The Sound of Silence: A License to be Selfish

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Abstract. We theoretically formulate the idea that if a person stays silent in a situation where a promise could have been made, then he or she will subsequently act as if having a license to be selfish. We then report results from an experimental test that provides some support.

1. Introduction

Several experiments have explored whether promises foster trust and cooperation. It was found that rich free-form promises do (e.g. Charness & Dufwenberg 2006, CD-06; Vanberg 2008) while short pre-fabricated circled promises do not (Charness & Dufwenberg 2010, CD-10).[1] CD-10 compare a treatment that does not allow for communication, with a treatment where subjects have a binary choice whether or not to circle a pre-fab promise.

The alternative to a promise, in a treatment that allows for communication, is *silence*. (i) What is the effect of choosing to be silent? (ii) Is it the same as being in a no-communication treatment? (iii) Would it ever be in a subject's interest to actively stay silent? No one explicitly set out to explore those questions.[2] Now we do. We have a theory that suggests a pattern of answers to questions (i)-(iii), which we test. In a nutshell: (i) Actively silent people should not be trusted; they feel they issued a fair warning, which eliminates their remorse of acting selfishly. (ii) *No*, because subjects in a no-communication treatment have no opportunity to issue a fair warning. (iii) *Yes*, staying silent may be in a subject's interest (but it depends on parameters, as we'll show).

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[1] For other relevant related work, see Ostrom *et al.* (1992), Braver (1995), Ellingsen & Johannesson (2004), Gneezy (2005), Kawagoe & Narita (2014), Ismayilov & Potters (2016), Balafoutas & Sutter (2017), Ederer & Stremitzer (2017), Krupka *et al*. (2017), Di Bartolomeo *et al*. (2019), and Cartwright (2019, Section 4).

[2] In principle the issue could have been considered by CD-10. However, they neither had such a goal nor enough data for a meaningful exploration as only four individuals made a bare promise in their study.

Exploring silence in settings where active communication is possible may at first seem esoteric. But, behold in particular (iii) above – we identify arguably plausible reasons why silence might be expected, hence shape economic outcomes. Moreover, on reflection one realizes that instances occur where people seem to choose silence over active communication. For example, think of a departmental meeting where the Chair is looking for people to serve on a boring committee and no one volunteers and in fact all colleagues look at the floor. If someone is forced to join the committee, will he or she do a good job?

Section 2 presents our theory. Section 3 describes our experimental design, including how we make use of Vanberg's (2008) "switching methodology" which is essential for drawing valid causal inferences. Section 4 reports results. Section 5 concludes.

2. Theory

Consider the following trust game, introduced by CD-06 and used in many studies (including CD-10). Payoffs reflect monetary payments, not necessarily utilities as individual choices may be affected by social preferences (e.g., inequity aversion, reciprocity, pangs of guilt):

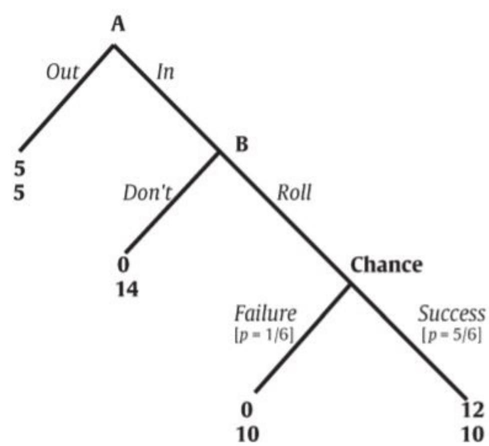

Figure 1 – Binary-choice trust game *à la* CD-06)

Now augment this game to allow for pre-play communication, from B to A. Namely, B has a binary choice. He may either send a promise that he will choose *Roll*, or he may stay silent. Call these choices *P* and *S*. The communication opportunity expands the ways in which social preferences may influence play. Previous literature, on this or related games (like that of Gneezy 2005), considered influence via guilt aversion (e.g., CD-06, Ederer & Stremitzer

2017, Battigalli *et al*., 2013; compare Battigalli & Dufwenberg 2007), intrinsic preferences for promise-keeping or internal consistency between declarations and actions (e.g., Ellingsen & Johannesson, 2004, Vanberg 2008, Ismayilov & Potters, 2016), or a combination of those (Di Bartolomeo *et al*., 2019). We explore a new idea: *selfish licensing*.

Consider the possibility that people feel bad, to some degree, when they choose opportunistically at the expense of someone, *unless they issued a warning*. If, somehow, they did issue a warning, then they would feel less bad engaging in opportunistic behavior, because they may afterwards justify their behavior in terms like "blame yourself since I told you so."

What does it mean to "issue a warning?" We propose that this can in principle be done by commission ("I tell you buddy, you're my friend but if you sit down with me at the poker table then beware that I take no prisoners"), or by omission in situations where someone could have indicated some kind of cooperative tendency but didn't (as in the example with the boring committee, given in the introduction). In the game at hand, we propose that the second possibility – warning-by-omission – applies. The active choice to be silent (S) gives B a psychological license to be selfish. By choosing S, when also the opportunity to choose P is available, the former choice will be taken to be a kind of warning, a signal of B's gamesman proclivity such that B would not feel bad choosing *Don't Roll*. By contrast, in the baseline setting, where there is no pre-play opportunity to communicate, no presumption of gamesman inclination is hinted at, and B obtains no license to be selfish.

To model this consideration, we analyze our game (mainly) from the viewpoint of player B. Specifically, B's utility equals his expected material payoff, except if play proceeds *In* + *Don't* and *there was no warning*. In such a case, $k > 0$, denoting a psychological cost to B of making an opportunistic choice at the expense of A, is deducted from B's material payoff to define his utility. In our context, the utility is thus $14 - k$ in the baseline as well as in the BP-setting following choice bare P (promise) while the utility instead equals 14 in the BP-setting following choice S (silence). Rationality thus implies that B chooses *Don't* after S + *In* (since $14 > 12 \cdot 5/6 = 10$). And after P + *In* player B chooses *Roll* if $10 > 14 - k$, or if $k > 4$, and conversely chooses *Don't* if $k < 4$.

Let x be the probability with which B believes that A will choose *In* following S, and let y be the probability with which B believes that A will choose *In* following P. Assume that $y > x$ – this is intuitive, and will be theoretically justified later. We can now solve for B's overall behavior. Consider first the case where $k < 4$. B will choose P-then- *Don't* if

$$(1 - y) \cdot 5 + y \cdot (14 - k) > (1 - x) \cdot 5 + x \cdot 14 \qquad \Leftrightarrow \qquad y > 9 \cdot x / (9 - k),$$

and if that inequality is reversed B will choose *S*-then-*Don't*. If instead $k > 4$ then B will choose *P*-then-*Roll* if

$$(1-y) \cdot 5 + y \cdot 10 > (1-x) \cdot 5 + x \cdot 14 \qquad \Leftrightarrow \qquad y > 9 \cdot x / 5,$$

and if that inequality is reversed B will choose *S*-then-*Don't*.

Suppose that player B is drawn from a population of scholars with different values of k, x, and y. Some B's would thus rely on *P*-then-*Don't*, some on *P*-then-*Roll*, and some on *S*-then-*Don't*. Note that a message of *S* is ominous, foreboding a choice of *Don't*. The sound of silence is a license to be selfish. Accordingly, A's expected material payoff from choosing *In* would be higher following *P* than following *S*.

We have not said anything about how player A is motivated, but if other things being equal higher expected material payoff is a good thing then we might expect that A is more likely to choose *In* following *P* than following *S*. This justifies our assumption that $y > x$.

3. Experimental design

The design involved 192 undergraduate students. Each of them played a session, which consisted of 8 rounds, with perfect stranger matching. Participants in each session were 32. We performed 6 sessions.

In each round, participants played the trust game described in Figure 1. As in CD-10, before playing, B had to decide whether to send a pre-fab promise to A (or to stay silent). As shown by CD-10, pre-fab promises do not shape expectations and *Roll* rates relative to the case when free form communication is allowed. We choose this (less effective) form of communication since our focus is not on promises, but on silence. Consistently with CD-10, we presume that promisors behave as if there were no communication.

The game designed by CD-10 is furthermore augmented with an additional key feature: the communication outcome (message or active silence) might not be delivered. Specifically, before B's choose their actions, the A-player of all pairs were switched with probability ½. All participants knew that A-players could thus be switched, and that only B's could observe if the A-player originally assigned to them were switched (hence second-order beliefs are unaffected by the switch). This way, we use the innovative switching technique introduced by Vanberg to test how people, who sent a *S* (warning), would behave if they could not communicate with someone. If B's originally paired A-player is switched the warning implicit in B's silence behavior is not conveyed to the A-player that B ultimately interacts with. Our idea is that the B-player understands this and hence acts as if he had not been able to convey any warning.

Formally, each round implemented the following sequence of five stages:

1. **Role assignment.** Roles A and B were randomly assigned and pairs were formed.
2. **Communication.** B's had to send to the originally matched A-player either the pre-compiled message "I promise to roll the dice" or stay silent (no message).
3. **A's action.** A had to decide whether to choose *In* or *Out*.
4. **Switching pairs.** Some A players were randomly switched (the probability of being switched being 1/2). Only B's were informed whether or not the A player with whom they were originally matched had been switched.[3]
5. **B's action.** B chose between *Roll* or *Don't*. Then all subjects were informed about their payoff for the round. A's were not informed whether they had been switched nor of B's choice; only payoffs were revealed.[4]

At the end of each session, one of the rounds was randomly chosen for payments determined by agents' choices. All the game payoffs were described in terms of "tokens," with 1 token = 0.5 euro. In addition, each subject received a fixed show-up fee of 2.50 tokens.

4. Experimental results

Our sample comprises 768 observations. Out of those, we obtain 638 pre-fab promises and 130 cases where B's choose to stay silent and not send a message. Then, 559/768 A's decided to continue the game (*In*) – in 468 cases after a pre-fab promise, whereas in only 71 cases after B's stayed silent. As we use the direct method, we obtain 559 observations for B's behavior (*Roll*/*Don't*).

Our results are described in Table 1. It reports, the percentage of choices of *In* (column (a)) and *Roll* (columns (b)-(c)), respectively. Rows give information about the kind of communication used by B. Standard deviations (s.d.) and the number of observations (obs.) are reported.

[3] The switch occurs only if A chooses *In*. If A chooses *Out* the round ends. Switched B's were furthermore allowed to observe whether the new A had received a promise or whether his/her partner were silent.

[4] A's could obtain a zero payoff either because B did not roll the dice or because the roll drawn was "the number one" when the B has chosen to do it.

Table 1 – A's and B's behavior (*In* and *Roll* rates)

	A'S *IN* RATES	B'S *ROLL* RATES	
		B WASN'T SWITCHED	B WAS SWITCHED
B'S COMMUNICATION:	(a)	(b)	(c)
(1) PROMISE	76% (s.d. 0.42/obs. 638)	49% (s.d. 0.50/obs. 250)	45% (s.d. 0.50/obs. 238)
(2) SILENT	55% (s.d. 0.50/obs. 130)	20% (s.d. 0.41/obs. 35)	36% (s.d. 0.49/obs. 36)

Comparing the percentages in column (a), A's are less likely to choose *In* when B's stay silent. Specifically, 488 out of 638 (76%) A's chose *In* when they received a promise, whereas 71 out of 130 B's (55%) choose *In* when no messages was sent to then (but rather they encountered silence) (76% vs. 55%: Z=1.99, p=0.046).[5]

Let us now focus on B's starting from promisors (row (1)). Our evidence here may be seen as supporting the results of CD-10, although the comparison is not based on treatments with and without communication. Rather, we can compare the situations when a bare promise was sent and then delivered, or not, to the eventual co-player (so no switch, or a switch). We find no significant difference. The behavior of bare promisors is unaffected whether the message is delivered or not. Specifically, the fraction of non-switched pre-fab promisors who chose to *Roll* (49%) is not significantly larger than who chose to *Roll* (45%) when switched and the pre-fab promises are not delivered (49% vs. 45%: Z=0.73, p=0.463).

The main novelty of our project is to focus on silences. We provide evidence that silence matters. Silent agents are less likely to *Roll* compared to bare promisors, but only when the "silence was delivered" (i.e., in the non-switched case). Specifically, among the non-switched cases (column (b)), the fraction of silent agents who chose to *Roll* (20%) is significantly smaller than who chose to *Roll* (49%) after a pre-fab promise (i.e., 20% vs. 49%: Z=2.20, p=0.027). But silence doesn't lead to a change in *Roll* rates when the originally matched co-player is switched; the fraction of B's who chose to *Roll* following a switch (36%) is larger than that observed (20%) for non-switched cases where the "silent" warnings were delivered (i.e., 36% vs. 20%: Z=1.99, p=0.046).[6]

[5] All reported statistics adopt the Wilcoxon signed rank test, which compares averages at the session level. Our data are independent at session level. We check the robustness of our results using the Fligner-Policello test, which confirms results of the Wilcoxon signed rank test, so we omit to report them. Our results are also robust when investigated by estimating a (probit) panel model that makes use of individual-level data. Estimation results are in line with the conclusions derived in the paper. Moreover, no trends across or between individuals are observed. Estimates are done using the generalized linear latent and mixed model (gllam) module of Stata and are available upon request from the authors.

[6] It is worth noting that in the switched case there is no difference between the case of bare promises or silence (i.e., 45% vs. 36%: Z=0.73, p=0.463).

4. Concluding remarks

Should active and passive non-communication be interpreted the same? Perhaps those who stay silent, when they could have made a promise, suffer less psychological cost of acting opportunistically than they would had they not had any communication opportunity. We formalized this idea and showed that there may be circumstances where active silence is chosen by folks who then cannot be trusted. We designed an experiment to test for empirical relevance, and we found some support. The sound of silence can be a license to be selfish.

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