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Repair Avoidance: When Faithful Informational Exchanges Don't Matter That Much

Bruno Galantucci,[a,b] Benjamin Langstein,[a] Eliyahu Spivack,[a] Nathaniel Paley[a]

[a]*Department of Psychology, Yeshiva University*
[b]*Department of Economics and Social Sciences, John Cabot University*

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Abstract

Common-sense intuition suggests that, when people are engaged in informational exchanges, they communicate so as to be reasonably sure that they perform the exchanges faithfully. Over the years, we have found evidence suggesting that this intuition, which is woven into several influential theories of human communication, may be misleading. We first summarize this evidence and discuss its potential limitations. Then, we present a new study that addresses the potential limitations. A confederate instructed participants to "pick up the skask" from a tray containing six objects and move it to a specific location. Since *skask* is a non-word invented by us, participants had to ask for clarification to perform the instruction faithfully. In contradiction with the intuition that people pursue faithfulness when engaged in informational exchanges, 29 of the 48 participants we tested performed the instruction without asking for clarification. We identified a possible cause for this behavior, which occurred more frequently when avoiding the clarification was unlikely to result in an overt consequence (an error in the execution of the instruction that could be noticed by the confederate or the experimenter). Other factors such as individual differences and the specific interpersonal dynamics of the experimental settings, if they played a role at all, did it to an extent that is unlikely to be comparable to that of the role played by overt consequences. Considered together, our various assessments of the extent to which people engage in faithful informational exchanges converge on a simple conclusion: Communicating faithfully is a substantially demanding task, and people often fail at it. We discuss the implications of this conclusion and speculate on its relevance for understanding the evolutionary past of human communication.

Keywords: Human communication; Conversation; Interpersonal interaction; Miscommunication; Situated cognition; Language; Communication biases; Conversational repair

Correspondence should be sent to Bruno Galantucci, Department of Psychology, Yeshiva College of Yeshiva University, 2495 Amsterdam Avenue, New York, NY 10033. E-mail: bruno.galantucci@gmail.com

1. Introduction

Let us imagine that, while sitting at a table, three friends engaged in the following exchanges.

Exchange 1.

Alice: Mario, can you please pass me the skask?

Mario: Sure.

[On the table there are a number of objects. Mario picks up a carafe of wine and passes it to Alice. In the meantime, Alice has left the room to answer a phone call.]

Exchange 2 (Mario's answers are all factually true.)

Oscar: Mario, I've never heard the word skask before, where does it come from?

Mario: I don't know, I've never heard it before either.

Oscar: Do you know what that word means?

Mario: No, I don't.

Oscar: Did I hear correctly that Alice asked you to pass her the skask?

Mario: Yes, that's what I've heard too.

Oscar: Did you already know that Alice wanted the carafe of wine?

Mario: No, I didn't.

Oscar: Let me understand, you passed the carafe to Alice without knowing what she actually asked you to pass?

Mario: Yes, that's what I did.

Oscar: Did you think she was being silly or making some kind of joke and went along with it?

Mario: No, I didn't think that.

Oscar: Do you suffer from any communication disability or mental illness?

Mario: No, I don't.

As Oscar's final question suggests, it is very hard to make sense of Exchange 1 in light of Exchange 2. Perhaps Exchange 1 could be an excerpt from a play of the *Theater of the Absurd*—something that purposively defies common sense—but definitely not an exchange likely to originate from ordinary circumstances. This simple intuition is woven, more or less explicitly, into several influential theories of human communication. For convenience, here we focus on two of them into which the intuition is woven rather explicitly. The first is a long-standing theory in linguistics and it is proposed by Hockett (1960). According to Hockett (1960), Exchange 1 is at odds with the basic requirement that senders and receivers of a message must share the same associative ties between the words they utter and the things to which the words refer. In his words:

> John says, "Please pass the sugar." Bill passes it. ... Thus there is an associative tie between the word sugar and a certain familiar substance: Bill does not pass the salt. (1960, p. 408)

The second theory is a well-established theory in psychology and is proposed by Clark and Wilkes-Gibbs (1986).[1] According to these authors, Exchange 1 is at odds with the

principle that senders and receivers of a message are mutually responsible for getting the actual meaning of the message across. In Clark and Wilkes-Gibbs's (1986) words:

> The participants in a conversation try to establish... the mutual belief that the listeners have understood what the speaker meant in the last utterance to a criterion sufficient for current purposes. (p. 33)

We believe that these rather diverse theories lead to similar conclusions concerning Exchange 1 because they share a common underlying assumption. The assumption is that, when people are engaged in informational exchanges, they communicate so as to be reasonably sure that they perform the exchanges faithfully (the *Faithfulness Assumption*).

At first sight, the Faithfulness Assumption does not appear to deserve much scrutiny as not only is it starkly appealing to intuition but it is also consistent with obvious facts. For instance, the existence of common cultural institutions such as financial markets or courts of law would make little sense if we did not assume that people are fully capable of performing faithful informational exchanges. Of course, people can also engage in acts of communication that do not necessarily entail faithful informational exchanges (henceforth, we will refer to these acts as *non-informational interactions*). For example, they can engage in moments of phatic communion (Malinowski, 1923), that is, interactions that focus primarily on the establishment and maintenance of pleasant social relations. However, since the faithful transfer of information is not a critical element of non-informational interactions, their existence is not at odds with the Faithfulness Assumption. Non-informational interactions simply fall outside of the assumption's scope. More problematic for the Faithfulness Assumption is the fact that miscommunication is not only rather common in daily life (Mustajoki, Sherstinova, & Tuomarla, 2018; Tzanne, 2000) but it also occurs in experimentally controlled exchanges which, much as Exchange 1, seem to fall squarely within the assumption's scope (Keysar, 2007). Could this be an indication that the Faithfulness Assumption might have limitations that go beyond the scope reduction imposed by non-informational interactions? Over the years, we have found evidence consistent with this hypothesis.

We stumbled onto the first piece of evidence while studying people engaged in the task of inventing novel communication systems in the absence of pre-established ones such as speaking or writing (Galantucci, 2005; Galantucci, Fowler, & Richardson, 2003). People turned out be quite good at the task—a finding consistent with field observations (e.g., Goldin-Meadow & Feldman, 1977; Kegl, Senghas, & Coppola, 1999; Sandler, Meir, Padden, & Aronoff, 2005)—but there were remarkable cases of failure. For the most part, these cases were due to the fact that people exhibited important limitations in their basic communicative abilities (Galantucci, 2009; Galantucci & Roberts, 2012; Galantucci & Steels, 2008). For instance, some individuals understood communicative acts performed by others—and used them for hours to inform their behavior—but did not reciprocate the acts, even when it was obvious that such reciprocation was necessary to avoid critical communication problems. These individuals did not seem to be even aware of the problems. Other individuals produced identical communicative acts but intended them to

convey different meanings depending on information that was accessible only to them (cf. Keysar & Henly, 2002; Lane & Ferreira, 2008 for similar phenomena with natural language use). Predictably, the recipients of these cryptic communicative acts repeatedly failed to understand them as intended. Yet, when this happened, the individuals who performed the cryptic acts reacted with surprise and disappointment, seemingly oblivious to the fact that their own behavior caused the failures. Considering that all of the individuals described above reported having normal communicative abilities, it became apparent that the challenging task had offered a new opportunity for assessing these abilities (Galantucci & Roberts, 2012). The findings were a bit surprising. When people could not use pre-established communication systems, not only did they sometimes show important limitations in their ability to communicate effectively but they also seemed to have little awareness of these limitations. These findings forced us to reconsider our initial assumptions concerning human communicative abilities, suggesting to us the hypothesis that the Faithfulness Assumption might have important limitations. Our first step in testing this hypothesis was to rule out the possibility that the findings from which it originated were artifacts caused by the highly unusual task of inventing a novel communication system. In consequence, we began investigating human communication in conditions much closer to the ordinary ones.

The Faithfulness Assumption has two critical entailments. The first is that people should monitor the faithfulness of the exchanges they perform, detecting any problem that might compromise it. The second is that, when people become aware of any such problem, they should address it. We began our test of the Faithfulness Assumption by focusing on the first entailment, measuring the extent to which people engaged in spontaneous conversations detected the presence of communication problems (Galantucci & Roberts, 2014; Galantucci, Roberts, & Langstein, 2018; Roberts, Langstein, & Galantucci, 2016). The measure relied on a simple method that has been often used to investigate attention in psychology: People are exposed to an oddity in a seemingly mundane event (e.g., a change in interlocutor during a one-to-one conversation; Fenn et al., 2011; Simons & Levin, 1998) and then they are interviewed to determine whether they noticed the oddity. In our case, the oddity was an obvious communication problem that we introduced in spontaneous conversations.

In a first study (Galantucci & Roberts, 2014), we assigned a simple communication task to two pairs of participants who did not know of each other's existence. In particular, each pair had to converse about a cartoon by sending instant messages from different physical locations. Unbeknownst to the participants, we occasionally crossed their conversations in such a way that each member of a pair ended up conversing with a member of the other pair. In other words, participants conversed multiple times with a stranger, who was involved in a different conversation focused on a different cartoon. The crossings of the conversations lasted 30 s, occurred four times over the course of 15 min, and the instant messaging platform did not reveal any overt sign of their existence. The logic of the study was straightforward: If people have limited ability to monitor the faithfulness of the messages they exchange, they may not detect the conversational incoherencies caused by the crossings. The prediction was tested in two experiments. In the first, aimed at

investigating narrowly focused conversations, each participant in a pair was told that their partner saw the same picture, colored differently, and that their task was to find the color differences. Five of the 15 participants for whom the crossings had created obvious conversational incoherencies failed to notice them. In a second experiment, aimed at investigating more broadly focused conversations, participants were told that their task was to discuss which of the celebrities depicted in the cartoon they would most and least like to spend a day with. Three of the 11 participants for whom the crossings had created obvious conversational incoherencies failed to notice them.

A follow-up study (Roberts et al., 2016) replicated the second experiment of the first study with a tighter manipulation of conversational coherence. Again unbeknownst to the participants, we swapped two messages in their conversation for messages of our own. These swaps were designed to consistently generate obvious incoherencies. They also allowed us to manipulate the kind of incoherence encountered by the participants. In one experiment, the incoherent message concerned a celebrity who was not in the cartoon and was not mentioned before in the conversation. For example, while Oprah was not in the cartoon and was not mentioned before in the conversation, the manipulated message would read: "Of these six, Oprah is kind of an icon for people like me." Fifteen out of 40 participants failed to notice the incoherent message. In a second experiment, the incoherent message implied that its sender was of a different gender from the gender expected by the receiver. For example, if the receiver's partner was a man, the manipulated message would read: "Hillary Clinton is an icon for women like me" (Hillary Clinton was one of the celebrities depicted in the cartoon). Participants met their partners at the start of the experiment and were in no doubt as to who they would be conversing with. Yet 11 out of 33 participants failed to notice the incoherent message. In sum, people engaged in instant messaging conversations showed important limitations in detecting obvious communication problems.

Our next step was to rule out the possibility that this finding was specific to instant messaging, an artificial medium that is clearly different from the natural medium of human communication.[2] We engaged 30 participants in one-to-one spontaneous conversations with a lab confederate (Galantucci et al., 2018). The conversations occurred face-to-face and were prompted by the task of jointly ranking in order of humorousness and oddness five "*would you rather*" questions (e.g., "would you rather live on a tree or in a cave?"). After about 8 min of mundane exchanges, the confederate uttered the sentence "*colorless green ideas sleep furiously*." The sentence is not only bound to cause a communication problem in almost any imaginable conversational context but is also devoid of meaning. Yet 20 of the 30 participants—when interviewed soon after the confederate uttered the sentence—believed that no such sentence had been introduced into their conversation. Moreover, of the 10 who thought that such a sentence had been introduced, only one could correctly recognize it in a list that included 20 other nonsensical sentences. In other words, people's capability to detect communication problems might be even lower in face-to-face conversations than in instant messaging exchanges.

Taken together, the three studies provide converging evidence consistent with the conclusion that people's sensitivity to communication problems has important limitations.

These limitations are in stark contrast with the Faithfulness Assumption. However, as pointed out by Micklos, Walker, and Fay (2020), this conclusion might be undermined by two potential confounds. On the one hand, the limitations we observed might have been caused by the fact that people encountered the communication problems while they were engaged in moments of non-informational interaction. With the exception of one experiment, in fact, all of our experiments involved spontaneous conversations aimed at performing tasks with low informational demands (the exception is the first experiment of the 2014 study). During these conversations, moments of non-informational interaction were a definite possibility. On the other hand, the limitations we observed might have been caused by memory failures. In particular, it is possible that the participants of our studies noticed the communication problems when they encountered them but then, when we interviewed them after the encounters, they failed to remember that they had noticed. While we believe that these potential confounds are unlikely to explain all of the 54 cases in which participants did not report having noticed the communication problems, we agree with Micklos and colleagues that a more stringent assessment of the Faithfulness Assumption requires ruling them out.

In the current study, we ruled out the possibility of non-informational interactions by inserting communication problems in highly structured exchanges that, much as Exchange 1 above, were not only informational in nature but also led to noticeable visible consequences. As for the possibility of memory failures, we ruled them out by observing people's immediate reactions to a communication problem they encountered. However, rather than focusing on whether people noticed the problem, we focused on whether they attempted to resolve it. We made this choice for three reasons. First, because it still allowed us to test a critical entailment of the Faithfulness Assumption. As we noted above, in fact, if people noticed communication problems but did not resolve them, the faithfulness of their informational exchanges would still be in jeopardy. Second, because the choice allowed us to circumvent the intricacies of measuring *noticing*, a response which can be difficult to sharply define (Nisbett & Wilson, 1977). Detecting whether people attempt to resolve a problem is a simpler endeavor. Third, because the choice provided an opportunity to test a hypothesis originating from some unpublished findings of the face-to-face study discussed above (Galantucci et al., 2018). Although we did not design that study to assess participants' immediate reactions, we had video footage of some of the trials in which the sentence "colorless green ideas sleep furiously" was uttered. According to the Faithfulness Assumption, one would expect the footage to document participants' attempts to address the communication problem, performing what is known as an other-initiated-repair (Schegloff, 2000; henceforth *repair*, for simplicity). The footage indeed contained a few repairs (e.g., *Exchange A* in Box 1, from *Clip A* in Appendix A) but also several unexpected responses such as laughing (e.g., *Exchange B*, from *Clip B*), completely ignoring the sentence (e.g., *Exchange C*, from *Clip C*), or even assenting to it (e.g., *Exchange D*, from *Clip D*).

Why did participants not repair such a conspicuously problematic sentence? Inspection of the footage suggested that, for some of them, it was possible that they were so focused on the flow of the conversation that they did not even process the sentence. This seemed

Box 1

Exchanges between the participants and the confederate

Exchange A	Confederate: colorless green ideas sleep furiously Participant A: what?
Exchange B	Confederate: colorless green ideas sleep furiously Participant B: [laughs]... all right... so we are at
Exchange C	Confederate: colorless green ideas sleep furiously Participant C: you never played would you rather?
Exchange D	Confederate: colorless green ideas sleep furiously Participant D: yeah

to be the case for Participants C and D in Box 1, who both reported not noticing the communication problem when interviewed at the end of the conversation. These participants seemed to exhibit a form of attentional deficit (cf. Fenn et al., 2011; Simons & Levin, 1998), which we labeled *content deafness* (Galantucci et al., 2018). In the case of other participants, however, this explanation was rather implausible. This seemed to be the case for Participant B in Box 1, for example, who indeed reported noticing the communication problem when interviewed at the end of the conversation. We hypothesize that participants like Participant B avoided the repair because the communication problem they encountered was unlikely to lead to overt consequences[3] (the *Overt Consequences Hypothesis*). The participant and the confederate, in fact, were engaged in a task that they could adequately perform even if they ignored a momentary communication problem. In particular, as mentioned above, they jointly ranked in order of humorousness and oddness five "*would you rather*" questions. This open-ended task led to rather casual conversations during which moments of non-informational interaction could have made irrelevant the potential consequences of a communication problem. As we will describe below, in the current study, we tested the Overt Consequences Hypothesis through an experimental manipulation.

2. Study

A confederate instructed participants to move ordinary objects from a tray (see Fig. 1A for an example) to a specific location on a chessboard. After a few mundane instructions, the confederate asked the participants to "*pick up the skask and move it to C5*." Since "skask" is a non-word created by us, the instruction contained a communication problem that was not only likely to be noticed but which also created a clear need for a repair. The repair was indeed critical for the faithfulness of the informational exchange: If participants avoided it, they might move a different object than the one intended by the

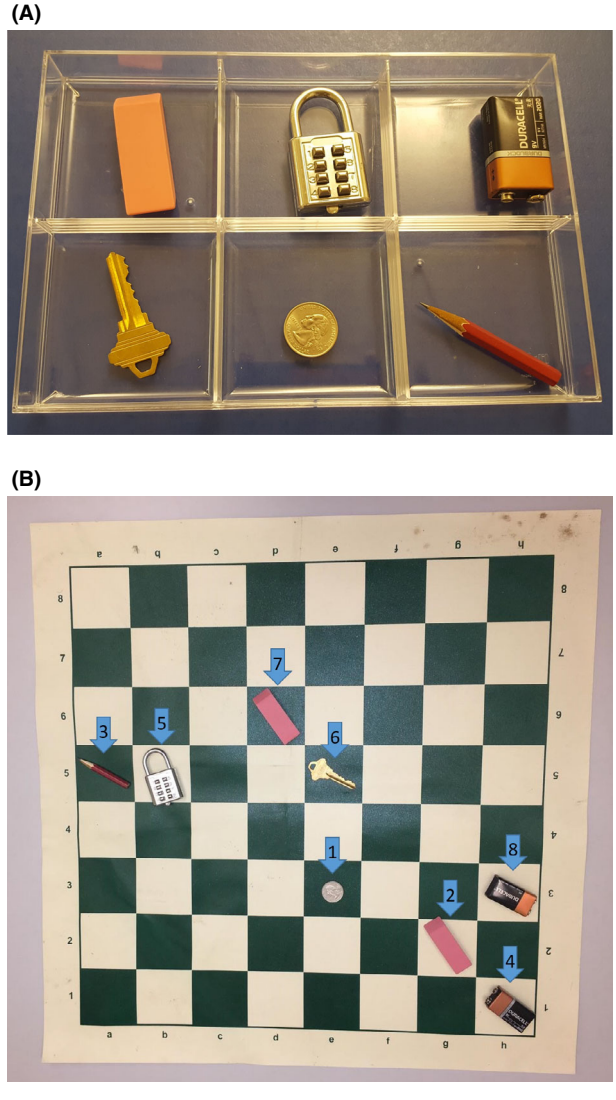

Fig. 1. (A) The tray used for the first mundane round of the game. (B) The picture illustrating where each object was to be placed and in what order the instructions were to be given in the first mundane round.

confederate. To ascertain whether participants performed a repair, we recorded their immediate reactions to the problem. A systematic inspection of these recordings provided an assessment of the Faithfulness Assumption that not only did not rely on participants' memory but it also originated from conversations that were unlikely to lead to non-informational interactions. If the hypothesis that the Faithfulness Assumption has important limitations is correct, the inspection should reveal a number of cases in which participants do not initiate a repair in reaction to the communication problem.

To test the Overt Consequences Hypothesis, we manipulated the probability that avoiding a repair could lead to overt consequences. In particular, two people—the confederate and an experimenter—had a clear view of the chessboard onto which the participant moved the objects. Furthermore, we informed participants that the session could be video recorded. In other words, if participants moved a different object than the one intended by the confederate, it was clear to them that someone was likely to notice the mistake. In the *high probability consequences* condition (henceforth *HPC* condition), four of the six objects on the tray had a well-known name while the other two did not (see Fig. 2A below). In consequence, avoiding a repair of the problematic instruction entailed a 50% probability of making a noticeable mistake. In the *low probability consequences* condition (henceforth *LPC* condition), five of the six objects on the tray had a well-known name while the sixth did not (see Fig. 2B below). In consequence, when participants were asked to "*pick up the skask and move it to C5*," they could avoid a repair while, reasoning by exclusion (cf. Golinkoff, Hirsh-Pasek, Bailey, & Wenger, 1992), they could still expect a rather low probability of making a noticeable mistake. Note that the probability is low but not null. The confederate, in fact, might have intended to refer to one of the objects with a well-known name and the instruction heard by the participant might have been the result of a communication problem. If the Overt Consequences Hypothesis is correct, participants in the HPC condition should be more likely to perform a repair than participants in the LPC condition.

3. Methods

3.1. Participants

Forty-eight English-speaking students from New York City, with no deficits in communicative ability, were recruited through online advertisements. Participation was compensated

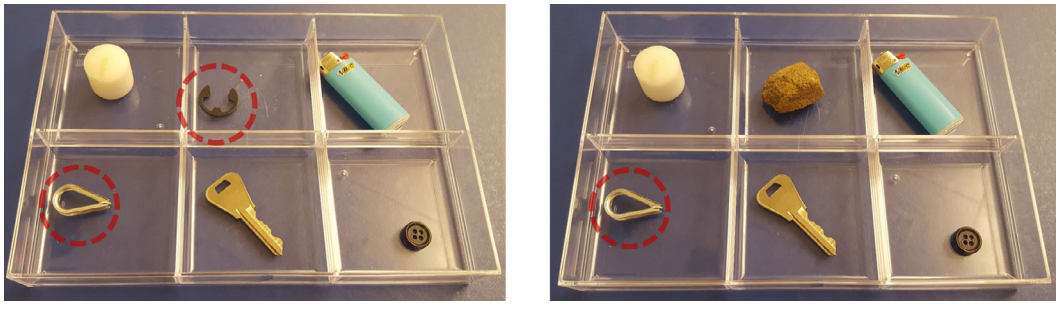

Fig. 2. (A) The six objects of the high probability consequences condition. The two objects without a well-known name are circled in red (at the hardware store where they were purchased, the object on the bottom row was sold as "wire rope thimble" while the object on the top row was sold as "retaining ring"); (B) The six objects of the low probability consequences condition. The object without a well-known name is circled in red (for half of the participants, it was the other object without a well-known name from A).

with $25. The average age of the participants was 26.73 years (SD = 8.21), and there were 15 female participants, 31 male participants, and 2 participants who preferred not to declare gender.

3.2. Ethical statement

Ethical approval was granted by the Institutional Review Board at Yeshiva University. All participants gave written consent to participate by signing a consent form. The consent form included a paragraph stating that any behavior that took place during the session could be video recorded.

3.3. Procedure

To dissimulate the goal of the study, the recruitment of the participants occurred through an ad that presented it as a study on "chess and cognition." Upon arriving at the lab, the participant and the confederate (one of us, NP) were introduced to one another as two participants who had been randomly paired up. The confederate's age (24 years) was similar to the average age of the participants and he was dressed casually. For the entire duration of the session, the confederate acted so as to minimize the likelihood that the participant would perceive his association with the lab, and the experimenter (one of us, ES) interacted with the confederate in the same manner as with the participant. For example, the experimenter asked the confederate for his name in the presence of the participant, as if they had never met before. For this reason, in the remaining part of this section we will refer to the confederate and the participant as "the participants" and make a distinction between them only when it is critical for the design of the study. After the participants reviewed and signed the consent form for the study, the experimenter escorted them into a room. He then told them that the session involved a number of tasks, without specifying how many. The first task was a cover task.

3.3.1. Cover task

The experimenter asked the participants to rank, in order of difficulty and humorousness, four "would you rather" questions taken from the same set of questions used for the 2018 study described above (Galantucci et al., 2018).

Along with the function of cover task, this task provided us with an opportunity to elicit two different interpersonal dynamics before the participants performed the critical task. On the one hand, we chose to elicit an interpersonal dynamic in which casual interactions between the participants were strongly discouraged. In particular, we instructed half of the participants (12 in the HPC condition and 12 in the LPC condition) to perform the task individually with pen and paper, while being seated so that they faced opposite directions. In other words, the participants (two strangers) had no particular reason to engage in casual interactions. In what follows, we will refer to this task as the "Formal Settings Cover Task." On the other hand, we chose to elicit an interpersonal dynamic in which casual interactions between the participants were strongly encouraged. In particular, we instructed the other half of the participants (12 in LPC condition and 12 in the

HPC condition) to perform the cover task jointly rather than individually. More in detail, similarly to the 2018 study (Galantucci et al., 2018), the experimenter instructed the participants to negotiate, through face-to-face conversations, a joint ranking of the four "would you rather" questions. As we learned in the 2018 study, this task elicits lively conversations that consist mostly of causal exchanges. In what follows, we will refer to this task as the "Informal Settings Cover Task."

While we had no clear expectations as to the effects of this manipulation, we performed it to investigate a scenario that could have affected our test of the Faithfulness Assumption. As described above, we designed the test with the intent of minimizing the likelihood of non-informational interactions. The experimental procedure we used for the Formal Settings Cover Task was optimal in this regard, but it was possible that it could overshoot its intended target. In particular, it was possible that, primed by an interpersonal dynamic in which there were essentially no spontaneous exchanges, the participants formed the belief that they were to avoid *any* spontaneous exchange, even in the following tasks. Participants in the Informal Settings Cover Task had no reason to form such belief.

For both cover tasks, the experimenter collected the rankings after 5 min had elapsed. He then asked each participant to answer in writing the following questions:

Question 1: On a scale from 0 to 10, how good of a job did you do in ranking the questions?
Question 2: On a scale from 0 to 10, how good of a time did you have with your partner on this first task?
Question 3: On a scale from 0 to 10, how good do you feel right now?

We asked these questions for two reasons. First, to further dissimulate the goal of the study, decoying participants who speculated about it into a very wide range of possibilities. The second reason was that of assessing the extent to which the manipulation of the cover task affected participants. Once participants had answered the three questions, the experimenter introduced a second task, the study's critical task.

3.3.2. Critical task

The experimenter asked the participants to sit across from each other at a round table (diameter 107 cm) and placed between them a large chess board (61 cm × 61 cm) with numbers and letters demarcating rows and columns. He told participants that they would be playing a simple game in which one of them would play the role of a *director* and the other the role of a *mover*. He then asked the participant to blindly pick from a box one of two pieces of paper to determine which role s/he would play. Unbeknownst to the participant, both pieces were labeled *mover*. In consequence, the participant always played the role of mover and the confederate always played the role of director. The experimenter told participants that, for each round of the game, the director would give instructions to the mover on how to move objects from a tray (see Fig. 1A for an example) onto the chessboard and then back into the tray. To explain how the director was to know which instructions to give in each round, the experimenter explained that he would give

the director a picture of the chessboard illustrating where each object was to be placed and in what order the instructions were to be given (Fig. 1B). After briefly showing the picture to the participants, the experimenter instructed the confederate to request a move by saying "please pick up the OBJECT and move it to COORDINATE" (e.g., "please pick up the candle and move it to C3") and, once the object had been placed on the board, to say "move it back." We adopted this procedure to maximize the likelihood that the only portions of the instructions that could lead to communication problems were the portions referring to the object and the coordinate.

Participants played four rounds of the game. The first two and the fourth rounds were mundane rounds in which the participant moved objects with well-known names (see Fig. 1 for an example). The third round contained the critical instruction "please pick up the skask and move it to C5[4]" (henceforth *critical instruction*). As described in the introduction, the 24 participants we randomly assigned to the HPC condition received the critical instruction while moving objects from the tray in Fig. 2A and the 24 participants we randomly assigned to the LPC condition received it while moving objects from the tray in Fig. 2B. In the HPC condition, the critical instructions occurred after four instructions involving each of the four objects with well-known names (e.g., "please pick up the lighter and move it to F2") and was followed by two more mundane instructions. In the LPC condition, the critical instructions occurred after five instructions involving each of the five objects with well-known names and was followed by one more mundane instruction. In other words, participants in this condition could easily infer the likely meaning of the critical instruction.[5] In consequence, while they might have still felt the need to initiate a repair to make sure they heard the instruction correctly, avoiding the repair was unlikely to lead to overt consequences. We adopted this procedure to maximize the contrast between the two conditions.

3.3.3. Questionnaire

After the completion of the critical task, the experimenter told the participant and the confederate that he would escort them to different rooms to answer a questionnaire. The questionnaire, which in reality was administered only to the participant, included two main sections.

3.3.3.1. Individual differences questionnaire: In the first section, we collected information about individual differences among the participants. This section included standard demographic information such as age and gender. It also included an inventory to measure the *Big Five* personality dimensions (BFI-2-S; Soto & John, 2017), a measure of social intelligence (the *Mind in the Eye* Test; Baron-Cohen, Wheelwright, Hill, Raste, & Plumb, 2001), and a measure of multilingualism (the *LEAP-Q* inventory; Marian, Blumenfeld & Kaushanskaya, 2007). We collected this information because we wanted to know whether it would enable us to predict the likelihood that participants performed a repair after receiving the critical instruction.

3.3.3.2. Session questionnaire: In the second section of the questionnaire, we collected information about the session. The section included the following questions:

Question A: What do you think was the purpose of the study?
Question B: Do you think that your partner was acting genuinely?
Question C: Do you remember hearing the word "skask" during the experiment?
Question D: (asked only if answer to Question C was "yes"): Do you know what "skask" means?
Question E: (asked only if answer to Question D was "yes"): What does "skask" mean?
Question F: (asked only if answer to Question D was "no"): Did you let your partner know that you didn't know the meaning of the word "skask"?
Question G: (asked only if answer to Question F was "no"): Which of the options below best accounts for why you didn't say anything when your partner used the word "skask"?

- I thought that asking for clarification would disrupt the flow of the conversation.
- I thought that it was clear enough which item my partner was referring to.
- Other (please specify)

Questions A and B provided a check of the fact that our cover procedures worked as intended, Questions C–G provided insight into participants' subjective experiences of the critical instruction.

4. Results

4.1. Experimental settings check

As revealed by their answers to Question A, none of the participants correctly guessed the purpose of the study. However, as revealed by Question B, four participants correctly guessed that the confederate was not acting genuinely. In consequence, the data from these participants were not used for the analyses that follow.

An inspection of the video recordings revealed that participants made virtually no mistakes in moving the correct objects (as well as in placing the objects in the correct locations on the chessboard). This suggests that our experimental settings worked as intended in making participants aware that their mistakes could be noticed.

An analysis of the answers to Questions 1–3 (the three questions we asked participants at the end of the cover task) revealed that the difference in cover task clearly affected participants' answers to Question 2 ("On a scale from 0 to 10, how good of a time did you have with your partner on this first task?"). In particular, the answers of the participants who performed the Formal Settings Cover Task (Mdn = 5, IQR = 1.5–7) were significantly lower than the answers of the participants who performed the Informal Settings Cover Task (Mdn = 8, IQR = 7–10), $U = 77.5$, $p < 001$, $r = .59$. As for Questions 1 and 3, the answers of the participants who performed the Formal Settings Cover Task (Question 1: Mdn = 8, IQR = 6.5–10; Question 3: Mdn = 7, IQR = 5–9) were also slightly different than the answers of the participants who performed the Informal Settings Cover

Task (Question 1: Mdn = 8, IQR = 7–10; Question 3: Mdn = 8, IQR = 7–9). These differences, however, were not significant. Before describing the impact of the difference in cover task on the occurrence of repair after the critical instruction, we will first describe the procedure we used to detect such occurrences.

4.2. Detecting the occurrence of a repair after the critical instruction

Two of us (E.S. and N.P.) independently inspected the recordings of the critical instruction to detect whether participants performed a repair. The criterion used for the detection was that participants were to be considered as not having performed a repair only if they performed the critical instruction without requesting any clarification from the confederate. In case of disagreement, the participant was to be considered as having performed a repair. There were no disagreements. The video clips for all of the participants who were considered as not having performed a repair can be viewed through the URL links in Appendix A. In addition, Clips 7 and 22 provide examples of participants who were considered as having performed a repair.

4.3. Repair occurrences for the critical instruction

For convenience, in Table 1, we provide a synopsis of the repair occurrences. As reported in the table, 15 of the 44 total participants (34.1%) performed a repair after the critical instruction and 29 (65.9%) did not (henceforth, we will refer to these participants as *repair avoiders*).[6]

4.3.1. Overt consequence hypothesis

In the HPC condition, 12 participants (54.5%) performed a repair after the critical instruction and 10 (45.5%) did not. In the LPC condition, three participants (13.6%) performed a repair after the critical instruction—confirming our intuition that even in this condition a repair was not entirely superfluous—and 19 participants (86.4%) did not. The difference in repair occurrences between the two conditions is statistically significant (p = .005, one-tailed Fisher's exact test, Cramer's V = 0.432).[7]

Table 1
Breakdown of repair occurrences

		LPC	HPC	Total
Low formality cover task	Repair	1 (9.1%)	4 (40%)	5 (23.8%)
	No repair	10 (90.9%)	6 (60%)	16 (76.2%)
	Total	11	10	21
High formality cover task	Repair	2 (18.2%)	8 (66.7%)	10 (43.5%)
	No repair	9 (81.8%)	4 (33.3%)	13 (56.5%)
	Total	11	12	23
Across cover tasks	Repair	3 (13.6%)	12 (54.5%)	15 (34.1%)
	No repair	19 (86.4%)	10 (45.5%)	29 (65.9%)
	Total	22	22	44

HPC, high probability consequences; LPC, low probability consequences.

Is it possible that, as suggested by our previous studies, the 10 repair-avoiders in the HPC condition did not even detect the obvious communication problem? This possibility seems to be inconsistent with the fact that none of the seven repair-avoiders in the HPC who were asked Questions G answered it by choosing the option "*I thought that it was clear enough which item my partner was referring to*."[8] In any event, to more soundly rule out the possibility of a failure in detecting the communication problem, we measured the time elapsed between the moment an instruction began (the utterance of the word "Please") and the moment in which the participant touched an object in the tray (the *Pick-up Time*).[9] If the repair-avoiders did realize that they had encountered a communication problem, then the Pick-up Times for the critical instruction should be longer than for the instructions that immediately preceded it. Fig. 3B illustrates the Pick-up Times for the critical instruction and the three preceding instructions. As is apparent in the figure, the Pick-up Times for the critical instruction were markedly longer than the Pick-up Times for each of the three preceding instructions. Indeed, the difference was statistically significant for each of the three pairwise comparisons (all paired-samples *t* tests yielded *p*s < .05, with all Cohen's *d*s > 1). For comparison, Fig. 3A illustrates the same measurements for 18 of the repairer avoiders in the LPC condition.[9] The measurements suggest much less marked differences in Pick-up Times. Indeed, the difference between the

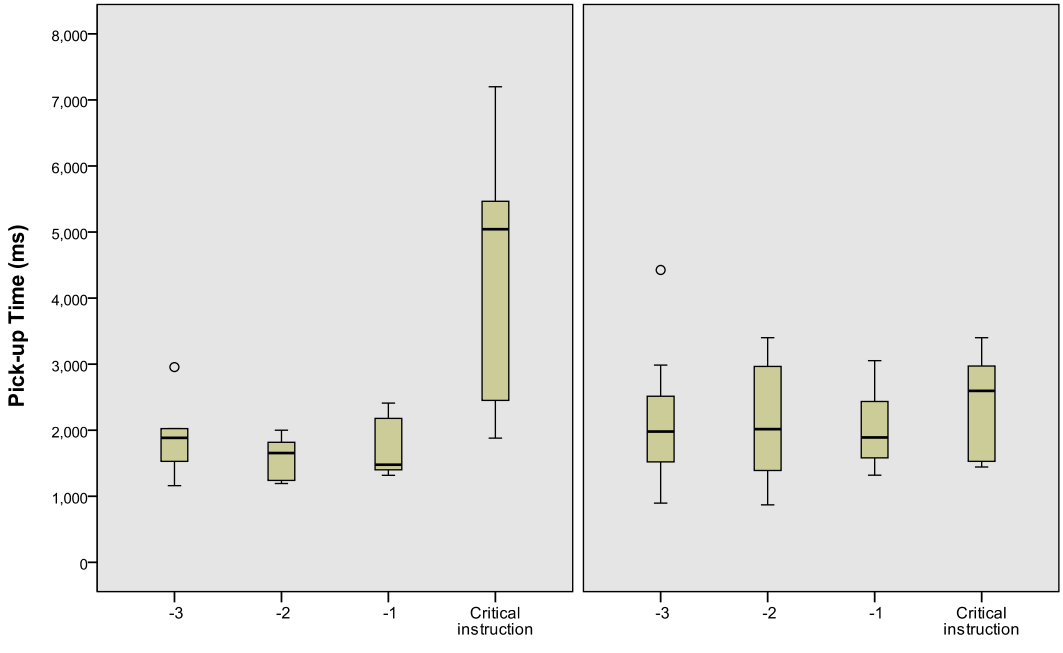

Fig. 3. (A) Boxplots for the Pick-up Times of the critical instruction and the three preceding instructions in the low probability consequences condition. (B) Boxplots for the Pick-up Times of the critical instruction and the three preceding instructions in the high probability consequences condition. (The boxes represent the interquartile ranges, the error bars the ranges, the horizontal line the medians, the circles the outliers, and the asterisks the extremes.)

Pick-up Times for the critical instruction and the preceding one was no longer significant, $t(17) = 0.18$; Cohen's $d = 0.04$, while the other two differences remained significant (both paired samples t tests yielded ps $< .05$) but were smaller in size (both Cohen's ds $< .6$). This contrast highlights the struggle faced by the repair-avoiders in the HPC condition. A majority of these 10 participants were clearly aware of the struggle. In answer to Questions C and D, in fact, six of them reported remembering hearing the word "skask" and not knowing what it meant. Sometimes, as in the case of Participant 2, there was little for us to infer, as the struggle was already evident in his overt behavior: He picked up both objects without a well-known name, smiled, and then moved one of the objects (see Clip 2). The four remaining repair-avoiders in the HPC condition reported not remembering hearing the word "skask." Considering that also their mean Pick-up Times were longer for the critical instruction than for each of the three preceding instructions, this finding is consistent with the possibility that memory failures might have contributed to the findings of our previous tests of the Faithfulness Assumption (Galantucci & Roberts, 2014; Galantucci et al., 2018; Roberts et al., 2016). This possibility, however, seems to have a limited explanatory scope because, in answer to Question C, 35 of the 44 participants reported remembering hearing the word "skask."

Intriguingly, in Answer to Question D, two of these participants reported knowing what the word "skask" means. One of them was in the HPC and performed a repair; the other was in the LPC and did not perform a repair.[10] We interpret this as the participants reporting that they *learned* what the word "skask" means during the course of the experiment.

4.3.2. Formality of settings

Five of the participants who performed the Formal Settings Cover Task (23.8%) performed a repair after the critical instruction and 16 (76.2%) did not. Ten of the participants who performed the Informal Settings Cover Task (43.5%) performed a repair after the critical instruction and 13 participants (56.5%) did not. The difference in repair occurrences between the two cover tasks is not statistically significant ($p = .213$, two-tailed Fisher's exact test, Cramer's $V = 0.207$).[11]

4.3.3. Individual differences

A binary logistic regression revealed that none of the individual differences we collected could reliably predict the occurrence of repair [all ps $> .12$; omnibus test of full model: $\chi^2(9, N = 44) = 3.8$, $p = .91$].[12]

5. Discussion and conclusions

The current study provided a clear result: Not only did participants frequently avoid repair after encountering a non-word that they had never heard before but also 10 of them did so even when, as in the HPC condition, this was likely to jeopardize the faithfulness of an informational exchange. Considering that there were no non-informational interactions during the critical task, the behavior of these 10 participants represents the starkest

violation the Faithfulness Assumption we have ever documented: People engaged in informational interactions may frequently forego the repair of a critical communication problem.

The current study also offered two insights concerning the reasons why people might avoid clearly needed repairs when engaged in informational exchanges. On the one hand, we found positive evidence in favor of the Overt Consequences Hypothesis: People are more likely to avoid repair when the avoidance is less likely to lead to overt consequences. This finding highlights the fact that people were sensitive to the potential costs implied in the avoidance. In other words, repair avoidance can be modulated by certain contextual factors such as the likelihood of incurring costs. On the other hand, our results suggest that the effects on repair avoidance of other contextual factors such as interpersonal dynamics and individual differences, if existent at all, are unlikely to be comparable in size to the effect of overt consequences. Furthermore, the relative small size of the effect of the interpersonal dynamics on repair avoidance suggests that the results of our study are relatively independent from the specific conditions we created through the manipulation embedded in the cover task. As discussed above, these conditions were aimed at ascertaining whether our test of the Faithfulness Assumption—which by design was focused on tightly controlled informational exchanges—was affected by the extent to which the experimental settings discouraged spontaneous exchanges.

Perhaps larger effects on repair avoidance could be found in the future by considering other hypotheses. For example, people might avoid repairs because they expect relevance from their interlocutors (Sperber & Wilson, 1986) and patiently wait for a deferred solution of the communication problems they encounter. Participant 2, a repair-avoider in the HPC condition, provides a good example of this possibility. In answer to Question G, in fact, he said that he avoided repair because "I figured a guess was probably good enough and that, given that he had an image of the board, he'd correct me if I picked wrongly."

Also, perhaps out of concerns for face management (Goffman, 1955), people may be reluctant to interrupt the flow of an interaction to let a stranger know that either he said something problematic or they do not know a word of their native language. Indeed, in answer to Question G, three repair-avoiders in the HPC chose the option "*I thought that asking for clarification would disrupt the flow of the conversation*." This explanation seems particularly compelling for three repair avoiders in the HPC condition who nonetheless spontaneously initiated repairs during the mundane rounds of the critical task.[13] For example Participant 6, when asked to "pick up the skask," picked up one of the two objects without a well-known name, without saying a word (see Clip 6). Yet, she initiated a repair a few rounds after the critical instruction, aimed at clarifying the correct location on the chessboard where to place the object she was picking up from the tray (see Clip 6a). In other words, while Participant 6 felt comfortable asking clarifications concerning instructions that she did not fully understand, she did not feel comfortable addressing the odd situation caused by the Critical Instruction. This occurred despite the fact that, as demonstrated by the repair she performed after the critical instruction, she seemed to care about performing the task accurately.

Finally, another possibility is that, as suggested by our previous studies (including those in which people invented new communication systems), the very ability to detect communication problems might be limited. As we noted above, this possibility cannot explain the results of the current study, but it may explain other occurrences of repair avoidance such as some of the ones we documented when discussing the footage of the 2018 study (Galantucci et al., 2018). In other words, as pointed out by Healey, De Ruiter, and Mills (2018), the study of miscommunication is a promising avenue for future research.

Before we discuss the general relevance of the line of research from which the current study originates, we note that this study serendipitously acquired specific relevance in light of a recent independent test of the Faithfulness Assumption (Micklos et al., 2020). To perform the test, Micklos and colleagues assigned a well-known referential communication task to a pair of participants. One participant (*the director*) communicated to the other (*the matcher*) which image to pick from a set of images (Clark & Wilkes-Gibbs, 1986; Krauss & Weinheimer, 1964). Since the images had no pre-established names, participants had to perform a number of accurate communicative exchanges to identify the correct image. These exchanges were performed through instant messaging conversations. Micklos and colleagues found that the matchers' mean confidence in having succeeded at the referential communication task was higher for successful trials than for unsuccessful ones. Furthermore, an analysis of the instant messaging conversations revealed that, when matchers signaled communication problems through repairs, the pair was less successful at the task and the directors were correspondingly less confident. From these results, Micklos and colleagues concluded that "interlocutors exhibit (a degree of) sensitivity to problems in communication, and use repairs to signal and address them" (Micklos et al., 2020, p. 14). We agree with the conclusion, which is consistent with cross-linguistic observations (Dingemanse et al., 2015). However, we believe that a thoroughgoing assessment of the Faithfulness Assumption entails two questions that are more stringent: Do people notice *every* communication problem they encounter and, when they do, do they *systematically* initiate a repair? Considering that in the current study we systematically observed whether people repaired a communication problem, the study provides an ideal opportunity to answer the second question. The answer is negative: People do not repair all of the communication problems they encounter.

As for the general relevance of the line of research from which the current study originates, we begin by noting that the existence of informational exchanges that violate the Faithfulness Assumption is consistent with two well-known facts.

First, it is consistent with the fact that miscommunication is rather common in daily life (Mustajoki et al., 2018; Tzanne, 2000). If people do not systematically repair the communication problems they encounter, or not even detect them, we should indeed expect miscommunication to be common. We note that this claim does not mean that people do not routinely perform faithful informational exchanges. If that were the case, in fact, we should expect communicational chaos, which is an expectation clearly inconsistent with the many achievements of human society. The claim simply means that sometimes people may not be fully aware of the requirements they need to meet to

perform faithful exchanges, or may find them challenging. When the latter scenario happens, people may struggle to meet the requirements, or simply ignore them. Also, it is possible that non-informational interactions, which clearly fall outside of the Faithfulness Assumption's scope, are not only pervasive in ordinary circumstances but also tightly interwoven with informational exchanges. If this were true, it would imply substantial limitations to the scope of the Faithfulness Assumption.

This brings us to the second fact consistent with the existence of informational exchanges that violate the Faithfulness Assumption. When the faithfulness of the exchanges is of the utmost importance, people typically enforce the use of strict communication protocols. For example, this is the case for exchanges performed by armed personnel or by airplane pilots and control tower operators. If people were naturally predisposed to consistently meet the requirements of the Faithfulness Assumption, enforcing the use of strict protocols would be unnecessary. While these protocols likely serve a number of functions, we believe that a critical one among them is to counteract people's natural tendency to perform unfaithful exchanges. Unfortunately, even in the presence of such protocols, many fatal accidents have been caused by miscommunications (Cushing, 1994). The fact that miscommunications can occur in contexts in which the cost is literally a matter of life and death highlights the fact that the cost of failure alone, although it may significantly affect the effectiveness of communication, it is not sufficient to fully ensure faithful communication.

The existence of informational exchanges that violate the Faithfulness Assumption is also consistent with the broad scientific claim that human cognition is designed to produce satisficing outcomes, not optimal ones (Simon, 1956). While this claim has been traditionally connected to the widespread existence of cognitive biases (Tversky & Kahneman, 1974), recent research has documented the existence of similar communication biases (Keysar & Henly, 2002; Keysar, Lin, & Barr, 2003; Lane & Ferreira, 2008; Savitsky, Keysar, Epley, Carter, & Swanson, 2011). These biases suggest the hypothesis that human communication too might be designed to produce satisficing outcomes, not optimal ones (Ferreira & Patson, 2007). We believe that our research on repair avoidance provides further evidence supporting this hypothesis. Because of methodological constraints, the evidence provided by the current study originates from strictly controlled conversational contexts. However, our previous studies suggest that converging evidence can be found in more natural conversational contexts (Galantucci & Roberts, 2014; Galantucci et al., 2018; Roberts et al., 2016).

In conclusion, we would like to briefly speculate on the possibility that the research we presented here may offer new insights concerning the evolution of human communication. In particular, our studies suggest the hypothesis that human communication may have first evolved with low levels of faithfulness. Exchanges with low levels of faithfulness, in fact, are less taxing for our communicative abilities but can still provide enough benefits to be evolutionarily advantageous. The overall level of faithfulness is likely to have increased over time, particularly since the advent of writing, and this would be consistent with the intuitive appeal of the Faithfulness Assumption. However, when the human communicative abilities are assessed under proper conditions, we might still

observe today the occurrence of exchanges that originate from the vestiges of a distant past. We believe that the hypothesis that human communication first evolved with low levels of faithfulness is complementary to the hypothesis that human communication first evolved as a form of "vocal grooming" (Dunbar, 1998) and hope that future research on people's present communicative abilities will explore this potential connection.

Authors' contributions

B. Galantucci developed the study concept. All authors contributed to the study design. E. Spivack and N. Paley collected and processed the data, under the supervision of B. Galantucci and B. Langstein. B. Galantucci performed the data analysis and interpretation. B. Galantucci drafted the manuscript. All authors approved the final version of the manuscript for submission. We thank for helpful comments Paola Castelli, Mark Dingemanse, Samuel Dratch, Nicolas Fay, Dov Fink, Carol Fowler, Uri Hasson, Simon Italiaander, Anthony Koutsoftas, Ariel Malka, Yosef Penner, Gareth Roberts, Gina Siddu Pilia, and two anonymous reviewers. Finally, we thank all the students who helped us during various stages of the project.

Open Research badges

This article has earned Open Data and Open Materials badges. Data and materials are available at https://osf.io/3x9c6/.

Notes

1. Note that this theory has deep scientific roots as it is meant to reconcile the idealized cooperation principle that the philosopher Grice (1975) proposed as a requirement for felicitous communication and the intricate system of conversational routines documented by sociologists (e.g., Sacks, Schegloff, & Jefferson, 1974; Schegloff, Jefferson, & Sacks, 1977).
2. For example, instant messaging lacks two critical features of face-to-face conversation: the copresence of the conversants in the same physical space and their mutual visibility (Clark & Brennan, 1991).
3. We use this term to refer to consequences that are not limited to the internal mental states of the participant. For example, if two interlocutors miscommunicated the location of an appointment, there will be the overt consequence of failing to find each other as well as other non-overt consequences such as the inferences each interlocutor may make about what caused the failure.

4. The confederate was instructed to utter the word "skask" with the same intonation he used for the words of the objects in the previous instructions.
5. Note that also the in first, second, and fourth rounds, it was always the case that each of the six objects was named once over the course of the first six instructions.
6. Incidentally, participants had rather accurate memories of these repair occurrences. In answer to Question F, in fact, all participants who did not perform a repair reported that correctly and only one participant who performed a repair incorrectly reported that he did not perform it.
7. The difference remains significant if the four excluded participants are included ($p = .003$, one-tailed Fisher's exact test, Cramer's $V = 0.43$).
8. By contrast, this response was chosen by eight of the repair-avoiders in the LPC.
9. The measurement could not be performed for one participant because of a technical failure during the recordings.
10. Incidentally, in answer to Question E, the former participant said that the word "skask" means "an object" and the latter that it means "C-shaped object."
11. The difference remains nonsignificant if the four excluded participants are included ($p = .371$, two-tailed Fisher's exact test, Cramer's $V = 0.172$).
12. The result is similar if the four excluded participants are included (all ps > .25; omnibus test of full model: χ^2 [9, $N = 48$] = 3.77, $p = .93$).
13. Note that these participants provide further confirmation that repair avoidance can occur even when people feel completely free to engage in spontaneous exchanges.
14. To protect the privacy of the participants, their faces have been blurred in these clips. In a few cases, such as the case of Participant 2 in Clip2, the b luring prevents readers from viewing facial expressions that are relevant for the points we make in the paper. Readers interested in viewing the unedited versions of the videos can contact BG at bruno.galantucci@gmail.com

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Appendix A: URL links for viewing the clips (permanent records of the clips are also available at DOI 10.17605/OSF.IO/3X9C6)

Clips A–D (footage from the study reported in Galantucci et al., 2018)

Clip A: Participant A
Clip B: Participant B
Clip C: Participant C
Clip D: Participant D

Clips 1–31 (footage from the current study)

Clip 1 (HPC condition): Participant 1

Clip 2 (HPC condition): Participant 2 (Because NP could not attend this session, there was a different confederate. It was again on of us, BL)
Clip 3 (HPC condition): Participant 3
Clip 4 (HPC condition): Participant 4
Clip 5 (HPC condition): Participant 5
Clip 6 (HPC condition): Participant 6
Clip 6a (HPC condition): Participant 6
Clip 7 (HPC condition): Participant 7—Example of repair
Clip 8 (LPC condition): Participant 8
Clip 9 (LPC condition): Participant 9
Clip 10 (LPC condition): Participant 10 (Because of a mistake, the confederate used a different coordinate for the instruction)
Clip 11 (LPC condition): Participant 11
Clip 12 (LPC condition): Participant 12
Clip 13 (LPC condition): Participant 13
Clip 14 (LPC condition): Participant 14
Clip 15 (LPC condition): Participant 15
Clip 16 (LPC condition): Participant 16
Clip 17 (LPC condition): Participant 17
Clip 18 (HPC condition): Participant 18
Clip 19 (HPC condition): Participant 19
Clip 20 (HPC condition): Participant 20
Clip 21 (HPC condition): Participant 21
Clip 22 (HPC condition): Participant 22—Example of repair
Clip 23 (LPC condition): Participant 23
Clip 24 (LPC condition): Participant 24
Clip 25 (LPC condition): Participant 25
Clip 26 (LPC condition): Participant 26
Clip 27 (LPC condition): Participant 27
Clip 28 (LPC condition): Participant 28
Clip 29(LPC condition): Participant 29
Clip 30 (LPC condition): Participant 30
Clip 31(LPC condition): Participant 31