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Laboratory Evidence on Face-to-Face: Why Experimental Economics is of Interest to Regional Economists

Björn Frank[1]

Abstract

The notion of face-to-face contacts has recently become very popular as a reason why firms still locate in proximity to others after the "death of distance." Controlled laboratory experiments provide direct and reliable evidence on the importance of face-to-face contacts. It is the purpose of this article to survey and to organize new and developing string of literature with a special focus on its importance for regional economics. However, the article might also serve to alert more experimentalists to the importance of their work for current regional science, of which they seem not to be aware.

Keywords

cooperation, death of distance, face-to-face, localized spillovers, trust

Introduction

A traditional source of localization economies is the cost advantage of local interaction with suppliers, cooperation partners, and customers (Marshall 1890, Book IV,

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chap. X). There is impressive empirical evidence that knowledge spillovers are bounded within narrow geographical ranges (surveyed by Ács and Varga 2005, or Döring and Schnellenbach 2006). However, insights into the microfoundations are still wanting, making it difficult to predict the effects of changing technology on location. For example, during the past twenty years, communicating at a distance has become easier and cheaper, while the variety of technical options has increased. One decade ago, the notion of a "death of distance" (Cairncross 2001) or "death of geography" was en vogue. Yet a visible weakening of localization economies mostly failed to appear.

Death is an unfortunate metaphor anyway. As far as we humans are concerned, we do not know, and will never know as long as we are living, what Death looks like,[1] but we can pretty well calculate a remaining life expectancy. For distance, the reverse is true. There is no indication at all when death will come, but we know, at least to some extent, what it looks like if it comes. In other words: What is necessary for communication over a distance to work so well that spatial proximity becomes unimportant? There is evidence on necessary features of communication tools if they are intended to replace, rather than just complement, face-to-face communication. It is the main purpose of this article to organize a major part of this body of evidence, namely, laboratory economic experiments.

In this survey, I aim to include all economic experiments that are relevant for understanding face-to-face interaction and its apparent persistence. In economic experiments, subjects are confronted with clearly defined (often strategic) decision problems, and their decisions are relevant for their actual monetary payoffs. This provisory definition serves as a minimum requirement for inclusion in this survey, and it already excludes most psychological experiments and approaches like role playing (with its unclear structure and lack of incentives).[2] However, there are additional requirements for good experimental practice often followed by economists (Hertwig and Ortmann 2001) on which I decided to be less strict.[3] In one respect, the experiments covered in this survey are atypical—necessarily, given the topic: Most economists let their subjects interact anonymously. Otherwise "the possibility of postgame interaction, positive or negative, may influence decisions" (Eckel 2007, 846n). Face-to-face experiments, however, cannot be completely anonymous. Yet in these cases experimenters take care that subjects paired together are strangers with a low probability of meeting again.

Nevertheless, the very fact that the subjects communicate with each other lessens a potential problem of typical experiments called experimenter demand effect (EDE; see Zizzo 2010). Especially when there is no contact with cosubjects, subjects are possibly concerned about the experimenter's expectation and his or her opinion about subjects' decisions. The only way to completely eliminate EDE and some similar problems of the laboratory environment (e.g., Levitt and List 2007) completely, though, are natural field experiments, where subjects are not aware that they are taking part in an experiment.[4] These are still experiments, because the experimenter retains control (Harrison 2005)—for example, subjects cannot self-select themselves

into treatments. The benefits of control become clear by imagining one had control outside the lab. Consider Charlot and Duranton (2004), who explore the relationship between workplace communication of employees and their wages. If the authors have had the kind of control experimenters have, they would have been able to randomly assign workers to workplaces with (different kinds of) communication devices and workplaces without, in different locations, observing afterward what the impact on wages is. In this case, the causal effect would have been clear, while the nonexperimental data they actually have forces them on a long, difficult detour: for investigating causality, they have to find instrumental variables for use in a two-stage least squares (2SLS) approach. Experimenters never need 2SLS.

In a sense, laboratory experiments and field studies on the same topic potentially corroborate each other;[5] only field studies can demonstrate the "real-world relevance" of a hypothesis while often laboratory evidence can help rule out a spurious relationship, or clarify causal mechanisms. In this particular case, Charlot and Duranton (2004) have shown that there is a likely impact of communication on local labor markets (also see Charlot and Duranton 2006; Gaspar and Gleaser 1998), while the laboratory evidence reported here helps to understand in greater detail how communication helps to achieve the goals of those who are involved in it.[6]

However, there is one line of research that is intentionally left out here: a considerable number of experiments focus on the benefits of communicating at all. For example, a number of experiments have compared only completely anonymous interaction, without any kind of communication, to face-to-face interactions (e.g., Dawes et al. 1988; Roth 1995; Schmidt and Zultan 2005; Valley et al. 2002). The results are interesting in their own right, but no proponent of the "death of distance" hypothesis has ever claimed that one can do without communication at all. Rather, the open question that is of interest for regional economics is this: What is lost when face-to-face interaction is substituted by communication via the Internet, where partners are at least known by name.

However, this would be a short survey if the only evidence were those experiments that directly compare face-to-face interaction with other communication channels, such as e-mail. By and large, it would be reduced to the first part of Trust and Cooperation section, reporting on experiments that show whether and how much trust and cooperation increase due to face-to-face interaction. Yet valuable insights can also be gained from indirect evidence. The experimentally Detecting Conditions and Reasons for Increased Trust and Cooperation subsection discusses experiments that shed some light on single features of face-to-face communication, such as smiling and eye contact, which might be responsible for the effects found in Measuring Increased Trust and Cooperation in Face-to-Face Experiments subsection. Furthermore, as face-to-face contact requires more spontaneous reactions than e-mail, what is the effect of spontaneity—that is, decision time—in controlled experiments? This will be discussed in Spontaneity section. Experiments that Regional Economists Would Like to See section discusses some research desiderata—that is, experiments that might be designed in order to shed light on aspects of face-to-face

communication that are important for regional science, but which have been overlooked so far by experimentalists. Discussion section concludes with a brief look at the implications for regional economics.

Trust and Cooperation

> A, B, C, D, and E meet over lunch. "Each of our firms benefits equally from this platform if we get it started," A remarks. "Yes, and the benefit is larger the more effort we put into it by the end of this year," B replies. All feel they should contribute considerable effort.
>
> F, in an e-mail to G, H, I and J, remarks: "Each of our firms benefits equally from this platform if we get it started." G replies: "Yes, and the benefit is larger the more effort we put into it by the end of this year." And it is even larger for me, H thinks, if the others do most of the work while I take care of my own business.

Measuring Increased Trust and Cooperation in Face-to-Face Experiments

Trust and cooperation are hard to disentangle. Conditional cooperators contribute to a group's public good if they expect others to do so, trusting that they will not egoistically exploit their strategic position. Hence many experimental designs do not allow one to be measured entirely without the other. It is difficult enough to measure trust and cooperation together even in simple experiments; the work by Valley, Moag, and Bazerman (1998) is a case in point.

They study bilateral negotiations with asymmetric information. Two subjects negotiate the price per share which the seller gets for a firm that henceforward, according to the experiment's framing, is to be managed by the buyer. There is something that only the seller knows: the true current value V of each share. From the buyer's perspective, it is equally distributed between \$0 and \$100. Furthermore, the buyer knows that the new value, once the firm is under the buyer's management, will be 1.5 times larger than the former value V. Hence selling the shares leads to a Pareto improvement.[7] However, the buyer should not bid for the shares. For any bid B, the seller will only accept if $B > V$. Getting the shares at price B means that the value for the buyer must be equally distributed between 0 and $1.5B$. Hence the expected value is lower than B, and the buyer should refrain from bidding unless he receives reliable information about V.

Simply asking the seller does not help from a purely game theoretic perspective, as the seller might lie. However, the seller is significantly less likely to lie in face-to-face interaction, only in 1 of the 14 (or 7 percent) negotiations that were taped, the seller lied about V, compared to 55 percent for telephone negotiations and 33 percent for (nonanonymous) written negotiations. Some buyers failed to take advantage of the features of face-to-face negotiation, however, and bid without asking for V. In these cases, sellers did not have to lie in order to get a price at which buyers lost money. Nevertheless, face-to-face interaction resulted in the highest number of Pareto improving deals.[8]

Other experimental designs are more straightforward with respect to the effects of face-to-face communication.[9] Frohlich and Oppenheimer (1998) let groups of five people play a fifteen-round multilateral prisoner's dilemma without any previous communication, with e-mail communication before each of the first eight rounds, or with face-to-face communication before each of the first eight rounds. In every round, each player can give any amount between 0 and 10 to the group, keeping the rest. The sum of all contributions is multiplied by 0.4, and each player receives the resulting amount. Without communication, each player gives 2.9 on average over rounds 1 to 8, being closer to the individually rational contribution of 0 than to the Pareto efficient contribution of 10. (If everyone contributes 0, everyone gets, or rather keeps, 10 per round. If everyone gives 10, everyone gets 20 per round.) With e-mail communication before each round, the average contribution in rounds 1 to 8 is higher: 7.6 on average. Yet with face-to-face communication it is even higher: 9.99!

Two things are noteworthy, however. First, from round 9 on, communication was no longer possible. Contributions in groups with previous face-to-face communication quickly collapse to the level of the e-mail groups (that also may no longer communicate), reaching the low level of the no communication group in the final round. It is an open question how persistent face-to-face communication should be to be effective;[10] in two studies reported on further below, face-to-face communication took place only before the first round, but its effects lasted throughout the experiment (Brosig, Ockenfels, and Weimann 2003; Bochet, Page, and Louis 2006). That a marked cooperation breakdown is observed only by Frohlich and Oppenheimer (1998) is possibly due to the fact that rounds 9 to 15 came unexpectedly for their participants;[11] the restart effect might have destroyed any previously accumulated feelings of group solidarity.

Second, Frohlich and Oppenheimer (1998) also try the "impartial PD," a game in which there is no conflict between individual and social rationality. The only problem is that this is not easy to comprehend for all participants. Purely self-interest reasoning, if correctly performed, will suffice to make participants give the full amount of 10. Here it is the purpose of communication not to build up empathy and trust, but to dispel any misunderstanding. Under these circumstances, face-to-face and e-mail communication work equally well (and better than no communication at all).

Whereas the conflict between social and individual rationality clearly vanishes in this alternative design by Frohlich and Oppenheimer (1998), Arunachalam and Dilla (1992, 1995) perform an experiment which features a bit of this conflict, but much less than in a prisoner's dilemma. Three subjects get paid if, and only if, they unanimously agree on one of 625 possible allocations (framed as vectors of transfer prices and further conditions for one upstream and two downstream divisions of a firm.) A majority of the allocations are Pareto efficient, and negotiations are severely hampered by the fact that every player only knows his own payoffs and may not communicate his payoff schedule. A random allocation would lead to a payoff of 6,600 points (leading to a $ 1.32 payoff) per subject, in the Kaldor-Hicks optimum each would get 9,800. Playing the game with a 25-minute face-to-face

communication phase leads to an average payoff of 7,270, compared to 7,018 for nonanonymous computer-mediated communication (chat) of equal duration. This small difference probably just reflects the speed disadvantage of electronic communication.

Independent of Frohlich and Oppenheimer (1998), a similar experiment was investigated by Rocco (1998). Groups of six participants played twenty-eight rounds of an experiment where the Nash equilibrium was Pareto dominated.[12] Face-to-face communication after rounds 10, 15, and 20 effectively helped participants to deviate from the individually rational decision and maximize group welfare instead in the second half of the experiment. E-mail communication among strangers, however, did not result in better cooperation. E-mail communication among people who had tried to solve a group task in face-to-face interaction on the day before the experiment was almost as successful in achieving cooperation as face-to-face interaction.

Bochet, Page, and Louis(2006) perform a four-person ten-round public goods game, comparing face-to-face interaction (lasting 5 minutes before the start of round 1) with a chat room treatment, allowing online discussion before the first, fourth, and seventh round, though messages revealing the players' identity, threatening or offering side payments were blocked. They find a lower difference between these two treatments than expected (according to Bochet, Page, and Louis 2006, 12); an average contribution of 81.4 percent of the endowment with chat room communication, compared to 96.2 percent face-to-face, averaged over ten rounds. Yet at least, in round 10, the average contribution has dropped to 78.1 percent of the endowment in the face-to-face treatment, but to 52.1 percent in the chat room treatment.

The most recent contribution to this line of research, with fairly unsurprising results, is by Naquin, Kurtzberg, and Belkin (2008), who perform a threshold public goods game; in a group of four, everyone gets two $7 certificates for meals in campus eateries if, and only if, at least three members contribute their initial endowment of one such certificate. Communicating nonanonymously via e-mail before their decision, 35.8 percent of the participants contributed to the group's public good, while face-to-face communication led to a 69.9 percent contribution rate.[13]

Summing up this subsection, face-to-face communication leads to better results than e-mails in strategic situations with a conflict between individual and collective rationality. This experimental evidence is convincing in terms of the number of replications and the unanimity of the results. However, it sheds no light, as it stands, on the reasons for the effects of face-to-face communication and hence on possible substitutes other than e-mail. This is why the additional experimental evidence, reported on in the next section, is needed.

Experimentally Detecting Conditions and Reasons for Increased Trust and Cooperation

Alternatives to e-mail. Compared to Frohlich and Oppenheimer (1998), a greater variety of communication channels is employed by Bos et al. (2001) in a

three-person variant of the prisoners' dilemma, and by Brosig, Ockenfels, and Weimann (2003) in a standard (four-person ten-round) public goods game (also reported on in Brosig 2006). This leads to additional insights concerning the triggers of trust and cooperation. Bos et al. (2001) find that communicating via a videoconference leads to levels of cooperation that are close to the results of face-to-face communication but significantly higher than in the case of chat room communication via the Internet. Unfortunately, Brosig, Ockenfels, and Weimann (2003) do not have a treatment with e-mail or chat room communication. Their main finding is that audio communication, compared to anonymous play, does not significantly increase the level of cooperation (on average, 48 and 57 percent, respectively, of the endowment are contributed to the group). Compared to these levels, video transmitted communication and face-to-face communication lead to higher levels of cooperation: 93 and 97 percent, respectively, which are not significantly different from each other.[14]

One might find the latter result surprising. Video conferences and face-to-face communication are not identical in every respect. In one case study on an interfirm team, Carletta, McEwan, and Anderson (1998) observed much less small talk in the virtual meeting. Their interpretation is that etiquette demands naturally occurring gaps in face-to-face meetings to be filled with chatting on diverse subjects, while letting a gap be a gap is o.k. in a videoconference. However, even before Brosig, Ockenfels, and Weimann (2003) tackled this question directly, there was indirect evidence that the differences between the two settings are too subtle to make behavior in face-to-face situations differ markedly from behavior in videoconferences: In a classic study on a multilateral prisoner's dilemma, Dawes, McTavish, and Shaklee (1977) found that communication had no effect as long as they only allowed only small talk, but no discussions of the problem at hand. The small talk element of face-to-face communication does not appear to be decisive.

Another finding of Brosig, Ockenfels, and Weimann (2003) was that pure identification (photographs of the group members being shown for 10 s before the game starts) did not lead to higher cooperation than purely anonymous play. Hence the obvious question is: what is it that video transmission can do which audio communication and photographs cannot?

Smiling. A possible candidate is the opportunity to smile. Scharlemann et al. (2001) find that people put more trust into others who are smiling. They play a variant of the trust game (Berg, Dickhaut, and McCabe 1995), in which their subjects (in the role of Player 1) have the choice between immediately getting £1 (with payoff of Player 2 being £0.50) or trusting Player 2, who then has the choice between rewarding the trust or not rewarding the trust. In the latter case, he gets £1.25, but Player 1 gets only £0.80. If Player 2 rewards the trust, his payoff is slightly lower (£1.20), but Player 1 also gets £1.20. Actually all subjects take the role of Player 1 and believe that their partner (Player 2) is a real person, and they are shown a photograph of Player 2. However, the photographs were from a Psychological Image Collection, with a smiling and a non-smiling picture taken from every model. Any

time that a Player 1 trusts Player 2, the computer simulates a "rewarding trust" decision of the presumed Player 2. This is the only experiment in this survey where subjects are deceived concerning their opponents, something that is considered as violation of a basic principle in experimental economics (Hertwig and Ortmann 2001, section 5). However, Scharlemann et al. (2001) argue that subjects were paid as promised, and real partners would not have changed their (perceived) choice situation, but would have made it more difficult to control their transmitted facial expression and to precisely measure the impact of a smile. If Player 2 does not smile, she or he is trusted, on average, in 55.0 percent of the decisions by Player 1. If Player 2 smiles, this rate increases to 68.3 percent.

Very recently the result obtained by Scharlemann et al. (2001) as been replicated with real persons in the role of Player 2, who were filmed when smiling (Centorrino et al. 2011). The more convincing the smile, the higher the perceived trustworthiness of Player 2, and the higher Player 1's willingness to send money. Hence means of communication might perform better than e-mail, and come closer to the positive effects of face-to-face interaction, if the expression of smiling can be transmitted.

Eye contact. A number of studies have shown that eye contact matters very directly. Compared to the completely anonymous control group, the difference is not real face-to-face interaction, but a simple image intended to activate the brain's eye-detection system. Specifically, the picture chosen by Burnham and Hare (2007) for this purpose shows "Kismet," a robot invented at Massachusetts Institute of Technology (MIT) with a typical metallic robot face but humanoid eyes. The authors let subjects play six rounds of a four-person public goods game, where no one meets the same counterpart twice. In every round, each player gets ten tokens (equivalent to US$2) and can place any amount between 0 and 10 into the group account. The amount is then doubled and divided between the four group members, which makes keeping the whole amount the dominant strategy. Averaged over subjects and rounds, the amount given in the control group is 4.17; in the experimental group, with Kismet shown on the screen during the experiment, it is 5.39, a (statistically significant) difference of 29 percent.

Working with a very similar difference between experimental and control group, Haley and Fessler (2005) let students play a dictator game, in which Player 1 (the "dictator") had to decide how to divide $10 between himself and Player 2, who has no active role in the experiment. The control group had the laboratory's label on the computer monitor, while for the experimental group, a stylized drawing of a pair of eyes appeared. On average, the amount allocated by Player 1 to Player 2 was $ 2.45 in the control group, but $ 3.79 in the experimental group, a difference mainly due to the higher share of people who gave a positive amount in the eyes group (0.88 vs. 0.55).[15]

Bateson, Nettle, and Roberts (2006) design an impressively simple field experiment and obtain evidence that confirms the laboratory studies. In a coffee room shared by forty-eight university staff members, they placed a poster showing the image of a pair of eyes in some weeks, and a flower poster in others. Payments for

tea, coffee, and milk in the room were made via an "honesty box,", a system that has no sanctions for nonpayment, although prices were clearly suggested. Actual payments were markedly, and statistically significantly, higher in weeks with eyes.

Given that the comparison of minimal social cues and no social cues turns out to be so impressive, why did the photographs of the participants in the experiments by Brosig, Ockenfels, and Weimann (2003), described above, not enhance cooperation? One possible explanation would be that these photographs were not shown while the decisions were made, but before the game, but admittedly this must remain speculative.

Anyway, it would be wrong to suggest that eye contact alone, even if it is appropriately timed, should be sufficient to induce *full* cooperation. Frey and Bohnet (1995) perform a four-person prisoner's dilemma game where participants could only choose between cooperation and defection (also reported in Bohnet and Frey 1995, 1999). They find that visual contact alone, with no talking allowed between players, significantly raises the cooperation rate, compared to complete anonymity, from 12 percent to 23 percent. Yet visual contact *plus* the chance to talk to each other had a much larger positive effect (a 78 percent cooperation rate). A similar two-person prisoner's dilemma experiment by Wichman (1970) yielded a similar, though less pronounced, result. Anonymity resulted in a 40.7 percent cooperation rate, visual contact alone in 47.7 percent, only hearing each other[16] in 72.1 percent, while unrestricted face-to-face contact led to a 87.0 percent cooperation rate, averaged over seventy rounds with fixed partners.

Summing up, eye contact is another important feature of face-to-face communication that contributes to increased trust and cooperation, compared to groups which communicate via e-mail. In some of the studies, eyes watching the experimental subjects were artificial or even stylized. We still lack a direct experimental comparison of this condition with "real" eye contact. What is clear, however, is that social dilemmas can hardly be overcome without eye contact of any kind.

Detecting lies. "Going 'eyeball-to-eyeball' is the typical business characterization of how to find out what someone truly has in mind in their conversations with others" (Winger 2005, 249). Are lies really written in the liar's face? Wang, Spezio, and Camerer (2010) let students play a sender–receiver game (Crawford and Sobel 1982), in which senders have private information about a true state of nature and often have an incentive to communicate it incorrectly to a receiver (i.e., to lie). While making their decisions, they were closely monitored. Specifically, their eye movements and pupil dilation were recorded. When subjects lied, their pupils expanded—more so the "larger" the lie. This does not necessarily tell us something about an advantage of face-to-face communication, as eye-tracking systems are absent in communication outside of certain laboratories. However, if there is something in the face about lying which can be measured, there might also be something that can be intuitively felt. Yet evidence on the reliability of this kind of evidence is difficult to obtain in the field. For example, Belot, Bhaskar, and van de Ven (2010)

report on a TV game show in which two participants who made it into the final round were playing a one-shot prisoners' dilemma variant. Some were lying when they promised to cooperate in this final round, but their opponents' belief about the likelihood of the promise being a lie was not revealed.[17] Fortunately, some experiments go further and also provide reliable evidence on lie detection.

A typical psychological experiment on detecting lies goes like this (Vrij 2008): one person is asked to lie (i.e., knowingly tell something she or he knows not to be true), and observing this, another person tries to find out whether the truth was told or not. However, monetary incentives are absent, and Holm and Kawagoe (2010) have recently demonstrated the importance of this point, when they found that many people claim to be able to detect lies, but few of them were willing to bet money on that ability in a simple bluffing game. Hence the major step toward an economic experiment is made when the first person has a material incentive to lie, and the second person has a material incentive to guess correctly.

The latter aspect is still absent in an original experiment by Frank, Gilovich, and Regan (1993). They let each subject play a one-shot two-person prisoner's dilemma against each of two further subjects, with whom they meet for 30 minutes before the decision was to be made. Promises could be made but were not enforceable—there was no sanction for lying. After these meetings, subjects were separated and asked to predict the other two players' decisions whether to cooperate or defect. These predictions turned out to be better than random guesses. However, the face-to-face meetings were not monitored. It cannot be ruled out that a few subjects were truthful about their intention to defect. In this case, the ability to detect lies would not be needed to explain their findings (Ockenfels and Selten 2000, 91). While this point has been partly invalidated by Brosig (2002), who was able to sort out subjects announcing their defection from the sample in her replication of the Frank, Gilovich, and Regan (1993) study, one further problem remains if we want to compare face-to-face interaction with alternatives such as e-mail: just like some false promises can be detected in a conversation, a few written lies might also be detected.

The typical psychological design previously sketched was paired with clear monetary incentives by Bond et al. (1985): subjects were paid more if they successfully lied about their last job, "success" depending on the number of detections by fellow subjects who were also paid depending on their success. These observers were right in 63.33 percent of all cases, compared to a 50 percent expected for random guessers. However, this success is not necessarily due to the fact that interaction was "almost" face-to-face, that is, via videotape. Liars might also be detected because of unintended verbal cues (Ekman 1985, 87–92; Vrij 2008, chap. 4) that might be transmitted via telephone or even e-mail as well.

Ockenfels and Selten (2000) performed an experiment which produced clearer results. They let two people freely negotiate over the division of DM 30 (about 15 euros). One of them has private information whether she or he has "costs" of DM 12, to be deducted from his share, or not. She or he can realistically only hope to get more than the "fair" share of DM 15 if she or he claims to have costs, whether

this is true or not. Hence some have an incentive to lie. Onlookers observing the negotiation are provided with an incentive to guess correctly who had costs—in other words, they had an incentive to detect lies. There was no control group in this experiment, but the relevant information was essentially binary, hence it is very reasonable to presume if it were performed using written communication only, guessing who is lying would be very difficult, resulting in a success rate of about 50 percent.

The finding by Ockenfels and Selten (2000) was that two objective features of the negotiation (an extremely quick agreement, or acceptance of a lower share by people without costs) sometimes helped onlookers to find out about true costs of the negotiators. Excluding these cases, the success rate was even below 50 percent (slightly but significantly). Live observation of the negotiation alone did not lead to a better than random guess about who was lying.

Many people, I presume, believe that one advantage of face-to-face contact is that untruthfulness is more easily uncovered. Experimental evidence does not support this view. Ending with a speculative note, the "psychic costs" of lying might nevertheless be larger in face-to-face communication. If this is true, some people trying to cheat will try to avoid that situation, hence the willingness to meet in person would be a reliable signal, but this is an untested hypothesis.

Spontaneity

> Meeting B over lunch, A makes a "final offer: $1000 for the equipment." B rejects by replying: "It's been a pleasure to meet you," evidently not meaning what he says, and walks away.
>
> C writes an e-mail to D, making a "final offer: $1000 for the equipment." D writes back: "Sorry for the late reply. In yesterday's e-mail you offered next to nothing, yet I accept, what can I do?"

If negotiations do not take place face-to-face, but via mail or e-mail, there is an automatic "cooling off period." If we are informed, by suitable experiments, on the effects of cooling off periods, we also get an idea about a certain aspect of face-to-face negotiations.

The first controlled economic experiment on the effect of a substantive cooling off period was run by Oechssler, Roider, and Schmitz (2008). Proposers in a mini-ultimatum game offered an 8:2 split or a 5:5 split. Responders decided whether to accept or reject. 24 hours later they were (surprisingly) given the chance to revise their decision. In one treatment, when possible payoffs were sure payments of 2, 5, or 8 euros, respectively, a cooling off period lowered the rejection rate neither markedly (from 42.6 percent to 39.4 percent) nor significantly. Yet in an alternative treatment with the same expected payoffs but more potential for temptation and regret (payoffs were designed as chances for a high price in a lottery), a cooling off period led to a large and significant drop in the rejection rate, from 27.7 percent to 20.5 percent. Grimm and Mengel (2011) replicated a similar result with a much

shorter cooling off period of 10 minutes, which turned out to be sufficient to markedly increase the acceptance rate in a standard ultimatum game. A stunning result is obtained for responders to whom 2 euros were offered (when 5 would have been the equal split and any other integer between 0 and 10 would have been possible). Only 16 percent of them accept when deciding immediately after receiving the offer, but 75 percent accept when they are kept busy for 10 minutes between receiving the offer and making the decision.

If, compared to usual laboratory conditions, a cooling off period leads to a lower rejection rate, increased time pressure might well increase the rejection rate. And this is exactly what Sutter, Kocher, and Strauß (2003) find. When responders playing the ultimatum game for the very first time had 10 s instead of 100 s for their decision, the rejection rate increased from 40.3 to 78.2 percent (while offers received were about the same for both groups of responders, which is noteworthy as proposers faced the same decision time as the responders they were faced with). Cappelletti, Güth, and Ploner (2011) recently also found higher rejection rates under time pressure (30 vs. 180 s in a slightly more demanding design).[18] The latter team of authors introduced time pressure in order to increase the weight of the affective system relative to the deliberative system, as the latter, located in a different neural area, takes more time. Hence they are conducting basic neuroeconomic research, but their results are also important in our context: compared with other settings for negotiations, a face-to-face interaction is most likely to require, or to provoke, spontaneous reactions. If spontaneity is typical for face-to-face interactions and if cooling off periods are typical for other types of negotiation, then all experimental evidence suggests that we should expect more rejections face-to-face.

Considering proposer and responder only, increasing rejection rates decreases efficiency. Anything that decreases rejection rates is welcomed by proposers, who can react by lowering their offers. On the other hand, anything that increases rejection rates is favored by responders, as far as proposers correctly anticipate the increased rejection likelihood and react accordingly. Hence we find that some negotiators should favor face-to-face interaction, some should not. From a regional economics perspective, if bargaining breaks down more often in face-to-face negotiations than in e-mail negotiations,[19] the sum of proposers' and responders' payoffs is lower in agglomerations with a lot of face-to-face contact. However, two qualifications come to mind:

First, the fact that face-to-face negotiations are bad for the proposer is somewhat counter-intuitive. Yet note that the proposer is not always a seller who wants to use the situation to get a quick (positive) reply from a surprised buyer. In the example at the beginning of this section, the proposer is a prospective buyer.

Second, the ultimatum game is a very stylized negotiation. Usually something like a counteroffer should be possible.

Yet a cautious conclusion might be put this way: inspecting available experimental evidence for mechanisms that makes agglomerations, and hence lot of face-to-face contacts, more attractive, the spontaneity of this type of interaction alone was not identified as a very likely candidate.

Experiments that Regional Economists Would Like to See

Table 1 briefly summarizes the results surveyed in this article. Taking all experiments surveyed here together, the clearest result is that face-to-face communication, compared to e-mail communication, increases trust and cooperation in most cases (except for pure coordination tasks). This is not only in line with the intuition of regional economists (e.g., Morgan 2004; McCann and Simonen 2005), it is also in line with folk wisdom—"face" was a synonym for credit in eighteenth-century slang, "to travel on one's face" meant to go upon credit (Partridge 1972).

Yet there is more in the experimental evidence than mere affirmation. Most importantly, does every alternative to face-to-face communication perform as disappointingly as e-mail? As Winger (2005, 250) puts it, "What is coming is (. . .) something that, by configuring bits of information, will give us images that have the characteristics of what we experience in physical settings." Winger seems to shy away from specifying a difference between "what is coming" and videoconferencing. On the latter, evidence is too sparse as yet,[20] but if the respective results by Bos et al. (2001) and Brosig, Ockenfels, and Weimann (2003) survive replications,[21] we can state that videoconferencing might be a substitute for face-to-face-communication when it comes to ensuring trust and cooperation.[22]

Ensuring trust and cooperation is not the only benefit of meeting face to face, however. Three main features of face-to-face communication are discussed in the regional science literature, while they are still disregarded or at least underresearched in experimental economics.

First, face-to-face communication might be more efficient—not necessarily in the sense of helping to avoid Pareto-inefficient bargaining breakdowns, but in a technical sense. As nonverbal and verbal communication flow at the same time, and immediate feedback helps to quickly remove misunderstandings (Storper and Venables 2004; Winger 2005, sections 4–6; and Hildrum 2007, 469 with further references). As a limiting case, codification of the relevant knowledge might be so poor that there might simply be no other way to communicate meaningfully than face-to-face (Ács and Varga 2005, 326; see Breschi and Lissoni 2001, 261–262 for some caveats)

How to measure the effects of richer signal transmission via face-to-face communication in the laboratory? One suggestion would be to allow a reduced face-to-face communication, with the eyes covered. The results would nicely complement existing knowledge about the isolated effects of smiling or eye contact, both of which have been shown to enhance trust and cooperation (Experimentally Detecting Conditions and Reasons for Increased Trust and Cooperation subsection). Maybe sunglasses would be sufficient to show an effect. Going to the extreme, one could cover subjects' faces completely with a mask, maybe with a neutral photograph of their face on it, and let them communicate with a device like Stephen Hawking's. This would definitely reduce all advantages of face-to-face contact with respect to communication efficiency; any remaining effects, programmed in the course of human evolution, would be directly due to the physical presence of the other/others.

Table 1. Overview of the Main Findings

Topic (section)	Finding		Source
Trust and cooperation	Typically increased in face-to-face interaction, compared to e-mail or chat room		Valley, Moag, and Bazerman (1998), Frohlich and Oppenheimer (1998), Naquin, Kurtzberg, and Belkin (2008), Rocco (1998), Bochet, Page, and Putterman (2006)
	Typically increased in face-to-face interaction, compared to audio communication		Valley, Moag, and Bazerman (1998), Brosig, Ockenfels, and Weimann (2003)
	Not increased in face-to-face interaction, compared to video conferencing		Brosig, Ockenfels, and Weimann (2003), Bos et al. (2001)
Inducement for increased trust and cooperation	Smiling	Causes part of the increase	Scharlemann et al. (2001), Centorrino et al., 2011
	Eye contact	Causes trust to increase (share of contribution of face-to-face effect unclear)	Burnham and Hare (2007), Bateson, Nettle and Roberts (2006), Haley and Fessler (2005), Rigdon et al. (2009)
	Lie detection	Evidence negative or inconclusive	Wang, Spezio, and Camerer (2010), Frank, Gilovich, and Regan (1993), Bond et al. (1985), Ockenfels and Selten (2000)
Coordination	Without a conflict between social and individual rationality, e-mail works as well as face-to-face contacts		Frohlich and Oppenheimer (1998), Arunachalam and Dilla (1992, 1995)
Spontaneity	Presumably increased in face-to-face situations, which does not increase efficiency	Oechssler, Roider, and Schmitz (2008), Sutter, Koche,r and Strauß (2003), Cappelletti, Güth, and Ploner (2011)	

Second, while face-to-face communication as such is often more efficient than electronic alternatives once it takes place, there are certain launching costs, so to speak, like the time to get to the meeting point; furthermore, possible scale economies of addressing a large number of people within a short time span cannot be utilized. As regional economists have pointed out, this is not necessarily a disadvantage of face-to-face communication, as it turns communication into a credible signal: face-to-face conversation is often an investment which only pays off for people who are interested in a long-term relationship. An e-mail (and maybe also even a video conference) is just too cheap to signal commitment (Leamer and Storper 2001; Storper and Venables 2004, 356).

The idea that face-to-face communication is a means to signal commitment could be implemented very directly in the laboratory. For example, let subjects have the choice whether to communicate cheaply via e-mail at a distance, or to carry the costs of meeting each other, although communication would still not be face-to-face but via artificially "expensive" e-mail. (More generally, endogenizing subjects' choice of communication media, something which so far has not been implemented in economic experiments, might reveal the experiences and expectations concerning communication in certain situations.[23])

A third point raised in the regional economics literature is that of "buzzing" and, more generally, interaction involving some random, unplanned, and undirected communication. This does not in any obvious way lend itself to a controlled laboratory experiment, but benefits are large for researchers discovering a less obvious way of investigating a phenomenon that Scitovsky (1986, p. 70–71) described as follows: "Not surprisingly, perhaps, economists are human. They sometimes do and sometimes do not find what they are looking for, but very seldom they find what they are *not* looking for." The same can be said about other humans. Yet finding out what one was not looking for can be very important and probably happens more often if communication is informal, unstructured, and face-to-face (e.g., Kraut, Egido, and Galegher 1988). Buzzing is a related concept, especially when considered as members of a "sectorally specialized networks" (Rodríguez-Pose and Crescenzi 2008, 383) benefitting from, and seeking, the geographic proximity of members of other such networks. (The term buzz was prominently introduced into regional economics by Storper and Venables 2004; see Vang 2005, or Asheim, Coenen, and Vang 2007, for a critical discussion.)

Discussion

There is a reason for the substantial research desiderata mentioned in the previous section: it was not the explicit purpose of the experiments covered in this overview to contribute to regional science.[24] Experimental economists are not different from theorists in that they often engage in solving "internal puzzles," that is, their research is inspired by other experiments or by a theoretical background that is typical for experimental economics, namely game theory and behavioral economics. Nevertheless, some experiments look as if they had been designed deliberately for

investigating questions that regional economists might have. The results, referred to in the section Experimentally Detecting Conditions and Reasons for Increased Trust and Cooperation subsection, suggest that for a new technology to be considered as a workable substitute for face-to-face contacts, it should at least be able to transmit smiles and continuous eye contact. As pointed out in Experiments that Regional Economists Would Like to See section these should be considered as necessary, rather than sufficient, conditions for a "death of distance." Yet knowing at least some necessary conditions is already useful. Consider a new alternative to videoconferencing: Avatar meetings on platforms such as Second Life (www.secondlife.com). A series of standard economics experiments has been replicated by Chesney, Chuah, and Hoffmann (2009) on Second Life, yet without a nonvirtual face-to-face control group (also see Atlas and Putterman 2011). Similar research might be done with smiling and eye-contacting variants of the Second Life avatars, just like the (much more expensive) "robotic telepresence" devices (www.robodynamics.com).

If face-to-face contacts do lose their importance as drivers of agglomeration, other reasons for regional concentration will remain intact. A small but important stream of literature tries to quantify the relative importance of the mechanisms at work (e.g.,, the extent to which workers' urban wage premium is realized not immediately after moving from a rural area into a city measures the importance of learning from others, see Heuermann, Halfdanarson, and Suedekum 2010, 4–5). At least for professions with intensive videoconferencing, we should expect a decline of the relative contribution of learning to agglomeration economies.

For explaining agglomeration economies, face-to-face contacts are one of a number of forces, hence if this particular force becomes a bit weaker due to the "death of distance," all taken together the effect might be small. This caveat does not hold for another question: given that for a certain task or project, the firm had decided to cooperate with a firm in another city, which firm in which city will be chosen? Many Marshallian externalities will be zero as soon as the two cooperating firms are in different cities, no matter how far away from each other. However, transaction costs matter. Let us assume that ten interfirm meetings are necessary, and let travel costs to Farawaycity be twice as high as travel costs to Nearbycity. If all meetings must be face-to-face, then this constitutes a good reason to mainly work with firms from Nearbycity. If only one kick-off meeting must be held face-to-face, while the other meetings can now be held via the Internet at zero cost, then still travel costs to Farawaycity are twice as high, but the absolute cost advantage is lower, and other criteria (such as other cost components) become decisive. Hence, we can expect to observe strong agglomeration economies within cities and at the same time an approximate "equalization of distances" between cities.

Of course, one might wonder whether the extent to which these effects have an impact on location decisions is limited by the extent to which managers are aware of them. We can safely presume that the basic management problems arising from virtual communication can be intuitively understood,[25] and they are discussed in the

popular business literature (Handy 1995). Furthermore, from an evolutionary perspective, it might suffice if those firms that act as if like they were aware of the problem survive with a higher probability than others.

There is no clear 1:1 correspondence between the stylized economic experiments and a set of useful categories of economic situations. One such set would be: horizontal intrafirm (teams), horizontal interfirm (projects), vertical interfirm (supplier–buyer). In all of these real-world situations, one might encounter ultimatum games, prisoners' dilemmas or other kinds of game that have been reconstructed in laboratory experiments. However, there is one important type of game which differs from the aforementioned ones in that players will *not* find other players' behavior unfair or exploitative, namely pure coordination games or variants such as Frohlich and Oppenheimer'' (1998) ''impartial PD'' described in Measuring Increased Trust and Cooperation in Face-to-Face Experiments subsection above. This corresponds to a number of important real-world constellations—for example, the case where motivation to contribute is intrinsic and hence free riding not an attractive option, but where efforts lead to nothing if they are entirely uncoordinated. Open source software programming, often highly decentralized, is a case in point.

However, even if different people share an interest, they need to be aware of each others' existence before they can start to coordinate. It was the modest purpose of this survey to introduce those experimentalists and regional scientists who are working on face-to-face communication to each other. If experimentalists take up the current interesting challenges from regional economics, and if regional economics theorize in response to experimental results, a broad variety of exciting new insights is bound to emerge.

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Notes

1. Popular images might be deceiving. Is he really dressed in a cloak with a large hood, and does he (or she) really wear a scythe? Last time I saw someone like that, it was just a peaceful mower. And where others have seen the death of distance, it turned out to be only peaceful e-mail communication.

2. For attempts to investigate possible effects of face-to-face interaction by role-playing, see, e.g., Sheffield (1989), Moore et al. (1999), Drolet and Morris (2000) and the references given therein on p. 27 and 29.
3. For example, unlike psychologists, economists as a basic principle do not deceive subjects (concerning rules, payoffs, opponents' incentives, etc.). In one paper covered in this survey, otherwise an economic experiment, this rule is broken for the purpose of gaining more relevant observations. Furthermore, economists often give subjects the opportunity to learn, using training rounds and/or repetitions of the same experiment to minimize the possibility that subjects misunderstand the rules and hence do not play the game actually intended by the experimenter. However, if one-shot experiments are really simple, their results can be meaningful, as is surely the case for those discussed below.
4. This does not hold for *artifactual field experiments*, which differ from laboratory experiments only in that subjects are not students, but typically intentionally chosen professionals like chess players or stock brokers. *Framed field experiments* let subjects act in a field context (e.g., like a trade fair), but still they are aware that they take part in a study. This classification is used by John List for his online bibliography of field experiments at www.fieldexperiments.com.
5. The popularity and publication success (see, e.g., Harrison and List 2004) of natural field experiments can be explained by the fact that they combine both, control like in experiments and external validity like in field studies.
6. A further example of a spurious relationship in our context is suggested by Döring and Schnellenbach (2006, 380): "Information about novelties flows more easily among agents located within the same area thanks to social bonds that foster reciprocal trust and frequent face-to-face contacts." If it would really be a confounding factor that is responsible for a relationship between trust and face-to-face contacts, this could be demonstrated in a suitable experiment.
7. In the experiment, described below, subjects in the role of seller earned $\$ 0.2(\text{sale price} - V)$, buyers earned $\$ 0.2(1.5 \cdot V - \text{sale price})$. Additionally, the authors report on a very similar experiment without monetary incentives, of which the main results are in line with the ones described here.
8. This holds true for 57.1 percent of all face-to-face negotiations (out of $n = 21$), which is significantly higher than the share of 22.2 percent reached for written communication. Yet the difference is not significant for comparison of face-to-face and telephone communication (38.1 percent). The latter, however, showed a significantly higher share of buyer's losses (47.6 percent) than face-to-face and written communication (23.8 percent and 25.9 percent, respectively). With an impasse rate of 52 percent, written communication brought negotiations closest to the theoretical prediction.
9. Most straightforward is Fetchenhauer, Groothuis, and Pradel (2010), who find that short video clips showing allocators in the dictator game help students to guess the amount of money they give to an anonymous receiver. However, there was no strategic interaction at all between those who were videotaped and those watching them. Belot, Bhaskar, and van de Ven (2012) obtained a similar result with video clips from a TV game show in which two people were playing a prisoner's dilemma variant.

10. Field studies suggest that face-to-face interaction might be part of a multiperiod communication process that also involves e-mails or phone calls for the less complex parts of a joint project, for example, hence face-to-face communication might even be a complement for phone calls (Charlot and Duranton 2006; Torre and Rallet 2005, 53–54).
11. I owe this point to Jeannette Brosig.
12. In each round, everyone received thirteen tokens and "invested" some of these, payoff being proportional to own number of tokens divided by total number invested by the group. In the Nash equilibrium, everyone invests ten tokens. The sum of payoffs is highest when everyone invests six.
13. In addition to their public good experiment, Naquin, Kurtzberg, and Belkin (2008) performed a second study, yet without monetary incentives and with loaded instructions: Subjects were put into the shoes of either a large commercial fisher, a small commercial fisher, a recreational competitive fisher, or a recreational tour fisher. They were asked by how much they would be willing to reduce their harvesting from a shark population, which would be depleted if everyone remained at his or her (group-specific) default value. Nonanonymous e-mail communication reduced the group harvest to 71 percent of the default value, compared to 55 percent for face-to-face communication. The interesting point about this study is that subjects were questioned about their perceived justification for being uncooperative, which is stronger for e-mail communication, contributing to the lower level of cooperation for this mode of communication. A replication with monetary incentives and a design without role-playing would be highly desirable.
14. Bos et al. (2001) also employed an audio communication treatment, with a cooperation level between that of chat room communication and that of videoconferencing. Due to small sample size, the respective differences are insignificant, however.
15. Rigdon et al. (2009) also play a dictator game, using a very schematic face—three dots that look remotely like a face ordered like the corners of an equilateral triangle—on the money allocation sheets where dictators in the experimental group had to state their decision. Sheets for the control group were similar, except that the three dots were rotated 180 degree (∴). Transfers by male dictators were about twice as high in the experimental group, compared to males from the control group, while no such effect was observed for females. This is puzzling, as no gender effect was found by Haley and Fessler (2005). As the stimulus used by Rigdon et al. (2009) is much more abstract than in the other studies discussed in this section, the gender effect might be a specific curiosity with low importance for real face-to-face interactions.
16. Which does not mean audio communication like in the study of Brosig, Ockenfels, and Weimann (2003) discussed above; subjects were just separated with a cardboard partition, which might contribute to differences between these two studies' results.
17. A stunningly simple laboratory demonstration that some—but not all—people are lying, given opportunity and incentives, is provided by Fischbacher and Heusi (2008). Some people display an aversion to lying as demonstrated by Gneezy (2005); for a discussion of the underlying motivation, see Hurkens and Kartik (2009), Charness and Dufwenberg (2010), or Chakravarty, Ma, and Maximiano (2011).

18. Like Sutter, Kocher, and Strauß (2003), Cappelletti, Güth, and Ploner (2011) did not match responders under time pressure with proposers enjoying a longer decision time, or vice versa. As a result, it is not possible to disentangle two possible effects of time pressure on proposers: first, their way of reflecting the decision task might be directly affected by time pressure. Second, proposers might expect the acceptance rate to be either higher or lower if responders are under time pressure. Hence the finding by Cappelletti, Güth, and Ploner (2011) that time pressure leads to (slightly but significantly) higher offers should provoke further research.
19. While direct experimental evidence is wanting on this conjecture, face-to-face ultimatum bargaining leads to less bargaining breakdowns than completely anonymous interaction, see Roth (1995) and Schmidt and Zultan (2005).
20. The experiment by Brosig, Weimann, and Yang (2004) does have a videoconferencing treatment, but it is missing in this survey, because of it there is no face-to-face treatment for comparison. For two of their trust game variants, however, these authors compare videoconferencing with e-mail communication, as well as with no communication at all. It is noteworthy that videoconferencing performs "better" than e-mail, if the criterion is the likelihood of arriving at an off-equilibrium bargaining result that is a Pareto improvement over the subgame perfect outcome. Likewise, e-mail communication performs better than no communication at all.
21. At least there is already support from a case study on a specific interfirm collaboration: "According to several project members, the group would have needed more face-to-face meetings to solve these problems had it not been for the video meetings." Hildrum (2007, 479). Similarly, Skyrme (1998, 30) writes on videoconferencing at BP: "Many problems at off-shore oil fields have been solved without resorting to jumping into the helicopter as was formerly the case."
22. See Miller and Storper (2008), section 2.1, for a comprehensive discussion and some caveats.
23. I owe this point to an anonymous referee. One step in this direction is Isaac and Walker (1991), who found subjects in a public goods game to be willing to pay for the opportunity to communicate at all, and Eckel and Petrie (2011), who find that many subjects in their experiment are willing to pay in order to see a photo of their partner in a trust game.
24. I am aware of only a small series of experiments that are clearly directly inspired by regional economics, but these are "Hotelling experiments" unrelated to the issue of face-to-face communication, see Brown-Kruse, Cronshaw, and Schenk (1993), Brown-Kruse and Schenk (2000), and Collins and Sherstyuk (2000).
25. See also the anecdotal evidence referred to in note 21.

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