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Bare promises: An experiment

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ABSTRACT

Is truth-value of a statement what lying aversion is all about? We propose an experimental test and find only limited support. In this context with 'bare promises', we also test for guilt aversion and again find only limited support.

1. Introduction

Previous research has demonstrated that people have some degree of aversion to lying.[1] An important research topic is concerned with why this is the case. Charness and Dufwenberg (2006) (henceforth, "CD") suggest that this happens because decision-makers dislike hurting others relative to what others expect to get.[2] In that paper and elsewhere, promises have been found to foster trust and cooperation.[3] With communication in the picture such "guilt-averse" decision-makers' preferences over choices may change with what is said, as words move beliefs. CD's experiments support this. In their trust-games-with-communication, second-movers often make colorful statements-of-intent ('promises') to exhibit trustworthy behavior. This feeds self-fulfilling circles of beliefs about beliefs that trust and cooperation will ensue.

However, based on data from a clever design, Vanberg (2008) calls CD's conclusions to question to some degree. He argues that "the effects of promises cannot be accounted for by changes in payoff expectations. This suggests that people have a preference for promise-keeping per se." Ellingsen and Johannesson (2004) were probably first to model this, via a "personal cost of being inconsistent," and Chen et al. (2008) and Kartik (2009) develop theory around the more general notion that decision-makers have a (belief-independent) cost-of-lying.

At its roots this concept makes reference to the *truth-value* of statements: decision-makers dislike making statements that are false. In this paper, we focus on a specific element of the overall research agenda: Is this truth-value all we need to capture an important aspect of human motivation, or does the context in which the statement was made matter? As in CD, we propose a design that augments a trust game with a pre-play communication stage and we elicit beliefs to allow us to test for guilt aversion.[4] However, while CD allowed for a full page of rich free-form written communication, in our new treatment the protocol is developed to be as bare as possible subject to being rich enough to allow a second-mover to issue a promise. If cost-

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[1] See for example Gneezy (2005). Brandts and Charness (2003) demonstrate that people dislike being told lies.

[2] To illustrate, in restaurants guilt-averse guests tip in proportion to how much they expect waitresses to expect to get, such as no tip in Italy, a couple of coins in Germany, and 16.5% in New York City ("double-the-tax"). For a theory of guilt aversion that applies to general games, see Battigalli and Dufwenberg (2007). For an early experiment finding support for guilt aversion in trust games, see Dufwenberg and Gneezy (2000).

[3] See e.g. Kerr and Kaufman-Gilliland (1994), Ellingsen and Johannesson (2004), Charness and Dufwenberg (2006), Sutter (2009), and Vanberg (2008).

[4] Some controversy has recently arisen regarding the reliability of such elicited-beliefs data. We do not address the issue in this paper; see Ellingsen et al. (forthcoming) and Reuben et al. (2009) for some evidence and counter-evidence.

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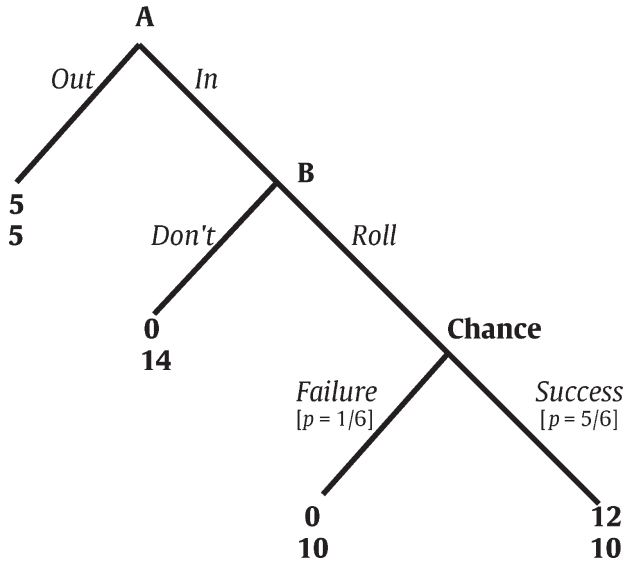
of-lying (or preference for promise-keeping) depends only on truth-value, this should still be sufficient to foster trust and cooperation.

Section 2 describes the design and reports results. Section 3 offers concluding remarks.

2. The Experiment

2.1. Backdrop

CD's game is reproduced below. The naming of players/strategies reflect experimental protocol. Payoffs reflect dollar payments, not necessarily utilities that may differ with social preferences, e.g. pangs of guilt, or cost-of-lying if there is pre-play communication.



CD's benchmark treatment involved no communication and mapped directly to the above game. CD also had a treatment with an opportunity for free-form pre-play communication from B to A; B could print up to a full page of text and send this to A. CD found strong effects on trust and cooperation following a statement-of-intent ("promise"): Comparing post-promise play in the communication-from-B-to-A treatment with play in the no-communication treatment, CD documented significant increases for *In*-rates as well as for *Roll*-rates. Overall, the rate of mutually-cooperative (*In*, *Roll*) strategy profiles was 67% following a free-form promise, far more than the less than 25% (*In*, *Roll*) rate in the no-communication treatment.[5] These changes in behavior were accompanied by significant changes in beliefs.

In what follows we examine whether a new *bare-promise treatment* generates changes relative to CD's no-communication treatment.

2.2. Design

As with CD, sessions were ran at UCSB in a large classroom divided by a center aisle with participants seated at spaced intervals. Our new bare-promise treatment involved three sessions each with 26–36 participants; there were 96 participants in total (no one participating twice). Average earnings were about $14 (including a $5 show-up fee); sessions took about one hour. Participants were referred to as "A" or "B". A coin was tossed to determine which side of the room was A and which was B. Identification numbers were shuffled and passed out face down, and participants were informed that these numbers would be used to determine pairings (one A with one B) and to track decisions for payoffs.[6]

Before playing the game, B could transmit a message to A. Each B was given two sheets of paper. One stated: "I promise to choose *Roll*;" the other was blank.[7] B placed one of the two sheets in an envelope that was conveyed to the appropriate A.

A and B then proceeded to play the game. A first chose *In* or *Out* and then B chose whether to *Roll* or *Don't Roll* a 6-sided die. B made this choice without knowing A's actual choice, but the instructions explained that B's choice would be immaterial if A chose *Out*. As in CD, we thus obtain an observation for every B ("the strategy method"). The outcome labeled "Success" in the figure occurred when the die came up 2, 3, 4, 5, or 6 after a *Roll* choice. After the decisions had been collected, a 6-sided die was rolled for each B; this was made clear to the participants in advance, to avoid the anticipated loss of public anonymity for B's who chose *Don't Roll*. This roll was determinative if and only if (*In, Roll*) had been chosen.

The outcomes and corresponding payoffs were described to the participants in this chart:

	A receives	B receives
A chooses *Out*	$5	$5
A chooses *In*, B chooses *Don't Roll*	$0	$14
A chooses *In*, B chooses *Roll*, die = 1	$0	$10
A chooses *In*, B chooses *Roll*, die = 2,3,4,5, or 6	$12	$10

A feature of CD's design concerned the provisions made for belief elicitation. We also elicit beliefs in this study. After collecting strategic choices, we passed out guess sheets. A's were asked to guess the proportion of B's who chose *Roll*, conditional on whether or not B sent a promise. Knowing that A's made this guess, B's were then asked to guess the average guess made by A's who chose *In*, conditional on whether or not B sent a promise. If a guess was within five percentage points of the correct answer, we rewarded the guesser with $2.50 (we also told participants we would pay $2.50 for all B guesses if no A's chose *In*).[8] These guesses constitute the data that we take to represent players' beliefs.

3. Results

A within-bare-promise-treatment comparison of the nature of play with and without promises yields no significant differences for *In*- or *Roll*-rates.[9] However, since only seven B's did not send a promise, the associated statistical tests on our binary-choice data lack power. To check robustness, we therefore compare behavior following a promise in the bare-promise-treatment (where we have 41 observations) with CD's no-communication treatment (where there were 45 observations).[10]

Fig. 1 gives a visual impression, and Table 1 provides the full details including the results of difference-in-proportions tests (Glasnapp and

[5] CD had many treatments; the rates given here refer to the treatment where the outside option for the first mover was (5.5), which is also the treatment compared in Vanberg (2008).

[6] Complete instructions are available from the authors on request.

[7] The instructions mentioned that a promise was not binding as otherwise some B's might have felt compelled to choose *Roll*. This approach was also used in Glaeser et al. (2000) and Andreoni (2005).

[8] How to best elicit beliefs is a thorny and important issue. We refer to Andersen et al. (2007) and Blanco et al. (2008) for some in-depth discussion about the pros and cons of various methods. Our scheme has the virtue of being simple to describe in instructions (as well as of staying close to CD). As our game is one-shot and we didn't mention guesses until after strategies were chosen, the belief elicitation should not affect participants' prior choices.

[9] 23/41 A's who received promises chose *In*, compared to 2/7 A's who did not receive promises. 25/41 B's who sent promises chose *Roll*, compared to 3/7 B's who did not send promises.

[10] Of course the baseline for different subject pools may well differ, so that strictly speaking one would prefer a control that is run simultaneously with the treatment. However, all of the sessions were conducted with UCSB students in the same room in all cases. We see little reason to expect a difference in the baseline behavior.

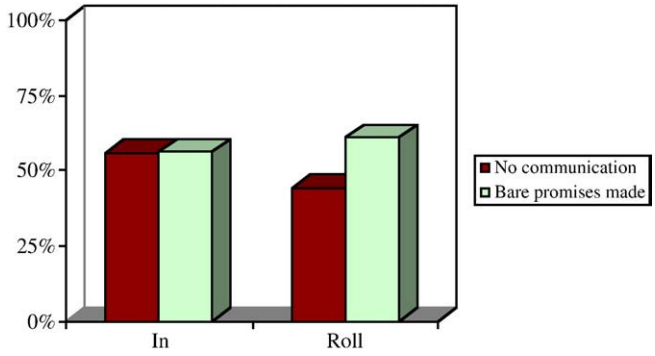


Fig. 1. Bare promises and behavior.

Poggio 1985). The lack of support for a trust-enhancing effect of bare promises is clear, as we observe little difference in A's behavior. Regarding the trustworthiness-enhancing effect of bare promises, there is an increase in the *Roll* rate of 16.6 percentage points; this difference is marginally significant on a one-tailed test. Overall, we interpret our results as providing some degree of support for truth-value based cost-of-lying, but the effects are not as large as those seen with CD's free-form promises, where the *Roll* rate after free-form promises was 75%. The observed rate after bare promises is roughly halfway between the rates with no communication and with richer and endogenous promises.

We have seen that bare promises have no effect on *In* rates and a marginal effect on *Roll* rates, so that the support for truth-value based cost-of-lying is not overwhelming. Do we find evidence of guilt aversion in our belief data? Recall that the test for guilt aversion is based on B's beliefs concerning A's beliefs. We find that the average belief of B's who choose *Roll* is 60.75, while the average belief of B's who choose *Don't* is 53.05.[11] While the direction of the difference is as predicted, the support for guilt aversion is not as strong here as in CD. The difference in beliefs of B's who choose *Roll* and *Don't Roll* is less than statistically significant ($Z=0.94$, $p=0.174$) on a one-tailed Wilcoxon ranksum test, while a simple probit regression of *Roll* against B's beliefs gives $Z=1.12$ ($p=0.131$, one-tailed) for the coefficient of B's beliefs).

4. Concluding remarks

One's regard for the truthfulness of a statement or promise is important for understanding economic interactions involving communication. We test whether violation-of-truth-value can by itself explain why people are averse to lying, by using a design that is rich enough to allow explicit promise-making and is otherwise as bare as possible. In comparison to not permitting communication at all, our data exhibit no effect at all on trust from bare promises, while trustworthiness after bare promises rises to a level intermediate between that observed with no communication and with rich, free-form promises. On balance, and in comparison with the richer promises in CD, these results suggest that bare promises had substantially less effect on behavior.[12] Overall, we find only limited support for truth-value based cost-of-lying; at the same time, we find only limited support for guilt aversion.

While a commitment-based story for promise-keeping may well be relevant, our new results suggest that to explain why people keep promises one needs something more nuanced than concern with truth-value only. It seems clear that context matters; for example, there are situations (such as used car sales, promises made by politicians, tax returns sent to the IRS, and testimony in traffic courts) where lying seems more-or-less expected and liars seem not to suffer much.[13] Thus, it appears that the issue of why some promises are more effective and credible than others has many shades that will require further study.

Table 1
Results across treatments.

	No communication	Bare promises made	Tests of differences
% *In*	25/45 (55.6%)	23/41 (56.1%)	$Z=0.05$, $p=0.480$
% *Roll*	20/45 (44.4%)	25/41 (61.0%)	$Z=1.53$, $p=0.063$

All tests are one-tailed, presuming an *ex-ante* lying-aversion hypothesis.

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[11] We measure the belief of a B-player who sent a promise as his or her guess of the average guess of those A's who chose *In* after receiving a promise. We measure the belief of a B-player who did not send a promise as his or her guess of the average guess of those A's who chose *In* after not receiving a promise.

[12] We do not wish to imply that bare communication cannot have large effects. Bare statements of intent have for example been found to be quite effective as an equilibrium-selection device in coordination games (see, e.g., Cooper et al., 1989, 1992; Charness 2000) where players to a large extent have joint interests.

[13] More examples come from a recent study by Erat. and Gneezy (2009) on "white lies", which *benefit* another person. They compare situations where the white-liar gains to cases where he or she is hurt, documenting subtle effects.