Mistakenly Seeking Solitude

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Connecting with others increases happiness, but strangers in close proximity routinely ignore each other. Why? Two reasons seem likely: Either solitude is a more positive experience than interacting with strangers, or people misunderstand the consequences of distant social connections. To examine the experience of connecting to strangers, we instructed commuters on trains and buses to connect with a stranger near them, to remain disconnected, or to commute as normal (Experiments 1a and 2a). In both contexts, participants reported a more positive (and no less productive) experience when they connected than when they did not. Separate participants in each context, however, expected precisely the opposite outcome, predicting a more positive experience in solitude (Experiments 1b and 2b). This mistaken preference for solitude stems partly from underestimating others' interest in connecting (Experiments 3a and 3b), which in turn keeps people from learning the actual consequences of social interaction (Experiments 4a and 4b). The pleasure of connection seems contagious: In a laboratory waiting room, participants who were *talked to* had equally positive experiences as those *instructed* to talk (Experiment 5). Human beings are social animals. Those who misunderstand the consequences of social interactions may not, in at least some contexts, be social enough for their own well-being.

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Humans are among the most social species on the planet, with brains uniquely adapted for living in large groups (Dunbar, 1998; Herrmann, Call, Hernàndez-Lloreda, Hare, & Tomasello, 2007; Sallet et al., 2011). Feeling socially connected increases happiness and health, whereas feeling disconnected is depressing and unhealthy (Holt-Lunstad, Smith, & Layton, 2010; House, Landis, & Umberson, 1988; Kahneman & Deaton, 2010; Myers & Diener, 1995). Nevertheless, modern life affords many opportunities for connecting with others that are routinely foregone. From trains to cabs to airplanes to waiting rooms, strangers may sit millimeters apart while completely ignoring each other, treating one another as objects rather than as sources of well-being. As Milgram and Sabini (1978) noted in their seminal study of modern urban life,

> The requirements of appropriate social behavior on the subway are, on the face of it, simple. [One] implicit rule . . . discourages passengers from talking to each other. Even though riders are often squeezed into very close proximity, they are rarely observed to converse. (pp. 32–33)

For a species that seems to benefit so much from connecting to others, why would people in close proximity so routinely seem to prefer isolation instead? Why are such highly social animals, at times, so distinctly unsocial?

There are two plausible answers to this apparent social paradox. One is that connecting with a stranger in conversation is truly less pleasant than remaining isolated for a variety of possible reasons. Preferring isolation in the company of random strangers may therefore maximize one's well-being. The other is that people systematically misunderstand the consequences of social connection, mistakenly thinking that isolation is more pleasant than connecting with a stranger, when the benefits of social connection actually extend to distant strangers as well. We designed a series of field and laboratory experiments to test between these two hypotheses, to identify underlying mechanisms and moderators for the behavior we observe, and to examine the broader consequences of distant social connections.

The Pleasure of Disconnection?

Modern life provides overwhelming opportunities for social engagement, and so, social connections have to be regulated. Just like other drive states such as hunger, where people consume food that appears satisfying and avoid food that appears nauseating, people regulate their social drive by connecting with people who seem satisfying (e.g., close others) but avoiding those who seem unsatisfying (e.g., distant others). For instance, a person's overall well-being appears to be driven by the quality of connections with *close* others rather than the quantity of connections with more *distant* others (Coan, Schaefer, & Davison, 2006; Cohen, 2004; Davis, Morris, & Kraus, 1998; King & Reis, 2012; Pinquart & Sorensen, 2003; Williams & Solano, 1983). People often evaluate distant strangers and outgroup members as relatively poor sources of social support—even lacking humanlike mental capacities of rational thought and secondary emotions (Cortes, Demoulin, Rodriguez, Rodriguez, & Leyens, 2005; Haslam, 2006; Haslam, Bain,

Douge, Lee, & Bastian, 2005). When faced with a seemingly unsatisfying stranger as a potential conversation partner, it could be sensible to avoid conversation and remain disconnected instead.

Indeed, at least some people seem to think that remaining disconnected from strangers is quite sensible. In a survey of 203 Amazon.com Mechanical Turk participants, we asked one group of people to report the likelihood that they would talk (yes or no) to a friend and a stranger (in counterbalanced order) in one of four locations (waiting room, train, airplane, and cab). Virtually none of these participants predicted that they would avoid talking to a friend (7%, 0%, 0%, and 0% responded "no" in each context, respectively), but a majority in each context thought they would avoid talking to a stranger (93%, 76%, 68%, and 51%, responded "no," respectively).[1] Few would forgo the potential pleasure of connecting with a friend, but most seem readily inclined to ignore a stranger. This makes sense if talking to a stranger really is less pleasant than sitting in solitude.

Mistakenly Seeking Solitude?

Connecting with strangers may not bring the same long-term benefits as connecting with friends, but our interest is in whether connecting with a stranger is less beneficial than remaining isolated altogether. It is possible that people misunderstand the consequences of distant social interactions such that people avoid talking to strangers because they expect it will be less pleasant than remaining isolated, when the opposite may actually be true.

Several existing findings suggest some misunderstanding about the consequences of social connection. In one series of experiments (Mallett, Wilson, & Gilbert, 2008), White participants expected to have more pleasant interactions with other White participants than Black participants. In reality, they had equally pleasant conversations with both groups. In another series (Zelenski et al., 2013), extraverts believed they would have a more pleasant experience interacting with another person in an extraverted fashion than introverts predicted. In fact, both introverts and extraverts reported a more positive experience while acting extraverted in a social interaction than while acting introverted.

In a third series of experiments (Dunn, Biesanz, Human, & Finn, 2007), undergraduates in one experiment expected to feel worse while waiting to interact with an opposite-sex stranger than while waiting to interact with their dating partner, but there were no mood differences among people actually waiting to interact with an opposite-sex stranger versus their romantic partner. A second experiment involving an actual 4-min interaction with either a romantic partner or an opposite-sex stranger found similar results. Although it is unclear whether these two results tell us something general about the hedonic benefits of interacting with strangers or something more specific about the pleasure of opposite-sex flirtation, they do suggest that people might misunderstand the pleasure of interacting with strangers and therefore mistakenly prefer isolation.

This misunderstanding of the actual consequences of social connection could take at least two different forms. Research on the impact bias (Gilbert, Driver-Linn, & Wilson, 2002) predicts a relatively mild misunderstanding in which people might expect that connecting with a stranger in conversation will be more negative than remaining isolated but that it will not be quite as negative in reality as they anticipate (e.g., Dunn et al., 2007;

Mallett et al., 2008). Talking to a stranger may be no worse (or only slightly worse) than remaining isolated.

A second pattern could reflect a more extreme misunderstanding, not just a mistake in the magnitude or duration of an effect but a mistake in the actual valence of an effect. In particular, in contexts that actually require interactions with others, people are happier when told to act extraverted—to be more assertive, adventurous, energetic, and talkative—than when told to act introverted (Fleeson, Malanos, & Achille, 2002; McNiel & Fleeson, 2006; Sandstrom & Dunn, 2014; Zelenski, Santoro, & Whelan, 2012). Critically, these methods either utilize diary or experience sampling studies in which the targets and contexts of the interactions are unclear (people are most likely interacting with friends in social contexts) or experiments that require engaging with a stranger (or a group of strangers) to complete a task. Nevertheless, they suggest it is possible, even in situations where social interaction is neither required nor the norm (such as on trains, on buses, or in waiting rooms), that engaging a stranger in conversation may actually be *more* pleasant than remaining isolated. This suggests a more profound misunderstanding of social interactions: Members of a highly social species may ignore other people because they expect that connecting with a stranger will be more negative than remaining isolated when in fact the exact opposite pattern is true.

Overview of Experiments

We conducted nine experiments, in both field and laboratory settings, to at least partly explain an apparent social paradox: why people who benefit greatly from social connection nevertheless prefer isolation amongst strangers. We began our research in two contexts where strangers come in very close proximity but almost never connect: on commuter trains and public buses. In each context, we first asked participants in one experiment (Experiments 1a and 2a) to talk to a stranger, sit in solitude, or do whatever they would normally do, to measure the actual consequences of distant social engagement versus isolation. This between-participants design allows us to measure whether interacting with a distant stranger is indeed less pleasant than remaining isolated in ecologically valid contexts (Keren & Raaijmakers, 1988).

In another experiment in each context (Experiments 1b and 2b), we then asked a separate group of participants to predict their experiences in the same conditions (talking to a stranger, sitting in solitude, or doing whatever they would normally do) to measure people's expectations of the outcomes of these conditions as precisely as possible. This within-participant comparison allows us

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[1] This survey included another between-participants condition that asked participants to remember their past behavior in each of the four contexts (waiting room, train, airplane, cab) to see whether their memory for their past interactions matched their predictions. Overall, they did. Participants in the memory condition considered the last time they were in each of the contexts with a friend and a stranger (counterbalanced order) and indicated whether they engaged in conversation ("yes," "no," "do not remember," or "not possible in this situation"). Simply looking at those who said "no," few participants again avoided talking to a friend (6%, 6%, 11%, and 12%, in each context, respectively), but many avoided talking to a stranger (86%, 86%, 59%, and 43%, respectively). The increased frequency of talking to a stranger in a cab—that is, to the driver—in both anticipated interaction and recalled interactions is a result we turn to in Experiments 4a and 4b.

to examine people's explicit theories about the consequences of these actions in direct comparison against each other (Hsee, 2000).

These field experiments tested whether isolation is truly preferable to connecting with a stranger and, if not, whether people's seeming preference for isolation among strangers comes from a mistaken belief that isolation will be a more positive experience than connecting with a stranger. Experiments 3a–4b tested between different possible mechanisms for the results observed in Experiments 1a–2b. Finally, Experiment 5 was a laboratory experiment that addressed two concerns with the field experiments: whether the effects of social connection affect only those we instructed or if they are contagious and extend to people who are *talked to* or *ignored* as well, and whether free choice affects the consequences of social connection. Overall, these experiments tested whether people are appropriately social in their everyday lives or perhaps not social enough for their own well-being.

Experiments 1a and 1b: Trains

Method

Experiment 1a procedure. Two research assistants recruited commuters just before boarding inbound morning commuter trains, surreptitiously recording their gender and ethnicity. The research assistants recruited people walking alone to the train platform in order to make it easier for participants in the connection condition to talk with a stranger (rather than with friends, family, or acquaintances they happened to be traveling with that day). One hundred eighteen commuters at the Homewood, Illinois, Metra station agreed to participate. We chose this particular station partly because it is one of the first on the rail line for the express trains, meaning that passengers are boarding a nearly empty train during the times when we conducted our experiments and would therefore be able to begin the experiment sitting alone (as is the strong norm) rather than choosing a stranger to sit next to.

Research assistants randomly assigned commuters to one of three conditions: connection, solitude, or control. Participants in the connection condition were told, "Please have a conversation with a new person on the train today. Try to make a connection. Find out something interesting about him or her and tell them something about you. The longer the conversation, the better. Your goal is to try to get to know your community neighbor this morning." Participants in the solitude condition were told, "Please keep to yourself and enjoy your solitude on the train today. Take this time to sit alone with your thoughts. Your goal is to focus on yourself and the day ahead of you." Finally, participants in the control condition were told, "Please do not make any changes to your normal commute. Your goal is to do as you would normally do." Although this condition is not an ideal control because participants are free in this condition to talk to others (almost certainly with friends) or sit alone in solitude, we nevertheless felt it useful to include for the sake of completeness.

Commuters then received the experimental survey and the Ten Item Personality Inventory (TIPI; Gosling, Rentfrow, & Swann, 2003) in a stamped and addressed envelope along with a $5 gift card as compensation. They were asked to follow the instructions (to talk, sit in solitude, or have their normal commute) during the

train ride and then open the envelope and complete the survey at the end of their train ride and mail it back.

The first page of the survey asked participants to report the time that they were completing the survey and to mark whether or not they did the following during their morning commute: talk to someone (friend or stranger not specified), talk on the phone, read, sleep, think, work, or other. Participants then completed three measures to assess the overall positivity of their commute: how happy and how sad they felt after their commute on response scales ranging from 0 (*Not at all happy/sad*) to 6 (*Very happy/sad*) and how pleasant their commute was, compared to their usual commute, on response scales ranging from −3 (*Much less pleasant*) to 3 (*Much more pleasant*). Because engaging a stranger in conversation may come at a cost to other activities one might do on a commuter train (such as work), we then asked participants to report how productive their commute was, compared to their usual commute, on a scale ranging from −3 (*Much less productive*) to 3 (*Much more productive*).

On the second page of the survey, we asked participants in the connection condition to write "as much as you can remember about the person with whom you spoke (name, ethnicity, age, occupation, etc.)," to estimate the length of their conversation (in minutes), and then to report how pleasant their conversation was on a scale ranging from 0 (*Not pleasant at all*) to 6 (*Very pleasant*) and their overall impression of their conversation partner on a scale ranging from −3 (*Very negative*) to 3 (*Very positive*). Finally, participants in all conditions marked the activities they *normally* do on their commute from a list of the same seven activities from the first page of the survey. The TIPI was always included after the experimental survey in the envelope.

Experiment 1b procedure. Research assistants recruited commuters in the same manner as Experiment 1a. One hundred five commuters from the Homewood, Illinois, Metra station agreed to participate. Those who agreed to participate received a stamped and addressed envelope containing the TIPI and the experimental survey in randomly determined order, along with a $5 gift card as compensation.

The experimental survey described the procedure of the actual experience experiment (Experiment 1a) as closely as possible. Participants imagined following the instructions from the control condition and, subsequently, the connection and solitude conditions in counterbalanced order. Therefore, the design included the same three conditions as Experiment 1a but manipulated within participants.

In the control condition, participants read,

> We would like you to imagine that you are participating in a study about commuting on the train. Imagine that, as you walk to the platform to catch your train in the morning, you see a student standing near the platform who asks you to participate in a study. Imagine that, just like you did today, you agree to participate and sign a consent form. Imagine that the student gives you these instructions for the study: "Please do not make any changes to your normal commute. Your goal is to do as you would normally do." Imagine that you follow these instructions and then complete a questionnaire at the end of your commute.

The connection and solitude conditions contained the same information except that the instructions were changed to match the actual instructions given in Experiment 1a. Following the

instructions for each condition, participants predicted their mood (how happy and how sad they would feel after a commute in that condition) and how pleasant and productive their commute would be compared to usual on the same rating scales described in Experiment 1a. At the end of the experimental survey, participants marked the activities they normally do on their commute from the same list of seven activities in the Experiment 1a survey.

Results

To obtain an overall measure of positivity, we first calculated positive mood (happy minus sad), then standardized positive mood and pleasantness, and then averaged those two measures into a single index. Positive mood and pleasantness were significantly correlated in all experiments reported in this manuscript (rs = .36, .55, .48, .55, .42, .59, .49, .52, .62, Experiments 1a–5, respectively, ps < .01).

Experiment 1a. Eighty-nine percent of participants who took a survey returned it in the mail, with no differences between experimental conditions (χ^2 = 2.36, p = .31). Of those, eight reported being unable to follow the instructions (one in the solitude condition, seven in the connection condition). Of these seven in the connection condition, all reported being unable to talk because nobody sat next to them on their train ride (a distinct possibility later in the mornings when the trains are not full). This left a final sample of 97 commuters in the following analyses (M_{age} = 49 years, SD_{age} = 13 years, 61% female).

Experiences. A one-way analysis of variance (ANOVA) showed that reported positivity of the commute experience varied across the three experimental conditions, $F(2, 96)$ = 3.10, p = .05, η^2 = 0.06.

Talking to a stranger on the train was not systematically unpleasant. In fact, participants in the connection condition reported having the most positive experience out of all three of our experimental conditions (see Figure 1). Most important, participants in the connection condition reported having a significantly more positive experience than participants in the solitude condition, $t(94)$ = 2.49, p = .02, d = 0.63.[2]

Because our control condition allows a wide range of activities, including talking to friends as well as sitting in solitude, the data from the control condition are somewhat difficult to interpret. Indeed, six participants in the control condition talked with a friend during the commute (based on the content of their description of the conversation), and these six participants reported a mean positivity comparable to participants in the connection condition (M = .20, SD = .77). Overall, ratings of positivity in the control condition fell almost perfectly in between the solitude, $t(94)$ = −1.22, p = .22, d = −0.25, and connection conditions, $t(94)$ = 1.39, p = .17, d = 0.29. Studies 2a and 4a allow for additional comparisons with a similar control condition to provide a more informed assessment of how connection versus isolation compares to whatever else participants might normally do in these contexts.

The positive experience that participants had in the connection condition did not appear to come at a significant cost to their reported productivity as there were no significant differences on this measure between the three experimental conditions, $F(2, 96)$ = 1.22, p = .30. In particular, reported productivity in the
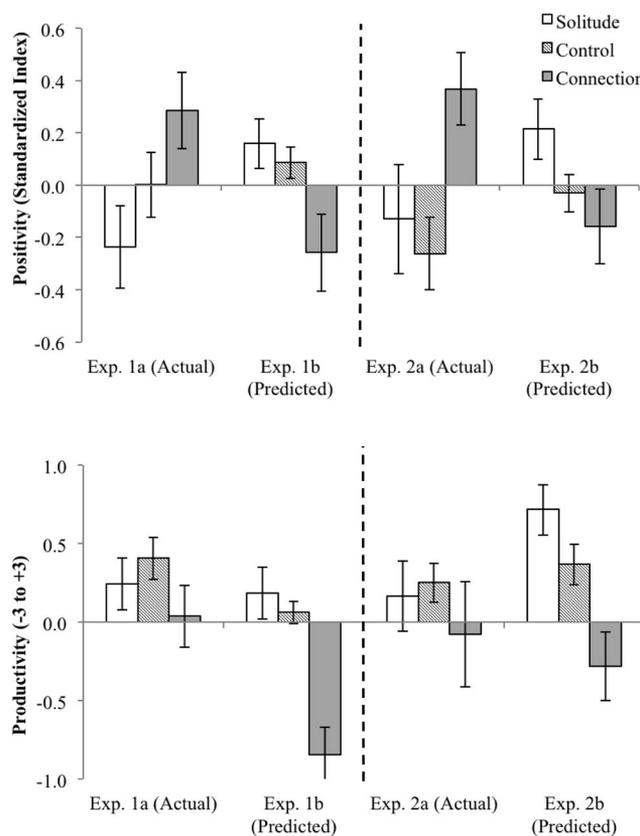

Figure 1. Actual and predicted positivity (top panel) and productivity (bottom panel) from Experiments 1a, 1b, 2a, and 2b. Error bars represent the standard error around the mean of each condition.

solitude and connection conditions was nearly identical (Ms = .24 and .04, SDs = .97 and 1.02, respectively), $t(94)$ < 1. Whether this null effect on productivity comes from people not being very productive on the train to begin with or from redefining what it means to be productive depending on their experimental condition is unclear. What is clear is that participants who talked to a stranger did not leave the train feeling that a potentially productive commute was wasted.

Conversation characteristics. Additional survey results showed that participants in the connection condition talked for an average of 14.2 min (SD = 11.3 min), had quite pleasant conversations overall (M = 5.1, SD = 0.9), and reported a positive impression of their partner (M = 2.2, SD = 1.1). Positivity of the commute did not significantly correlate with either the reported pleasantness of conversation or the impression of the conversation partner (ps > .10). Of course, these null effects do not mean that the quality of a conversation is not related to a person's evaluation of his or her experience in that conversation. Instead, none of our participants reported having a truly negative con-

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[2] The effect of talking compared to sitting in solitude on positivity of commute experience remained significant after including the eight commuters who did not follow instructions, $t(102)$ = 2.11, p = .04. It also remained significant after controlling for normal train activities in a linear regression (β = 0.24, p < .05).

versation with a truly unpleasant person, and hence, there was little variability in participants' evaluations to detect a relationship with their resulting evaluations of the experience. We suspect these generally positive conversations are not an accident but rather reflect a general feature of short-term interactions that we discuss further in the General Discussion. Length of the conversation, however, did correlate with positivity ($r = .43$, $p = .03$). People liked their conversation partners, had pleasant conversations, and had more positive commutes the longer their conversations lasted.

Personality. The Appendix shows the correlations between participants' personality as measured by the TIPI and positivity of commute in each of the experimental conditions. There are few significant correlations with our experimental conditions. Perhaps more important, the difference in positivity between the connection and solitude conditions remained significant even after controlling for the Big Five personality factors in a linear regression ($\beta = 0.31$, $p < .01$), and the difference between these experimental conditions did not interact with any of the personality factors ($ps > .10$). As is often the case, strong situational manipulations tend to influence people similarly, regardless of their personality type (Bem & Allen, 1974; Mischel, 1973).

Experiment 1b. Sixty-six commuters ($M_{age} = 44$ years, $SD_{age} = 13$ years, 66% female) completed the survey during their train ride and mailed it back (a 63% response rate).

Predicted experiences. If commuters in Experiment 1a had a more positive experience connecting with a stranger, then why do people in such circumstances so rarely do so? Experiment 1b suggests an answer. Commuters predicted precisely the opposite pattern of the actual experiences observed in Experiment 1a, $F(1, 64) = 4.69$, $p = .03$, $\eta^2 = 0.07$, not simply mispredicting the magnitude of these experiences but mispredicting their relative valence. In particular, Figure 1 shows that commuters predicted that they would have a significantly less positive commute in the connection condition than in both the solitude condition, $t(64) = 2.46$, $p = .02$, $d = 0.41$, and the control condition, $t(64) = 2.18$, $p = .03$, $d = 0.45$. Compared to the actual experiences of participants in these conditions, connecting with strangers is *surprisingly* positive.

Not only did participants expect to have the least positive commute in the connection condition, they also expected to have the least productive commute $F(1, 64) = 20.69$, $p < .01$, $\eta^2 = 0.24$. Participants predicted that they would have a significantly less productive commute in the connection condition than in either the solitude condition, $t(64) = 5.04$, $p < .01$, $d = 0.74$, or the control condition, $t(64) = 2.72$, $p < .01$, $d = 0.83$. Overall, connecting with a stranger was seen as being an unpleasant and unproductive use of time, neither of which appeared to be true among those who actually did so in Experiment 1a.

Personality. The Appendix again shows the correlations between participants' personality as measured by the TIPI and positivity of commute in each of the experimental conditions. Some significant and intuitive correlations emerged among participants in the connection condition. The predicted difference in positivity between the connection and solitude conditions remained significant even after controlling for the Big Five personality factors in a repeated measures ANOVA, $F(1, 55) = 4.77$, $p = .03$, $\eta_p^2 = 0.08$.

The difference between these experimental conditions did not interact with any of the personality factors ($ps > .05$). We continue testing for potential personality moderators in the following experiments.

Discussion

Commuters on a train into downtown Chicago reported a significantly more positive commute when they connected with a stranger than when they sat in solitude, and yet they predicted precisely the opposite pattern of experiences. This pattern of results demonstrates a severe misunderstanding of the psychological consequences of social engagement. This mistake is particularly unfortunate for a person's well-being given that commuting is consistently reported to be one of the least pleasant experiences in the average person's day (e.g., Kahneman, Krueger, Schkade, Schwarz, & Stone, 2004). This experiment suggests that a surprising antidote for an otherwise unpleasant experience could be sitting very close by.

Experiments 2a and 2b: Buses

Experiments 1a and 1b provide an ecologically valid setting for our predictions (particularly compared to a university laboratory setting with undergraduate participants; Henrich, Heine, & Norenzayan, 2010), but any field setting readily calls to mind idiosyncratic features that limit its generalizability. In addition, participants in Experiments 1a and 1b completed the questionnaires at the end of their commute, at a time when we could not control the participants' context or ensure 100% response rates. We therefore sought convergent evidence in Experiments 2a and 2b in a different field experiment (public buses) with final surveys completed in a laboratory.

Method

Experiment 2a procedure. Participants recruited from a laboratory participant pool in downtown Chicago enrolled in the experiment by completing an online survey. The survey explained that they could only participate if they took public transportation to the laboratory in the morning, that they would receive a phone call on the morning of their commute to the laboratory, and that they would be compensated $5 for their time. Eligible and interested participants then signed an electronic consent form to enroll; reported their age, gender, and ethnicity; and indicated the time they preferred to be called and the phone number to call. Eighty-seven participants answered the telephone when called on the morning of the experiment.

When the experimenter called participants, she asked them to follow the same control, connection, or solitude condition instructions from Experiment 1a (randomly assigned), modified slightly to accommodate differences in the change of context. She subsequently asked participants to brainstorm how they could follow the instructions, in an attempt to increase the odds that they would actually do so. Once in the laboratory after their commute, participants completed a computerized survey with the same questions as Experiment 1a. Participants also indicated what mode of public

transportation they had used to get to the laboratory. They received $5 for their participation.[3]

Experiment 2b procedure. Sixty-one people who had previously completed studies at the same laboratory as Experiment 2a participated in exchange for entry into a $30 Amazon.com gift card raffle. Demographics collected from a prior survey revealed our sample had a median age between 21 and 25 years and was 71% female. The experimental survey, completed online by participants, described the procedure of the actual experience experiment (Experiment 2a) as closely as possible. Participants imagined following the instructions from the control condition and, subsequently, the connection and solitude conditions in counterbalanced order. The design therefore included the same three conditions as Experiment 2a but was within participants.

In the control condition, participants read,

> You are participating in a study about commuting using public transportation. Imagine that you will take public transportation to the downtown research laboratory. Imagine further that a research assistant will call you in the morning before your commute to give you the instructions for the study. The research assistant gives you these instructions: "Please commute to the laboratory and then complete a short survey after your commute." Imagine that you follow these instructions and then complete a questionnaire at the end of your commute.

The connection and solitude conditions contained the same information except that instructions were added to match the actual instructions given in Experiment 2a. Following the instructions for each condition, participants predicted their mood (how happy and how sad they would feel after a commute in that condition) and how pleasant and productive their commute would be compared to usual on the same rating scales described in Experiment 2a.

Unlike Experiment 2a, participants did not complete the TIPI, and we did not ask about their normal commuting activities.

Results

Experiment 2a. Of 87 participants who answered their phones, 75 commuted to the laboratory and completed the experimental survey (an 86.2% response rate that did not vary by experimental condition, $\chi^2 = 2.89$, $p > .10$). All participants reported following instructions, yielding a final sample of 75 participants ($M_{age} = 27$ years, $SD_{age} = 7$ years, 49% female).

Experiences. As in Experiment 1a, connecting with a stranger was not unpleasant. In fact, Figure 1 shows that participants in the connection condition again reported the most positive experience of our experimental conditions, $F(2, 74) = 4.09$, $p = .02$, $\eta^2 = 0.10$. Participants in the connection condition reported a significantly more positive experience than participants in the solitude condition, $t(72) = 2.14$, $p = .03$, $d = 0.56$,[4] and also a more positive experience than participants in the control condition, $t(72) = 2.69$, $p = .01$, $d = 0.91$.

Again replicating the results of Experiment 1a, we found no significant difference in the reported productivity of the commute, $F(2, 73) = 0.47$, $p = .63$. Reported productivity in the solitude and connection conditions was nearly identical ($Ms = .05$ and $-.15$, $SDs = .88$ and 1.39, respectively), $t(72) < 1$.

Conversation characteristics. Additional survey results showed that participants in the connection condition talked for an average of 9.8 min ($SD = 6.1$ min), had a relatively pleasant conversation

($M = 4.2$, $SD = 1.4$), and reported a positive impression of their partner ($M = 1.9$, $SD = 1.2$). As in Experiment 1a, positivity of the commute was not significantly correlated with the pleasantness of the conversation or the impression of the partner but was positively correlated with the length of the commute ($r = .44$, $p = .03$). The longer participants connected with a stranger, the more positive their commuting experience was.

Personality. The Appendix again shows the correlations between participants' personality as measured by the TIPI and positivity of the commute in each of the experimental conditions. The difference in positivity between the connection and solitude conditions remained significant even after controlling for the Big Five personality factors in a linear regression ($\beta = 0.38$, $p < .01$). The difference between these experimental conditions did not interact with any of the personality factors ($ps > .10$).

Experiment 2b.

Predictions. As in Experiment 1b, participants again predicted having the most negative experience connecting with a stranger and most positive experience sitting in solitude, precisely the opposite pattern of experiences than we actually observed in Experiment 2a, $F(1, 59) = 3.28$, $p = .04$, $\eta^2 = 0.05$. As shown in Figure 1, participants again expected to have a significantly less positive experience in the connection condition than in the solitude condition, $t(60) = 2.03$, $p < .05$, $d = 0.37$. Predictions in the control condition fell roughly in between, not differing from the connection condition, $t(59) = 0.89$, $p = .38$, but differing from the solitude condition, $t(59) = 2.29$, $p = .03$.

Predictions for productivity followed a similar pattern as Experiment 1b, $F(1, 59) = 9.68$, $p < .01$, $\eta^2 = 0.14$, such that participants again anticipated a less productive commute in the connection condition than in both the solitude condition, $t(60) = 3.74$, $p < .01$, $d = 0.66$, and the control condition, $t(60) = 2.72$, $p = .01$, $d = 0.47$.

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[3] To explore whether any effects from the morning commute might affect participants' evaluations of their entire day, we e-mailed them a link to a final survey at approximately 7:00 p.m., promising entry into a raffle to win an iPod Shuffle if they completed the survey. The overall response rate was 92%, with no differences between experimental conditions ($\chi^2 = 1.11$, $p > .10$). This second survey first asked commuters to enter their participant number and then asked if they had talked to someone (or planned to talk to someone) on their commute home that night, with "yes" or "no" response options. Commuters rated how their day was overall on a scale ranging from -3 (*Very bad*) to 3 (*Very good*) and how happy they felt during the day, from 0 (*Not at all happy*) to 6 (*Very happy*). Finally, they entered comments into a textbox if they had any. This survey allowed us to examine whether any effects from the morning commute carried through to color their entire day. Experimental condition affected the extent to which participants talked to someone (or planned to talk to someone) on their evening commute, $F(2, 66) = 3.67$, $p = .03$. Interestingly, participants in the solitude condition reported being directionally more likely to talk to someone during their evening commute than those in the control condition, $t(66) = 1.68$, $p = .10$, and more than those in the connection condition, $t(66) = 2.68$, $p = .01$. Whether this is simply seeking a variety of experiences or learning that sitting in solitude was not very pleasant is unclear. There were no significant differences in participants' evaluations of their overall day by experimental condition ($p > .10$). Having a more pleasant commute in the morning did not have an equally large effect on the rest of participants' days.

[4] The difference on the positivity of the commute between the connection and control conditions of commute experience remained significant after controlling for normal commuting activities in a linear regression ($\beta = 0.29$, $p = .03$).

Discussion

People riding on a public bus had a more positive experience when they talked to a nearby stranger than when they sat in isolation and yet predicted precisely the opposite. This again suggests that people may routinely choose to sit in isolation because they mistakenly think it will be more pleasant than talking to a nearby stranger.

One possible alternative interpretation is that the pleasure of connecting with a stranger in Experiments 1a and 1b comes not from actual social connection but rather from simply violating social norms. That is, people on both the commuter trains and public buses almost never engage strangers in conversation, and violating this norm may be what people enjoy rather than actually connecting with a stranger.

We think two points argue strongly against this alternative. First, many behaviors would violate social norms and would also be unquestionably miserable experiences. For instance, one Internet reader who commented on a news report of Experiment 1a suggested that the experiment's authors might want to tie themselves to the front of the train engine "if they like noise on the train so much." We did not feel compelled to test this norm violation empirically. Riders could also travel naked, lie in the aisle, sing in falsetto, or perform any number of other creative norm violations that need no experiment to confirm that they would be more miserable than sitting in quiet solitude. Violating norms is clearly insufficient for a positive experience. Second, the large empirical literature on conformity actually demonstrates that violating social norms is a systematically *negative* experience that people try to avoid by going along with whatever others are doing (Miller, 2006). If anything, the counternormative nature of talking to strangers in the contexts we studied would seem to work against a positive experience—perhaps leading to social censure from others—rather than in favor of it.

Experiments 3a and 3b: Barriers to Engagement or Bad Experiences?

Experiments 1 and 2 provide an answer to our first research question: People may avoid connecting with strangers and choose isolation instead because they misunderstand the consequences of social interaction, not because connecting with strangers is actually more negative than remaining isolated. Indeed, participants' expectations about the consequences of social interaction were not just mistaken about the magnitude of an emotional experience; they were mistaken about the valence of the experience. Commuters expected that isolation would be more pleasant than connection, when precisely the opposite was true.

These results, however, raise a second major question. If connecting with strangers is truly more pleasurable than sitting in isolation, then why do people not learn this and then behave differently? There are at least two plausible mechanisms for this significant misunderstanding.

First, there may be some barrier that keeps people from connecting with strangers and thereby learning that their expectations are mistaken. The most obvious barrier could come from the existing social norms that discourage connecting with strangers (see also Milgram & Sabini, 1978). These norms could evolve out of a complete disinterest in connecting with strangers, but they

could also evolve even among highly social people who are otherwise very interested in connecting with strangers. People may feel like they are being polite by not intruding on another person, fear being rejected when attempting to start a conversation, or feel that they have little or nothing in common with a stranger. This could create the perfect context for *pluralistic ignorance* (e.g., Prentice & Miller, 1993), whereby people believe that others are less interested in connecting than they are themselves. If other people's silence around strangers is interpreted as disinterest rather than as politeness, then attempting conversation would seem more unpleasant than it would actually be (Miller & McFarland, 1991; Vorauer & Ratner, 1996). Highly social animals could sit in the company of strangers, all be interested in connecting with each other, and yet misread others' silence as disinterest and therefore prefer solitude. This pluralistic ignorance, whereby people consistently think others are less interested in connecting than they are themselves, not only could make an attempted conversation seem unpleasant but could also create a barrier to learning that one's expectations are mistaken.

Second, expectations can be based on memories of past experiences (Wirtz, Kruger, Scollon, & Diener, 2003). Past negative experiences of talking to strangers may be more memorable than positive or even typical experiences (Hastie & Kumar, 1979). If a prediction is biased by one's memory of unusually negative interactions (Morewedge, Gilbert, & Wilson, 2005) or biased by the relative ease of imagining negative outcomes (Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001), then the average actual experience would be better than expected. By this account, it is not a lack of experience connecting with strangers that creates mistaken expectations but rather learning the wrong lesson from past experiences because of biases in imagination or memory.

We tested between these two possibilities—one of misreading others' behavior that creates a barrier to engagement and the other of negativity bias in memory or imagination—in two surveys of train and bus commuters (Experiments 3a and 3b) recruited from the same populations as Experiments 1 and 2, respectively. To test for a barrier to engagement, participants imagined trying to have a conversation and then predicted others' interest and willingness to talk. If people misread others' behavior as disinterest, then participants will systematically think they are more interested in connecting than others are and also underestimate others' willingness to connect. To test whether negativity bias in memory leads to mistaken expectations, participants were randomly assigned to imagine having a positive conversation, a negative conversation, or simply a conversation (the control condition). If expectations are biased by memories for negative past experiences or the ease of imagining negative experiences, then those who imagine simply having a conversation will make predictions more similar to those in the negative conversation condition than in the positive conversation condition.

Method

Experiment 3a procedure. Research assistants recruited commuters in the same manner as Experiment 1a. Eighty-six commuters from the Homewood, Illinois, Metra station agreed to participate. Those who agreed to participate received a stamped and addressed envelope containing the experimental survey, along with a banana as compensation.

In the experimental survey completed during their commute, participants first imagined following the connection condition instructions from Experiment 1a. They reported how interested they thought the other person would be to talk to them and how interested they would be to talk to the other person on a scale ranging from 0 (*Not at all interested*) to 6 (*Very interested*), how difficult it would be to start the conversation on a scale ranging from 0 (*Not at all difficult*) to 6 (*Very difficult*), and then circled the percentage of people they thought would be willing to talk to them (from 0% to 100% in 10% increments).

On the second page, all participants were asked to imagine having a conversation. This conversation varied by random assignment to one of three between-participant conditions. Participants randomly assigned to the control condition imagined simply having a conversation for the duration of the ride. Participants randomly assigned to the positive condition imagined a positive interaction in which the conversation partner was extremely interesting, and those assigned to the negative condition imagined a conversation partner who was extremely uninteresting. Participants then predicted how much they would have in common with the other person on a scale ranging from 0 (*Very little in common*) to 6 (*A lot in common*) and then predicted how stimulating the conversation would be on a scale ranging from 0 (*Not at all stimulating*) to 6 (*Very stimulating*). Last, they completed the same questions from prior experiments about their predicted happiness, sadness, pleasantness, and productiveness of commute.

Experiment 3b procedure. Sixty-five people recruited from the same subject pool as Experiment 2b participated in exchange for entry into a $30 Amazon.com gift card raffle. Demographics collected from a prior survey revealed our sample had a median age between 21 and 25 years and was 77% female. The computerized survey described the procedure of the actual experience experiment (Experiment 2a) as closely as possible, following the same procedure as Experiment 3a, modified where necessary to match the change in context from trains to buses.

Results

Experiment 3a. Sixty-four commuters ($M_{age} = 38$ years, $SD_{age} = 13$ years, 74% female, 74% response rate) completed the survey during their train ride and mailed it back.

As shown in Table 1, participants reported being significantly more interested in talking to others than they thought others were in talking to them, $t(63) = 4.77$, $p < .01$, $d = 0.44$. Participants also predicted that fewer than 47% of commuters would be willing to engage in conversation and that it would be relatively difficult to start the conversation ($M = 4.38$, $SD = 1.69$). In reality, all who returned our surveys reported talking with the person they attempted to connect with, and not a single person reported being rebuffed. The seven who reported being unable to talk in Experiment 1a reported that nobody sat next to them. Commuters appeared to think that talking to a stranger posed a meaningful risk of social rejection. As far as we can tell, it posed no risk at all.

If predicted interactions are biased by memories of especially negative interactions, then predictions in the control condition should mimic those in the negative interaction condition. That is, when people spontaneously think of having a conversation with a stranger, the context that comes to mind should be closer to an explicitly negative interaction than an explicitly positive interaction. As shown in Table 1, they are not. Participants in the negative condition predicted having less in common, $t(61) = -5.08$, $p < .01$, $d = 1.53$; a less stimulating conversation, $t(61) = -3.99$, $p < .01$, $d = 1.18$; and a less positive experience, $t(60) = -2.86$, $p < .01$, $d = 0.93$, than participants in the control condition. In contrast, participants in the positive condition predicted having more in common, $t(61) = 2.41$, $p = .02$, $d = 0.81$; a marginally more stimulating conversation, $t(61) = 1.85$, $p = .07$, $d = 0.57$; a less productive commute, $t(60) = 2.35$, $p = .02$, $d = 0.82$; but not a significantly more positive experience, $t(60) = 0.64$, $p > .10$, than did participants in the control condition. There was no systematic evidence that control condition predictions were negatively biased.

Table 1

Estimates of Own and Others' Sociality and Predicted Positivity and Productivity Following an Imagined Positive, Negative, or Control Conversation (Experiments 3a and 3b)

	Experiment			
Measure	3a: Train		3b: Bus	
Perceived sociality				
Your interest in talking	3.11 (1.70) } $p < .01$		3.18 (1.65) } $p < .01$	
Others' interest in talking	2.44 (1.34)		2.55 (1.48)	
Percentage willing to talk	46.4		44.6	
Predicted conversation				
Positivity				
Positive conversation	0.35 (0.87) } $p = .52$		0.55 (0.64) } $p = .12$	
Control conversation	$p < .01$ { 0.20 (0.58)		$p < .01$ { 0.19 (0.54)	
Negative conversation	−0.47 (0.58)		−0.67 (0.94)	
Productivity				
Positive conversation	−1.26 (1.33) } $p = .02$		−0.14 (1.96) } $p = .78$	
Control conversation	$p = .47$ { −0.33 (0.91)		$p = .17$ { 0.00 (1.27)	
Negative conversation	−0.61 (1.44)		−0.70 (1.64)	

Note. Values are means, with standard deviations in parentheses. The *p* values indicate the statistical significance of pairwise comparisons between conditions. Evaluations of interest in talking (both *yours* and *others'*) are shown on the same scale ranging from 0 to 6, with more positive numbers indicating more interest in talking.

Experiment 3b. Once again, participants reported being significantly more interested in talking to others than they thought others were in talking to them, $t(64) = 3.21$, $p < .01$, $d = 0.41$; predicted fewer than 45% of commuters would be willing to talk with them; and predicted it would be difficult to start a conversation ($M = 4.03$, $SD = 1.8$). Again, all participants who arrived to the laboratory in Experiment 2a reported talking to the person they attempted to connect with. Participants predicted that whether a stranger would agree to talk or not was roughly the same as a coin flip. Again, as far as we can tell, it is closer to a sure thing.

As shown in Table 1, the control condition predictions again showed no evidence of being negatively biased. Participants in the negative condition imagined having less in common, $t(62) = -5.35$, $p < .01$, $d = 1.69$; a less stimulating conversation, $t(62) = -5.33$, $p < .01$, $d = 1.55$; and a less positive experience, $t(62) = -3.89$, $p < .01$, $d = 1.13$, than participants in the control condition. No significant differences emerged in evaluation between the positive and control conditions.

Discussion

These results suggest that misunderstanding the consequences of social connection comes, at least in part, from barriers to engaging in conversation rather than from biased memory for past conversations or biased imagination for conversations once engaged. Solitude seems preferable to connecting with a stranger, it appears, because people interpret others' actions as signs of disinterest and therefore do not engage in the very conversations that would correct their expectations.

This mechanism suggests that misunderstanding the consequences of social interaction comes from inexperience connecting with strangers rather than from negative experiences doing so. This further implies an important moderator of both the misunderstanding of social connection and the preference for isolation observed in Experiments 1 and 2. In particular, it implies that those who are more likely to connect with strangers in conversation, or those in contexts that more readily enable such connections, should obtain the experience necessary to calibrate one's expectations. The mistaken preference for solitude we observed earlier should therefore emerge among those who rarely engage strangers in conversation or in contexts (like those of Experiments 1 and 2) that create barriers to engaging strangers in conversation. We tested this idea in Experiments 4a and 4b by studying a context that more naturally enables conversations between strangers: taxicabs.

Experiments 4a and 4b: Calibrating Through Experience

A person may sit on a train, bus, or plane or in a waiting room without engaging a stranger in conversation because there is no explicit need to do so. This is not true in a taxicab, where a person must engage in at least some minimal conversation with the driver at the beginning to state one's destination. In addition, the private nature of the cab, especially when traveling alone, is more likely to encourage conversation with a driver than a more public setting like a train, plane, bus, or large waiting room. Indeed, in the survey we described in the introduction, 49% of respondents said they would be likely to talk to a stranger (presumably the driver) in a cab. This stands in stark contrast to only 23% of people in

Experiment 1a who said they normally talk to strangers on the train and 0% of people in the control condition who actually reported talking to a stranger. If experience calibrates expectations, then those who routinely talk to their drivers should make more calibrated predictions than those who rarely talk. Those who rarely talk should make the same mispredictions about the experience of connecting with a stranger as in Experiments 1b and 2b.

Method

Experiment 4a procedure. Two research assistants recruited travelers waiting in the taxicab line to leave the Chicago Midway International Airport. Only people traveling alone were asked to participate. One hundred fifty travelers agreed to participate in exchange for a candy bar. When travelers signed the consent form, they also reported how tired they felt on a scale from 0 (*Not at all tired*) to 6 (*Very tired*).

Research assistants randomly assigned participants to the control, connection, or solitude conditions. The corresponding instructions were identical to Experiment 1a except that participants in the connection condition were asked to talk to their cab driver instead of talk to someone else on the train. Travelers received the experimental survey and the TIPI in a stamped and addressed envelope. They were asked to follow the instructions (to talk, sit in solitude, or have a normal ride) during the cab ride and then open the envelope and complete the survey at the end of their cab ride and mail it back. Travelers were also told that they would be entered into a $30 gift card lottery if they returned their survey.

The experimental survey was identical to Experiment 1a except for four initial questions asking participants about the ride (its length and purpose), where they live (in Chicago or visiting, and zip code), and a final question asking how tired they felt (on the same scale as on the consent form). Additionally, the list of normal cab ride activities was slightly modified from our previous experiments: Instead of asking whether or not participants normally talk to "someone," we asked whether they talk to their driver as well as whether they talk to someone else in the cab. The TIPI was always included after the experimental survey in the envelope.

Experiment 4b procedure. Research assistants recruited travelers in the same manner as Experiment 4a. Seventy travelers agreed to participate and received a stamped and addressed envelope containing the TIPI and the experimental survey in randomly determined order, along with a candy bar as compensation.

The experimental survey described the procedure of the actual experience experiment (Experiment 4a) as closely as possible. Participants imagined following the instructions from the control condition and, subsequently, the connection and solitude conditions in counterbalanced order. Therefore, the design included the same three conditions as Experiment 4a but manipulated within participants.

In the control condition, participants read,

> We would like you to imagine that you are participating in a study about commuting in cabs. Imagine that, as you leave the airport to catch your cab, you see a student standing near the cab stand who asks you to participate in a study. Imagine that, just like you did today, you agree to participate and sign a consent form. Imagine that the student gives you these instructions for the study: "Please do not make any changes to what you would normally do during your cab ride today. Your goal is to do as you normally would when you are in a cab."

Imagine that you follow these instructions and then complete a questionnaire at the end of your commute.

The connection and solitude conditions contained the same information except that the instructions were changed to match the actual instructions given in Experiment 4a. Following the instructions for each condition, participants predicted their mood (how happy and how sad they would feel after a ride in that condition) and how pleasant and productive their ride would be compared to usual on the same rating scales described in Experiment 4a. At the end of the experimental survey, participants marked the activities they normally do on their commute from the same list of eight activities in the Experiment 4a survey.

Results

Experiment 4a. One hundred four travelers returned a completed survey (a 69.3% response rate, with no difference by experimental condition, $\chi^2 = 3.64$, $p = .16$). Of those, 11 reported being unable to follow instructions, all of whom were in the solitude condition. This meant that the rate of following instructions varied by experimental condition ($\chi^2 = 21.36$, $p < .01$). Our final sample of people who returned a survey and followed instructions was therefore 93 travelers ($M_{age} = 39$ years, $SD_{age} = 13$ years, 49% female).

We expected that participants would be more likely to report normally talking to strangers—namely, the driver—in cabs than we observed in Experiments 1a–3b. Indeed, unlike the prior experiments where talking to strangers was unusual, a majority of participants (65%) checked the box indicating that they normally talked to their driver.

Experiences. As before, a one-way ANOVA showed that reported positivity of the commute experience varied across the three experimental conditions, $F(2, 92) = 6.84$, $p < .01$, $\eta^2 = 0.13$. As shown in Figure 2, commuters has a significantly more positive experience in the connection condition than in the solitude condition, $t(90) = 3.58$, $p < .01$, $d = 0.93$.[5] In the control condition, participants had a significantly more positive commute than in the solitude condition, $t(90) = 2.75$, $p = .01$, $d = 0.58$, but did not differ significantly from the connection condition, $t(90) = 1.01$, $p = .32$. There was no interaction with whether participants indicated routinely talking to their driver or not, $F(2, 86) = 1.23$, $p = .29$. If anything, the result was directionally larger for loners than for talkers.

There were no differences in reported productivity between any of the three experimental conditions, $F(2, 91) = 1.14$, $p = .32$. Reported productivity in the solitude and connection conditions was nearly identical ($Ms = -.11$ and $-.03$, $SDs = .97$ and $.57$, respectively), $t(90) < 1$, with no interaction by whether participants indicated routinely talking to their driver or not.

Conversation characteristics. Additional survey results showed that participants in the connection condition talked for an average of 19 min ($SD = 10.3$ min), had relatively pleasant conversations ($M = 4.8$, $SD = 1.0$), and had a relatively positive impression of their driver ($M = 1.9$, $SD = 1.2$). Unlike Experiments 1a and 2a, positivity of the commute was correlated with both the pleasantness of the conversation ($r = .60$, $p < .01$) and their impression of the driver ($r = .55$, $p < .01$). Positivity of the commute was also positively correlated with the length of the conversation, but only marginally so ($r = .31$, $p = .10$).
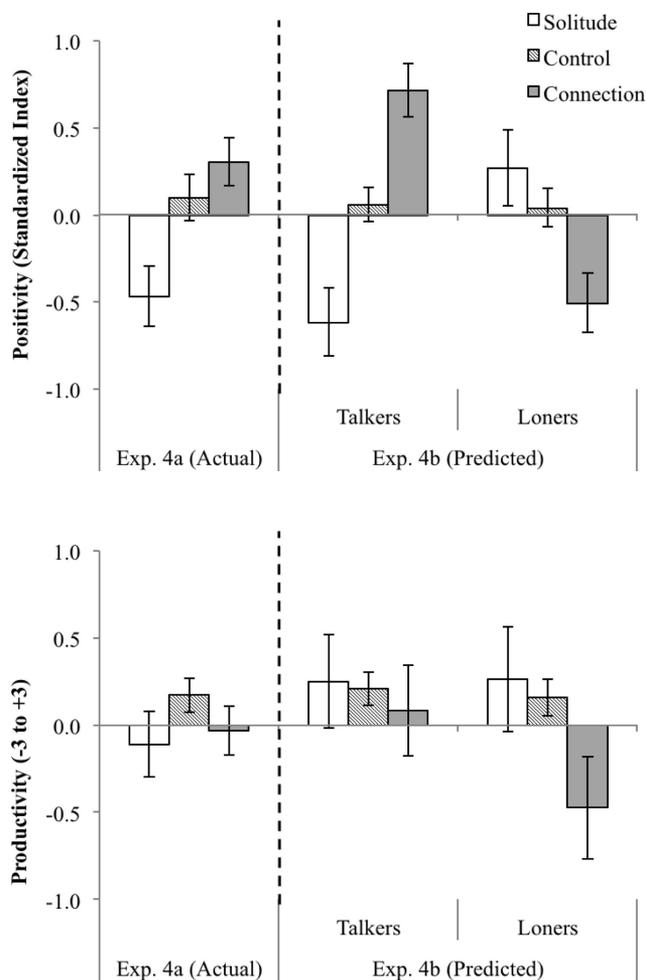

Figure 2. Actual and predicted positivity (top panel) and productivity (bottom panel) from Experiments 4a and 4b. Error bars represent the standard error around the mean of each condition.

Personality. The Appendix again shows the correlations between participants' personality as measured by the TIPI and reported positivity in each of the experimental conditions. The difference in positivity between the connection and solitude conditions remained significant even after controlling for the Big Five personality factors in a linear regression ($\beta = 0.42$, $p < .01$). The difference between these experimental conditions did not interact with any of the personality factors ($ps > .10$).

Additional analysis. To assess another potential cost of conversation compared to isolation, we asked participants to report how tired they were. In particular, conversation may be relatively exhausting and isolation relatively energizing. We found no support for this possibility, as there were no significant differences in reported tiredness between the connection and solitude conditions either before the commute ($Ms = 3.13$ and 2.70, $SDs = 1.38$ and 1.35, respectively), after the commute ($Ms = 3.10$ and 3.07, $SDs =$

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[5] The effect of talking compared to sitting in solitude on positivity of commute experience remained significant after controlling for normal cab activities in a linear regression ($\beta = 0.40$, $p < .01$).

1.69 and 1.49, respectively), or in the difference between them, $t(55) = 1.22$, $p > .10$. Connecting with others does not appear to be especially tiring, nor does solitude in this context appear particularly energizing.

Experiment 4b. Forty three travelers ($M_{age} = 38$ years, $SD_{age} = 13$ years, 52% female) completed the survey during their cab ride and mailed it back (a 61% response rate).

Predicted experiences. Unlike prior experiments, cab riders predicted no overall difference in positivity between the three experimental conditions, $F(2, 82) = 1.42$, $p = .25$. This is consistent with our hypothesis that misunderstanding the consequences of social action would be reduced in contexts where engaging with random strangers is more common. Indeed, 56% of riders checked the box indicating they normally talked to their drivers (talkers), whereas the remaining reported not normally talking (loners). As shown in Figure 2, participants' predictions were significantly moderated by whether they were talkers or loners, $F(2, 82) = 18.31$, $p < .01$, $\eta^2 = 0.30$. Consistent with learning from experience, talkers predicted a significantly more positive experience in the connection condition than in the solitude condition, $t(23) = 5.32$, $p < .01$, $d = 1.52$. Consistent with failing to learn because of inexperience, loners showed the same pattern of predictions observed among train (Experiment 1b) and bus (Experiment 2b) commuters. That is, loners predicted a less positive experience in the connection than in the solitude condition, $t(18) = -2.35$, $p = .03$, $d = 0.93$. Predictions in the control conditions matched what talkers and loners said they normally do (i.e., connection and control conditions were similar for talkers, whereas solitude and control conditions were similar for loners). This stands in contrast to what we observed among the actual experiences of participants in Experiment 4a, in which prior experience as a talker or loner did not affect the actual positivity of the commute.

Cab riders also predicted no difference in productivity among the three conditions, $F(2, 82) = 1.92$, $p = .15$, an effect that did not differ between talkers or loners, $F(2, 82) = 0.80$, $p = .45$.

Personality. The Appendix again shows the correlations between participants' personality as measured by the TIPI and positivity of ride in each of the experimental conditions. The predicted difference in positivity between the connection and solitude conditions remained nonsignificant after controlling for the Big Five personality factors in a repeated measures ANOVA, $F(1, 34) = 0.01$, $p > .10$. More important, the interaction between experimental condition and whether participants normally talked to their drivers remained significant after controlling for personality factors, $F(1, 34) = 22.07$, $p < .01$, $\eta_p^2 = 0.39$. The difference between the experimental conditions did not interact with any of the personality factors ($ps > .10$).

Discussion

When expectations and reality diverge, the gap can be explained either by a failure of learning or a failure of imagination. Experiments 4a and 4b clearly suggest that failing to understand the consequences of social connection stem from a failure of learning. The talkers who reported routinely engaging a stranger in conversation—in this case, a cab driver—had the most calibrated predictions. They expected that connecting with the driver would make the cab ride more pleasant than sitting quietly in solitude.

Talkers' expectations were right: In Experiment 4a, those who talked to their driver reported a more positive experience than those who sat in solitude. In contrast, the loners who did not normally talk with their driver showed the same pattern of misunderstanding observed in Experiments 1 and 2. The loners predicted that they would have a more pleasant experience in isolation than in conversation. Loners' expectations were wrong: in fact, precisely the opposite of what these participants actually experienced in Experiment 4a.

Cab riders' predictions were guided by the amount of information they had from past behavior. Their actual experience, however, was not. Cab riders enjoyed their commute more when they talked to the drivers than when they sat in solitude regardless of whether they normally talked to their driver or not.

Experiment 5: Is the Pleasure of Connection Contagious?

Our experiments suggest that people fail to maximize their own well-being because they mistakenly prefer isolation over connecting with strangers. However, the broader implications of correcting this misunderstanding are unclear for three reasons. First, we could only sample participants who agreed to be in our experiments, not their conversation partners. Talking to a stranger may be surprisingly pleasant, but *being talked to* by a stranger could be predictably miserable, meaning that the overall positivity of a train, plane, cab, or waiting room of talkers could be no higher than one full of loners. Second, all participants in our experiments were, by scientific necessity, told to behave one way or another. For a variety of reasons, participants may only get pleasure from a conversation they were instructed to have—for instance, because they achieved their (imposed) goal to talk, because they find it easier to initiate a required conversation, or because they would perceive themselves to be lonely when voluntarily initiating a conversation where the norm is not to talk. Finally, all participants in the preceding solitude conditions sat in isolation, but in close proximity, to another participant. It is possible that this experience of being "alone together" made the solitude conditions particularly unpleasant. To test this possibility, Experiment 5 included a control condition in which participants simply were left in solitude, alone.

To measure evaluations from both members of the conversation dyad and also to make people feel that they had free choice to talk (instead of feeling required), we created a situation resembling a waiting room at, say, the dentist. Two participants had a 10-min break in the middle of unrelated tasks in a laboratory experiment. During this waiting period, we instructed one participant to either talk to the other person in the room or to remain isolated and avoid conversation. We gave the other participant no instructions. Among those given instructions, some were told they did not have to follow the instructions if they did not want to (although 79 of 80 pairs did so), thereby giving them some sense of free choice over their behavior (e.g., Cooper, 1980). For other instructed participants, their free choice was not emphasized (as in the preceding experiments). We then measured both participants' evaluations of the waiting period.

If being talked to is just as pleasant as initiating a conversation, and if initiating a conversation is just as pleasant under a sense of

free choice as no choice, then explicit instruction or forced conversation is not what makes connecting with a stranger pleasant.

Method

Participants. One hundred seventy-nine people (20 pairs in each of the four randomly assigned dyad conditions and 19 people in the control condition) recruited to a laboratory in downtown Chicago participated in exchange for $6 ($M_{age}$ = 32 years, SD_{age} = 13 years, 39.1% female).

Procedure. We recruited unacquainted participants in dyads and randomly assigned each participant into one of nine possible conditions in a 2 (participant: instructed vs. not instructed) × 2 (condition: connection vs. solitude) × 2 (instruction type: free choice vs. no choice) experimental design, plus a single control condition in which participants sat alone. The participant factor was manipulated within dyad, whereas condition and instruction type were manipulated between dyads.

The experimenter asked participants to leave their belongings in the laboratory waiting room and then instructed them to complete a consent form and a 44-item personality measure (the Big Five Inventory; John, Donahue, & Kentle, 1991) in separate rooms. The experimenter then randomly assigned one participant in each dyad (or the sole control participant) to be in the uninstructed condition and told this participant that there would be a short delay while preparing materials for the second part of the experiment.

The experimenter told the other participant, in the instructed condition, that there would be a short delay. The instructed condition participants were randomly assigned to one of four possible conditions: 2 (instruction type: free choice vs. no choice) × 2 (condition: connection vs. solitude). In the free-choice solitude condition, the experimenter told instructed participants,

> Would you mind not talking at all to that other participant? Just leave him or her alone for the entire time, and keep to yourself. Of course, you don't have to do this. If you really want to talk to the person, we can't stop you, but it would be great if you could choose to just keep to yourself and not talk to that person. Again, not talking to the other participant is optional for the study. It is your choice.

In the no-choice solitude condition, the experimenter told instructed participants,

> Please do not talk to that other participant. Just leave him or her alone for the entire time, and keep to yourself. It is important for you to keep to yourself and not talk to that person. Not talking to the other participant is a mandatory requirement for this study.

In the free-choice connection condition, the experimenter told instructed participants,

> Would you mind chatting for a while with that other participant? Just make small talk with him or her while you wait. Of course, you don't have to do this. If you really want to sit in silence, we can't stop you, but it would be great if you could choose to engage the other person in conversation for as long as I'm gone. Again, talking to the other participant is optional for this study. It is your choice.

In the no-choice connection condition, the experimenter told instructed participants,

> Please chat for a while with that other participant. Just make small talk with him or her while you wait. It is important for you to engage the other person in conversation for as long as I'm gone. Talking to the other participant is a mandatory requirement for this study.

The experimenter then led both participants into the waiting room and left them in the room along with the uninstructed participants. After 10 min, participants were separated into individual cubicles to complete the experimental survey about their waiting experience.

In order to ensure compliance for each step of this relatively long (30-min) experiment, participants completed and signed a Decision Sheet for each component of the study: before the personality survey, before their wait experience, and before their final experimental survey. Decision Sheets also clarified whether the instructions were mandatory or optional, consistent with participants' experimental condition, to reinforce the free-choice manipulation.

Materials. The experimental survey first asked participants to mark all of the things they did during their break from the following list: talked to someone, talked on the phone, read a book or newspaper, slept, thought (by yourself), worked on a computer, worked on a phone, or other (with a space to write the activity). Participants then rated how pleasant and productive their break was on a scale ranging from 0 (*Not pleasant/productive at all*) to 6 (*Very pleasant/productive*) and how happy and sad they felt after the break from 0 (*Not at all happy/sad*) to 6 (*Very happy/sad*).

The second page of the survey included secondary measures to be completed only by participants who spoke to the other participant during the break. We first asked participants to "write as much as you can remember about the person with whom you spoke (name, ethnicity, age, occupation, etc.) and the conversation you had." Then we asked participants to estimate how long they talked in minutes, whether or not they knew the other person before the experiment, how pleasant their conversation with the person was on a scale ranging from 0 (*Not at all pleasant*) to 6 (*Very pleasant*), and what their overall impression of the person was on a scale ranging from −3 (*Very negative*) to 3 (*Very positive*).

The final page of the survey included a manipulation check that asked participants if the experimenter gave them instructions on how to behave prior to their wait. If participants reported they had received instructions, it then asked them to rate how much control they had in following the instructions from 1 (*I had no choice but to follow instructions—they seemed entirely mandatory*) to 4 (*It was completely my choice to follow instructions—they seemed entirely optional*). As an exploratory measure, all participants then estimated how long the break was (in minutes) and how long it felt on a scale ranging from 1 (*Time dragged*) to 7 (*Time flew*). Finally, participants reported their demographics (gender, age, ethnicity, education level, and whether English was their first language).

Results and Discussion

One pair of participants in the solitude free-choice condition did not follow experimental instructions and both reported talking to each other during the session. The following analyses are not meaningfully affected by either including or excluding this one group. We have included them here to be conservative, but note that the hypothesized effects get slightly stronger when this pair is removed. Degrees of freedom vary slightly because some participants skipped items on the final survey.

Manipulation check. Even though 79 of 80 pairs of participants in the following analyses followed the experimenter's instructions, participants in the free-choice condition reported feeling that they had more free choice in whether to talk to their partner or not during the waiting period ($M = 3.34$, $SD = 0.90$) than participants in the no-choice condition ($M = 1.85$, $SD = 1.05$), $F(1, 62) = 37.16$, $p < .01$, $\eta_p^2 = 0.38$.

Positivity. As shown in Figure 3, participants had a more positive experience when they connected with each other than when they avoided each other, regardless of the other experimental conditions. A 2 (participant: instructed vs. not instructed) × 2 (condition: connection vs. solitude) × 2 (instruction type: free choice vs. no choice) mixed-model ANOVA on reported positivity revealed only a significant main effect for condition, $F(1, 78) = 23.63$, $p < .01$, $\eta_p^2 = 0.24$. None of the other main effects or interactions approached significance, $Fs(1,76) < 1.05$. Apparently, being talked to by a stranger is every bit as positive as talking to one, regardless of whether the initiator has a sense of free choice or not.

If anything, the difference between the connection and solitude conditions was somewhat stronger among instructed participants in the free-choice condition than in the no-choice condition, $F(1, 76) = 3.16$, $p = .08$. Having no choice to either connect or sit in solitude does not seem to be a likely explanation for the results of our preceding experiments.[6]

Participants in the control condition, who sat in the room alone, reported an experience ($M = -0.18$, $SD = 0.74$) that was statistically indistinguishable from uninstructed participants in the solitude condition ($M = -0.25$, $SD = 0.81$), $t(96) = -0.31$, $p = .76$, but that was significantly less positive than uninstructed participants in the connection condition ($M = 0.33$, $SD = 0.65$), $t(96) = 2.51$, $p = .01$, $d = 0.51$. Sitting in solitude for 10 min is significantly less pleasant than engaging a stranger in conversation, regardless of whether one is sitting alone or in close proximity to a stranger. Being alone together does not seem to explain the results of the solitude condition in the preceding experiments.

Productivity. Unlike the preceding experiments, participants in Experiment 5 also reported having a more productive experience when they connected than when they sat in isolation. A 2 (participant: instructed vs. not instructed) × 2 (condition: connection vs. solitude) × 2 (instruction type: free choice vs. no choice) mixed-model ANOVA on reported productivity revealed a main effect of condition, $F(1, 76) = 8.61$, $p < .01$, $\eta_p^2 = 0.10$. There was no main effect of participant and no interactions, $Fs(1, 78) < 1.56$, except for a marginal interaction of experimental condition and instruction type, $F(1, 76) = 2.79$, $p = .099$, $\eta_p^2 = 0.04$. Decomposing the interaction, participants in the no-choice condition showed a significantly smaller effect of condition ($M_{connection} = 3.05$, $SD = 2.91$, vs. $M_{solitude} = 2.68$, $SD = 2.91$) than participants in the free-choice condition ($M_{connection} = 4.05$, $SD = 2.88$, vs. $M_{solitude} = 2.45$, $SD = 2.88$), $Fs(1, 38) = 2.55$ and 6.63, $ps = .12$ and $.01$, $\eta_p^2s = 0.06$ and 0.15. There was also a marginal main effect of instruction type, $F(1, 76) = 2.79$, $p = .099$, $\eta_p^2 = 0.04$, such that participants in the no-choice condition felt more productive than those in the free-choice condition ($Ms = 3.34$ vs. 2.78, $SDs = 2.13$ vs. 2.13). Finally, participants in the control condition, who sat in the room alone, reported the same level of productivity as participants in the connection and solitude conditions, $F(2, 96) = 0.58$, $p = .56$.

Participants felt somewhat more productive when talking than when sitting in solitude, but this was strongest for participants instructed to talk. This suggests that the sense of productivity in this experiment comes partly from following the experimenter's instructions. Consistent with the previous experiments, we do not conclude that connecting with a stranger is systemically perceived as productive but rather that, for whatever reason, it is not systematically perceived as an *un*productive waste of time.

Personality. The Appendix shows the correlations between participants' personalities as measured by the Big Five Inventory and positivity of their waiting experiences in each of the experimental conditions. As in the preceding experiments, there is no consistent pattern of moderation. We report them for the sake of completeness. The difference in positivity between the connection and solitude conditions remained significant even after controlling for the Big Five personality factors in linear regressions for both the instructed ($\beta = 0.47$, $p < .01$) and uninstructed participants ($\beta = 0.32$, $p < .01$).[7]

The results affirm the positive consequences of distant social connection observed in the preceding experiments (see also Sandstrom & Dunn, 2014). Connecting with a stranger in a laboratory context was more pleasant than sitting in solitude for those who initiated the conversation as well as for those who were initiated into it, both for those who initiated conversation with a sense of free choice and for those given no sense of free choice. Solitude,

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[6] We note that Experiment 5 is a more elaborate version of another experiment we conducted that follows a very similar procedure but includes only the no-choice condition. This simpler experiment replicates the main effects in the no-choice condition of Experiment 5, bolstering support for the reliability of this result. In this experiment, participants ($N = 88$) again experienced a 10-min wait in a laboratory. One participant was explicitly told either to connect with the other participant or to sit in solitude. The other (uninstructed) participant was told nothing. All dependent measures were identical to Experiment 5. Results again demonstrated that participants had a more positive experience when they connected with each other ($M = 0.47$, $SD = 0.68$) than when they avoided each other ($M = -0.47$, $SD = 0.90$), regardless of whether they were the instructed or uninstructed participant. A 2 (condition: connection vs. solitude) × 2 (participant: instructed vs. uninstructed) ANOVA on reported positivity revealed only a significant main effect for condition, $F(1, 38) = 28.09$, $p < .01$, $\eta^2 = 0.43$. The main effect of participant, $F(1, 38) = 1.42$, $p = .24$, and the overall interaction, $F(1, 38) = 0.32$, $p = .58$, was nonsignificant. Again, being talked to by a stranger is every bit as positive as talking to one. Participants in this simpler experiment also reported having a more productive experience when they connected ($M = 2.98$, $SD = 1.70$) than when they sat in isolation ($M = 2.11$, $SD = 1.77$). A 2 (condition: connection vs. solitude) × 2 (participant: instructed vs. uninstructed) ANOVA on reported productivity revealed only a significant main effect of condition, $F(1, 38) = 6.80$, $p = .01$, $\eta^2 = 0.15$. The main effect of participant, $F(1, 38) = 0.00$, $p = .95$, and the overall interaction, $F(1, 38) = 0.86$, $p = .36$, were nonsignificant.

[7] Estimated wait time: It is often said that time flies when you are having fun. Consistent with this possibility, participants in the connection condition reported time flew more ($M = 4.80$, $SD = 1.92$) than those in the solitude condition ($M = 3.95$, $SD = 1.90$), $F(1, 73) = 7.61$, $p < .01$, $\eta_p^2 = 0.09$. Whether participants were instructed or uninstructed and whether their instructions involved free choice or no choice had no significant main effect or interaction on their perception of time, $Fs(1, 73) < 0.57$. Participants' estimations of their actual waiting time, however, was not affected by any of the experimental conditions, $Fs(1, 70) < 1.43$, except for the instruction condition, $F(1, 70) = 8.57$, $p < .01$, $\eta_p^2 = 0.11$. Participants who were uninstructed more accurately guessed the 10-min break time ($M = 9.69$ min, $SD = 4.79$), whereas instructed participants believed the break was nonsignificantly shorter ($M = 7.56$ min, $SD = 4.23$).
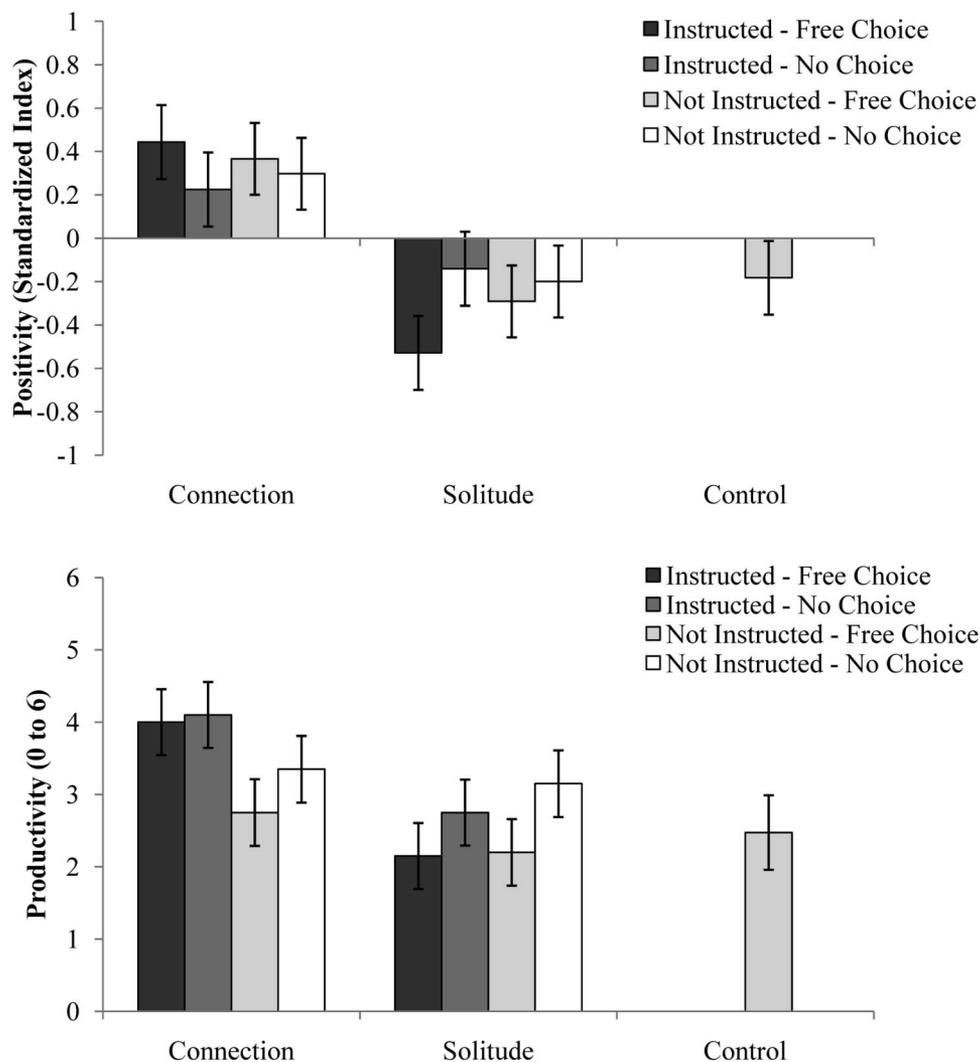
Figure 3. Positivity (top panel) and productivity (bottom panel) from Experiment 5. Error bars represent the standard error around the mean of each condition.

in contrast, was relatively unpleasant in this context whether experienced in the company of another person or not. The benefit of social connection, or the cost of isolation, seems both contagious and robust.

General Discussion

Aristotle famously argued that man is by nature a social animal, but people in the company of strangers often look to be anything but social. Instead of treating each other as possible sources of well-being, strangers in close proximity often ignore each other completely, treating each other more like objects than like fellow social beings. For one of the most highly social species on the planet, whose members benefit significantly from forming connections with other people, this seems paradoxical. Why would highly social animals in the company of strangers so routinely ignore each other?

A series of nine experiments conducted on trains, buses, and taxicabs and in a laboratory suggests an answer: People misunder-

stand the consequences of social connection. On trains and buses, participants in both Experiments 1b and 2b predicted that they would have a more positive commute sitting in solitude than connecting with a random stranger. These predictions are consistent with the common behavior in these contexts, where conversations between strangers are rare. Yet, consistent with the broad benefits of social connection and the pain of social isolation, participants in Experiments 1a and 2a who actually experienced these two situations had precisely the opposite experience: They had a more positive commute when they connected with a stranger than when they sat in solitude. People seem to ignore strangers because they mistakenly think that forming a connection with them would be systematically unpleasant, whereas isolation would be pleasurable. Humans may indeed be social animals but may not always be social enough for their own well-being.

Of course, life is not always lived to maximize well-being. People may therefore put off positive interactions because it comes at a cost for some other goal. When commuting, a person may

want to get work done. When riding in a cab after a long plane flight, a little sleep might be rejuvenating. When sitting in a waiting room, homework may need to get done. Indeed, in both Experiments 1b and 2b, participants predicted that their socializing would come at a cost to productivity, expecting they would have a less productive commute if they connected with a stranger than if they sat in solitude. Neither experiment, however, found that participants who actually connected with a stranger reported having a less productive commute when they connected with a stranger than when they sat in solitude. Either people do not get as much done on the train sitting alone as they expect to (Buehler, Griffin, & Ross, 1994), or forming a new connection comes to be defined as a reasonably productive use of time after having done it. Likewise, cab riders in Experiment 4a did not report being any more tired after connecting with the driver than when sitting in solitude, and those sitting in a waiting room in Experiment 5 actually reported having a more productive time when they talked than when they sat in isolation. These results do not mean that social interaction is never distracting or draining, but they do mean that the benefits our participants received from connecting with a stranger in our experiments did not come with easily imagined costs.

Such a dramatic disconnect between the actual and anticipated impacts of connecting with a stranger is puzzling. If connecting with a stranger is so much more pleasant than sitting in solitude, then why do people expect precisely the opposite? At least part of the answer comes from Experiments 3a and 3b. In order to have beliefs that accurately reflect reality, a person must learn about reality either directly or indirectly. Any barrier that keeps a person from learning from reality could create mistaken beliefs.

Experiments 3a and 3b both suggest that one possible barrier is a widely shared belief that other people are not interested in connecting, a belief that could come directly from existing behavioral norms. Just as in emergencies, where a concerned onlooker who hesitates to intervene can conclude that other equally concerned onlookers doing the very same thing are not actually concerned at all (Miller & McFarland, 1987), so too do strangers who follow the existing social norms of isolation seem to exhibit *pluralistic ignorance,* which leaves the majority of people feeling that others are not as interested in connecting as they are themselves. This belief serves as an obvious barrier to connecting with a distant other, one that could keep people from gaining the very experience they would need to accurately understand the hedonic consequences of social interaction. Indeed, Experiments 4a and 4b make this point directly. Cab riders had a more positive commute when they talked to their driver than when they sat in isolation, an effect completely anticipated by people who reported routinely talking to their drivers but completely unanticipated by people who reported rarely talking to their drivers. A preference for solitude in the company of strangers seems to come at least partly from failing to learn from experience.

This research broadly suggests that people could improve their own momentary well-being—and that of others—by simply being more social with strangers, trying to create connections where one might otherwise choose isolation. Our experiments, however, contained some necessary artifacts that may create concerns about accepting this broader implication. In particular, participants in our experiments were asked to engage others in conversation as part of an experiment. Although Experiment 5 clearly showed that being

the recipient of connection is just as positive as being the instigator of it, perhaps this feature served as an icebreaker that allowed people to have a more pleasant conversation than they might have had otherwise. Perhaps being able to start the conversation by saying, "I'm in an experiment and was asked to talk," enabled our results?

Regrettably, we did not ask how participants began their conversations in the experiments, but this alternative did occur to us afterward. We therefore e-mailed all of the participants from the connection conditions of Experiments 1a, 2a, and 4a who gave us valid e-mail addresses ($n = 62$) to ask if they could recall how they started their conversation. This distant retrospection is obviously less valid than asking people immediately after the experiment, but we assumed that participating in the experiment would be memorable enough for at least some people to give informative evidence. The overall response rate of the participants we e-mailed was 50% ($n = 31$). Of these, 16% reported being unable to remember how they started their conversation. Of the remaining who did remember how they started their conversation, 29% asked about current events (e.g., "What do you think of the weather?", discussing an upcoming election), and 39% asked a personal question (e.g., "How are you?", "Where are you from?", or commented on the other person's hair/clothes/shoes, etc.) The final 16% began in other idiosyncratic ways, such as talking about themselves or saying "bless you" after a sneeze. No participant reported starting the conversation by mentioning the experiment. Instead, they seemed to do what most do to start a conversation— they tried to say something worth responding to.

However, even if all of these participants are misremembering their conversation or if all of the participants who did not respond to our survey actually began their conversation by mentioning the experiment (a possibility we find very implausible), this would not invalidate our results. It would only show that what intuitively seems to be a weak opening line might be a surprisingly good one.

Moderators, Likely and Unlikely

Our experiments demonstrate a consistent disconnect between the anticipated and actual consequences of connecting with a stranger. However, there are surely important moderators of either the anticipated or actual experience of social connection that would affect the disconnect we have documented. In fact, our experiments suggest both a likely moderator and an unlikely moderator.

A likely moderator, suggested by Experiments 3 and 4, is the frequency with which people actually connect to strangers. Actual contact with outgroup members tends to lead to more positive attitudes about them (Pettigrew & Tropp, 2006), and actual experience connecting to distant others should lead to more calibrated expectations about future interactions (as we observed among cab riders who reported routinely talking to their drivers in Experiment 4a). Indeed, we suspect that the variance in existing cultural norms about engaging strangers in conversation (Forgas & Bond, 1985; Triandis, 1972) would moderate the magnitude of the disconnect we observed. In cultures or contexts where connecting with strangers is more routine and encouraged, we would expect calibration between the expected and actual outcomes of distant social interactions because people in these cultures and contexts learn about the consequences of social interaction through their own experi-

ence. But in cultures or contexts where connecting with strangers is more actively discouraged, such as in the contexts we studied, we would expect a mistaken preference for solitude simply because people have failed to learn that their expectations are wrong.

An unlikely moderator suggested by our experiments is a person's existing personality. We found no evidence that global personality types, measured (in all but Experiment 2b) by a short or long measure of the Big Five Personality Inventory, influenced either predicted or actual experiences with social interaction in any consistent way across our experiments. Instead, the actual consequences of social interaction in Experiments 1a, 2a, 4a, and 5 did not differ consistently by personality type. Personality did not significantly moderate the actual effects of connecting with a stranger, of sitting in isolation, or of the difference between the two. This is consistent with a general finding that strong situational contexts tend to affect people with differing personality types relatively similarly (Mischel, 1973) and also with the more directly related finding that people who are asked to behave extravertedly in a social interaction report having a more pleasant experience than those asked to behave introvertedly regardless of their trait level of extraversion (Fleeson et al., 2002; McNiel & Fleeson, 2006; Zelenski et al., 2012). Pleasant social interactions seem pleasant for most people, as isolation seems unpleasant for most people.

Interestingly, two experiments (Experiments 1b and 4b) also failed to find consistent personality effects on the *predicted* consequences of connecting with strangers. These results may appear somewhat discrepant with one published series of experiments in which trait extraversion predicted how positively people expected to feel in social situations where they were instructed to behave either extravertedly (i.e., to act bold, talkative, energetic, active, assertive, and adventurous) or introvertedly (Zelenski et al., 2013). For instance, extraverts predicted that they would have a less pleasant experience if they were instructed to act introverted than if instructed to act extraverted while completing a jigsaw puzzle with another person, getting to know another person, or planning a day together in a group. Although Experiment 1b found directionally consistent results, with extraversion positively correlated with predicted positivity of the connection experience, we did not replicate the direction of this result in Experiment 4b, and the relationship between extraversion and predicted positivity did not vary between experimental conditions. Why did our extraverts not think that sitting in isolation on the train would be unpleasant?

One possible answer is that our experiments were underpowered to detect any stable personality moderators. However, because our prediction experiments were all within participants involving relatively large samples (*n*s = 66 and 43 in Experiments 1b and 4b, respectively), these experiments would at least theoretically be able to detect modest effects. Another possible answer is that we used only a 10-item measure of the Big Five in our prediction experiments (Gosling et al., 2003) rather than a more specific measure of extraversion in Zelenski et al. (2013; Saucier, 1994). Although the shorter version of the Big Five appears to be an empirically valid measure of the longer and more elaborate version (Gosling et al., 2003), it is certainly possible that two different measures of the same thing yield different results because they are not, in fact, measuring exactly the same thing.

However, we think our findings suggest a third, more interesting possibility. Notice that extraversion is a much broader concept than connecting with a stranger, one that operationally involves talking but also involves behaving in a bold, adventurous, energetic, active, and assertive fashion. These latter behaviors are relatively self-focused and need not involve any interaction with others. Predicting the consequences of extraversion may then focus more one's own *actions* than others' *reactions*. In contrast, our participants predicted the consequences of connecting with a stranger, a prediction that Experiments 3a and 3b suggest is guided by a judgment of how others are likely to respond (Dunn et al., 2007; Mallett et al., 2008; Vorauer & Miller, 1997). Note also that few would predict that connecting with a friend would be an unpleasant experience, presumably because a friend would react favorably compared to the uncertain (and potentially negative) reactions of seemingly disinterested strangers. Extraversion, and perhaps all of the individual difference measures, failed to predict the anticipated consequences of social connection in any consistent fashion, we believe, because the latter is guided by social judgments as much as, or perhaps more so than, by self-assessments.

Experiment 4b is consistent with this possibility. Here, participants who learned about others' reactions through their own personal experience made different and more calibrated predictions than those who failed to learn from such experience, regardless of their own trait extraversion (or any of the other Big Five traits). Indeed, even measures of trait extraversion in that experiment did not differ between those who reported normally talking to their drivers versus those who reported normally remaining silent, either in riders' actual experiences, Experiment 4a, $t(89) = 0.60$, *ns*, or in riders' predicted experiences, Experiment 4b, paired $t(41) = 0.47$, *ns*. People may fail to connect with strangers because others seem disinterested, rather than because it is inconsistent with one's own personality.

This suggests that people do not so much have a preference for solitude in the presence of strangers as they do a fear of the negative consequences that might come from attempted interactions. We suspect this is why there are specific social functions and customs intended to alleviate this fear, whether it is a cocktail party, a social networking event, or a welcome reception—all places where strangers come in contact but are known to be interested in connecting. We also suspect this is why concepts like icebreakers and pickup lines exist, customs intended to make it easier to start a conversation, whereas ice-makers and drop-dead lines that would end a conversation are relatively uncommon. Removing the barrier to starting a conversation, rather than trying to increase a person's own trait extraversion, may therefore be the most effective way to encourage interactions with distant strangers.

Extraversion, Social Connection, and Well-Being

Prior research suggests that acting extravertedly—that is, acting bold, assertive, energetic, active, adventurous, and talkative (the exact list has varied by study)—in laboratory experiments involving group tasks like solving jigsaw puzzles and planning a day together generally leads to greater positive affect than acting introvertedly—lethargic, passive, and quiet—in those same situations (Fleeson et al., 2002; Zelenski et al., 2013). Our studies extend these findings in at least two major ways. First, our experiments manipulated the goal of connecting with a stranger directly, whereas acting extravertedly affects a wide variety of behaviors,

not all of which require engagement with other people. Indeed, in one experiment, the subcomponents of state extraversion significantly correlated with 17 different goals, ranging from trying to convey information (being talkative) to trying to have fun (being spontaneous) to trying to strive hard for something (being bold) to trying to be attractive or interesting (being energetic; McCabe & Fleeson, 2012). Exactly why trait and state extraversion increases well-being is unclear. By restricting our manipulation to one goal, our experiments suggest that social connection is at least one potentially important component of this more general result.

Second, asking participants to act introvertedly in a context in which they must necessarily interact with other people (e.g., a group decision task) creates a mismatch between their behavior and the situation. Our studies do the opposite: The behavioral norms in Experiments 1, 2, and 3 strongly pushed for remaining disengaged from strangers, whereas we asked participants in some conditions to talk to a stranger. This means that the behavior–situation mismatch occurred in the connection conditions rather than in the solitude conditions. This provides very strong evidence that participants' positive experience connecting with a stranger does not come simply from an alignment between participants behavior and the surrounding situation but that connecting with a stranger is positive even when it is inconsistent with the prevailing social norm.

Qualifications

Of course, our results do not demonstrate that all interactions with strangers will be pleasant, any more than studies showing the hedonic benefits of close family and friends demonstrate that all family relationships are pleasant. Moreover, we suspect that there are some important boundary conditions on the pleasures of connecting with strangers that our experiments were unable to test. Our experiments tested interactions that lasted anywhere from a few minutes to as long as 40 min (considerably longer than existing laboratory experiments), but they did not require repeated interactions or particularly long interactions with the same random stranger. Nobody in the connection condition, for instance, spent the weekend with a stranger on a train. Indeed, some research suggests that liking for a stranger may peak at a relatively short interaction and then decline over time as more is learned about another person (Norton, Frost, & Ariely, 2007).

If, however, the amount of time spent in conversation with a distant stranger is inversely related to its pleasantness at some point along the time spectrum, then this only makes the results of our experiments even more surprising. On trains and buses and in waiting rooms, the duration of the conversation is relatively limited. These could be the kinds of brief *social snacks* (Gardner, Pickett, & Knowles, 2005) with distant others that are maximally pleasant and yet people still routinely avoid them. Understanding how the anticipated length of an interaction affects the likelihood of engaging someone in an interaction, compared to its actual effects on the quality of an experience, is a potentially rich domain for future research.

Likewise, our experiments examined the consequences of only a single social connection. We did not test the consequences of talking repeatedly to strangers over a longer period of time. It could be people habituate to the pleasures of social engagement quite quickly. Although possible, existing research on adaptation suggests that the pleasures of connecting with strangers could be a repeated source of well-being. In particular, people habituate more quickly to identical repeated experiences than to subtly different repeated experiences (Epstein, Caggiula, Rodefer, Wisneiwski, & Mitchell, 1993; Redden, 2008; Rolls, Van Duijvenvoorde, & Rolls, 1984; Schumann, Petty, & Clemons, 1990). The 10th identical chocolate will not be as delightful as the first, but the 10th subtly different dessert could be every bit as delightful as the first. Notice that conversations are more like subtly different desserts than they are completely identical chocolates because each conversation with a new stranger is unique. We would therefore predict that the 10th conversation with a stranger on a morning commute would still be a significantly more pleasant experience than the 10th foregone conversation. We think testing this hypothesis in particular, and the consequences of repeated social engagement more generally, is a very promising avenue for future research.

Concluding Thought

Being civil toward distant others or random strangers is typically believed to benefit others—society at large or those who are befriended. The results of our experiments, however, join a growing body of research suggesting positive consequences of prosociality for oneself. Whether it is spending money on others versus oneself (Dunn, Aknin, & Norton, 2008), behaving equitably rather than selfishly (Condon & DeSteno, 2011), or expressing gratitude versus disdain (Algoe, Gable, & Maisel, 2010), prosociality seems not only to benefit others but also to benefit oneself. On an increasingly crowded planet, misunderstanding the benefits of social engagement could be increasingly problematic. At least in this respect, the hedonist who seeks happiness and the idealist who seeks civility should choose the same path.

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Appendix

Correlations Between Reported Personality Traits (as Measured by the Ten Item Personality Inventory or Big Five) and Positivity of Commute or Wait

Experimental conditions	Extraversion	Agreeableness	Openness to Experience	Conscientiousness	Emotional Stability
Experiments 1a & 1b: Trains					
Actual (Experiment 1a)					
Control ($n = 37$)	0.208	0.063	0.200	0.211	0.278
Connection ($n = 27$)	−0.080	0.116	0.278	0.299	0.227
Solitude ($n = 32$)	0.086	0.339	0.432**	0.017	0.428**
Predicted (Experiment 1b, $n = 66$)					
Control	0.168	0.089	−0.031	−0.008	−0.306**
Connection	0.259*	0.141	0.425**	0.277*	0.330**
Solitude	0.187	0.108	0.150	0.205	0.099
Experiment 2a: Buses					
Actual (Experiment 2a)					
Control ($n = 24$)	0.111	0.407*	0.419*	0.333	0.197
Connection ($n = 26$)	0.287	0.113	0.051	0.379	0.269
Solitude ($n = 25$)	−0.323	0.140	0.306	0.532**	0.053
Experiments 4a & 4b: Cabs					
Actual (Experiment 4a)					
Control ($n = 36$)	0.066	0.166	0.381*	0.180	0.019
Connection ($n = 27$)	−0.127	0.176	0.030	−0.089	−0.241
Solitude ($n = 30$)	−0.028	−0.283	−0.217	−0.153	−0.039
Predicted (Experiment 4b, $n = 43$)					
Control	0.148	−0.142	0.154	0.044	0.032
Connection	−0.169	0.242	−0.164	−0.039	0.191
Solitude	0.027	−0.206	−0.220	0.116	0.004
Experiment 5: Is the pleasure of connection contagious?					
Instructed					
Connection–no choice ($n = 20$)	0.396	0.275	−0.088	−0.122	0.081
Connection–free choice ($n = 20$)	0.222	0.730**	0.213	0.374	−0.525*
Solitude–no choice ($n = 20$)	0.254	0.356	0.160	0.105	−0.289
Solitude–free choice ($n = 20$)	0.396	0.275	−0.088	−0.122	0.081
Not instructed					
Connection–no choice ($n = 20$)	0.217	0.406	0.142	0.047	−0.337
Connection–free choice ($n = 20$)	0.295	0.781**	0.319	0.249	−0.063
Solitude–no choice ($n = 20$)	0.120	0.256	−0.006	0.583**	−0.306
Solitude–free choice ($n = 20$)	0.628**	0.525**	0.082	0.309	−0.625**
Control ($n = 19$)	−0.030	0.219	0.470*	0.290	−0.254

* $p < .05$.   ** $p < .01$.