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The Effect of Communication Channels on Promise-Making and Promise-Keeping: Experimental Evidence

Julian Conrads* Tommaso Reggiani†

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Abstract

In modern organizations, new communication channels are reshaping the way in which people get in touch, interact and cooperate. This paper, adopting an experimental economics framework, investigates the effect of different communication channels on promise-making and promise-keeping in an organizational context. Inspired by Ellingsen and Johannesson (2004), five experimental treatments differ with respect to the communication channel employed to solicit a promise of cooperation, i.e., face-to-face, phone call, chat room, and two different sorts of computer-mediated communication. The more direct and synchronous (face-to-face, phone, chat room) the interpersonal interaction is, the higher the propensity of an agent to make a promise. Treatment effects, however, vanish if we then look at the actual promise-keeping rates across treatments, as more indirect channels (computer-mediated) do not perform statistically worse than the direct and synchronous channels.

*University of Cologne, Cologne (Germany). Email:julian.conrads@uni-koeln.de.
†LUMSA University, Rome - campus Palermo - (Italy); IZA, Bonn (Germany). Email:t.reggiani@lumsa.it (corresponding author).

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1 Introduction

In modern organizations, new communication channels are reshaping the way in which people get in touch, interact and cooperate. Emails, online polls, chat rooms, and conference calls are replacing face-to-face interactions in many situations. At the same time, non-binding and spontaneous cooperation between members of the staff has become a key factor in dealing with the increasing complexity in modern organizations (The Economist 2016a,b, Walther 2011). In addition to that, given the increasing size of corporations (e.g., in multinational companies), employees sometimes receive spontaneous requests from colleagues who are complete strangers, possibly sitting in an opposite corner of the world. Ellingsen and Johannesson (2004) found that just a simple indirect and anonymous written communication helps to reinforce promise-keeping in a stylized *hold-up problem* (gain-from-trade set-up). However, in their conclusions they asked *"How would (promise) behavior be affected if interactions were oral and face-to-face rather than written and anonymous?"* (pp. 417-418). Our study addresses this specific question raised by Ellingsen and Johannesson (2004)[1] and looks at its organizational implications.

The effects of different channels of communication have already been analyzed in other fields of social interactions. Harbring (2006), for example, examines the impact of unilateral communication (announcements) vs. bilateral communication (exchange of emails) in promoting agents' performance. They find that bilateral communication increases the performance of the agents both under tournament- and team-based incentive schemes. Conversely, when the unilateral communication technology is put in place, detrimental effects in terms of cheating behavior arise only under the tournament-based incentive scheme.

Brosig, Weimann, and Ockenfels (2003) focus on the effects of different communication channels on cooperation in several standard public good games. The authors vary the communication channel applied in pre-play communication, e.g., auditory or visual channels, either bidirectional or unidirectional. They find that bidirectional face-to-face communication is crucial for enhancing cooperation.

Bicchieri and Lev-On (2007) and Bochet, Page, and Putterman (2006) also find that face-to-face communication increases cooperation in social dilemmas, but also that written communication through a chat room that preserves anonymity and excludes facial/verbal expression is almost as efficient.[2]

Valley, Moag, and Bazerman (1998) study a bilateral negotiation game with asym-

[1] The same paper has informed the most recent experimental research on promises, such as Charness and Dufwenberg (2006), Vanberg (2008), and more recently Ma, Meng, and Shen (2015).

[2] Balliet (2010) provides a meta study on the effects of communication on social dilemmas. The author finds large positive effects of communication on cooperation, which is moderated by the type of communication, i.e., face-to-face communication has a stronger effect compared to written messages.

metric information, finding different degrees of trust, truth-telling and efficiency across communication channels. Higher levels of truth-telling allow subjects negotiating face-to-face to achieve higher joint benefits than those negotiating by telephone or in writing. In other studies, fixed- or free-form communication in trust games is analyzed, either in face-to-face or in written form. The studies find that pre-play communication increases trust and trustworthiness compared to no communication (Roth 1995, Charness and Dufwenberg 2010). In this vein, Ben-Ner, Putterman, and Ren (2011) find that verbal communication in a chat room significantly increases trusting and trustworthiness compared to no or other fixed-forms of communication. Hoffman, McCabe, and Smith (1996) and Bohnet and Frey (1999) assume that decreasing social distance increases pro-social behavior in dictator games. The latter authors argue that identification of the "other" causes more prosociality (see also Charness and Gneezy 2008, Gaechter and Fehr 1999).

We investigate the effects of alternative communication channels on promise-making and promise-keeping. While Ellingsen and Johannesson (2004) analyze promises in a very abstract environment, we opt for a less stylized set-up that better resembles an organizational context where a broken promise can be a source of direct costs as well as delays or frictions in the organizational flow.

Our controlled experiment employs a simple promise-making/promise-keeping task, in which subjects are asked about their willingness to voluntarily commit to taking part in a short online survey for scientific purposes within the next 24 hours, without monetary compensation.

A baseline face-to-face interaction is compared to a phone call, a chat room-based interaction[3] a computer-mediated interaction "office", and a further computer-mediated interaction "remote", i.e., online.[4]

Under face-to-face, phone call, and chat-based conditions - which are distinguished by a synchronous interaction between the parties - promise-making rates prove to be significantly higher than under the two non-synchronous and computer-mediated conditions. Despite these differences in promise-making, no significant differences in promise-keeping rates are observed across treatments.

The paper is organized as follows: Section 2 introduces the experimental design; testable hypotheses are derived in section 3, in light of the technical features of the different communication channels; results are presented in section 4, and final considerations are found in section 5.

[3]Chat room-based communication tools are increasingly diffusing into large business organization (e.g., SLACK or CHATTER; The Economist 2016a, The Economist 2016b). We thank an anonymous referee for having directed our attention to this emerging phenomenon.

[4]This treatment reproduces either a freelance working relationship or interaction with a colleague within the firm who is located in an overseas office.

2 Experimental Design

The experiment employed a simple task such that both promise-making and promise-keeping could be tracked and matched. Neither fixed nor contingent incentives were at stake since we are interested in studying promise-making and promise-keeping not in a "contractual"[5] fashion but in a more genuine "helping"[6] setting, as it captures the essence of a genuine promise. From Latin *pro-mittere*(*pro*: for you/for your benefit; *mittere*: to give a present), a genuine promise is by nature free from any formal obligation, enforcement or exchange in return. It is a pure commitment to help. A promise entailing something in return does not represent a proper promise but instead a non-binding agreement of mutual exchange.

The experiment took place on the day immediately following another experimental task by Conrads and Lotz (2015), which focused on the effects of different communication channels on lying behavior. In this experiment, subjects were asked to privately flip a coin four times and report the outcome via different communication channels (i.e., face-to-face, phone, written chat, and two types of computer mediated communication). The number of reported "heads" increased subjects' payoffs, thus offering subjects the chance to increase their payoffs by over-reporting the true outcomes of the coin flips. We operationalized this incentivized study and added our non-incentivized promise task in the same sessions, stressing to participants that the two activities were independent of one another. The promise task was not announced in advance. In the data analysis, we control for subjects' behavior in the incentivized task, which was conducted beforehand, and we do not observe any significant pattern, either statistically or in term of size. See below for further details.

Subjects were asked for their willingness to promise to participate in a short online survey for scientific purposes within the next 24 hours (starting every subsequent hour).[7] In case a subject agreed take part in the survey, he/she received the information concerning the URL link needed to access to the online survey (see Script A.1 in the Appendix). Subjects then had a one-day window to fill in the questionnaire.

Thus, two variables of interest are at hand, i.e., first, whether or not a subject promises to take part in the online survey and, second, whether or not the promise is actually kept (see Figure 1 for the decision tree). We conducted this experiment directly after the independent experimental task mentioned above for two main reasons: (I) the task at hand was rather simple and quick, and (II) by design we wanted to exclude any ancillary incentives, except for the spontaneous propensity of the subjects to promise to cooperate unconditionally.

[5] E.g. Ellingsen and Johannesson (2004),Charness and Dufwenberg (2006), Vanberg (2008).

[6] See Branas Garza (2007) and Berger, Herbertz, and Sliwka (2011) on helping behavior at the workplace.

[7] The topic of the survey was about the individual perception of different NGOs in terms of trust and reputation. The content of the survey was not announced during the promise-making phase.

..::: Figure 1: about here :::...

Implementing a standard between-subjects design, we exogenously varied among the different treatments the communication channel used to approach the potential volunteers asking for their help.

In the first treatment - *Face-to-Face* (henceforth: *F-t-F*) - subjects were approached in person by the same research assistant in their laboratory cubicles and directly asked for cooperation in participation in the online survey. Subjects then had to report face-to-face to the research assistant about their positive or negative decision.

In the second treatment - *Phone* - the very same research assistant approached the subjects through a call. Each subject was equipped with a headset and headphone for this purpose. Using SKYPE Call (with both the chat room and the video conference functions turned off), the research assistant called subjects and asked whether they were willing to participate in the online survey. Subjects had to report via phone about their positive or negative decision.

In the third treatment - *Chat* - the research assistant approached the subjects through a chat room. Using SKYPE Chat (with both the phone and video conference function turned off), subjects were sent a chat message and asked whether they were willing to participate in the online survey. Subjects had to respond via the chat room about their positive or negative decision.

In the fourth treatment - *PC-Lab* - no direct verbal communication channel was adopted. Subjects in this treatment were asked to state their willingness to promise to participate in the online survey via the displays of their PCs in the lab.

In the fifth treatment - *PC-Online* - subjects participated in an online experiment outside the lab. As in the *PC-Lab* treatment, subjects were asked to indicate their willingness to promise to participate in the survey via the displays of their PCs but, different from *PC-Lab*, they never are present in person in the lab. This took place following the online treatment in Conrads and Lotz (2015). -

A total of 302 subjects (with a mean age of 24 and 51% being female) participated in the experiment. The treatments *F-t-F*, *Phone* and *PC-Lab* were conducted at the "elfe" laboratory of the University of Duisburg-Essen (5th, 6th, 7thNovember 2013; 19th May 2016). The treatment *PC-Online* (29th November 2013) was entirely conducted online [8] (see Table A.4 in the Appendix for details on the sequence of the sessions). Subjects were recruited from a large pool of over 2,000 students of the University of Duisburg-Essen via ORSEE (Greiner, 2015).

In treatments *F-t-F*, *Phone*, *Chat* and *PC-Lab*, 60 subjects participated in each treatment, and 62 subjects took part in the *PC-Online* treatment. The experiment was pro-

[8] Like in the *PC-Lab* treatment subjects (i) first engage in the unrelated task, (ii) the outcome/payment is communicated, and finally (iii) the promise-making task is administrated.

grammed using the BoXS software (Seithe, 2010). Each laboratory session (*F-t-F*, *Phone, Chat* and *PC-Lab)* involved 12 or 15 participants. Approximately 30% of the participants were economics or business administration majors; the other 70% were enrolled in different fields, such as law and natural sciences. Participants in lab sessions were randomly allocated to fully-private and soundproof cubicles.[9] The content of communication was held constant in all of the different treatments, i.e., the same script was employed independent of verbal (*F-t-F* and *Phone*) or non-verbal communication (*Chat, PC-Lab* and *PC-Online*). In treatments with non-verbal communication, the identical text was applied in written form on computer screens. The wording adopted in all four treatments was the following:

[10]

> *"Independent of the previous experiment there is a five-minute online survey that you can fill in at home. You can participate in the scientific survey within the next 24 hours. The count-down will start in 1 hour. Participation is voluntary and will not be paid. If you promise to participate, before leaving the laboratory you will receive a voucher with a link to get access to the survey. Do you promise to participate in the online survey?"*

In the treatments with verbal communication *F-t-F* and *Phone* the same research assistant always communicated with the subjects.[11] In case a subject agreed to take part in the online survey, a paper-based voucher with an individualized URL link to access to the survey platform was handed to the subjects before they left the lab sessions (*F-t-F*, *Phone*, *Chat, PC-Lab*).

For the *PC-Online* treatment, subjects were first approached via ORSEE to register for the online experiment by Conrads and Lotz (2015). They then received an identical mask to the one administrated under the *PC-Lab* condition in order to ask for their promise. Finally, after the online session, subjects who made the promise received the voucher via email, [12] including the individualized URL link for accessing the survey

[9]This is a key technical feature of the "elfe" laboratory of the University of Duisburg-Essen. For this reason this venue is well suited to host studies focusing on communication channels.

[10]In treatment *PC-Online,* subjects were informed that they would receive an email containing an URL link to get to the survey platform (see Script. A.1 in the Appendix). In this case, the following wording was adopted *"[...] If you promise to participate, within five minutes you will receive a voucher via email with the link to access to the survey".*

[11]Note that the same female research assistant (24 years old) was intentionally chosen to communicate with the subjects because the experimenters, males and above the age of a typical university student, might bias subjects' actions due to obedience or authority concerns; see Karakostas and Zizzo (2014). During the experimental sessions the subjects did not encounter any other research assistants or experimenters. In order to control for gender-related effects (Solnick 1999, Eckel and Grossman 2001), in each treatment almost 1/2 of the subjects were males and 1/2 were females (see Table A.2 in the Appendix).

[12]The sender was the academic association HEIRS - Happiness, Economics and Interpersonal Relations; University of Milan-Bicocca < `http://www.heirs.it/` > <

platform (see Script A.1 in the Appendix). The format of the voucher was constant across treatments.[13] In addition, on the voucher it was clearly stated that subjects could fill in the online questionnaire within the next 24 hours.[14] Due to the individualized links, it could be tracked whether subjects actually kept the promise of filling in the online survey or not.

3 Features of Communication Channels and Behavioral Hypotheses

In this section, the specific features of the applied communication channels will be elaborated on. After this, behavioral hypotheses will be derived, which can be scrutinized by looking at the findings of the four experimental treatments.

Several technical features of the applied communication channels might be important for promise-making and -keeping. Table 1 summarizes the taxonomy of the different communication channels with respect to the specific characteristics.

Social distance arguments regarding communication channels have been the subject of many studies analyzing pro-social behavior. Hoffman, McCabe, and Smith (1996) and Bohnet and Frey (1999) assume that decreasing social distance increases pro-social behavior, e.g., making and keeping a promise. Three key aspects of social closeness prominently discussed in the literature may explain this increase in pro-sociality: identification, anonymity, and social norm activation. Bohnet and Frey (1999) argue that the increase in pro-sociality is due to higher degrees of *identification* in socially close interactions. Gaechter and Fehr (1999) apply the converse argument and define social distance as the degree of *anonymity* within a social interaction. Socially distant interactions are characterized by a higher degree of anonymity, which may lead to less pro-social behavior.

`info@heirs.it` >. This scientific society has no connection with the University of Duisburg-Essen. The subject of the email was: "online survey access link."

[13] In order to minimize subjects' transaction costs, we provided shortened URLs (e.g., `http://goo.gl/s3aCrd`) that are quick and easy to type in all different internet browsers. This is also to keep the setting as constant as possible compared to the *PC-Online* treatment, where subjects received an already active URL link in the body of the emailed voucher, and they just needed to click on it in order to get access to the survey (see Script A.1 in the Appendix). Table A.3 in the Appendix reports how the promptness in fulfilling the promise was not different under the alternative experimental conditions. In all the treatments, subjects took on average 6 hours and 30 minutes before filling the survey. This result provides evidence about the fact that receiving an already "active link" (*PC-Online* treatment) does not represent and advantage in terms of promise-keeping.

[14] We monitored the activity on the online survey platform both before and after the provided 24 hours time window. No one visited the survey platform before the actual start (1 hour buffer time after promise-making). The survey platform was monitored during the subsequent two days, no one filled in the survey after the deadline.

Moreover, Bicchieri and Lev-On (2007) and Schram and Charness (2015) argue that the *activation of social norms* is influenced by the social distance of an interaction. In anonymous interactions, only intrinsic moral norms to behave pro-socially are active, whereas in closer interactions both intrinsic moral norms and social norms prevail. With respect to communication, these three aspects of social distance, i.e., identification, anonymity, and social norm activation, vary in the degree to which they are prevalent under specific channels of communication. Thus, the highest degree of social distance can be assumed under online communication, whereas face-to-face communication is characterized by a high degree of social closeness (see Table 1 for a summary of the features of the applied communication channels).

Following Hancock, Thom-Santelli, and Ritchie (2004), communication channels can be differentiated by their degree of synchronicity and recordability.

The **synchronicity** of an interaction describes whether messages can be exchanged instantaneously and in real time. The communication channels face-to-face, phone and written chat are synchronous compared to non-synchronous channels like emails, where usually no instantaneous messages are exchanged. Under synchronous channels people in general do not have the time to extensively reflect on the content they want to communicate.

In addition, synchronous channels vary in their degree of anonymity, e.g., written chat communication is more anonymous compared to phone or face-to-face interactions. A strictly connected aspect, which overlaps with anonymity, is represented by the degree to which so called **"telltale clues"** (human touch) can be transmitted. Following Frank (1988), these clues refer to facial or verbal expressions that may influence behavior, e.g., blushing or tone of voice. For instance, refusing a promise face-to-face might be harder since the counterpart may see the embarrassment of the refusing party.

Recordability of communication refers to the question of whether the content of an interaction is automatically documented, for example, as in chat rooms or email communications. As a consequence, choices and statements can be easily retrieved and hard evidence about them can be verified ex-post.

Hence the *Chat* treatment[15] - a chat room being synchronous but anonymous and recordable at the same time - allows us to disentangle the influence of synchronicity (e.g., as in *F-t-F* and *Phone*) and anonymity (no "telltale clues") (e.g., as in *PC-Lab* and *PC-Online*).

..:::: Table 1: about here ::::..

[15] We thank an anonymous referee for his/her advice on this point.

Given the characteristics of the different communication channels, two main testable hypotheses can be derived:

Hypothesis (1a) - Social distance: Promise-making rates decrease with greater social distance. More promises are made in treatments employing communication channels having a higher degree of social closeness.

$$\textit{F-t-F} \succ \textit{Phone} \succ \textit{Chat} \succ \textit{PC-Lab} \succ \textit{PC-Online}$$

Hypothesis (1b) - "Telltale clues" *vs.* Synchronicity: Promise-making is promoted by "telltale clues" rather than synchronicity because direct interaction creates more identification[16] with the counterpart than synchronicity *per se*.

$$(\textit{F-t-F} \geq \textit{Phone}) > (\textit{Chat} \geq \textit{PC-Lab} \geq \textit{PC-Online})$$

Hypothesis (2) - Recordability: Promise-keeping rates are higher when communication channels entail recordability. Promises are kept more often in treatments with recordable communication since the promises made can be easily verified ex-post.

$$[(\textit{PC-Online} = \textit{PC-Lab} \geq \textit{Chat}) > (\textit{Phone} \geq \textit{F-t-F})] \mid \textit{Promise-making} = 1$$

4 Results

Figure 2 depicts (see also Table 2 and Figure 2), by treatment, the share of subjects who made the promise to participate in the online survey and the share of subjects who actually kept the promise. Looking at the promise-making frequencies in the baseline condition *F-t-F,* 88% of the subjects made a positive promise. Almost identical shares of 85% made the promise in the *Phone* condition and 78% under the *Chat* condition. The proportions drop drastically to 67% and 53% under *PC-Lab* and *PC-Online*, respectively. Despite these differences in the promise-making rates, the proportion of subjects who made the promise and actually kept it was rather constant across treatments.

..:::: Table 2: about here ::::..

[16] See Frank (1988).

Result (1a): Hypothesis confirmed
Promises-making rates decrease with greater social distance: More promises are made in treatments employing communication channels having a higher degree of social closeness.

$$\textit{F-t-F} \succ \textit{Phone} \succ \textit{Chat} \succ \textit{PC-Lab} \succ \textit{PC-Online}$$

In order to avoid multiple testing issues (List, Shaikh, and Xu 2016) generated by multiple comparisons (due to repeated proportion tests), a conservative Jonckheere-Terpstra test for ordered alternatives is adopted.[17] It reveals how increasing the social distance (from *F-t-F* to *PC-Online*) leads to a significant decline in promise-making rates ($p<0.001$, for descending ordered alternative; See Figure 2).

Result (1b): Hypothesis rejected
Promise-making behavior is promoted by the synchronicity property of the communication channel rather than "telltale clues" (human touch) during the interaction.

$$\textit{(F-t-F} = \textit{Phone} = \textit{Chat)} > \textit{(PC-Lab} = \textit{PC-Online)}$$

Both LPM and Probit model (Table 3) reveal that there is no statistical difference in promise-making (dependent variable: 1 if promise is made, 0 if promise is denied) between the *F-t-F, Phone* and *Chat treatments.*

In model 3 of Table 3, the *F-t-F* (constant, 83%), the *Phone* (treatment dummy; 3 percentage points, p=0.79) and the *Chat* coefficients (treatment dummy; 16 percentage points , p=0.149) turn out to be statistically indistinguishable.

Taking *F-t-F* as reference treatment, promise-making decreases significantly by 36 percentage points (p=0.001) under *PC-Lab* and 43 percentage points ($p<0.001$) under *PC-Online* (both treatment dummies), respectively.[18] Controls that refer to demographics and the behavior/gains in the previous experimental task[19] are not statistically significant

[17] The Jonckheere–Terpstra test is a non-parametric test for more than two independent samples, similar to the Kruskal–Wallis test. Unlike Kruskal–Wallis, Jonckheere–Terpstra tests for ordered differences between treatments, and hence it requires an ordinal ranking of the test variable. For a more detailed description of the test, see Hollander, Wolfe, and Chicken (2015).

[18] The coefficients for *PC-Lab* and *PC-Online* (both asynchronous channels) turn to be not statistically different from each other (Wald-test p=0.509).

[19] It is the number of reported *tails* in the coin flip task by Conrads and Lotz (2015). In this experimental task, subjects had to privately flip a coin four times. Each time tails was reported, a subject earned 1 Euro in addition to a fixed payment of 7 Euros for filling in a socio-demographic questionnaire. Thus, subjects had an incentive to over-report the true outcome of the four coin flips. The independent variable labeled '*Reported outcome*' in Table 3, 4, A.2 and A.3 captures the payoffs earned by subjects in the previous experimental task (this control variable varies between 0 and 4). '*Reported outcome*'

at any conventional level and small in their relative sizes. Since the research assistant interacting (under *F-t-F* and *Phone*) with the subjects was a female, this could have left space for gender/chivalry-related concerns[20] (Solnick 1999, Eckel and Grossman 2001). The interacted LPM regression about promise-making (Table 3, model 3) enriches the baseline models (Table 3, models 1 and 2), introducing interaction terms concerning treatments and gender (*Male*). If gender/chivalry concerns were present, the interaction terms should be positive and significant (please remember that the research assistant was a female), especially the one interacting *Phone*Male* as well as the baseline coefficient for *Male* (it refers to the *F-t-F* treatment).

The proper computation of the marginal effects of interaction terms based on two dummies in nonlinear models represents an open issue in the literature (Ai and Norton 2003, Greene 2010). For this reason we rely on a more intuitive LPM estimation -model 3- (please note that the LPM -model 1- and the corresponding Probit specification -model 2- deliver very similar estimates) that allows for a simpler interpretation of the coefficients. Also in this specification the baseline control for *Male* (it refers to the *F-t-F* treatment) is small and not statistically significant, and the same is confirmed for the interaction between *Phone*Male*. These two results (together with the balanced gender composition of the different groups exposed to the treatments, see Footnote 11 and Table A.2 in the Appendix) confirm that the general finding is not driven by gender/chivalry-rated effects. These effects could only be realized in treatments that feature a direct interaction between the subjects and the research assistant, such that the gender is revealed by her body appearance (*F-t-F*) or the tone of her voice (*Phone*).

Both the interactions *Chat*Male* and *PC-Online*Male* are not statistically significant. The interaction term *PC-Lab*Male* represents an exception as is marginally significant at level. This suggests that males exposed to the *PC-Lab* treatment tend to commit more frequently than females exposed to the same treatment. On the other hand, it is extremely difficult to link this effect to proper gender/chivalry-rated concerns because during the PC-mediated promise-making phase the identity of the research assistant plays no role.

...::: Figure 2: about here :::...

...::: Table 3: about here :::...

can be interpreted both as behavioral outcome and, as a consequence, payoff earned in Conrads and Lotz (2015) task, e.g., if a subject reported tails four times she would also earn 4 Euros.

[20] We thank an anonymous referee for this remark (see also Footnote 11).

Result (2): Hypothesis rejected
Promises-keeping rates are not affected by recordabilty concerns.

$$[(PC\text{-}Lab = PC\text{-}Online = Chat) = (Phone = F\text{-}t\text{-}F)] \mid Promise = 1$$

Moving to the promise-keeping margin, the shares of subjects who have made the promise and fully kept it[21] do not show large variations across treatments (see Figure 3 and Table 3). Under *F-t-F*, 36% of the subjects keep the promise. The 51% and 36% were consistent under the *Phone* and *Chat* condition, respectively. As far it concerns asynchronous channels, 40% and 36% of the committed subjects actually keep the promise under *PC-Lab* and *PC-Online*, respectively.

..::: Figure 3: about here :::...

Both LPM and Probit regressions (Table 3, models 4, 5, and 6) reveal that the baseline probability of keeping the promise (1 if promise is kept, 0 if promise is not kept) observed under *F-t-F* does not vary significantly in the *Phone* treatment (treatment dummy; 17 percentage points, p=0.188 -model 6-). In line with this observation, no statistically significant differences can be detected if more directly recordable tools are considered, such as *Chat* and *PC-based*. Also, in this case, all controls that refer to demographics, as well as to the behavior/gains in the previous experimental task, are not statistically significant. Also the covariates capturing the interactions between gender and treatments (in model 6) turn out to be not significant at any conventional level. The significance of the intercation term *Chat*Male* represents the only exception, but it is extremely difficult to link this positive effect to proper gender/chivalry-rated concerns because during the Chat interaction the identity of the research assistant plays no role.

The null result detected in the regression analysis is confirmed by non-parametric analysis. The Jonckheere–Terpstra tests fails to reject the null hypothesis of equality between treatments (see Figure 3): Decreasing the degree of recordability (from *PC-based* to *F-t-F*), promise-keeping rates remain statistically indistinguishable across treatments (p=0.619, descending ordered alternative).

[21] No subject started the survey without fully completing it. No one accessed the survey platform before the starting time or after the deadline.

5 Conclusion

"How would (promise) behavior be affected if interactions were oral and face-to-face rather than written and anonymous?" (Ellingsen and Johannesson 2004, pp. 417-418). Our study documents how "oral" *vs.* "written" concerns do not lie at the core of the question. Instead, the "synchronicity" *vs.* "asynchronicity" property of the communication channel seems to be the key element in promise-making.

In our experimental study, around 85% of the subjects make a promise to complete an unconditional cooperation task when approached through a synchronous and interactive communication channel (*F-t-F*, *Phone*, *Chat*), while only about 60% make a promise when communications took place under asynchronous and indirect computer-mediated *PC-Lab* and *PC-Online* conditions. As promise-making rates between face-to-face and written chat communication are not statistically different, it appears that synchronicity is more relevant for promise-making than "telltale clues", i.e., in the *Chat* treatment, subjects interacted under (I) a high degree of anonymity and (II) via a technologically-mediated channel, but (III) in a context of synchronous communication (like face-to-face or by phone). This synchronicity may also lead to a rather automatic than reflective handling of the request to make a promise. The automatic response may be more pro-social (i.e., making the promise) in contrast to a reflective assessment of the request, which might lead to an anticipation of the resulting effort and a possible refusal of the request (e.g., see Haidt 2001). This first finding matches the results by Bicchieri and Lev-On (2007) and Bochet, Page, and Putterman (2006). They find that face-to-face communication increases cooperation in social dilemmas, but also that written communication through a chat room - that preserves anonymity and excludes facial/verbal expression - is almost as effective.

On the one hand, the main finding concerning the importance of "synchronicity" aspects seems to capture the trend in the real business world, where chat rooms are regarded as quite effective and *"[...] managers oblige their underlings to add new collaborative tools such as Slack and Chatter to existing ones such as e-mail and telephones"* (The Economist 2016a, and The Economist 2016b).

On the other hand, promise-keeping does not turn out to be influenced by the communication channel: The average promise-keeping rate is quite constant across treatments. Recordability concerns (*Chat*, *PC-Lab*, *PC-Online*) do not contribute to the achievement of higher promise-keeping rates. According to this null result, the increasing diffusion of indirect communication channels seems to be justified as their adoption entails clear savings in terms of transaction costs.

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Figures and Tables

Figure 1: Decision tree

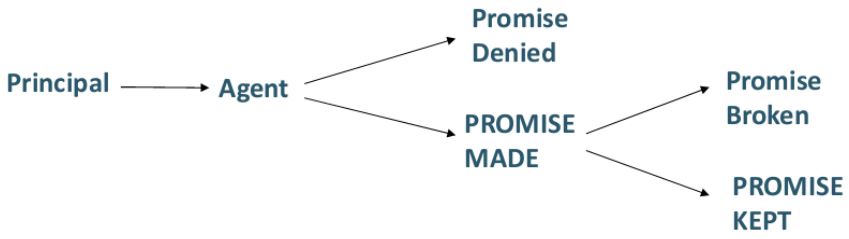


Table 1: Characteristics of the different communication channels

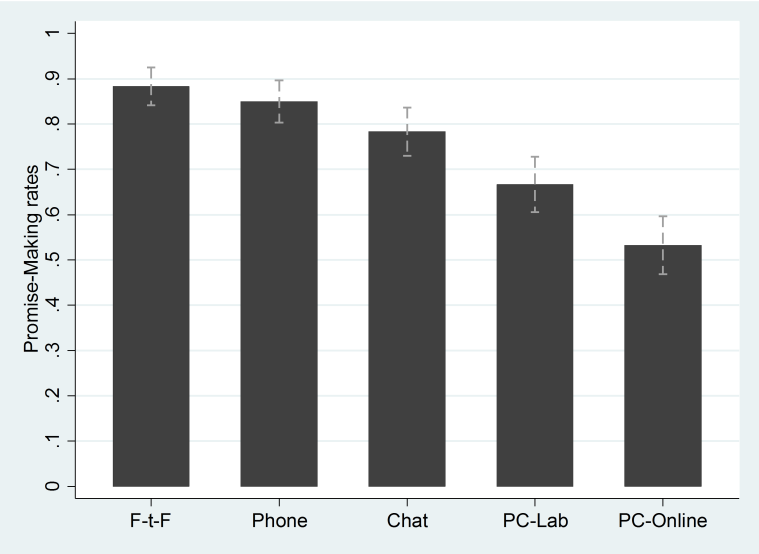
	F-t-F	*Phone*	*Chat*	*PC-Lab*	*PC-Online*
Social distance	1	2	3	4	5
Technical Features					
- Telltale clues	✓	✓			
- Synchronicity	✓	✓	✓		
- Recordability			✓	✓	✓

Notes: With respect to the aspects of social distance, 1 stands for the lowest degree and 5 for the highest degree of a respective aspect. Check marks represent the presence of a technical feature.

Table 2: Descriptive statistics

Treatment	Obs.	Promise-making rate	Promise-keeping rate
F-t-F	60	88% (53/60)	36% (19/53)
Phone	60	85% (51/60)	51% (26/51)
Chat	60	78% (47/60)	36% (17/47)
PC-Lab	60	67% (40/60)	40% (16/40)
PC-Online	62	53% (33/62)	36% (12/33)

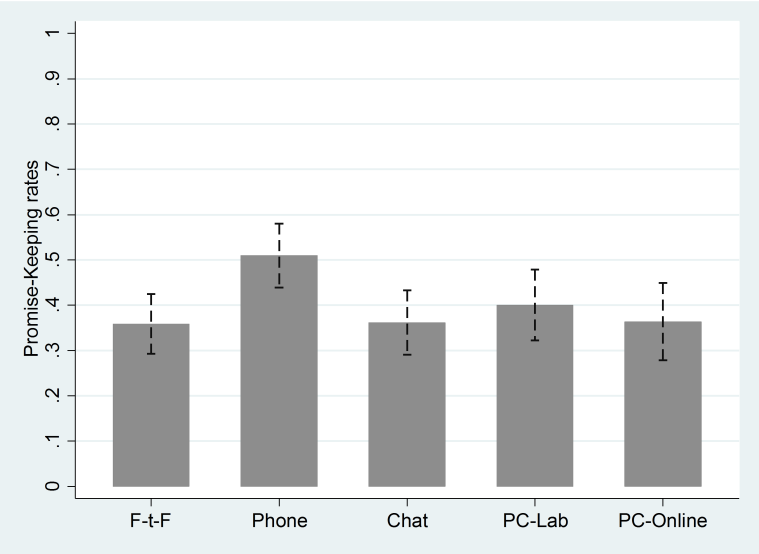
Figure 2: Promise-Making rates, by treatment



Notes: Relative frequencies of promise-making (black bars) in each experimental treatment.

Error bars based on the standard deviations of the means (dashed bars) for each experimental treatment.

Figure 3: Promise-Keeping rates, by treatment



Notes: Relative frequencies of promise-keeping (grey bars) in each experimental treatment.

Error bars based on the standard deviations of the means (dashed bars) for each experimental treatment.

Table 3: Regressions analysis
Treatment Effects

Variable	(1) Promise-Making LPM	(2) Promise-Making Probit-Mrg. Effects	(3) Promise-Making LPM	(4) Promise-Keeping LPM	(5) Promise-Keeping Probit-Mrg. Effects	(6) Promise-Keeping LPM
Phone	-0.031	-0.048	-0.029	0.157	0.159	0.176
Chat	-0.100	-0.137	-0.157	0.002	0.003	-0.215
Pc-Lab	-0.215***	-0.264***	-0.364***	0.049	0.050	-0.007
Pc-Online	-0.353***	-0.399***	-0.434***	-0.006	-0.006	-0.080
Male	0.019	0.013	-0.097	-0.008	-0.008	-0.137
Age	0.002	0.002	0.002	0.007	0.007	0.006
Reported outcome	-0.001	0.001	-0.000	-0.002	-0.002	-0.007
*Phone*Male*			-0.005			-0.043
*Chat*Male*			0.115			0.448**
*Pc-Lab*Male*			0.297*			0.118
*Pc-Online*Male*			0.168			0.159
Constant *[F-t-F]*	0.825***	0.838***	0.884***	0.206	0.225	0.308
Observations	302	302	302	224	224	224
(pseudo)R^2	0.089	(0.077)	0.11	0.019	(0.014)	0.052

OLS Linear Probability Model and Probit regressions; marginal effects are reported. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.
Phone, *Chat PC-Lab*, *PC-Online* treatment dummies; *Male* gender dummy=1 if male; *Age* discrete variable; *Reported outcome* discrete variable ranging between 0 and 4. *F-t-F* treatment dummy omitted for collinearity reasons, reference level (Constant).

Appendix

Script A.1: Voucher

Further instructions after making the promise to access to the online survey

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Thank you very much that you agreed to participate in the online survey.

Within the next 24 hours
from MM/DD/YY , HH/MM to MM/DD/YY , HH/MM

you reach the survey platform by using the following link:

< `http://goo.gl/s3aCrd` >

Table A.2: Gender composition, by treatment

Treatment	Males	Females	
F-t-F	30	30	60
Phone	30	30	60
Chat	29	31	60
PC-Lab	30	30	60
PC-Online	28	34	62
	147	155	302

Table A.3: OLS Linear Regression
Promptness in keeping the promise

Variable	Promptness (minutes)
Phone	31.305
	(102.719)
Chat	61.462
	(116.336)
Pc-Lab	110.680
	(116.412)
Pc-Online	37.337
	(126.872)
Male	10.082
	(74.487)
Age -cent.-	14.222*
	(8.394)
Reported oucome	-34.801
	(40.753)
Constant *[F-t-F]*	426.055***
	(138.980)
Observations	90
R^2	0.057

Outcome variable: minutes of procrastination in filling in the survey from the start of the survey time window. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.
Phone, *Chat* , *PC-Lab*, *PC-Online* treatment dummies; *F-t-F* treatment dummy omitted for collinearity reasons, reference level (Constant); *Male* gender dummy =1 if male; *Reported outcome* discrete variable ranging between 0 and 4; *Age* variable is centered around the mean $(Age_i - \overline{Age})$ in order to allow for a more intuitive interpretation of the constant term estimate.

Tab. A.4: Sequence of experimental treatments and sessions

Session	Treatment	Date	Session time	Survey start	Survey end	Observations
1	*F-t-F*	05/11/2013	10am – 11am	12pm – 05/11/13	12pm – 06/11/13	12
2	*F-t-F*	05/11/2013	11pm – 12pm	01pm – 05/11/13	01pm – 06/11/13	12
3	*F-t-F*	05/11/2013	01pm – 02pm	03pm – 05/11/13	03pm – 06/11/13	12
4	*F-t-F*	05/11/2013	02pm – 03pm	04pm – 05/11/13	04pm – 06/11/13	12
5	*F-t-F*	05/11/2013	03pm – 04pm	05pm – 05/11/13	05pm – 06/11/13	12
6	*Phone*	06/11/2013	10am – 11am	12pm – 06/11/13	12pm – 07/11/13	12
7	*Phone*	06/11/2013	11pm – 12pm	01pm – 06/11/13	01pm – 07/11/13	12
8	*Phone*	06/11/2013	01pm – 02pm	03pm – 06/11/13	03pm – 07/11/13	12
9	*Phone*	06/11/2013	02pm – 03pm	04pm – 06/11/13	04pm – 07/11/13	12
10	*Phone*	06/11/2013	03pm – 04pm	05pm – 06/11/13	05pm – 07/11/13	12
11	*PC-Lab*	07/11/2013	10am – 11am	12pm – 07/11/13	12pm – 08/11/13	12
12	*PC-Lab*	07/11/2013	11pm – 12pm	01pm – 07/11/13	01pm – 08/11/13	12
13	*PC-Lab*	07/11/2013	01pm – 02pm	03pm – 07/11/13	03pm – 08/11/13	12
14	*PC-Lab*	07/11/2013	02pm – 03pm	04pm – 07/11/13	04pm – 08/11/13	12
15	*PC-Lab*	07/11/2013	03pm – 04pm	05pm – 07/11/13	05pm – 08/11/13	12
16	*PC-Online*	29/11/2013	10am – 11am	12pm – 29/11/13	12pm – 30/11/13	62
17	*Chat*	19/05/2016	10am – 11am	12pm – 19/05/16	12pm – 20/05/16	15
18	*Chat*	19/05/2016	12pm – 13pm	14pm – 19/05/16	14pm – 20/05/16	15
19	*Chat*	19/05/2016	14pm – 15pm	16pm – 19/05/16	16pm – 20/05/16	15
20	*Chat*	19/05/2016	16pm – 17pm	18pm – 19/05/16	18pm – 20/05/16	15