table 1, the target judgments, such as the weight of a giraffe, were lower when preceded by judgments of smaller quantities, such as the weight of a raccoon. Moreover, they were reduced more when participants encountered two prior judgments (e.g., raccoon and wolf) than when they encountered one.

Since each participant made one target judgment in each of the four conditions, we conducted a within subject ANOVA with target judgment condition as the lone factor. After performing a log transformation on the raw responses to reduce skewness and computing a z-score for each domain, we compared z-scores across conditions, and found a significant main effect [$F(3,1656) = 18.7, p < .001$]. Post-hoc pair wise tests revealed that target judgments were significantly lower when preceded by judgments of a smaller quantity [p 's $< .001$], and making two such judgments resulted in lower estimates than in

any of the other conditions [p 's < .001]. There was no difference between the *Small* and *Medium* anchor conditions [p > .5].

Discussion

The target judgments (the weight of a giraffe, a lead bowling ball, the population of Japan, and the cost of manufacturing a television set) were all markedly reduced by preceding judgments of smaller quantities. Moreover, this effect was stronger when participants made two such judgments (the *Both* condition) than when they only made one (the *Small* or *Medium* conditions)⁴. But, what is it about the previous questions that causes the effect? One possibility is that these effects are caused by the accessibility of the smaller numbers preceding the target judgment, which is sometimes termed basic numeric priming (see Critcher and Gilovich 2008; Wilson et al. 1996; Wong and Kwong 2000). This account would predict anchoring regardless of what the preceding numbers referred to, if anything. Alternatively, these effects could be caused by the activation of the *concept* of smallness, which would predict anchoring whether the preceding judgments involved numeric responses or not. This account would predict that anchoring would occur when preceding judgments involved smaller objects, even when the response is non-numerical. Finally, it might be necessary to render numerical judgments on the same dimension in order to generate the effect. Study 2 was designed to help discriminate among these hypotheses.

⁴ While our studies focus on the effect of small anchors, in another study not reported in the paper, we found that judgments of small objects are *larger* when preceded by judgments of large objects. Thus the effect works in both directions.

STUDY 2

Method

In exchange for a chance to win Amazon gift certificates, 322 respondents participated in an online survey⁵. Participants were randomly assigned to four conditions, all of which ultimately involved estimating the average weight of an adult giraffe. In the *Control* condition, respondents made only that judgment. In the *Both* condition they first estimated the weight of a raccoon and wolf, respectively. In a third condition (*Numeric Primes*) we replaced judgments of the weight of raccoons and wolves with two unrelated judgments suspected to yield similar numeric estimates: specifically, participants were asked to estimate the number of states east of the Mississippi river, and the average life expectancy of a Japanese woman. If the anchoring effects found in study 1 resulted from the production of smaller numbers, *per se*, we would expect the *Numeric Primes* condition to yield similar results. In a fourth condition (*Conceptual Primes*), images of a raccoon and a wolf were shown and respondents were merely asked to identify these animals, before estimating the giraffe's weight. If the effect were merely due to the target judgment being presented in the context of smaller objects, then this condition should yield a similar effect.

Results

Table 2 presents the mean estimates by condition. A between subjects ANOVA performed on the log of the estimates yielded a significant effect of condition [$F(3,315) = 3.0, p < .05$]. As in Study 1, post-hoc pair wise tests revealed that estimates of the giraffe's weight was significantly reduced when preceded by estimates of the two smaller

⁵ 3 extreme outliers whose estimates were more than 7 Inter quartile ranges from the median were removed from the data

animals than in each of the other three conditions [p 's < .05]. Neither the numeric primes nor the conceptual primes conditions revealed a significant effect.

•• Table 2 ••

Discussion

These results suggest that in order to generate the anchoring effects, respondents must generate numeric responses on a related dimension. Results from the *Numeric Primes* condition show that merely writing down small numbers had no effect on subsequent judgments. Moreover, the results from the *Conceptual Primes* condition demonstrate that merely grouping a giraffe with smaller animals was not enough to generate anchoring. One problem with this condition is that even though we did not explicitly ask participants to estimate the weight of the smaller animals, they might have automatically done so anyway, which would explain why the estimate of the giraffe is somewhat smaller in this condition, even if the effect is not significant.

While the effect seems to require numerical judgments of related objects, it is possible that the effect is merely caused by participants making inferences about the target's size based on the preceding questions. Maybe participants infer that giraffes are smaller than they thought because the experimenters chose to include them with questions about the weight of a raccoon. However, a follow-up study undermines this interpretation. Fifty students at MIT were shown the design of study 1 and asked to rank the mean estimates of giraffe, when judged alone, when preceded by a judgment of a raccoon, or when preceded by judgments of a raccoon and wolf. Over 70%

(mis)predicted that judgments of a giraffe's weight would be *larger* when preceded by smaller animals, contrary to the effect actually exhibited [$p < .01$ by a sign test].⁶ If the inclusion of smaller animals in the judgmental context was seen as communicating that giraffes were not that big, we would have expected them to apply this theory to their predictions of the experimental results. But this did not occur.

In the paradigm of sequential judgments made with no explicit comparative standard, anchoring appears to occur only when the preceding judgments are related to the target judgment. However, it remains unclear *how* they must be related. Is it sufficient for prior judgments to involve the same *dimension* (i.e., will estimating the weight of *any* small object reduce the estimates of the giraffe's weight)? Or must the preceding judgments be within the same category as the target? We tested these competing hypotheses in the following study by holding the judged dimension constant (pounds), while manipulating the similarity of the preceding judgments to the target judgment.

STUDY 3

Method

We recruited 488 respondents to participate in an online survey in exchange for a chance to win Amazon gift certificates. As before, respondents estimated the weight of an average adult giraffe (in pounds). In the control condition, that was the only judgment. In three other conditions, respondents first estimated the weight of two smaller *mammals* (a raccoon and a wolf), two smaller *birds* (a wild turkey and an

⁶ Most of the incorrect predictions (32/35) were that the weight of the giraffe would be smallest in the *None* condition, while only (3/35) were that it would be smallest in the *Small* condition, suggesting a strong belief in contrast effects.

emperor penguin) or two smaller *objects* (a tricycle and a window air conditioner). These pairs of stimuli were chosen because they yield similar numerical estimates to raccoon and wolf, but differ in terms of their conceptual similarity to a giraffe.

•• Table 3 ••

Results

As Table 3 reveals, the degree of anchoring induced by prior estimates is strongly related to the categorical relationship between those questions and the target questions. The giraffe was judged to be lighter following estimates of smaller *mammals* or *birds*, but not smaller *objects*. Post-hoc pair wise comparisons support the significance of the differences evident in the table [p 's < .01]. Moreover, though the *birds* and *mammals* conditions did not significantly differ [$p = .2$], the birds appeared to be somewhat less influential, which supports the idea that the influence of prior stimuli diminishes with increasing conceptual distance from the target judgment (Brown 1953; Strack and Mussweiler 1997).

Discussion

The preceding studies help delineate the boundary conditions of anchoring in sequential judgments, but do not explain *why* the effect occurs. Studies 1-3 argue against basic numeric priming, basic conceptual priming, or some "demand effects" whereby respondents make inferences from the experimental decision to group giraffes with raccoons and wolves.

In contrast with other theories of anchoring (Chapman and Johnson 1999; Mussweiler and Strack 1999; Strack and Mussweiler 1997), we propose that this effect reflects a shift in the numeric language people use to *represent* or *communicate* their subjective impressions, rather than a change in those impressions, *per se*. This implies that preceding judgments (about, say, a raccoon's weight) should not affect judgments about a giraffe's size which are expressed in different dimensions or in different units. We test this hypothesis in study 4.

STUDY 4

Method

We recruited 218 respondents to complete a brief paper survey. Half of all the participants estimated the weight of a raccoon. All participants then estimated three features of a giraffe: its weight (in pounds), its height (in feet), and its weight relative to a grand piano, on a scale ranging from 1 (piano is much heavier) to 7 (giraffe was much heavier) with 4 as a midpoint (they weigh about the same).

Results and Discussion

Table 4 reports the results. As in previous studies, those who first estimated the weight of a raccoon generated much smaller estimates of a giraffe's weight [$t(213) = 3.2$, $p < .005$]⁷. Notably, however, the raccoon judgment did not affect estimates of the giraffe's height [$p > .8$] or its weight relative to a grand piano [$p > .8$].

••• Table 4 •••

⁷ As in the previous studies the t-test was performed on the log transformed values.

These results are not readily explained in terms of the theory most commonly invoked to explain anchoring in this paradigm: that explicit comparisons to provided standards activate information consistent with the provided anchor which then lingers to influence the subsequent judgments (Mussweiler and Strack 1999, 2000; Strack and Mussweiler 1997). If information consistent with a giraffe being as small as a raccoon was more accessible at the moment of the target judgment(s), one would expect giraffes to be judged as smaller on *all* judgments that capture this construct, including their height, their weight relative to piano, the number of lions that could feast on one, and so on.

By contrast, we propose that the putative anchoring stimuli may not affect subjective impressions of the target judgmental object (in this case, a giraffe's mass) but only the numeric language that is selected to communicate those impressions. For instance, when participants map their subjective impression of the giraffe's mass onto some number of pounds, the mapping can be influenced by preceding judgments such that a smaller number of pounds is required to communicate the same subjective impression of mass. To reiterate, when contrasted with a number like 23 (as in a 23 pound raccoon), big numbers, like 1000 are perceived to be larger than they are perceived without this provided contrast. Correspondingly, respondents select smaller numbers when asked to communicate their subjective impression of a giraffe's mass in terms of these units. In other words, a numeric contrast effect in the mapping stage results in an assimilation effect in the response stage, whenever a response is requested in the same units as the anchoring stimulus. This explains why, in study 4, the raccoon judgment

affected only the pounds judgment and not the other judgments that should correspond to a giraffe's size.

Our theory that anchoring is caused by numeric contrast effects in the mapping stage, implies that if the ordinary task were inverted (if respondents were provided with the desired response and asked to select the corresponding stimulus), the presence of the smaller animals would increase, rather than decrease, the magnitude of the produced response (see Figure 1). For example, if respondents were asked to generate an exemplar of an animal that weighs 1000 pounds those who were first asked to judge the weight of a much smaller animals (like a raccoon or wolf) should select *larger* exemplars -- because 1000 now connotes a larger thing. We test this prediction in study 5.

... Figure 1 ...

STUDY 5

Method

We recruited 158 respondents to complete a brief online survey. All participants were shown a list of 15 animals ordered by weight, from very small (mouse) to very large (elephant) and were asked to select the animal whose average adult weight was closest to 1000 pounds. Half of the participants estimated the weight of an average adult wolf (*Anchor* condition) before answering this question while the other half did not (*No Anchor* condition).

Results

To analyze the data, we compared the rank of the selected animal, which ranged from 1 (mouse) to 15 (elephant).⁸ As predicted, respondents who first estimated the weight of a wolf selected significantly larger animals when asked to choose one weighing 1000 pounds [$M = 11.6$, $SE = .39$] than those who did not [$M = 10.5$, $SE = .39$; $t(159) = 2.01$, $p < .05$]. The average response of the participants in the no anchor condition was half way in between the 10th animal on the list (polar bear) and the 11th (camel), which corresponds roughly to an animal weighing 1120 pounds, while the response of the participants in the anchor condition was almost half way between the 11th (camel) and the 12th (Giraffe), which corresponds roughly to an animal weighing 1670 pounds. This result strongly supports the proposed theory, in which contrast effects at the mapping stage induce either assimilation effects (studies 1-4) or contrast effects (study 5) at the output stage. This account may also help explain why two anchors have a stronger effect than one, since the posited effect essentially occurs twice -- the small stimulus affects the number selected to represent the medium stimulus, which in turn further affects the mapping when a number is selected to represent the large stimulus.

GENERAL DISCUSSION

Sequential exposure to multiple judgmental objects is a common occurrence in the real world. People rate a series of interviewees, encounter initial and subsequent offers in negotiation, and so on. We show here that preceding judgments need not be offered as comparative standards to anchor subsequent judgments. This is not widely

⁸ The correct answer corresponded to the 10th animal on the list, which was a polar bear (average adult weight: 990 pounds).

appreciated. Most experts we confronted failed to predict these effects, as did most respondents (see discussion of study 2).

Our results contribute to an understanding of both the nature and boundary conditions of anchoring. In contrast with theories of selective accessibility (Chapman and Johnson 1999; Strack and Mussweiler 1997), we contend that the psychological mechanism of anchoring is often not through changes in subjective impressions, but via a change in the mapping between subjective impressions and overt numeric responses. Furthermore, in contrast with some theories, we found evidence of anchoring only when sequential judgments pertained to the same category of objects and when responses were expressed in the same units. We also demonstrate that anchoring depends on more than the value of the immediately preceding stimulus, as multiple anchors increase the degree of anchoring.

These findings suggest a profound challenge in measuring people's beliefs. Even when respondents express judgments on externally defined scales such as pounds and meters, their responses appear to reflect local contextual influences. Thus, supposedly objective, externally anchored scales are also affected by the context in much the same way as the numbers on transparently ambiguous response scales. The integrity of objective physical standards of a pound (or 1000 pounds) can be defined by the Weights and Measures Act, but this will not stop people from temporarily adopting idiosyncratic standards for just how much (or little) that is.

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TABLE 1

Mean estimates by condition of the anchor and target judgments (Study 1)

Judgmental Domain	Small Anchor	Medium Anchor	Target Judgment
Weight of animals (pounds)	Raccoon	Wolf	Giraffe
			901
	21		735
		86	792
	19	64	657
Weight of bowling-ball sized spheres (pounds)	Rubber	Glass	Lead
			150
	14		108
		30	93
	15	26	83
Population of countries (millions)	Finland	Italy	Japan
			124
	20		116
		38	129
	12	32	91
Manufacturing cost of products (dollars)	Clock Radio	Fax Machine	Television
			\$610
	\$8		\$457
		\$53	\$369
	\$7	\$31	\$323

TABLE 2

Mean estimates by condition of the anchor and target judgments (Study 2)

Condition	Judgment 1	Judgment 2	Target (giraffe)
Control			1105
Numeric Primes	# of U.S. states east of Mississippi: 22	life expectancy of Japanese woman (yrs): 81	1100
Conceptual Primes	identify depicted animal (raccoon): 96% identified	identify depicted animal (wolf): 94% identified	936
Both	weight of raccoon: 27	weight of wolf: 72	735

TABLE 3

Mean estimates by condition of the anchor and target judgments (Study 3)

Condition	Judgment 1 (weight of...)	Judgment 2 (weight of...)	Target (giraffe)
Control			1065
Objects	Tricycle 18	Air Conditioner 72	1170
Birds	Turkey 31	Penguin 66	757
Mammals	Raccoon 23	Wolf 77	698

TABLE 4

Mean estimates by condition of the anchor and target judgments (Study 4)

Condition	Judgment 1	Target Judgments		
Estimate	Raccoon Weight (lbs)	Weight of giraffe (lbs)	Height of giraffe (ft)	Giraffe Weight (vs. piano)
None Condition		1117	17.5	4.5
Anchor Condition	22	760	17.3	4.5

FIGURE 1

Representation of the relationship between subjective impressions and the objective scale responses. Figure 1A represents how the mapping occurs when no anchor is present. Figure 1B represents the shift in the mapping after an anchor question. Figure 1C represents the predicted contrast effect when the direction of the mapping is reversed.

Figure 1A

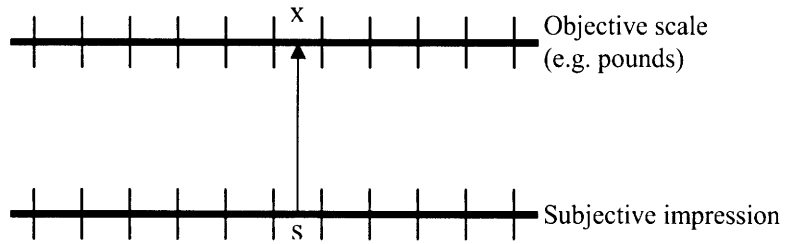


Figure 1B

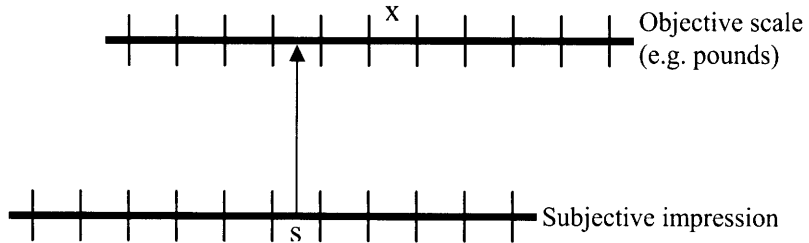
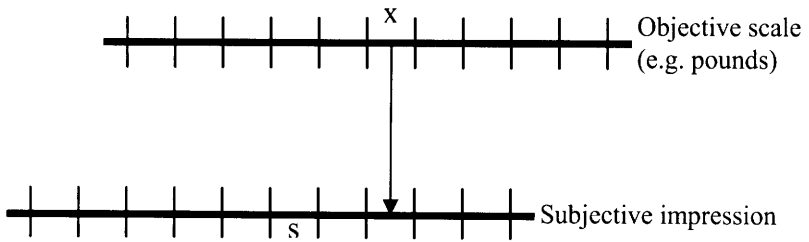


Figure 1C



Essay 2

The Impact of Regular Activities on Well-being

ABSTRACT

Because of people's tendency to adapt, few events in life have a lasting impact on subjective well-being. We suggest that while major events may not provide lasting increases in well-being, certain seemingly minor events – such as attending religious services or exercising – may do so by providing small but frequent boosts. In two studies we demonstrate the existence of such boosts to well-being, and provide evidence that these boosts may be cumulative. We suggest that shifting focus from the impact of major life changes on well-being to the impact of seemingly minor repeated behaviors is crucial for understanding how best to improve well-being.

Understanding the determinants of subjective well-being has important implications for economics. At the micro level, well-being has been shown to affect the behavior of individuals. For example, some studies have found that there is a positive relation between workers' stable happiness and their work performance (Wright and Staw 1999). People under positive moods have also been shown to be more creative problem solvers, more likely to attain a mutually favorable outcome while bargaining, and more willing to seek variety among positive choices (Isen 2000).

At the macro level, policymakers must often decide among various programs that differ in the advantages they provide to the public. Understanding what factors truly improve well-being can guide such decisions, and help avoid choosing programs that bring expensive and non lasting benefits. In contrast to models in which utility is inferred from people's choices, recent economic conceptualizations of utility include people's subjective feelings of utility – their reports of their subjective well-being – as an important input in determining overall utility. These reports of subjective well-being are increasingly a factor in determining the overall utility of some public policy decisions (e.g. Di Tella and MacCulloch 2006; Kahneman et al. 2004a). As a result, the scientific study of subjective well-being has received increased attention from economists (Frey and Stutzer 2002; Kahneman et al. 2004b).

The literature on subjective well-being offers a paradox, however, that must be resolved before the construct can be fruitfully applied to improve people's lives: Most studies have shown that people's overall level of happiness seems stubbornly impervious to change. While people accumulate experiences that most predict would affect their

well-being, subjective well-being appears to be surprisingly stable. Indeed, if major life events such as winning a lottery fail to have a substantial lasting impact on well-being (Brickman, Coates, and Janoff-Bulman 1978), it is hard to imagine that any single event could accomplish the feat. Therefore, it would seem that any policy geared towards maximizing subjective well-being would be doomed to fail from the outset. We believe, however, that it is possible to make lasting changes in subjective well-being if one focuses on the right types of behaviors. In this paper, we shift from a focus on the impact of single major life events toward a focus on the impact of seemingly minor behaviors such as exercising or attending religious services on well-being. We suggest that while single major events may be unlikely to have a lasting impact, smaller minor behaviors provide small boosts to well-being that can lead to real changes in overall well-being, especially if they are repeated with sufficient frequency over time: one can't win the lottery every day, but one can exercise or attend religious services regularly, and these repeated behaviors may be enough to increase well-being over time.

Adaptation and the Stability of Well-Being

Brickman and Campbell (1971) coined the term “hedonic treadmill” to describe the now widely-accepted notion that though people continue to accrue experiences and objects that make them happy – or unhappy – their overall level of well-being tends to remain fairly static. The logic behind this argument stems from adaptation level theory (Helson 1964), which argues that people perceive objects not in any absolute sense, but rather relative to a level established by previous experiences. Therefore, when people experience a positive event, two effects take place: in the short run, well-being increases;

in the long run, however, people habituate to their new circumstances, which diminishes the positive effect of that event. In the most famous demonstration of this course of events, Brickman et al. (1978) interviewed a sample of lottery winners, as well as a sample of accident victims who had become paralyzed. The sample was chosen such that the major life event had happened to them within the previous year and a half, but at least a month before the interview, to allow for adaptation to occur. Their results showed that the lottery winners did not rate themselves as happier than the control group, and while the accident victims rated themselves as less happy than the other groups, they still rated themselves above the midpoint of the scale. Similarly, Suh, Diener, and Fujita (1996) showed that while major life events that had occurred within the previous 3 months predicted wellbeing, those occurring further back in time did not. Other studies have shown that there is no difference in well-being between people who had recently experienced a romantic break up versus those who had not, between assistant professors who had been denied tenure and those who had gotten it, or between those whose preferred gubernatorial candidates had won or lost (Gilbert et al. 1998). Taken together, these results suggest that most events have no lasting impact on our well-being (see Diener, Lucas, and Scollon 2006; Frederick and Loewenstein 1999; Frey and Stutzer 2002).

Worse still, of those investigations that have demonstrated a lasting impact of major events on well-being, most have been in negative domains. Dijkers (1997) conducted a meta-analysis of 22 studies that looked at the relationship between spinal cord injury and quality of life, and found that degree of disability had a negative effect on quality of life. Lucas et al. (2003) examined the effects of marital status over time with a

large representative sample. Although marriage had a strong initial positive effect, this effect disappeared after just one year. People who had been widowed, on the other hand, never fully adapted and remained less happy than their baseline before the event occurred.

What then does account for people's positive levels of well-being, if not major life events? Many studies have shown that there are strong effects of both genetic predisposition and stable personality traits on well-being. Lykken and Tellegen (1996) found that half of the variance associated with well-being was associated with genetic variation, and that this accounted for 80% of the stable component of the subjective well-being measure (see also Suh et al. 1996). Indeed, Headey and Wearing (1989) suggested that rather than life events causing changes in well-being, life events are to some degree endogenously caused by personality. Personality models of well-being thus suggest that though there may be short-lived effects of external shocks on well-being, people return to their baseline in the long run, a baseline which is determined primarily by their personalities and the events that those personalities cause them to pursue.

Can Well-Being be Improved?

All of the previously cited research paints a rather discouraging picture about people's ability to increase their own well-being. Although some major negative events seem to be able to create lasting changes (Dijkers 1997; Lucas 2005; Lucas et al. 2004), there seems to be little people can do to improve their well-being. At most, people may hope for temporary lifts from major life events – from marriage, from winning the lottery – which quickly fade as they return to their usual baseline predetermined by genes and

personality – requiring them to ever peruse the hedonic treadmill in the hope of finding some temporary increase in happiness. Given the frequency of winning the lottery or getting married (most people will do both a maximum of a few times), the odds of improving well-being seem low. Of course, this is not as tragic as it sounds, since most people’s equilibrium state is somewhat happy (Diener and Diener 1996). Nonetheless, it seems as though people generally should not even bother to pursue goals that make them happy (and not try to avoid many of the activities that they expect to cause them unhappiness), as achieving them will not have any lasting impact. It also appears as though any economic policy aimed at improving people’s welfare is just a waste of time and money, since it will have no long-term effect. But is there truly nothing that people can do to improve their well-being?

We suggest that shifting from a focus on the impact of major life events to a focus on minor life events – the kinds of small activities people partake in every day – offers insight into how people might increase their well-being. Indeed, in contrast to the research reviewed above, some studies have shown that particular behaviors – such as religion and exercise – are related to higher levels of well-being. Importantly, these kinds of behaviors are repeated behaviors, rather than single-shot life events. We suggest that the cumulative impact of repeating minor but positive life events in the short-term – such as choosing to attend religious services each week or to work out several days a week – may be sufficient to increase well-being in the long-term.

We chose religious practice and exercise as our initial behaviors because both have been linked to well-being, and are precisely the kinds of minor repeated behaviors we propose may improve well-being. Indeed, religiosity and religious involvement have

overwhelmingly been found to correlate with many measures of well-being. Myers (2000) reports data from a national sample showing that those who are most involved with their religion are almost twice as likely to report being “very happy” than those with the least involvement (see also Ferris 2002). In a large cross-sectional national sample, Ellison (1991) found that religious variables accounted for 5 to 7 percent of the variance in life satisfaction (see also Witter et al. 1985). Religious involvement has also been found to be positively related to more objective measures of well-being such as mental and physical health (e.g. Hackney and Sanders 2003; Larson et al. 1992; Seybold and Hill 2001). While not as widely-studied, physical exercise is known to generate endorphins that improve mood (Thoren et al. 1990), regular engagement in exercise has been shown to have a positive impact on well-being (Biddle 2000), and of course countless studies demonstrate the benefits of exercise for physical well-being (see Penedo and Dahn 2005; Ross and Hayes 1988).

How Might Repeated Minor Events Lead to Increased Well-Being?

How is it that religious involvement and exercise improve subjective well-being when most other factors – including events that seem to dwarf these in significance – seem to matter little? Why don’t people adapt to religion and exercise, behaviors that most people have been engaged in their entire life, as they do to most other things? We suggest that these behaviors have a causal effect on well-being because they give regular and reliable – albeit small – boosts to well-being each time a member participates in one of these activities. Though each boost is likely not large and fades over time, both

religion and exercise encourage regular participation, which might cause these small boosts to aggregate over time, leading to increased well-being.

Some evidence for the impact of small repeated events comes from the finding that commuting, a regular daily activity, is rated as a highly negative experience (Kahneman et al. 2004a), and commuting time correlates negatively with subjective well-being (Stutzer and Frey 2004). In addition, Lyubomirsky, Sheldon, and Schkade (2005) suggest that intentional activities, discrete actions in which people choose to engage, can lead to higher levels of well-being because they draw attention to positive events, thus preventing them from fading into the background. However none of these studies have documented whether these activities cause small boosts in well-being every time they are performed, as we propose.

OVERVIEW

In order to test whether people in fact do get small boosts from engaging in religious activity, we measured the subjective well-being of people as they entered and exited religious services (Study 1), and as they entered and exited the gym and yoga (Study 2). We expected to observe increases in well-being from before to after such behaviors. In both studies, we also assessed people's reported frequency of such behaviors, to investigate whether increased frequency of engaging in these behaviors (and thus more frequent small boosts) was related to overall higher well-being.

STUDY 1

Method

Participants

Teams of undergraduate research assistants surveyed places of worship for twelve religions in the Boston/Cambridge area (see Table 1 for religions represented, number of places of worship surveyed, and number of members of each religion who participated). In total, 2,095 people participated (1032 male, 1063 female), with a mean age of 36.7.

•• Table 1 ••

Procedure

Participants were approached either before services or after services. We were concerned that approaching the same participants both pre- and post-service would create strong demand effects, with participants being motivated to report increased mood in order to justify their attendance. Therefore, we ensured that surveyors did not question the same participant twice, making this a true between-subjects design and minimizing these demand effects.

Participants were asked to rate, on a scale from 1 (very bad) to 100 (very good), “How do you feel right now?” “How satisfied are you with your life in general?” and “How satisfied are you with your spiritual and religious life?” While the limitations of doing field research limited us to just these three questions, previous research has suggested that even single item measures of well-being correlate well with more intensive surveys (e.g. Sandvick, Diener, and Seidlitz 1993). Participants also reported how many

times they had attended services in the last month and their age; surveyors recorded their gender.

Results

Since our three measures of well-being were highly correlated, we averaged them to create a composite measure of well-being [Cronbach's $\alpha = .80$]. The average well-being reported in our sample was 81.2 [$SD = 13.0$]; in line with previous research, people in our sample tended to be happy (Diener and Diener 1996).

We next looked at the effect of attending a religious service on well-being by comparing the composite measure of participants surveyed before services and the different set of participants who were surveyed after services. As Figure 1 shows, attending a religious service provided a small and positive boost to reported well-being, and this was true across all of the surveyed religions. Collapsing across all religions, those surveyed after their religious service [$M = 82.8, SD = 12.0$] reported a significantly higher level of well-being than those surveyed before [$M = 79.6, SD = 13.8; t(2093) = 5.67, p < .001$].

••• Figure 1 •••

Our first result showed that people tend to get a small positive boost in well-being from attending a specific religious service. We suggest that it is the aggregation of these small boosts over time that contributes to the positive relationship between religiosity and well-being. If this is the case, we would expect people who had attended more services in

the previous month to report a higher level of well-being at baseline (before they had received a positive boost from attending the service). We therefore explored the relationship of our composite measure of well-being to the number of times participants reported attending services in the previous month. On average, participants indicated that they had attended services 4.0 [$SD = 3.7$] times in the previous month, and as expected, the frequency of attendance was positively related to well-being, $\beta = .75$, $t(2048) = 9.97$, $p < .001$: For every extra time a person had attended a religious service in the previous month, their baseline well-being was .75 points higher. As can be seen in Figure 2, this effect is close to linear – the more people attend, the happier they are.

•• Figure 2 ••

Discussion

These data offer an account for the relationship between religion and well-being, and help to explain why it is that religious people tend to be happier than non-religious people. While previous studies have shown that religious involvement is correlated with well-being, that correlation could have been caused by happier people being more religious, or some third factor. In Study 1, participants were randomly surveyed either before or after services, thus allowing us to conclude that religious adherents in fact report a higher level of well-being after participating in a religious service than before. Those actively involved in religion get small boosts to their well-being every time they attend a religious service, and when people attend religious services frequently enough,

these boosts seem to lead to overall higher levels of well-being.⁹ While this second result is purely correlational, there is good reason to believe that attending services frequently causes higher levels of wellbeing, rather than the other way around. As we have shown, people get boosts from attending religious services, showing that attending a service can cause a single shot improvement to well-being. If people choose activities in order to maximize their wellbeing, one would expect the least happy people to attend the most often, since they would benefit most from these boosts, leading to a negative correlation rather than the positive one we found. In addition, Litwin (2007) found that after controlling for covariates such as social involvement and physical health, religious involvement only improved mortality risk for those who attended services regularly, consistent with our theory that lasting change only occurs with frequent involvement.

STUDY 2

Study 1 showed that religion is a behavior in which people can engage to get off the hedonic treadmill. Every time people attend a service they get small boosts, which over time seem to lead to a permanent change in their baseline level of well-being. While the relationship of religion and well-being is among the most-studied, there is no reason to think that the regular practice of religion is privileged in its positive impact. In Study 2,

⁹ An alternative explanation for our results – which may resonate with non-religious readers who were forced to attend religious services as children – is that rather than benefiting from attending religious services, the upward change merely reflects relief that they are over. This is unlikely, however, because participants reported very high levels of well-being before services had begun.

we investigate another set of behaviors which involve frequent discrete events: Physical activity, which has also been found to provide long-term benefits for well-being. We explore whether two activities – going to the gym and practicing yoga – provide small boosts to well-being with each iteration, and whether the frequency of engaging in these activities (and thus of getting these boosts) predicts baseline well-being.

Method

Participants

Teams of undergraduate research assistants surveyed a gym and two yoga classes in the Boston/Cambridge area. In total, 224 people participated (122 male, 102 female), with a mean age of 39.1. 164 were surveyed outside the gym, while 60 were surveyed outside of a yoga class.

Procedure

As in Study 1, participants were approached as they were either entering or exiting their gym or yoga classes, and surveyors again did not question the same participant twice. Participants completed the same survey as in Study 1, which included the three measures of well-being, as well as the frequency with which they had attended their gym or yoga class in the previous month.

Results and Discussion

As in Study 1, we computed a composite measure by averaging the three well-being scales [Cronbach's $\alpha = .74$]. The mean reported level of well-being in our sample

was of 76.2 [$SD = 13.8$], indicating that overall most of the people in our sample were happy.

Was there an effect of engaging in physical activity similar to the one we observed with religious participation? As can be seen in Figure 3, engaging in physical activity also provided a positive boost to well-being. Averaging across both groups, people who were surveyed after they engaged in physical activity reported significantly higher levels of well-being [$M = 79.2, SD = 13.4$] than those surveyed before they engaged in physical activity [$M = 72.7, SD = 13.4; t(222) = 3.65, p < .001$]. As with people who attended religious services, we found that people who engaged in physical activity received a small positive boost to their well-being.

... Figure 3 ...

We next examined whether the frequency with which people had engaged in these activities in the previous month predicted their baseline well-being. On average, our participants indicated that they had attended the gym or a yoga class 12.0 [$SD = 10.3$] times in the previous month. We again found a positive relation between our composite measure of well-being and frequency of attendance, $\beta = .33, t(99) = 2.25, p < .05$: for each extra time they had attended their gym or yoga class in the previous month, participants experienced an increase in their well-being of about a third of a point. As in Study 1, there seems to be a cumulative effect of the small boosts of engaging in these behaviors, such that greater frequency was associated with greater well-being.

GENERAL DISCUSSION

The data reported here address a seeming paradox: Despite the many studies showing that very few events can have a lasting impact on subjective well-being because people adapt to their circumstances, some research suggests that certain behaviors are positively related to well-being. We posited that the reason that behaviors such as religious involvement and physical activity have a lasting effect on well-being is that these involve frequent small boosts to well-being, with a non-activity period preceding each activity, and are quite different from the infrequent large changes provided by major life events. We further suggested that while the effect of each one of these boosts might be small, people who engage in these activities often enough will end up with higher well-being. Using a paradigm in which we surveyed some participants before they attended religious services or exercised and others as they left these activities, Study 1 showed that people reported higher well-being after religious services, while Study 2 showed a similar effect for attending the gym or a yoga class. Equally important, frequency of engaging in these activities was a positive predictor of people's baseline well-being, suggesting that these small boosts have a cumulative positive effect on well-being.

Our findings imply that, in contrast to the notion of an inescapable hedonic treadmill, it is not pointless for people to seek to improve their well-being. However, improvement may not come from major events such as winning the lottery, despite the seemingly life-changing nature of such examples. Rather, it seems like the key for long lasting changes to well-being is to engage in activities that provide small and frequent boosts, which in the long run will lead to improved well-being, one small step at a time.

In light of our results, we think it not coincidental that Karl Marx called religion the “opium of the masses,” while athletes frequently refer to the “runner’s high” that comes with strenuous exercise. In some sense, both attending religious services and exercising work like a drug in their impact on well-being; while the benefits of the former may be more psychological and the latter more physiological in nature, the two seem to have similar positive effects. If people are engaged in a rational pursuit of higher well-being, it is not surprising that people pursue these activities more and more to continue to receive the cumulative benefits. While it is likely the case that not everyone would benefit from these two activities, we suggest that everyone can and should find an activity with similar characteristics in order to create lasting improvements in their well-being.

Our findings also suggest that policies aimed at improving welfare are not a pointless endeavor, as the hedonic treadmill suggests. However, one must be careful when choosing these policies. Single shot events such as a one time tax refund will probably have little lasting impact on the well-being of the country, while policies that lead to small but repeated gains are likely to succeed. Future research should explore what are the best policies for achieving lasting change.

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TABLE 1

Number of participants and places of worship surveyed for twelve religions (Study 1).

Religion	Number of places of worship surveyed	Number of participants
Baha'i	1	39
Baptist	8	499
Catholic	4	120
Christian	6	161
Congregational	3	263
Episcopalian	5	438
Greek Orthodox	1	99
Lutheran	2	109
Methodist	3	208
Mormon	1	57
Quaker	2	42
Unitarian	1	60

FIGURE 1

Well-being of twelve religions before and after religious services (Study 1). Error bars indicate standard error.

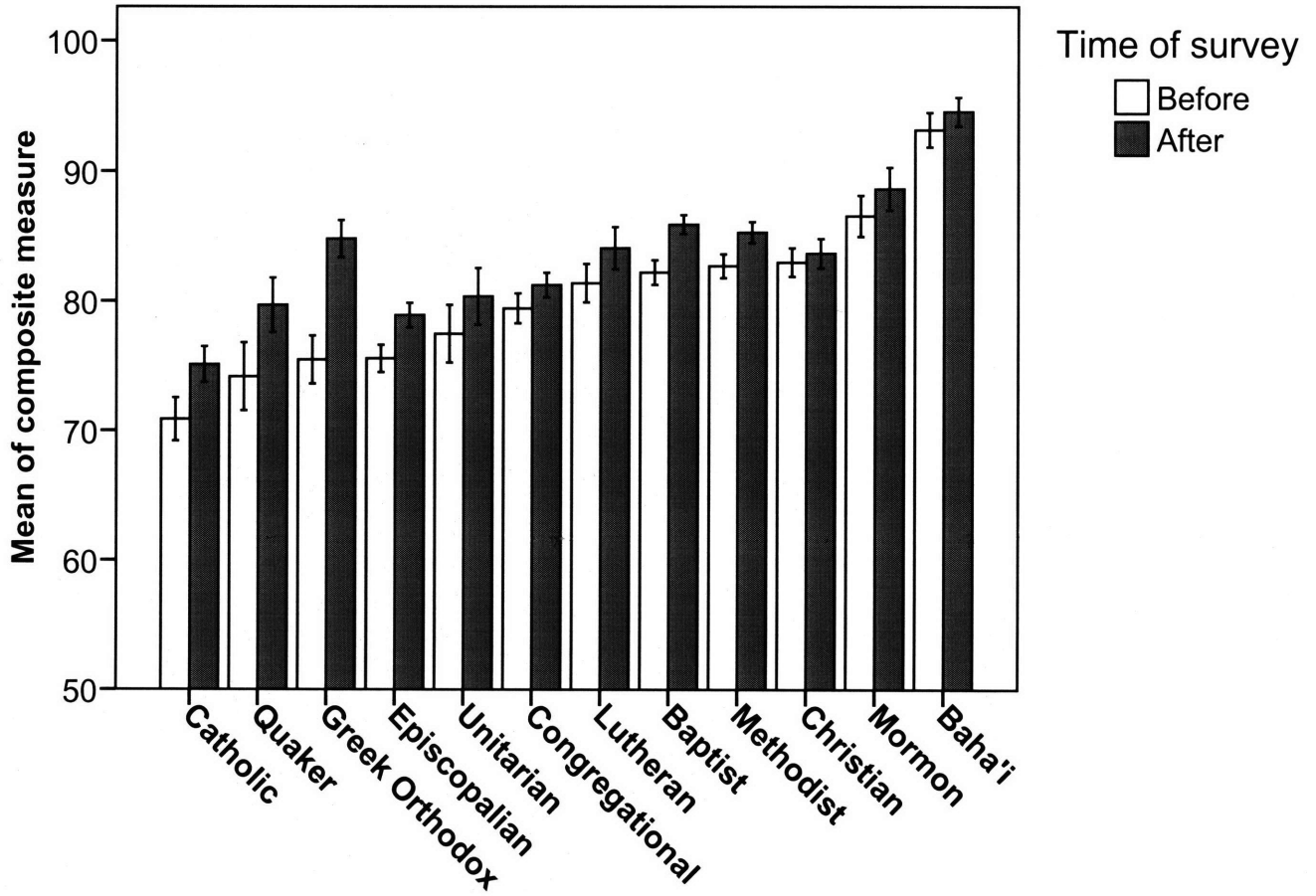


FIGURE 2

The impact of frequency of attendance of religious services on well-being (Study 1).

Error bars indicate standard error.

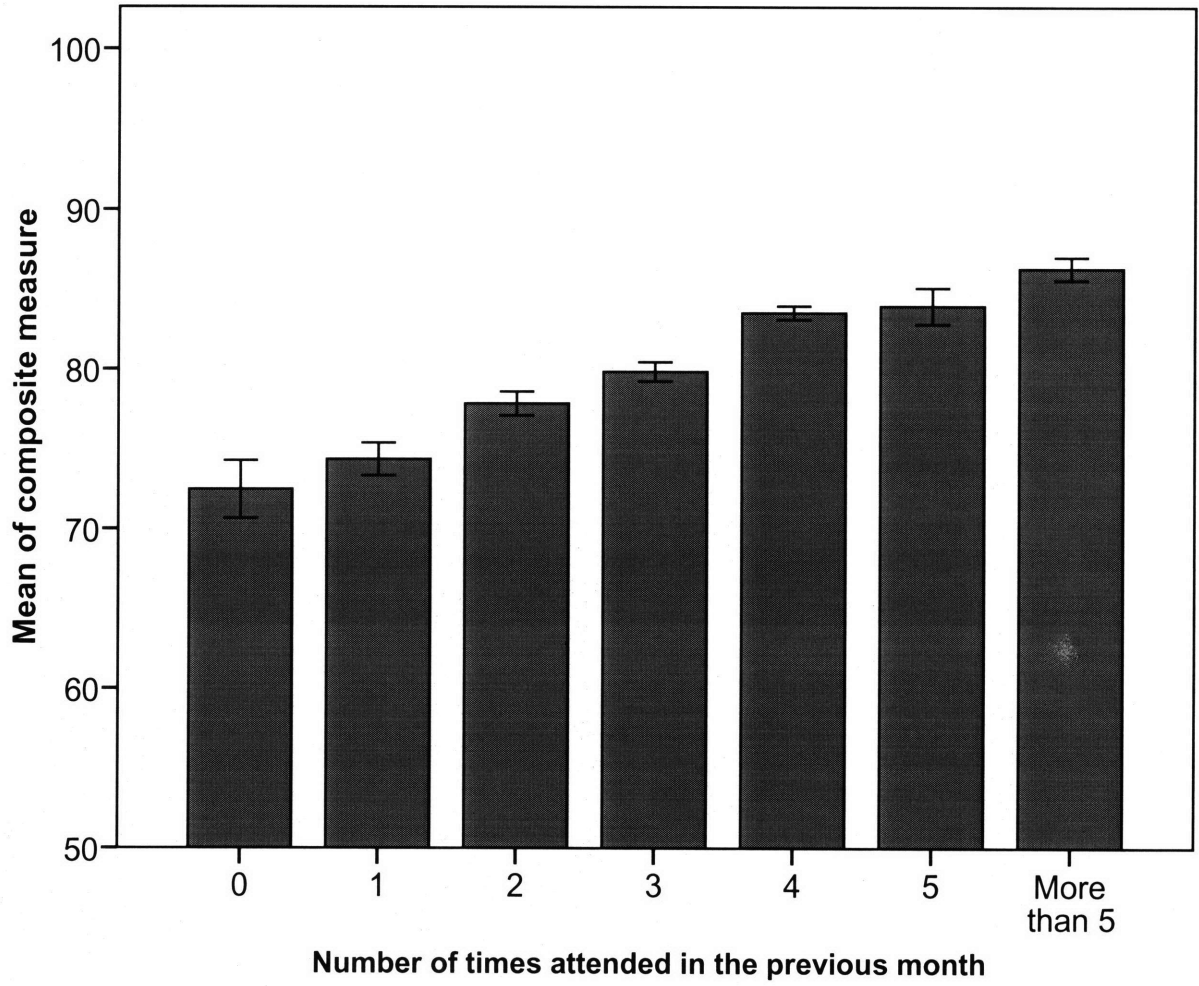
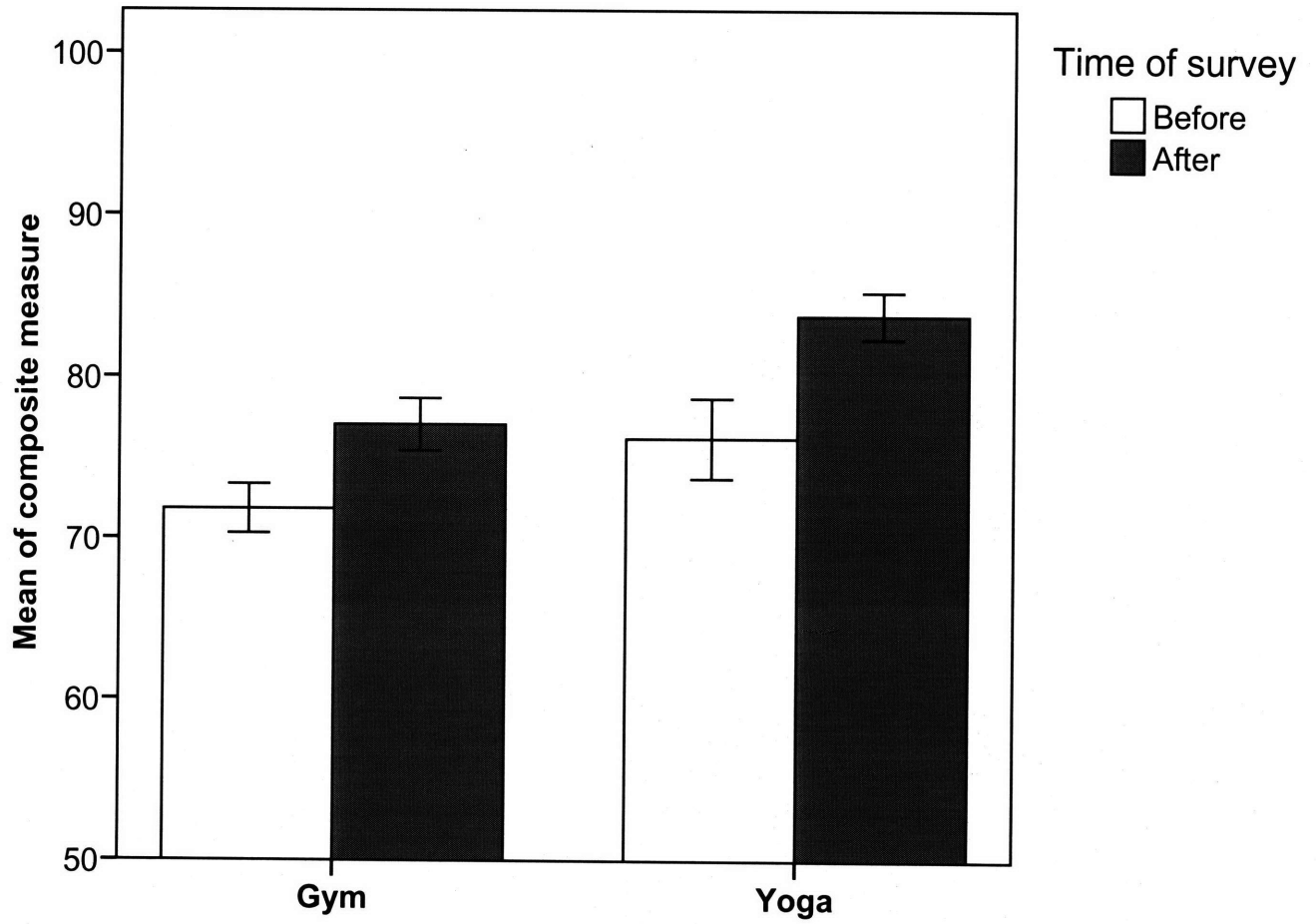


FIGURE 3

Well-being before and after attending the gym or yoga class (Study 2). Error bars indicate standard error.



Essay 3

The Doomsday Device: A Mechanism for Avoiding Conflict

ABSTRACT

Arms races are a common occurrence in the business world. In the current paper, we propose that such arms races can be avoided by the use of commitment to an extreme response, and we test this idea experimentally. Our findings suggest that actual commitment is an effective but underused tool for avoiding the escalation of conflict. We also find that while cheap talk is not effective in the short run, it is useful for establishing a reputation for future interactions.

Arms races have a long history in the social and natural sciences (Dawkins and Krebs 1979; Richardson 1960). In these contests, players are mainly concerned with their standing relative to their opponents rather than with a fixed goal. For example, during the cold war the United States and the Soviet Union kept increasing their nuclear stockpiles in order to not fall behind the other nation, at the peak, accumulating as many as 70,000 warheads between them (Norris and Kristensen 2006). An interesting feature of arms races is that even though the players are competing, they also benefit if they can coordinate to avoid the conflict from escalating. For instance, if both nations had a way to coordinate on building half of the number of warheads they did, they would be no worse off militarily, since only their relative standing matters, while the money saved could have been used for more productive projects. Consequently it is important to find effective methods for achieving such coordination.

Similar conflicts arise in the business world, where firms constantly compete with each other. They cut prices to lure costumers away from their competitors, they increase their advertising to increase their brand recognition, and they constantly make small improvements to their products to get a slight edge over their competitors. The problem of these behaviors is that just as one firm can cut its prices, increase its advertising and change its product, so can its competitor. And so, while these behaviors might bring some short term gains to the firm engaging in them, they will also create an arms race where the long run outcome can involve lower profits for both firms.

Examples of such detrimental effects abound. For example, last year Intel cut the price of its Core 2 quad processor by 50% to put pressure on its closest competitor

Advanced Micro. The price war between these two firms that ensued lead to a 42% decline in annual profit at Intel and a \$166 million loss for Advanced Micro (Culpan 2007). Similarly, Dodge is set to release its 2008 Viper with 600 horsepower, in response to the Corvette that has 500hp. Of course, GM is rumored to be readying a 750hp Corvette to further escalate the arms race (Carney 2007). Arms races can also be observed in advertising expenditure, where each competitor tries to spend more than the other in order to get more recognition. For instance, in the 2004 presidential election, Bush spent \$1.14 billion while Kerry spent \$1.08 billion (Edsall and Grimaldi 2004). Most likely little would have changed if they both had been able to coordinate on a much lower sum, but their inability to credibly do so lead to an arms race resulting in a lot of money being wasted (and many annoyed people who were overwhelmed by the amount of advertising). But is it possible to avoid such arms races?

Ironically, one way to avoid the escalation of arms races is to commit to an extreme escalation. Such an idea has existed in military arms races for many years, and was depicted in the movie “Dr. Stangelove or: How I Learned to Stop Worrying and Love the Bomb”. In this movie the Soviet Union built a “Doomsday” device, which could not be deactivated under any circumstance, and would destroy the whole world if they were attacked by a nuclear weapon. While acts of this nature can be seen as extremely aggressive, they can also be incredibly effective for avoiding an escalation that neither party wants. Indeed, by committing to an extreme aggressive retaliation, both parties can credibly coordinate on a peaceful one they both prefer.

In the current paper, we investigate in an experimental lab setting the effectiveness of such a doomsday device as a tool that can be used to avoid the escalation

of conflict in business settings. In particular, we examine four key questions: First, we test whether such a commitment device is an effective tool for avoiding the escalation of conflict. Second, we test whether actual commitment is necessary, or whether cheap talk is enough to prevent the escalation. Third, we study what are the long term effects of such strategies. Finally, we examine whether our participants understand the benefits of the device and choose to use it when they are allowed to decide whether to implement it or not. In the next section we present the specific model of conflict we use in our experiment, followed by an overview of the research on commitment and cheap talk. After that, we present our experimental design, and the main experimental results.

A Model of Conflict

In our experiments we use a variation of the dollar auction game (Shubik 1971), where a specific amount of money is auctioned off in an all-pay auction. In our setup, participants competed in pairs and were told that they each represented a firm that could spend money on research and development of a product. Whichever firm spent the most money developed the better product and thus got the entire market (worth \$5 in our experiment). Participants were allowed to increase their bids sequentially and to keep the game simple, at each stage the participants' only options were to spend 10 cents more than their opponent had spent, or to drop out and lose their investment. Figure 1 depicts the extensive form of the first 5 periods of the game. Since the game doesn't end until one of the firms drops out, the total number of rounds in the game varies.

... Figure 1 ...

All pay auctions provide a good setup for examining conflict escalation since previous studies have shown that people tend to overbid in these contests. Gneezy and Smorodinsky (2006) ran an experiment where participants could bid simultaneously for a fixed amount of money. All players had to pay their bid regardless of whether they won the prize, and only the highest bidder got the prize. In their experiment most participants overbid, and thus lost money on average, and this behavior persisted over the various rounds of the experiment, suggesting that the bias would not go away with more experience. Similar results were found in the case where participants have independent and private valuations for the prize (Noussair and Silver 2006), and in sequential all-pay auctions where the prize is probabilistically determined as a function of the amount spent (Millner and Pratt 1989).

Besides being a situation where we could reasonably expect conflict escalation, we used an all-pay auction because it is a specific case of a much broader model of competition made popular by Tullock (1980). Variations of this model have been used within the field of marketing to model the effects of advertising on market share (Friedman 1958; Schmalensee 1976). It has also been used to model research and development competitions (Che and Gale 2003; Leininger 1991), the lobbying process (Baye, Kovenock, and De Vries 1993), athletic tournaments and career games (Rosen 1986), market exit by firms (Fudenberg and Tirole 1986), predatory pricing (Roth 1996) and even conflicts between animals (Bishop, Cannings, and Smith 1978).

Mechanisms to Avoid Conflict Escalation

Conflict escalation seems to be a common occurrence, but is it unavoidable? In our experiments we test whether commitment to an extreme response can prevent such an escalation thereby leading both parties to a better outcome. Specifically, in our experiment we include a condition where one of the two firms is by design committed to always increase its spending. If such a commitment is an effective tool for avoiding conflict escalation, we expect the committed firm to perform much better, and even the uncommitted one to improve its outcome in many instances, since it will not engage in a costly conflict.

The benefits of commitment have a long history within game theory. Schelling (1960) discussed how various forms of pre-commitment can alter the structure of the game, allowing players to get payoffs that otherwise were not feasible. Many subsequent theoretical models showed that the ability to commit can lead to better outcomes when firms compete on price (Fershtman and Judd 1987; Sklivas 1987), quantity (Fershtman, Judd, and Kalai 1991), and when firms compete in contests similar to the one used in our experiment (Baik and Kim 1997; Konrad, Peters, and Wärneryd 2004; Kräkel 2005). The theoretical prediction for our experimental setup is simple. When one of the players is committed to always increase their spending, the other player should drop out before he spends any money.

Although in theory commitment should be very effective at defusing the conflict, it might not work in practice. Many studies have shown that people don't only care about their monetary payoffs but also about the fairness of the results. Because of this, they are willing to sacrifice monetary payoffs in order to enforce a fair outcome. For example, in

the ultimatum game, where one player is 'committed' to move first, participants often turn down offers that are lower than 30 percent of the total payoff (Camerer and Thaler 1995; Güth, Schmittberger, and Schwarze 1982). Other experiments which allow participants to punish their opponents have shown that people are willing to spend money in order to punish an unfair opponent, even when there is no direct monetary benefit from this action (Fehr and Gächter 2000).

Given that people have such a strong preference for fairness, it is not surprising that experiments that have tested the effect of commitment have found that it often doesn't have the impact predicted by theory. Some experiments have compared Cournot versus Stackelberg games, where a Stackelberg game can be seen as a modification to Cournot where one firm can commit to move first, and thus have a higher profit. These studies have found little advantage for the Stackelberg leader, since they typically get punished if their output is higher than the cooperative Cournot one (Huck, Muller, and Normann 2001). Similar results held when the choice to move first was given to the players, rather than externally enforced (Fonseca, Huck, and Normann 2005; Huck, Muller, and Normann 2002). Fershtman and Gneezy (2001) looked at the case of explicit commitment in an ultimatum game, and although the ability to commit helped the responder, its impact was much lower than what would be predicted by theory. Finally, Huck et al. (2004) looked at explicit commitment through agents in Cournot markets, and found that participants rarely chose aggressive contracts, and those who did were punished.

Overall these results suggest that commitment doesn't always work as theory would predict. The committed firms tend to get punished if they use their power to force

an unfair outcome, and therefore benefit little from the advantageous position obtained by the commitment. However, this does not mean that commitment never works. In the preceding experiments, there was a fair outcome and the purpose of the commitment was for one player to use his power to get a larger share of the profits. Because of this, the opponents reacted negatively and punished the committed player when he tried to abuse his power. But there are other situations where commitment can be useful. Specifically commitment can be used as a coordination device allowing firms to avoid costly arms races neither firm wants to engage in. As in our leading example from “Dr. Strangelove”, by committing to an extreme response, firms can coordinate on an outcome that is mutually beneficial, and that might not have been possible without the commitment. Our experiments test the value of commitment in such a situation.

While we believe that commitment will be an effective device for avoiding the escalation of conflict, it might be the case that the actual commitment is not necessary. Since neither firm wants to engage in an arms race, allowing one firm to communicate the intention of commitment, without actually committing, might be enough to coordinate on a low conflict outcome. In our experiments we also test whether firms can coordinate on a low conflict outcome through cheap talk by including a condition where one of the firms sends a message indicating that it is committed to always increase its spending, but where it is clear that this is just cheap talk. The message conveys the same intentions as the true commitment condition, and thus firms might also use it to coordinate. However, since it does not represent a true commitment, the player receiving such a message might ignore it and escalate the conflict.

The effects of cheap talk have also been widely studied from both a theoretical and experimental perspective. Even though any game involving cheap talk has a ‘babbling’ equilibrium, in which the sender’s message is uninformative and it is ignored by the receiver, cheap talk has been shown to be effective in many situations. Crawford and Sobel (1982) proposed a model where cheap talk can convey private information, and showed that the amount of information conveyed is related to how aligned the preferences of the two agents are. Other theoretical models have shown that cheap talk can be effective in situations such as coordination of when to enter a market (Farrell 1987), the adoption of new industry standards (Farrell and Saloner 1985) and bargaining (Farrell and Gibbons 1989; Matthews and Postlewaite 1989). In general, the theories show that cheap talk can be effective as long as the incentives of the players are sufficiently aligned (Farrell and Rabin 1996).

The experimental evidence has generally supported this intuition. Dickhaut et al. (1995) manipulated the alignment of the incentives of two players, and found that as the alignment of the incentives of the players decreased, so did the information transmitted in the message, and the weight put on the received message. Other studies have shown that cheap talk can be used as an effective coordination device when incentives are perfectly aligned (Cooper et al. 1989, 1992). Cheap talk can also be effective when incentives are only partially aligned, such as in public good games (Palfrey and Rosenthal 1991), ultimatum games (Croson, Boles, and Murnighan 2003) and double auctions (Valley et al. 2002). For a survey of experimental research involving cheap talk see (Crawford 1998). A recent study has also looked at the effect of cheap talk on an all-pay auction similar to the one we used in our experimental setup, and found that cheap talk can help

achieve better outcomes (Harbring 2006). However, this paper differed from ours in that it included a symmetric equilibrium where both players bid the maximum amount and got the prize with a 50% chance. So there was a way for cheap talk in this set up to help participants coordinate on a lower and equal bid – which was the case.

In summary, the theory on commitment suggests that it can easily alter the outcome of games by allowing a player to use strategies that otherwise would not be possible. However the empirical evidence has not supported this claim strongly. People have a preference for fairness and they are willing to punish others if they abuse the power they obtain from commitment. The theory on cheap talk suggests that in some situations cheap talk can help players coordinate, however the usefulness of cheap talk depends on the alignment of the players incentives. The empirical evidence generally supports these claims, as cheap talk has been shown to be helpful in situations where the incentives of the players are sufficiently aligned. What we want to test empirically is how effective cheap talk and commitment to an extreme response are at preventing the escalation of conflict. This is a situation where the players are competing for a prize, and thus it is more difficult to coordinate than when the incentives are aligned. However, both players are worse off if they allow the conflict to escalate, and therefore would like to be able to coordinate on a lower rather than a higher conflict outcome.

EXPERIMENTAL OVERVIEW

In order to test our research hypotheses, we ran two studies. In the first study we examined the effectiveness of cheap talk and commitment to a ‘doomsday’ device at reducing the escalation of conflict in our research and development arms race. In order to

do so, participants were randomly assigned to conditions where they had to use one of these strategies, or to a control condition where no mechanism was implemented to avoid the escalation of conflict.

In the first study we also examined the long term consequences of these strategies. One concern regarding the use of commitment is that once the committed firm loses this power, the other might retaliate, making commitment a risky long term strategy. In particular, if the non-committed firm perceived the advantage rendered by the commitment as unfair, they might behave much more aggressively once the commitment is no longer an option for its competitor. If this were the case, then commitment would only be useful to the extent that a firm is sure it can maintain the commitment active.

In contrast to actual commitment, cheap talk might have positive long term consequences. If the cheap talk is treated as credible, it will allow the firm to build up a reputation. Once no further communication is possible, this reputation should hold, and therefore it will benefit the firm that sent cheap talk. On the other hand, if the cheap talk is merely used as a coordination device for a particular round, it will not have any long term impact on payoffs.

In order to test the long term consequences of these strategies, we included a second round with the same partner where there was no commitment or cheap talk involved. The participants did not know about this round until the first round of competition ended, thus ensuring that the first round was seen as a one shot game, with no long term strategic implications.

While we believe that commitment and cheap talk could be very effective strategies at reducing the escalation of an arms race, people might not appreciate their

usefulness. In order to test whether our participants are willing to implement them, in our second study we allowed one of the players from each pair to choose if they wanted to implement the ‘doomsday’ machine, send cheap talk or do nothing before engaging in our research and development arms race.

STUDY 1

Method

We recruited a total of 132 participants in groups varying from 10 to 20 participants. Once in the lab, each participant was assigned to a computer, and was randomly paired with another participant in the room. None of the participants knew who their opponent was, and all of the interaction was done through the computer.

In this experiment participants were told that they represented one of two firms that were competing against each other in the development of a product. The firm with the better product got the entire market, which was worth \$5, while the other one got nothing. The way they competed was by spending money on research and development of their product; whichever firm spent the most, had the better product, and thus got the \$5. Since spending on research and development is a sunk cost, whatever they spent during the competition was deducted from their total earnings at the end. In other words, the participant representing the firm that spent the most on R&D got the \$5 minus whatever they spent, while the participant representing the other firm lost whatever they had spent on R&D (See appendix 1 for an exact copy of the instructions). Participants were given a \$15 show up fee in addition to their earnings (or losses) during the task.

At the beginning of the competition, firm 1 faced the decision whether to spend 10 cents to enter the market, or to exit. If firm 1 entered the market, firm 2 had the opportunity to increase its spending by 20 cents or to exit. From then on, whichever firm had spent less money, had the option to increase its spending by 20 cents (10 cents more than the other firm), or to exit the market, and lose all of the money it had spent so far. The competition continued until one of the participants chose to not increase their spending. See figure 1 for the extensive form representation of the first 5 periods of the game.

Before the experiment began, participants were reminded that all of the money involved was real money, and that the amount they made or lost would be added to their total earnings for the experimental session. They were also asked to answer a short questionnaire on the computer before proceeding to make sure they understood the instructions. During this period all questions from the participants were clarified in order to ensure that they fully understood the procedure.

Each pair of participants was randomly assigned to one of three experimental treatments:

Control condition

In this treatment the participants proceeded directly to the competition after having completed the questionnaire.

Cheap-talk condition

In this treatment, firm 1 was forced to send the following message to firm 2: *'I am committed to win this game. Every time you increase your spending, I will increase mine, so there is no way you can win'*. In addition, the screen included the following message from the experimenter: *'Just to make it clear, this is what they are saying but in fact they are not obligated in any way to use this strategy.'* Firm 2 did not know that firm 1 was forced to do this, all they knew is that their opponent had sent them this message. The competition began after firm 2 finished reading this message.

Commitment condition

In this treatment, firm 1 was assigned to a commitment condition, where the computer would automatically increase firm 1's spending every time firm 2 increased its spending and the game would only end when firm 2 stopped spending. In this condition, firm 2 received the following message: *'The other firm is fully committed to an automated spending strategy. It no longer has control over when it will increase its spending or when it will stop. The computer will automatically increase its spending every time you increase yours. The game will only end when you stop bidding.'*

At the end of the competition (when one of the firms stopped spending more), the two participants were told that they would participate in another round of the same task (which they did not know about previously) against the same opponent, and proceeded to the following round. In this round all pairs were in the control condition, such that no

new messages were exchanged, and all participants in the commitment condition were informed that firm 1 was no longer committed during this round.

*Results*¹⁰

Control condition

We first examined the results of the control treatment to see how much money the participants spent on average before they dropped out, and the average earnings of the companies. Would participants be able to foresee that even though for any given choice it was strictly better to increase their spending, the same held true for the other firm, and thus both could end up losing a lot of money by going deep into this competition?

Our results suggest that participants did not realize this, and ended up losing money on average even though they had the option of never entering the competition in the first place. Table 1 shows the percentage of times firm 1 or 2 won the competition (that is, they got the \$5 by spending more), the mean and median spending for the two companies, and the mean and median earnings for the first round.

Overall the results of the control condition suggest that people do not foresee how much their opponent will compete, and end up entering a competition that harms both of them. On average the competition lasted 40 rounds, with each round making both players worse off than if they had stopped earlier. Because of this 68% of the participants in the control condition ended up losing money [$p < .05$ by Binomial test].

•• Table 1 ••

¹⁰ Because of the high degree of non-Normality of the distributions of our results, we used non-parametric statistics for the hypothesis tests. All pair-wise tests were performed using Mann-Whitney tests unless specified otherwise.

Mechanisms to reduce conflict

We next examined whether cheap talk and commitment were effective mechanisms for reducing the escalation of conflict in the control condition. Cheap talk was not an effective mechanism to reduce conflict. The amount spent by firm 1 in the cheap talk condition was slightly higher [$M = \$5.1$, $S.E. = 1.4$], than in the control condition [$M = \$3.9$, $S.E. = .7$]. This difference was not statistically significant [$Z = .08$, $p = .98$]. Commitment on the other hand was an effective mechanism for reducing conflict. The amount spend by firm 1 [$M = \$.2$, $S.E. = .1$] was significantly lower in the commitment condition than in the control condition [$Z = 4.32$, $p < .001$]. Figure 2 displays the entire distribution of spending by firm 1 in the three conditions.

••• Figure 2 •••

The reduced spending resulted in higher earnings for the person representing firm 1. The earnings for firm 1 were significantly higher in the commitment condition [$M = \$4.8$, $S.E. = .1$] than in the control condition [$M = -\$1.4$, $S.E. = .8$; $Z = 5.10$, $p < .001$]. There was no statistically significant difference, however, in the earnings of the people in the control condition and those in the cheap talk condition [$M = -\$3.2$, $S.E. = 1.4$; $Z = .67$, $p = .50$]. Figure 3 displays the entire distribution of earnings of firm 1 in each of the conditions.

••• Figure 3 •••

While it is clear that the commitment benefits firm 1, what is more surprising is that it can also benefit firm 2, even though it can never win in this condition. Since the earnings in the control condition of firm 2 on average were negative, it will be better off to the extent that firm 1's commitment prevents it from entering the competition at all. Indeed, the average earnings of firm 2 in the commitment condition [$M = -\$1$, $S.E. = .1$] were marginally higher than those in the control condition [$M = -\$1.4$, $S.E. = 1.0$; $Z = 1.78$, $p < .08$]. While firm 2's earnings in the commitment condition were also higher than those in the cheap talk condition [$M = -\$2.1$, $S.E. = 1.5$], this difference was not statistically significant [$Z = .72$, $p > .4$]. There was also no statistically significant difference between the earnings of firm 2 in the control condition and the cheap talk conditions [$Z = .23$, $p = .82$]. Figure 4 displays the entire distribution of earnings for firm 2 in each of the conditions.

••• Figure 4 •••

Long Term Effects

One interesting question that arises from the results of the first round is how these outcomes will impact future interactions between the two players. Will those who won in the first round be more likely to win again, or will concerns of fairness lead to the opposite result? Moreover, will the impact of the first round on the second one depend on the condition they were initially assigned to? (even though in the second round they were all effectively in the control condition)

For example, It might be the case the firm 2 will behave more aggressively after firm 1 had been committed in the first round, since this might have been perceived as an unfair situation. On the other hand, it might be the case that firm 2 will treat the commitment in the first round as a useful device to avoid conflict escalation, and thus not compete more in the second round. Cheap talk might also have interesting long term consequences. Even though it didn't have much of an effect in the first round, the participants who actually won after sending cheap talk might have built a stronger reputation, since they backed up their cheap talk with actions, making their statement about them being strong competitors more credible. We next examine the results from the second round to address these questions.

Figure 5 shows the distribution of the earnings for both firms in the second round split by which condition they had been assigned to in the first round. These results show two clear patterns. First of all, earnings are higher across all conditions compared to the results for the first round, indicating that most players dropped out much earlier in the second round. This is not surprising, since many participants lost a lot of money in the first round, and did not want to engage in another costly conflict. Also, in the second round they were paired with the same opponent, so they had information on how much the other was willing to compete in order to win, and thus knew when it was not worthwhile for them to enter the competition.

... Figure 5 ...

Second, when comparing the different treatments, a few interesting patterns emerge. The earnings for the firms in the 3 conditions are pretty similar, and none of the pair-wise comparisons are statistically significant, suggesting that on average the effect of the condition did not carry over to the next round. Participants who had previously been committed did not seem to gain any advantage, nor suffer any detriment from this.

We next looked at the probability that the firm that won the competition in the first round would win it again in the second. On one hand, the firm that won in the first round might be more likely to win in the second, since it could have established a reputation as a tough competitor. On the other hand, there might be fairness concerns, and the firm that won in the first round might let the other firm get the prize the second time around. In order to test this, we compared the probability of winning, and the average earnings of the firms in the control and cheap talk conditions conditional on their outcome in the first round.¹¹

When collapsing across both conditions, we found that when firm 1 won in the first round, it won 66.7% of the time in round 2, while it only won 29.6% of the time in round 2 after having dropped out in round 1. This difference was statistically significant [$\chi^2_{(1)} = 6.0, p < .05$]. We further found that this difference depended on the condition. While there wasn't a statistically significant difference between the control and cheap talk conditions in the probability of firm 1 winning the second round, conditional on it having lost the first one [$\chi^2_{(1)} = 2.4, p > .1$], participants representing firm 1 in the cheap talk condition (89%) were significantly more likely to win the second round than those representing firm 1 in the control condition (36%) when firm 1 had won the first round

¹¹ We could not perform the same analysis for the commitment condition, since firm 1 always won in the first round.

$[\chi^2_{(1)} = 5.7, p < .05]$. These results show that although cheap talk didn't have much of an effect in the first round, it allowed firm 1 to build a reputation more effectively, since conditional on it winning in the first round, it was more likely to win again in the second round compared to the firms that won in the first round, but didn't send any cheap talk. These results were mimicked in the earnings. Conditional on having dropped out in the first round, there was no statistically significant difference in the earnings of firm 1 in the second round between the control condition [$M = -\$0.01$, $S.E. = .54$] and the cheap talk condition [$M = \$.2$, $S.E. = .93$; $Z = .25$, $p = .81$]. However, conditional on having won the first round, the participants in the cheap talk condition [$M = \$3.3$, $S.E. = .7$] had significantly higher earnings than those in the control condition [$M = -2.0$, $S.E. = 2.5$; $Z = 2.4$, $p < .05$].

Discussion

In the first study we tested two mechanisms for avoiding the escalation of unwanted conflict. Our results showed that commitment to an extreme response was a very effective mechanism, while cheap talk didn't have much of an effect. Interestingly, even firm 2 tended to do better in the commitment condition than in the other two, even though it could never win the prize.

We also found interesting long term effects for the various strategies. First of all, contrary to what fairness would predict, the firms that were committed in the first round, did not perform any worse in the second round than the firms in the control condition. Having lost their asymmetric power, they did not feel the need to give up sooner in the second round so that each of the firms could win one of the rounds. Similarly, firm 2 in

the commitment condition didn't seem to compete any stronger in the second round to make up for the initial unfair power balance.

Second, while cheap talk didn't seem very effective in the first round, it seemed useful for building a long term reputation. Participants who sent cheap talk and won in the first round were much more likely to win in the second round compared to those in the control condition who won in the first round.

The results of the first study make it clear that commitment is a powerful tool for avoiding the escalation of conflict. Had our participants been given a choice about which condition they would prefer to be in, the choice would be clear, since the committed firms did vastly better than the other ones. But, will people realize how useful commitment is, and how quickly arms races can escalate without it? We test this in our second study, where we give the participants representing firm 1 the choice of which 'condition' they want to be in.

STUDY 2

Method

We recruited a total of 36 participants from the same population as those in study 1. The procedure was very similar to that of study 1, except that before the competition began, the three conditions in study 1 were described to the participant representing firm 1, and s/he was asked to choose which one s/he would prefer (See appendix 2 for the exact description they saw before making the choice). It was explained that firm 2 would be notified about their commitment if they committed, or would be sent the described message if they chose the cheap talk. They were also informed that firm 2 would not

know that they had this choice. After firm 1 made its choice, firm 2 read the message it received (if any) and then they began the competition.

Results

The results of study 2 were very similar to those in study 1. Participants who chose to commit made more money than those in the other conditions, and this difference was statistically significant [$Z = 2.1, p < .05$]. Table 2 displays the mean earnings of the participants in this study split by the choice of firm 1.

••• Table 2 •••

What about the choices of firm 1, did participants choose the commitment condition which leads to higher profits than the other two? Figure 6 shows the percentage of participants choosing each of the 3 treatments. As the figure shows, participants did not foresee the usefulness of commitment before entering the competition. Only 12% of our participants chose to commit when given the option, and because of this, most participants did not do as well as they could have.

••• Figure 6 •••

Discussion

In our second study we showed that only a small percentage of our participants chose to commit, even though the results from both studies demonstrated that this option

is better than the other two. We believe that this is one of the reasons why arms races are so commonly observed in the real world. Before these conflicts begin, most people do not foresee how quickly they can escalate, and thus fail to invoke mechanisms that can prevent this escalation. People also seem to fail to predict how powerful some of these mechanisms are for reducing the escalation.

GENERAL DISCUSSION

Firms are constantly struggling to get an upper hand in the market by making small changes to undercut their competitors. In the short run many of these changes can lead to higher profits, but in the long run these strategies often lead to an arms race and lower profits for all firms. In this paper we used an all pay auction as an experimental model of this situation. Under this model firms can invest resources in order to have the best product, and win the market. However, it is always beneficial for the firm that has spent less to spend a bit more than its competitor to gain the upper hand.

In our control treatment we found that participants indeed competed too much, leading to negative average earnings. We then tested two mechanisms for reducing conflict: cheap talk and commitment. Our results clearly showed that commitment was an effective tool for avoiding conflict. Contrary to predictions of previous studies that implied that fairness concerns might inhibit the effectiveness of commitment, participants quickly dropped out of the competition when the other firm was committed. We believe that the reason our results are different from previous findings is that in our setting the commitment mechanism was used as a coordination device that helped the players avoid an arms race neither wanted. In previous studies commitment was used to force unfair

allocations when fair ones were possible, leading to retaliation from the players who perceived the result as unfair. Further evidence that the commitment mechanism was perceived as a coordination device came from the second round where commitment was no longer available. Had the participants representing firm 2 perceived it as an unfair advantage, they would have competed more fiercely in the second round to make the final allocation more even, however we did not find any evidence of this behavior.

Our results for cheap talk were mixed. While the cheap talk didn't seem to have an effect in the first round, it seemed to bring benefits in the second round to those who had won the competition in the first round. Participants in the cheap talk condition were more able to build a reputation for being a tough competitor, reflected in the fact that those who had won in the first round, almost always did so in the second, which was not the case for the participants in the control condition.

Considering how powerful commitment was in our experiment, we examined whether our participants would be willing to use this strategy. Our results clearly indicated that they were not, as almost all participants avoided commitment, even though this option clearly led to the best outcome. We believe that our participants did not realize how quickly the conflict could spiral downward in the control condition, and how effective true commitment was at stopping this escalation of conflict.

Commitment devices are sometimes observed within the market. For example, some firms will issue coupons that guarantee they will beat any price offered by their competitors. These strategies could be perceived as aggressive, but our results suggest that rather than increasing competition, these commitments can allow firms to coordinate on a lower conflict and higher profit equilibrium. As long as the firms see these as tools

for coordination, rather than threats, all firms can benefit. Nonetheless, our results also indicate that people don't seem to be aware of these benefits, and choose not to use them when available.

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TABLE 1

Descriptive statistics for the control condition: Proportion of the times that each firm won the competition, as well as the mean and median spending and profits for both firms.

	Winning percentage	Mean amount spent on R&D	Median amount spent on R&D	Mean profits	Median profits
Firm 1	0.5	\$3.95	\$4.3	-\$1.45	-\$0.9
Firm 2	0.5	\$3.945	\$4.2	-\$1.445	-\$1.1

TABLE 2

Means and standard deviations of the earnings in Study 2 split by Firm 1 choice

	Mean earnings for firm 1	Mean earnings for firm 2
Control (N = 10)	-\$1.8 (5.2)	-\$1.8 (5.9)
Cheap-talk (N = 5)	\$1.1 (2.7)	\$0.1 (4.4)
Commitment (N = 2)	\$4.9 (0.0)	\$0.0 (0.0)

FIGURE 1

Extensive form of the first 5 periods of the game in the experiments

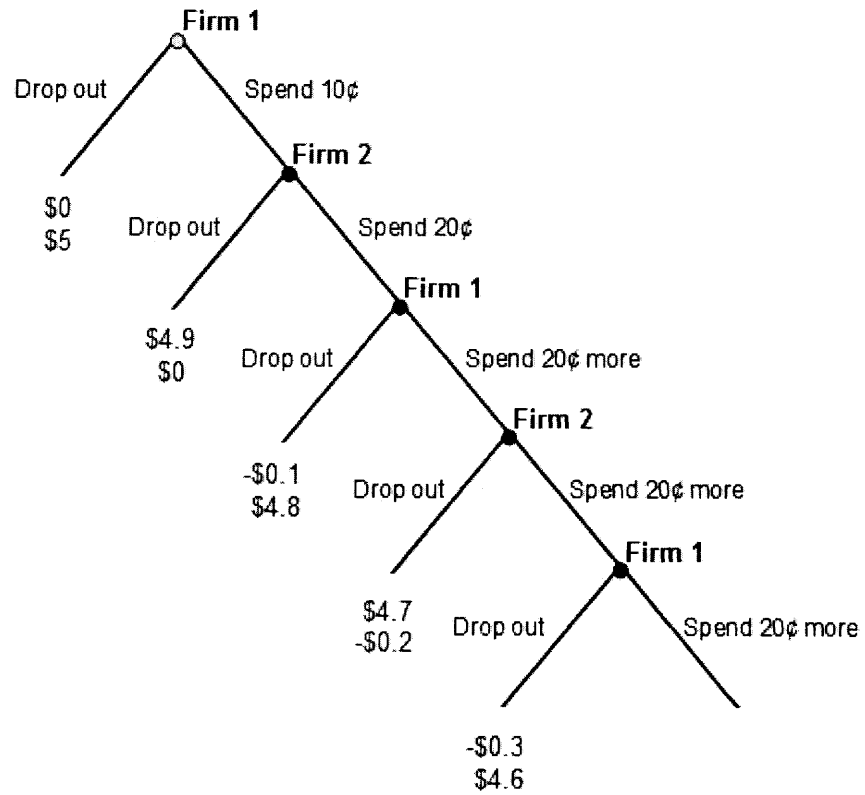


FIGURE 2

Distribution of spending by Firm 1 split by condition.

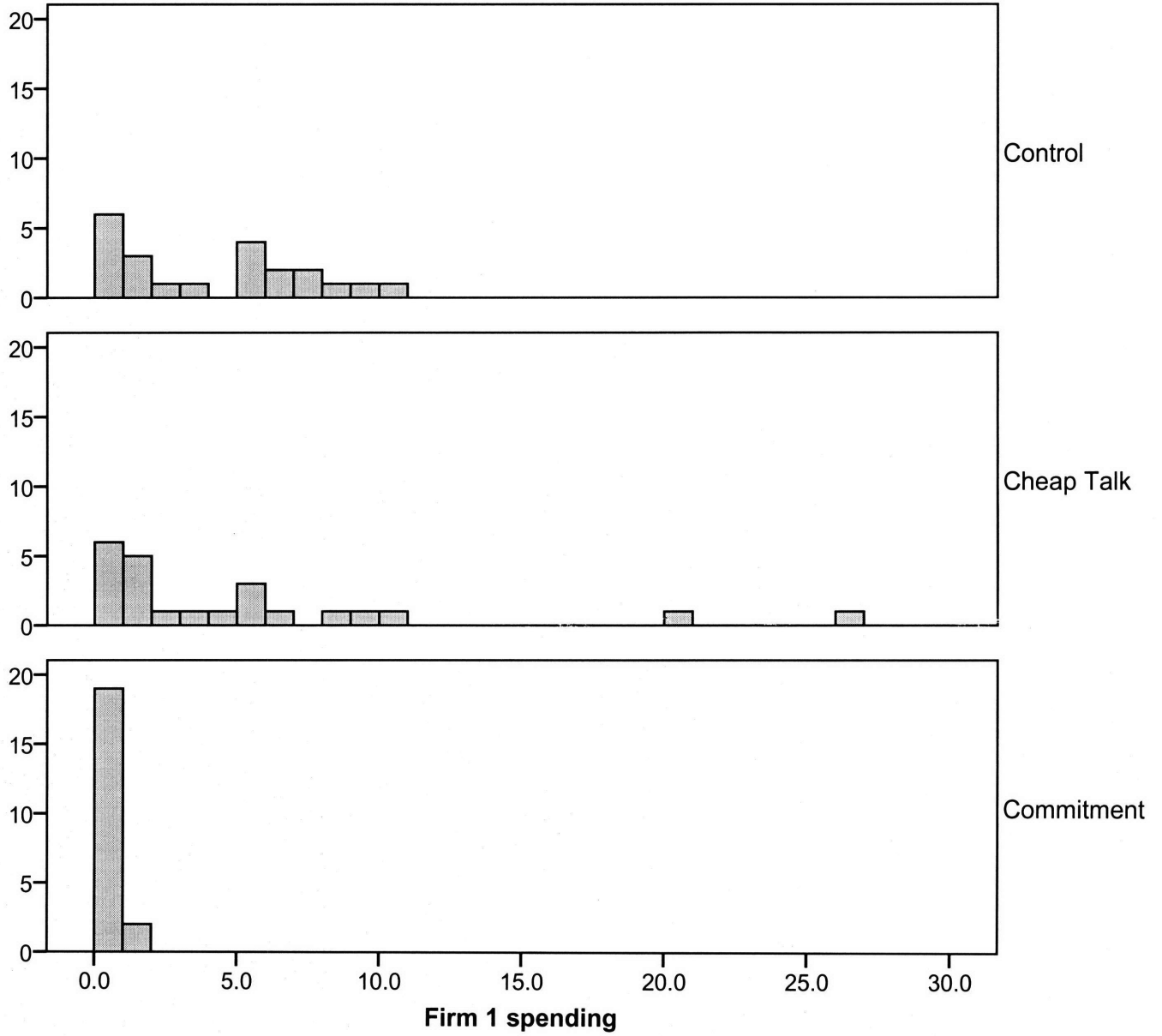


FIGURE 3

Distribution of profits for Firm 1 split by condition.

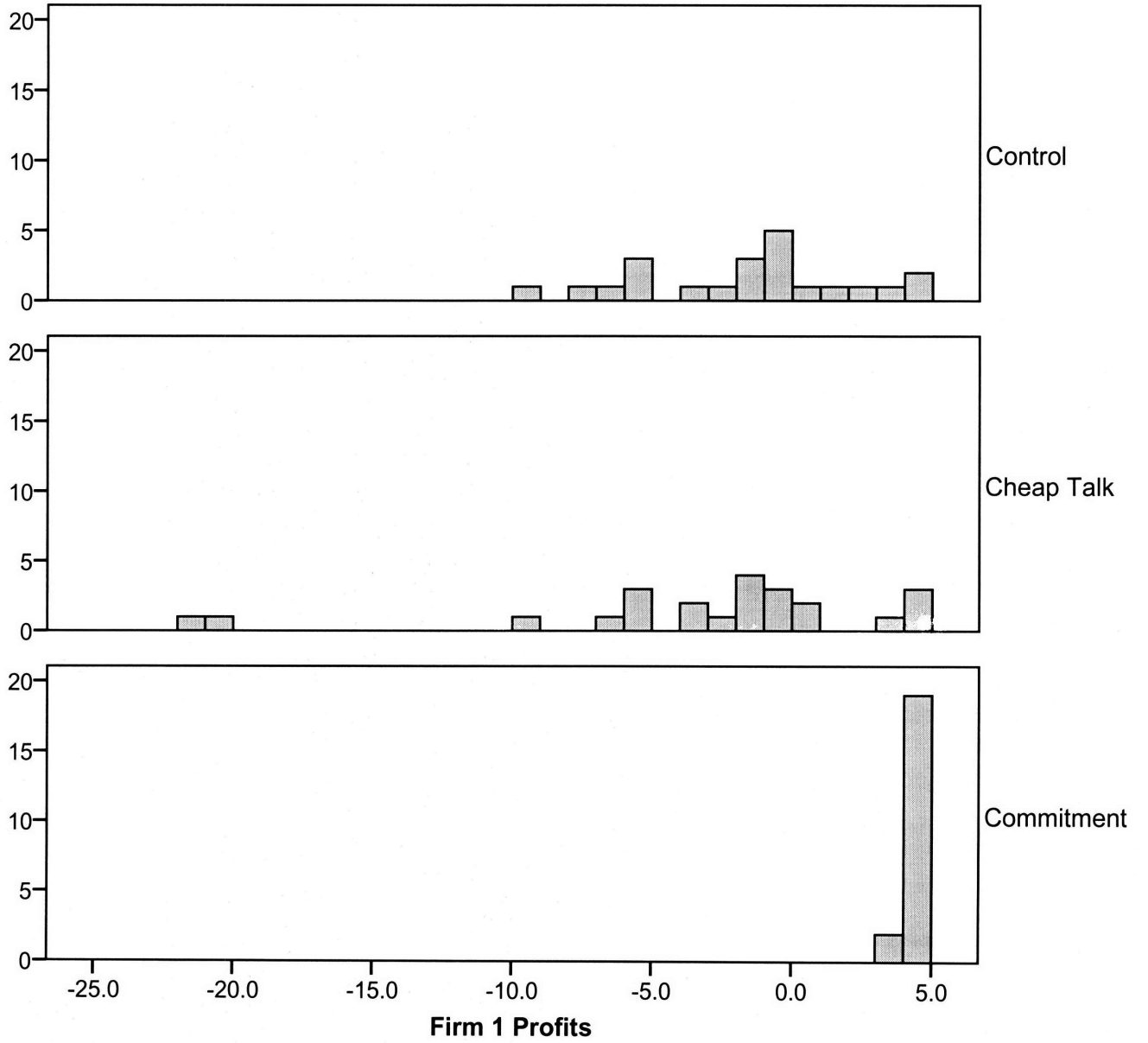


FIGURE 4

Distribution of profits for Firm 2 split by condition.

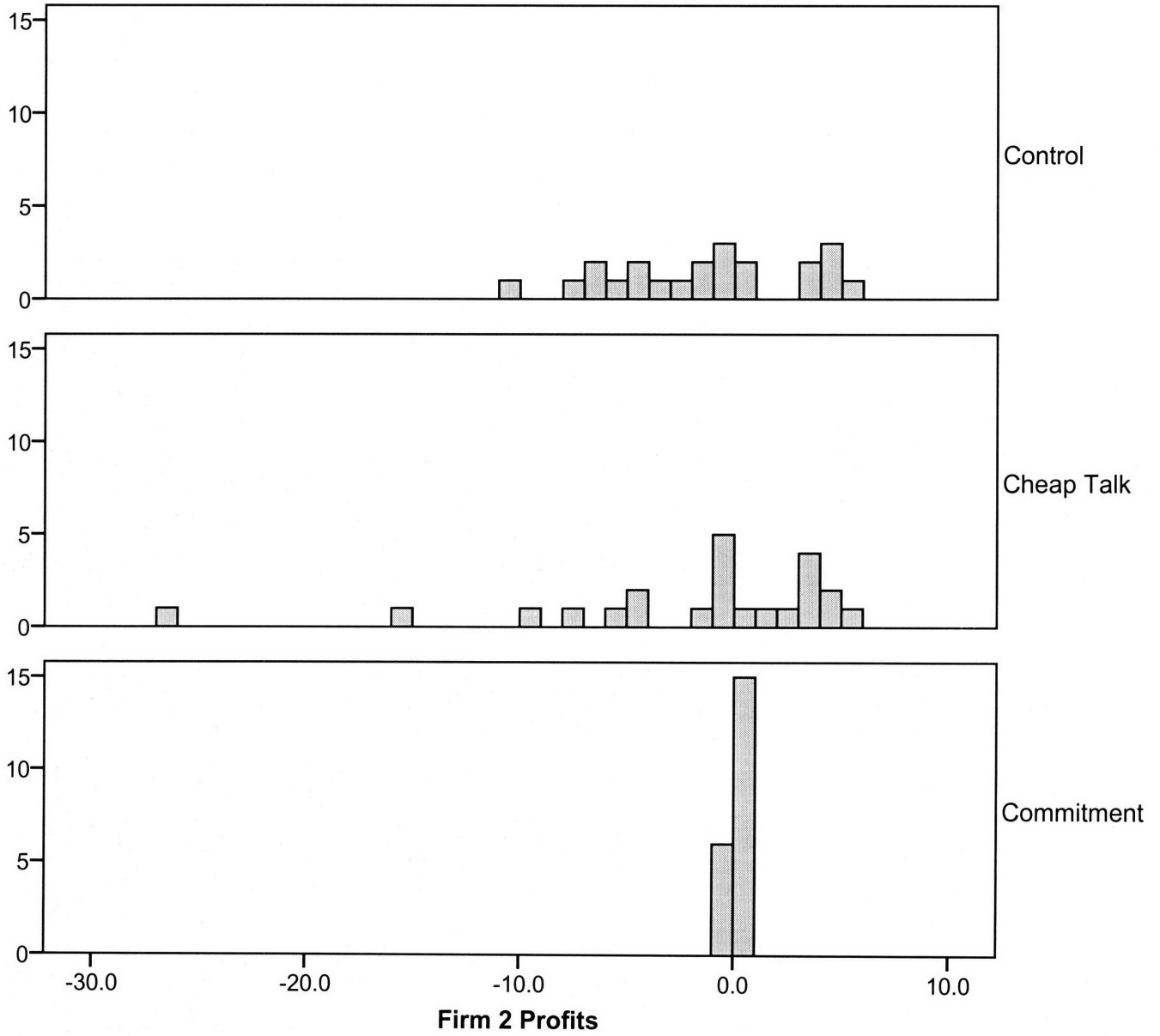


FIGURE 5

Distribution of profits for both firms in round 2 split by round 1 condition.

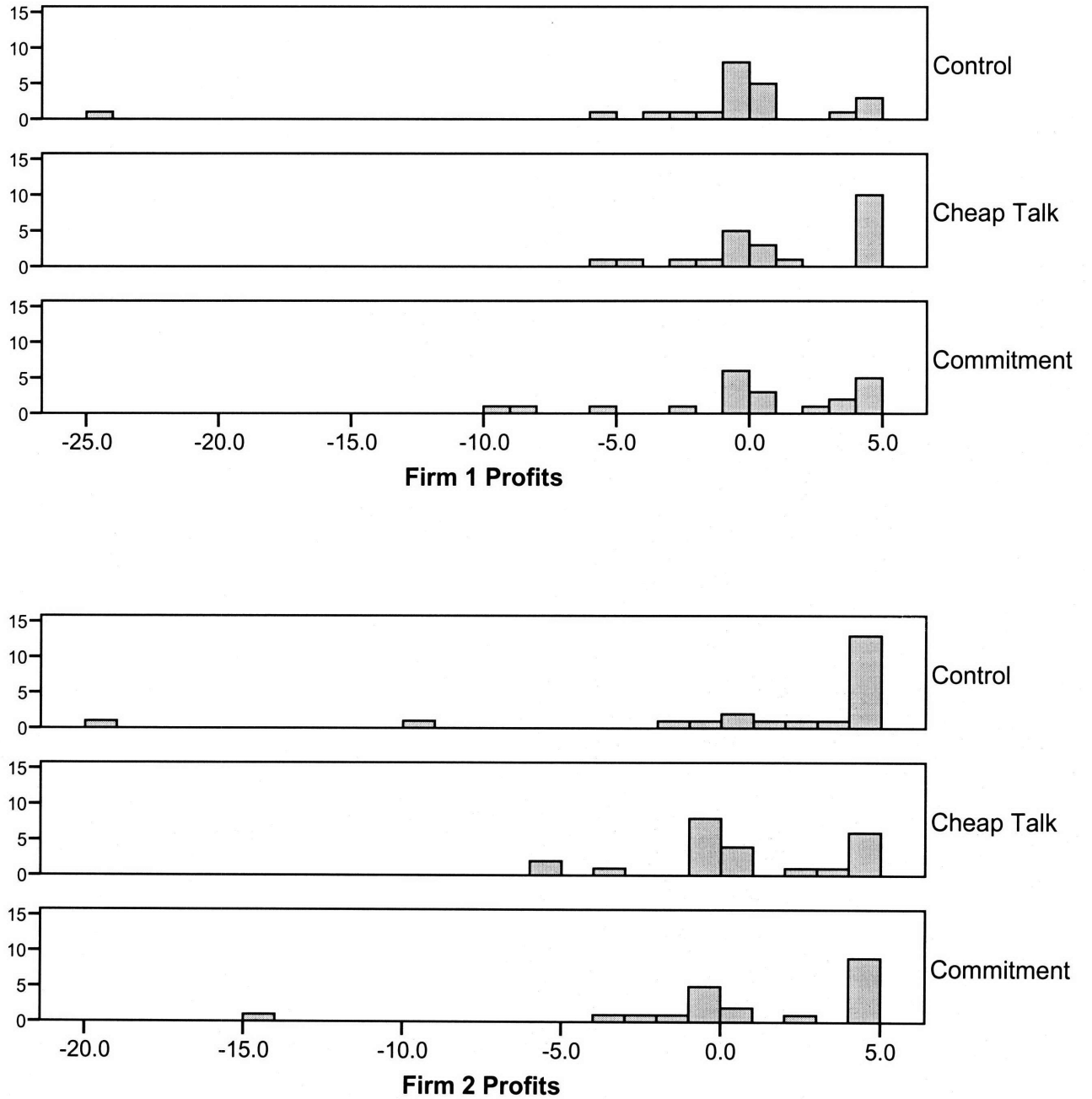
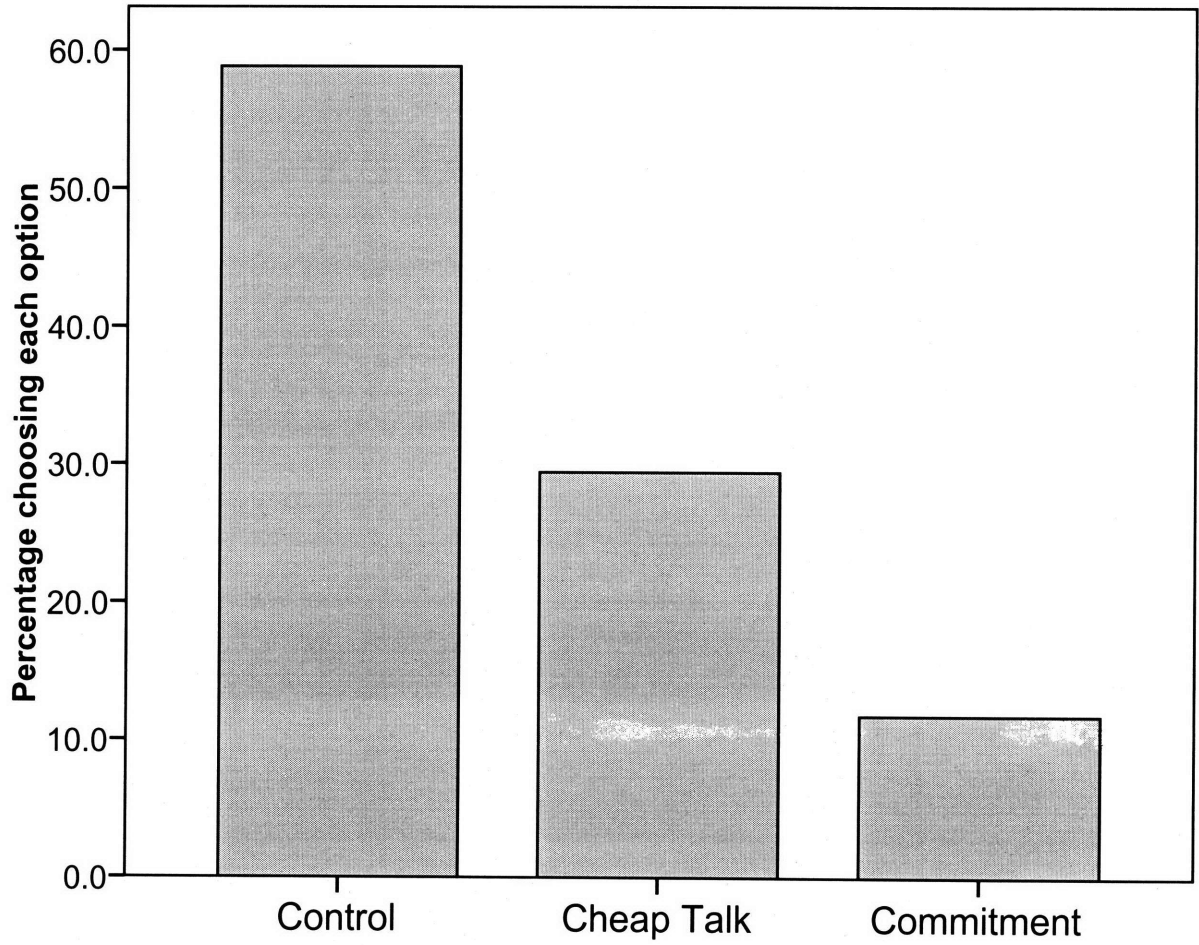


FIGURE 6

Percentage of participants choosing each strategy in Study 2.



APPENDIX 1

Instruction for Company 1

You are a manager of a company (called Company 1) that is competing against another company in the development of a product.

This is a “winner takes all” market, which means that the company that will have the better product will get all the sales (worth \$5 in this experiment).

Both companies have the same ability to produce good products, so the only question is how much they are each willing to invest in research and development (R&D). The company that will invest more in R&D will get the whole market.

Your task is to decide how much to invest in R&D.

Your first decision is whether you want to invest 10 cents to start developing your product. If you do, the other company (represented by another randomly chosen participant) can decide if they want to invest 20 cents. At that point, you can decide if you want to get out of the competition, or invest an additional 20 cents, etc. The competition ends when one of the companies decides to drop out and at that point the company with the highest investment in R&D gets the whole market (worth \$5).

Note that, since your spending is on R&D, money that had been spent cannot be recovered. This means that if after spending \$2, you decide to drop out, your payment for this game will be minus \$2 (to be deducted from your total earnings for the session). On the other hand, the other company drops out, you will get the payment for the whole market (\$5) minus your investments in R&D up to that point.

If you have any questions, please raise your hand. To continue to the next screen, please press k on the keyboard. Note that this is real money, and whatever you gain or lose in this experiment will be added or deducted from your total for the session.

APPENDIX 2

Before the bidding begins, you have the option of doing nothing, sending a message to the other firm, or of committing to always increase your spending.

If you click on the button called 'Message', the other firm will be sent the following message on your behalf: "I am committed to win this game. Every time you increase your spending, I will increase mine, so there is no way you can win". The other firm will know however, that you do not have to follow this strategy if you don't want to.

If you click on the button called 'Commit', you will be committed to a strategy where you will no longer be able to decide when to increase your spending or when to stop spending. The computer will automatically increase your spending every time the other firm increases theirs, and the game will only end when the other firm stops its spending. The other firm will be notified of this commitment.

If you click on the button called 'None', you will begin the game without sending a message or committing.

Regardless of which option you choose, the other firm will not know that you had other options. Please select one of the options: