YOU CAN HANDLE THE TRUTH:
MISPREDICTING THE CONSEQUENCES OF HONEST COMMUNICATION

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ABSTRACT

People highly value the moral principle of honesty, and yet, they frequently avoid being honest with others. In the present research, we explore the actual and predicted consequences of honesty in everyday life. We utilize field and laboratory experiments that feature two types of honesty interventions: 1) instructing individuals to focus on complete honesty across their interactions for a period of time, and 2) instructing individuals to engage in specific honest conversations that they frequently avoid in everyday life. In Studies 1a and 1b, we randomly assigned individuals to either be (or imagine being) honest, kind, or conscious of their communication in every conversation with every person in their life for three days. We find that people significantly mispredict the consequences of honesty: focusing on honesty (but not kindness or communication-consciousness) is more pleasurable, meaningful, socially connecting, and does less relational harm than individuals expect. We extend our investigation by examining the consequences of specific well-controlled honest conversations for both communicators and their relational partners in two preregistered laboratory experiments. In Study 2 we examine the predicted and actual consequences of honestly disclosing personal information, and in Study 3 we examine the predicted and actual consequences of honestly sharing negative feedback. Our results suggest that individuals broadly misunderstand the consequences of increased honesty because they overestimate how negatively others will react to their honesty. Overall, this research contributes to our understanding of affective forecasting processes and uncovers fundamental insights on how communication and moral values shape well-being.

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Honesty is one of the most fundamental moral values in human life. Honesty is among the most important traits for interpersonal judgment (Anderson, 1968; Goodwin, Piazza, & Rozin, 2014) and dominates philosophical and religious teachings across time and cultures. Given the importance of honesty to individuals' sense of self and morality, why do individuals so frequently avoid being honest with others? One reason is that being honest often feels unkind or uncomfortable. Even seemingly mundane questions, such as "How are you?" can trigger a difficult conversation if a communicator believes that answering the question honestly might be uncomfortable for them or another party.

People routinely face difficult conversations – conversations in which honesty could be aversive (Stone, Patton, & Heen, 2010). They face these conversations in their personal lives when sharing their feelings with friends and family members, and in their professional lives when deciding how to deliver negative news and critical feedback. Though these difficult conversations are part of everyday life, navigating them can elicit distress and anxiety (e.g., Margolis & Molinsky, 2008; Molinsky & Margolis, 2005). As such, individuals often choose to be dishonest during difficult conversations, or avoid engaging in such conversations altogether, and instead focus on being pleasant and creating smooth social interactions (Lee, 1993; Rosen & Tesser, 1970; Tesser, Rosen, & Tesser, 1971).

Is forgoing honesty a good decision? On the one hand, it is possible that avoiding honesty – particularly during difficult conversations – promotes social connection and leads to more enjoyable interactions. On the other hand, it is possible that individuals *expect* honesty to be far

more unpleasant and socially damaging than it truly is. And, perhaps being completely honest also yields unexpected benefits, such as a heightened sense of meaning.

In one field experiment (as well as a pilot field experiment reported in the online supplemental materials) and two preregistered laboratory experiments, we examine the predicted and actual effects of honesty on psychological well-being and social connection. We examine the consequences of two types of honesty interventions: 1) instructing individuals to focus on complete honesty across their interactions for a period of time, and 2) instructing individuals to engage in specific honest conversations that they frequently avoid in everyday life. We compare honest communication to kind communication, as well as a neutral control condition, and we examine how honesty influences three distinct facets of well-being: hedonic well-being, eudaimonic well-being, and social connection. Hedonic well-being is characterized by pleasure, enjoyment, and happiness. In the hedonic view, well-being consists of the presence of pleasure and the absence of pain (Ryan & Deci, 2001). Eudaimonic well-being is characterized by meaning, fulfillment, and individual autonomy. In the eudaimonic view, well-being consists of the actualization of human potentials, rather than pleasure (Waterman, 1990, 1993). Social connection, in addition to one's sense of pleasure and meaning, is an important component of overall physical and subjective well-being (Helliwell & Putnam, 2004; Myers & Diener, 1995).

This research has important theoretical and practical contributions. First, we document the psychological forces that push people away from communicating honestly. As will be revealed through our studies, individuals find honest communication to be more enjoyable, meaningful and socially connecting than they expect. Our results suggest that individuals misunderstand the *personal* consequences of increased honesty because they misunderstand the

social consequences of honesty. Specifically, communicators assume that others will react more negatively to honest conversations than they actually do.

Our studies also reveal that individuals' aversion to honesty may be misguided. The choice to be less than fully honest with others is driven by individuals' inaccurate expectations of how unenjoyable honest conversations will be. However, individuals' post hoc appreciation for honest conversations is driven by how meaningful those conversations are. These results suggest that affective forecasting failures can lead individuals to miss out on meaningful experiences.

Additionally, by doing a deep dive into the intrapersonal and interpersonal consequences of honesty, this work helps to bridge the gap between our understanding of normative and behavioral ethics. For centuries, ethicists have touted the moral significance of different virtuous behaviors, but psychologists have only recently begun to examine the experience and consequences of enacting or violating these virtues (Aknin et al., 2013; Aknin, Dunn, & Norton, 2012; Dunn, Aknin, & Norton, 2008; Emmons & McCullough, 2003; Gino, Kouchaki, & Galinsky, 2015; Hofmann, Wisneski, Brandt, & Skitka, 2014; Lyubomirsky, Shelden, & Schkade, 2005; Rudd, Norton, & Aaker, 2014). To our knowledge, our work is the first to examine how specific moral principles and styles of communication influence all three fundamental forms of well-being (hedonic, eudaimonic, and social connection).

Defining Honesty

A key reason that being honest during difficult conversations is a source of stress for so many people is that there is uncertainty about how one's conversational partner will react to the information that is divulged. Rather than engage in such conversations, many people choose to cloak their personal thoughts and feelings in social niceties or avoid having difficult conversations altogether (Stone et al., 2010). The result is a glaring lack of honesty.

We define honesty as *speaking in accordance with one's own beliefs, thoughts and feelings.* Our definition of honesty builds on a recent definition of authenticity by Gino, Kouchaki, and Galinsky (2015), who define authenticity as *acting* "in accordance with one's own sense of self, emotions, and values." According to Gino, Kouchaki, and Galinsky, inauthenticity entails being untrue to oneself, whereas being dishonest entails being untrue to others. In other words, authenticity and inauthenticity are not limited to communication, and do not require a target. Conversely, we define honesty and dishonesty as forms of *communication*, which require a target (i.e. listener).

We note three important facets about our definition of honesty. First, a communicator's *beliefs, thoughts and feelings* are independent of reality. That is, individuals can be honest by communicating something that they believe to be true, even if that belief is, in fact, false. Thus, individuals can be honest about an objective fact, but they can also be honest about their inner experience and opinions (DePaulo & Bell, 1996). We are primarily interested in honesty about individuals' inner experiences (i.e., their private opinions and histories) in the present manuscript. It is important to understand the consequences of honestly communicating one's inner experience because sharing this information is relatively discretionary (compared to sharing objective negative news, such as layoffs), but is nonetheless the basis for important social and organizational processes (e.g., self-disclosure and interpersonal feedback).

Second, we define honesty based on the content that a communicator shares, rather than the tone with which they share that content. In other words, individuals can, but need not, be honest in a blunt or harsh manner. Third, we conceptualize honesty as a communication style

that involves openness and truth-telling, rather than a set of actions that involves the avoidance of cheating and duplicity.[1]

Honesty, Pleasure, and Social Connection

One reason people may avoid being honest is that honesty often appears to conflict with kindness: truthfully sharing one's opinions and feelings can hurt others and create social tension during difficult conversations. Human beings are hardwired to detect pain and suffering in others (Craig, 2009), and consequently, they often prioritize the prevention of interpersonal harm over other principles, such as honesty (Gray, Young, & Waytz, 2012; Haidt & Graham, 2007; Schein & Gray, 2017). The primacy of harm avoidance influences a host of moral and social decisions, including everyday communication. Most people recognize that one goal of social communication is to avoid embarrassing others and to help others "save face" (Goffman 1967), which can lead people to be overly polite and avoid sharing negative opinions, rather than being completely honest (DePaulo & Bell, 1996).

Individuals also prioritize harm avoidance when they judge others' communication. For example, individuals often judge those who tell prosocial lies (i.e., statements that prioritize benevolence over honesty) as more ethical and trustworthy than those who are honest but harm others (Levine & Schweitzer, 2014, 2015). Despite the strength of individuals' intuitions about the social harm associated with difficult truths, we know very little about when these intuitions are correct or incorrect. Although recent research demonstrates that targets do indeed resent communicators who speak truths that cause objective harm (Levine & Schweitzer, 2014, 2015),

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[1] We note that we are not focused on reactions to the word "honesty" per se, but rather we investigate the consequences of engaging in more honest communication than people typically do (see Study S1 in our online supplement for more on this distinction).

much less is known about the perceived and actual harm associated with the truths that individuals withhold in everyday life.

The belief that honesty during difficult conversations will cause interpersonal harm leads communicators to worry about how enacting this harm will affect their own emotional state and their relationships. When individuals are faced with the prospect of harming others, they often anticipate feeling negative emotions such as guilt and shame (Cohen, Wolf, Panter, & Insko, 2011; Tangney, Stuewig, & Mashek, 2007). This also occurs in the domain of communication. For example, individuals who have to deliver bad news to others often avoid doing so because they feel guilty about not sharing the person's fate (Heider, 1958; Tesser & Rosen, 1972). In their classic research on the "MUM effect" (keeping mum about undesirable information), Tesser and Rosen (1972) found that individuals were reluctant to tell another (fictitious) participant that they would receive painful electric shocks during an experiment, particularly when the communicators did not share the same bad fate. Work by the same author team revealed that the reluctance to deliver bad news persists even in situations in which communicators and targets have no preexisting relationships and anticipate no future interactions, and when the information that needs to be delivered is objective and urgent (Rosen & Tesser, 1970). Tesser and Rosen interpret these results as suggesting that the MUM effect occurs because communicators anticipate internal discomfort rather than relational costs (Tesser & Rosen, 1972).

Related research, however, argues that relational costs do play a role. For example, Bond and Anderson (1987) demonstrate that individuals only exhibit the MUM effect when they are visible to the target, suggesting that the reluctance to share negative information is not driven solely by one's internal emotions, but rather a concern about how the other person will react and

judge the communicator. Similarly, within organizations, Fisher (1979) finds that individuals exaggerate the positivity of feedback to poor performers, which is mediated by the belief that poor performers will like them less. Research within close relationships also demonstrates that friends avoid telling each other that they do not like their friend's romantic partner because they fear harming their relationship with their friend (Mayer, 1957) and that they avoid honestly disclosing personal information (i.e., self-disclosure), in part, because they worry about others' judgments and social distance (Rosenfeld, 1979). Across contexts, individuals also tend to avoid honestly sharing their opinions in the presence of others who do not share these views (e.g., Tetlock, 1985; Tetlock, Skitka, & Boettger, 1989), presumably because they fear interpersonal judgment.

Despite the large literature on the anticipated costs of honesty during difficult conversations, little is known about whether these anticipated costs are accurate. That is, existing research demonstrates that people avoid being honest because they associate honesty with interpersonal harm and discomfort, and as a result, expect to incur affective (i.e., hedonic) and social costs when they are completely honest. Thus far, however it is unclear whether individuals are well-calibrated in their estimation of such costs. Does honesty cause as much interpersonal pain and tension as people expect? Is it unpleasant and socially isolating to be honest with others? It is possible that communicators overestimate the costs of honesty, and thus, could afford to be more honest than they are without suffering negative affective or social consequences. On the other hand, it is possible that individuals understand the costs of honesty with reasonable accuracy and their reluctance to engage in difficult conversation reflects a rational cost-benefit calculation. In the present research, we examine these possibilities.

We hypothesize that communicators overestimate both the hedonic and social costs of honesty. Research on emotion-regulation, secrecy, and the performance of necessary evils suggest that this may be the case. For example, individuals who honestly express their emotions experience lower stress and blood pressure, and develop higher levels of intimacy than individuals who hide their emotions (Butler et al., 2003; Srivastava, Tamir, McGonigal, John, & Gross 2009). On the other hand, individuals who harbor secrets have poorer health than individuals who do not (Slepian, Chun, & Mason, 2017). Research on the experience of performing "necessary evils," such as delivering terminal prognoses or critical performance feedback (e.g., Margolis & Molinsky, 2008; Molinsky & Margolis, 2005), also suggests that communicators' concerns about the costs of honesty might be overstated. This body of research finds that despite prior assumptions that the prospect of harm-doing triggers psychological disengagement (Bandura, 1999), many professionals who are taxed with communicating unpleasant truths to others (e.g., doctors, managers, corrections officers) are able to psychologically engage and connect with the targets of their communication. Furthermore, communicating conflict directly rather than indirectly can in many circumstances help de-escalate negative conflict spirals and improve interpersonal relationships, (Weingart, Behfar, Bendersky, Todorova, & Jehn, 2015).

Theoretically, our prediction of an affective forecasting error about honesty is informed by a growing body of research documenting other ways in which individuals mispredict how their conversations will affect them. In particular, individuals frequently underestimate the pleasure they derive from conversing with outgroup members (Mallet, Wilson, & Gilbert, 2008) and strangers (Dunn, Biesanz, Human, & Finn, 2007; Epley & Shroeder, 2014). Mallet et al. (2008) found that individuals mispredict the consequences of interacting with outgroup members

because they expect outgroup members to be more different from themselves than they actually are. Epley and Schroeder (2014) found that communicators mistakenly predict that talking to a stranger will be less pleasant than sitting in isolation, when in fact, connecting with a stranger is more pleasant than sitting in isolation because people expect others to be less interested in connecting than they actually are. These papers document two reasons people systematically mispredict the consequences of social interactions: 1) they misunderstand the nature of who they will interact with, and 2) they misunderstand whether other individuals are interested in social interaction in the first place.

We posit a third reason that people mispredict the pleasure of particular conversations: they misunderstand how others will react to the *content* of the conversation. Specifically, we hypothesize that individuals overestimate how negatively others will react when they share honest information that is typically left unsaid (e.g., intimate personal information or interpersonal critiques). This may happen for at least two reasons. First, it is possible that people believe that the norm of not being completely honest with others reflects a preference. That is, just as individuals' infer that others are disinterested in talking to strangers from the norm of silence among strangers (Epley & Schroeder, 2014), people may infer that others are uninterested in – or worse yet, would be offended by – hearing their intimate secrets or critical opinions, based on norms of politeness and conflict avoidance. Furthermore, individuals may suffer from a focusing illusion (e.g., Gilbert & Wilson, 2000; Loewenstein & Schkade, 1999; Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000) and narrowly focus on the exact information shared, rather than recognizing the broader context in which that information is shared. For example, individuals may overestimate the costs of providing negative feedback because they assume the listener will focus on the specific criticism provided, rather than the

speaker's intent or the relationship in which the feedback is shared. Either, or both, of these processes could give rise to systematic mispredictions of others' reactions to honesty.

The central purpose of the current research is to examine whether individuals' assumptions regarding the hedonic and social costs of increased honesty in everyday life are misguided. We hypothesize that they are, and we explore whether mispredictions of others' reactions to honesty underlie this error.

Honesty and Meaning

A secondary goal of this research is to broadly examine the consequences of honest communication. Thus, in addition to examining the relationship between honesty and hedonic well-being and social connection, we consider how honesty influences one's sense of meaning, or eudaimonic well-being. To communicate honestly, individuals must look inwards and consult their personal feelings and opinions. This process may increase self-actualization and produce feelings of personal control and autonomy, key components of eudaimonia (Deci & Ryan, 2008). Research on the experience of inauthenticity is consistent with this proposition. Behaving inauthentically by misrepresenting one's emotions or by conforming to social norms that are inconsistent with one's personal beliefs lowers moral self-regard and one's sense of moral purity (Gino et al., 2015). Moral identity is closely linked to sense of self (Aquino & Reed, 2002), thus decrements in moral identity may undermine one's sense of meaning and purpose as well (i.e., eudaimonic well-being). Consequently, we expect honesty to increase eudaimonic well-being.

We did not have any a priori predictions about whether individuals would accurately predict the relationship between honesty and eudaimonic well-being, thus this aspect of the current investigation is more exploratory and inductive. Nonetheless, examining eudaimonic well-being forecasts provides insight into individuals' relative ability to predict hedonic versus

eudaimonic outcomes, thus shedding new light on the types of outcomes individuals accurately and inaccurately forecast.

Overview of Studies

We rely on field experiments and laboratory experiments to examine the predicted and actual consequences of increased honesty in interpersonal communication. First, we conducted a pilot field experiment in which we randomly assigned 117 participants to be completely honest, kind, or conscious of their communication (the control condition) in every social interaction for three days. Participants in this study forecasted the experience and then judged the experience every day for three days. Because Study 1a replicates the main results of the Pilot Study, we report this Pilot Study in our online supplemental materials only.

Our Pilot Study informed our first field experiment (Study 1a). Study 1a featured the same experimental manipulations that we used in our Pilot Study (i.e., we randomly assigned participants to be completely honest, kind, or conscious of their communication for three days) but we refined our protocol and our measures. We complemented the field experiment with a forecasting experiment (Study 1b) in which a separate sample of participants made predictions about the experience of being honest, kind, or conscious of their communication in every social interaction for three days.

In Study 1, we compare the consequences of focusing on honesty, focusing on kindness, and being conscious of one's communication (our neutral control condition) for several reasons. First, we wanted to examine whether individuals mispredict the consequences of honesty in particular, or if they mispredict the consequences of focusing on any moral principle when communicating. We chose kindness as a comparison because it is the natural counterpart to honesty during difficult conversations, the context in which we expect individuals to avoid

honesty most frequently. Second, the kindness and neutral control conditions in our field experiments allow us to draw conclusions about the absolute consequences of honesty, consistent with our secondary research goal. By including a neutral control condition in the research design, we can examine how honesty influences well-being above and beyond participants' reactions to participating in the study itself. Note, however, that the control condition does not perfectly reflect individuals' routine communication (i.e., in the absence of an intervention).

Two weeks after the field experiment ended, participants responded to open-ended questions about the nature of their interactions during the experiment and the consequences thereof. To better understand the mechanisms underlying our effects and the nature of participants' experiences, we analyzed participants' open-ended responses (Study 1c). These analyses suggest that focusing on honesty increased the frequency with which individuals engaged in difficult conversations and genuine self-expression in their everyday lives, and that individuals were surprised that others reacted more positively to their honesty than they anticipated. To extend these initial findings, in Studies 2 and 3, we examine the predicted and actual consequences of engaging in specific, well-controlled, honest conversations with pairs of close relational partners. In Study 2, individuals had an honest conversation about a series of difficult topics with a close relational partner, and in Study 3, individuals honestly provided negative interpersonal feedback to a close relational partner. In Study 3, we also test whether communicators' (mis)predictions about the personal consequences of honesty are mediated by their (mis)predictions about others' reactions to honesty. Across our studies, we decided our sample sizes in advance, and we report all manipulations and measures we collected. The institutional review board (IRB) of the University of Pennsylvania approved all aspects of the Pilot Study and Study 1, and the IRB of the University of Chicago approved all aspects of

Studies 2 and 3. Data and materials from our studies can be found on the Open Science Framework: https://tinyurl.com/y8kaj356.[2]

Study 1

Study 1 contained two separate samples: Experiencers (Study 1a) and Forecasters (Study 1b). Experiencers were randomly assigned to be completely honest, kind, or conscious of their communication in every interaction for three days. Forecasters made predictions about all three experiences, but did not actually engage in the experience. Because our hypotheses involve the contrast between forecasts and experiences and we only collected forecasts in Study 1b, we first describe the methods of both studies, and then present our analyses comparing Study 1a to 1b.

To test the feasibility of the design and explore initial predictions, we ran a very similar Pilot Study prior to running Study 1. The main difference between the Pilot Study and Study 1 was that the Pilot Study included additional exploratory measures and featured a within-subjects design, such that participants made forecasts about the three-day communication study before engaging in it. The Pilot Study and Study 1 yielded nearly identical results. We report full information about the Pilot Study in our online supplemental materials.

Study 1a: Experiencers

Sample. Our goal was to recruit 50 participants per cell in Study 1 based on our Pilot Study and recommended research practices (Simmons, Nelson, & Simonsohn, 2013). One-hundred fifty-seven adults initially enrolled in our study, 154 of whom completed at least one

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[2] Due to privacy concerns, we stripped participants' open-ended responses (about their difficult conversations) from our posted data sets. These data are available from the authors upon request.

daily survey (98.1%; 70.8% female[3], mean age = 22), and 144 of whom completed the two-week follow-up (91.7%).

Method. We ran Study 1a on three consecutive Thursdays during March and April of 2016. We conducted the study over consecutive weeks so that all participants would participate in the study for two weekdays (Thursday, Friday) and one weekend day (Saturday), in order to maximize the number of different types of relational partners participants interacted with.

We advertised Study 1 to a panel of adults and students in Philadelphia, Pennsylvania. All individuals who had ever completed a study at this laboratory received an email informing them that a new study was available and that to learn more, they should complete a short introductory survey. The introductory survey contained information about a study called the "Challenging Exercise" study, which was an optional 3-day experiment that would challenge the way they communicate with others. In exchange for their participation, participants would earn $20 and the chance to win an iPad mini. Participants were informed of the time commitment of the study and the potential distress that could be caused by participating. However, they were not provided any information about the experimental conditions at this time.

Participants had to provide their participant ID to indicate their interest and that they understood the nature of the study. Participants who provided their participant ID were eligible to sign up for the study online through the laboratory's online portal.

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[3] We conducted exploratory analyses in every study to examine potential gender effects. Although gender occasionally interacts with our manipulations, we do not find consistent patterns of gender effects across studies. Because we did not develop hypotheses about gender, and because the findings were not consistent across the studies, we do not discuss gender further. Further information about gender in our studies is available on the Open Science Framework: https://tinyurl.com/y8kaj356.

Assignment to condition. Participants who signed up for the study were required to come to the laboratory to receive specific instructions about the study. When participants arrived at the lab, they were assigned to one of three experimental conditions: honesty, kindness, or a control condition. We randomized condition at the session-level. That is, each session of participants (i.e., the group of participants that arrived at the lab during the same time) was assigned to the same condition. Participants learned about the experimental condition verbally, and had the opportunity to ask questions. Because of the verbal instructions, it was necessary to have each session of participants assigned to a single condition. There were 30 sessions in all; 10 honesty sessions, 10 kindness sessions, and 10 control sessions.

A trained research assistant, who was blind to our hypotheses, first provided some basic information about the study and prompted individuals to consider the tradeoff between honesty and kindness during difficult conversations. Then s/he instructed participants how to behave for the next three days according to their experimental condition and invited questions from participants to ensure their understanding of the protocol.

Specifically, the research assistant explained:

[*Honesty* condition]

Throughout the next three days – that means today, tomorrow, and the following day - please strive to be absolutely honest in every conversation you have with every person you talk to. Really try to be completely candid and open when you are sharing your thoughts, feelings, and opinions with others. You should be honest in every conversation you have, in every interaction, with every person in your life. Even though this may be difficult, you should do your absolute best to be honest.

[*Kindness* condition]

Throughout the next three days – that means today, tomorrow, and the following day - please strive to be kind in every conversation you have with every person you talk to. Really try to be caring and considerate when you are sharing your thoughts, feelings, and opinions. You should be kind in every conversation you have, in every interaction, with every person in your life. Even though this may be difficult, you should do your absolute best to be kind.

[Communication-consciousness– *Control* condition]

Throughout the next three days – that means today, tomorrow, and the following day - please be conscious of the way you communicate with others. Please act as you normally would throughout the length of this study. You should not change your behavior, but you should be conscious of it.

Participants were instructed not to tell anyone about the experiment, including their relational and conversational partners. We include the full script for each condition in Appendix A.

Participants were then directed to a computerized survey. Within the survey, participants first provided demographic information. Then, they re-read the instructions associated with their condition and responded to a one-item comprehension check, asking them what their goal in the study was (response-options: "To be honest in all of my communication", "To be kind in all my communication," or "To communicate as I normally do, but be conscious of my communication"). Participants had to answer the comprehension check correctly to proceed with the study.

Next, participants provided their email address to indicate their continued consent, and to allow us to contact them with their nightly surveys. Finally, participants were asked to confirm their commitment to the study by typing the following statement into the survey, "*For the next three days, I will [communicate honestly, communicate kindly, be conscious of my communication].*"

Before leaving the laboratory, participants were reminded of their study condition and were instructed to begin the study immediately. Participants were told that they would receive their first nightly survey that evening at 6pm. Participants had to say aloud, "I agree to participate." Upon leaving the laboratory, participants received a small card that read

"Communicate Honestly," "Communicate Kindly," or "Be Conscious of Your Communication" and the dates of the study, and directed participants to email the experimenter if they had any questions.

   Nightly surveys. We tracked participants' behavior over three consecutive days. We emailed participants a nightly survey on Thursday and Friday at 6pm, and on Saturday at 12pm. We emailed participants earlier in the day on Saturday to ensure they would not miss the survey.

   When completing the nightly surveys, participants first responded to several open-ended questions. We list all questions that we asked and discuss the coding and results associated with these questions in the online supplemental materials.

   Then, participants responded to our focal measures. Using five-point bipolar rating scales, participants rated the degree to which their experience in the study that day was difficult-easy, unpleasant-pleasant, unenjoyable-enjoyable, stressful-relaxing, meaningless-meaningful, constraining-liberating, unfulfilling-fulfilling, futile-enriching, socially isolating-socially connecting, and alienating-uniting. We combined the first four items into a single measure of enjoyment (daily αs > .82; this is our measure of hedonic well-being). We combined the middle four items (meaningful, liberating, fulfilling, enriching) into a single measure of meaning (daily αs > .77; this is our measure of eudaimonic well-being). We combined the final two items into a single measure of social connection (daily rs > .60). Our measures of hedonic and eudaimonic well-being are consistent with existing research (e.g., Huta & Ryan, 2010; 2015) that conceptualizes the hedonic motives as seeking enjoyment, pleasure, and relaxation, and eudaimonic motives seeking to learn something or do what you believe in.

   Next, participants rated their agreement (1 = *strongly disagree*, 7 = *strongly agree*) with two manipulation check items: "I was completely honest and candid in every conversation I had

today" and "I was kind and compassionate during every conversation I had today." Finally, we asked participants to reflect on their experience that day and to explain how they either did or did not comply with the experiment. We also asked them to write about any challenges they faced and how it felt to focus on [honesty, kindness, their communication]. Participants were given the lead experimenter's email address and were invited to reach out to her with questions or concerns at any time.

Reflection survey. Two weeks after participants completed the third and final day of the experiment, they were emailed a final reflection survey. Participants first responded to several open-ended questions, asking them what they learned, how their behavior and communication had changed, what difficulties they had, any surprises they faced, and how their relationships changed. We provide more details about these open-ended questions in Study 1c and Appendix B.

In the two-week follow-up survey, participants also rated their agreement with 23 statements intended to assess the degree to which the experiment had lasting impact on their honesty and kindness, improved their hedonic and eudaimonic well-being, as well as improved and harmed their relationships. Importantly, participants also assessed the degree to which they appreciated the experience. We provide all items and the scale reliabilities in Appendix C (all rs > .71, αs > .91).

Before exiting the survey, participants indicated whether they would prefer their $20 payment via PayPal, Venmo or by receiving an Amazon.com gift card. We randomly selected one participant to win the iPad mini and we compensated all participants within one week.

--- Table 1 about here ---

Study 1b: Forecasters

Sample. We ran Study 1b shortly after we ran Study 1a, recruiting from the same participation pool, with the stipulation that participants who completed Study 1a were not eligible to complete Study 1b. Two-hundred twenty-three participants (65.0% female, mean age = 22) participated in Study 1b and were included in our analyses. An additional four participants completed the survey, but indicated they had already participated in Study 1a (despite passing our initial screener). These participants were deleted from the dataset before any analyses were conducted.

Method. Following the procedure of Epley and Schroeder (2014), we included the same conditions in the forecasting study (Study 1b) as we included in the experience study (Study 1a), but we manipulated the conditions within-, rather than between-, subjects. Specifically, participants learned about an experiment that was taking place, called "the Challenging Exercise" study. We described the protocol of the Challenging Exercise study (Study 1a) very closely. Participants learned that individuals who enrolled in the Challenging Exercise study would have to make modifications to their communication for three days and complete nightly surveys, and that the experience might cause discomfort. Then, all participants learned about all three conditions of the study—honesty, kindness, and communication-consciousness—and read the exact instructions that participants in the Challenging Exercise study (Study 1a) actually received. After reading about each of the conditions of the study, participants made a hypothetical choice and selected the one condition they would want to participate in. Note that participants did not actually engage in the Challenging Exercise study (i.e., the three-day communication interventions).

Then, participants were asked to imagine participating in each condition of the study and to imagine being honest [being kind, being conscious of their communication] for three days.

Participants forecasted their level of enjoyment, social connection, and meaning in each of the experimental conditions using the same items we used in Study 1a (all rs > .65, αs > .80).

Next, we asked participants to imagine that they had completed the study and were evaluating the experience of being honest [being kind, being conscious of their communication] two weeks later. Participants then responded to the same 23 items we collected during the follow-up reflection survey of Study 1a (see Appendix B; all rs > .45, αs > .73). Then, we asked participants to imagine they actually had to participate in one condition in the study. Participants selected the one condition they would want to participate in. Participants provided demographic information before they were dismissed.

Results

Analytical approach. Within each construct (e.g., enjoyment, social connection, meaning), we present Forecasters' predictions (Study 1b), followed by Experiencers' actual ratings (Study 1a). To analyze Experiencers' actual ratings in Study 1a we created daily average variables by taking the three-day average of the respective dependent variables (e.g., daily enjoyment, social connection, and meaning) that we collected during the experiment. We conduct our manipulation check and experienced enjoyment, social connection, and meaning analyses using all participants who responded to at least one daily survey and we conduct our subjective long-term impact analyses using all participants who completed at least one daily survey as well as completed the two-week follow-up. Table 1 depicts the number and percentage of participants who began and completed each stage of the experiment across our conditions. To check the robustness of our results, we also replicated our findings using missing data analyses with five sets of multiple imputation data, and none of our results are substantively different when analyzing the raw data compared to the imputed data.

We present the results of planned t-tests between predictions (Study 1b) and actual ratings (Study 1a) of each construct, within each condition. Finally, we examine Forecasters' choices. We present all descriptive statistics, as well as all non-focal results (daily manipulation checks, Long-term honesty and kindness) in Table 2.

<div align="center">---Tables 1 and 2 about here---</div>

Daily experience measures.

Enjoyment.

Forecasted Enjoyment. A repeated measures ANOVA revealed a significant effect of *Condition, F*(2,442) = 96.23, $p < .001$, $\eta_p^2 = .30$. Forecasters (i.e., participants in Study 1b) expected the *Honesty* condition to be less enjoyable than the *Control* and *Kindness* conditions (*ts* > 9.48, *p*s < .001, *d*s > .85).

Actual Enjoyment. Actual enjoyment from Experiencers (i.e., participants in Study 1a) did not mirror the forecasts. A one-way ANOVA revealed a marginal effect of Condition, F(2,151) = 2.76, $p = .066$, $\eta_p^2 = .035$. Unlike the Forecasters, Experiencers rated the *Honesty* condition as no more or less enjoyable than the *Control* or *Kindness* conditions (*ps* > .17). In fact, the marginal trend was such that the *Control* condition was slightly *less* enjoyable than the *Honesty* and *Kindness* conditions.

Consistent with our hypothesis, participants in Study 1b expected *Honesty* to be significantly less enjoyable than participants in Study 1a actually experienced it to be (*t*(270) = 6.86, $p < .001$, $d = 1.15$). Importantly, Forecasters did not mispredict the enjoyment associated with the *Kindness* or *Control* conditions (*ps* > .68). We depict these results in Figure 1.

<div align="center">---Figure 1 about here---</div>

Social connection.

Forecasted social connection. A repeated measures ANOVA revealed a significant effect of *Condition, F*(2,442) = 141.12, *p* < .001, η_p^2 = .39. Forecasters (i.e., participants in Study 1b) expected the *Honesty* condition to be less socially connecting than the *Control* condition and the *Kindness* condition (*ts* > 7.8, *p*s < .001, *ds* > .64).

Actual social connection. Actual social connection did not mirror these forecasts. A one-way ANOVA revealed a significant effect of *Condition*, *F*(2,151) = 4.89, *p* = .009, η_p^2 = .061, but unlike the forecasts, participants in the *Honesty* condition of Study 1a actually felt marginally more socially connected than participants in the *Control* condition (*t*(94) =1.79, *p* = .076, *d* = .36), and no more or less connected than participants in the *Kindness* condition (*p* = .20). We depict these results in Figure 2.

Consistent with our predictions, participants in Study 1b expected *Honesty* to be significantly less socially connecting than participants in Study 1a actually experienced it to be (*t*(270) = 5.46, *p* < .001, *d* = 0.97). Interestingly, participants overestimated the social connection associated with *Kindness* (*t*(278) = 2.87, *p* = .004, *d* = 0.45), but they did not mispredict the social connection associated with the *Control* condition (*p* = .84). These results are shown in Figure 2.

---Figure 2 about here---

Meaning.

Forecasted meaning. A repeated measures ANOVA revealed a marginal effect of *Condition, F*(2,442) = 2.47, *p* = .086, η_p^2 = .01. Forecasters (i.e., participants in Study 1b) expected the *Honesty* condition to be less meaningful than the *Kindness* condition (*t*(221) = 2.12, *p* = .035, *d* = .17) and no different than the *Control* condition (*p* = .67).

Actual meaning. A one-way ANOVA revealed a significant effect of *Condition* on meaning, $F(2,151) = 7.05$, $p = .001$, $\eta_p^2 = .085$, but unlike the forecasts, participants in the *Honesty* condition experienced similar levels of meaning compared to participants in the *Kindness* condition ($p = .17$), and greater meaning than participants in the *Control* condition ($t(94) = 3.70$, $p < .001$, $d = .79$).

Interestingly, participants in Study 1b expected *Honesty* to be somewhat less meaningful than participants in Study 1a actually experienced it to be ($t(270) = 1.76$, $p = .088$, $d = 0.30$). Participants did not mispredict the meaning associated with the *Kindness* condition ($p = .44$), and they actually overestimated the meaning associated with the *Control* condition ($t(268) = 2.09$, $p = .037$, $d = 0.37$). We depict these results in Figure 3.

Although Forecasters marginally underestimated the meaning associated with honesty than the Experiencers reported, the magnitude of this misprediction was substantially smaller ($d = .30$) than mispredictions of enjoyment ($d = 1.12$) and social connection ($d = .97$).

---Figure 3 about here---

Subjective long-term impact.

Long-term hedonic well-being.

Predicted long-term hedonic well-being. A one-way ANOVA revealed a significant effect of *Condition* on predicted long-term enjoyment, $F(2,422) = 52.54$, $p < .001$, $\eta_p^2 = .19$. Forecasters (i.e., participants in Study 1b) expected to derive less long-term enjoyment in the *Honesty* condition than in the *Control* and *Kindness* conditions (ts > 4.21, ps < .001, ds > .36).

Actual long-term hedonic well-being. Actual long-term hedonic well-being did not mirror these forecasts. A one-way ANOVA revealed a significant effect of *Condition* on long-term enjoyment, $F(2,141) = 3.64$, $p = .029$, $\eta_p^2 = .049$, such that participants in the *Honesty* condition

in Study 1a derived marginally greater enjoyment than participants in the *Control* condition

($t(84) = 1.94$, $p = .054$, $d = .43$), and similar levels of enjoyment as participants in the *Kindness*

condition ($p = .59$).

Further supporting our predictions, Forecasters (i.e., participants in Study 1b)

underestimated the long-term enjoyment associated with *Honesty* ($t(264) = 2.26$, $p = .025$, $d =$

0.39). However, they actually overestimated the long-term enjoyment associated with the

Control condition ($t(264) = 2.45$, $p = .015$, $d = 0.41$) and the *Kindness* condition ($t(279) = 2.47$,

$p = .014$, $d = 0.37$).

Long-term relational improvement.

Predicted long-term relational improvement. A one-way ANOVA revealed a significant

effect of *Condition* on predicted long-term relational improvement, $F(2,422) = 26.82$, $p < .001$,

$\eta_p^2 = .11$. Forecasters (i.e., participants in Study 1b) expected to improve their relationships less

in the *Honesty* condition than in the *Control* and *Kindness* conditions (ts > 5.98, ps $< .001$, ds >

.51).

Actual long-term relational improvement. Actual relational improvement did not mirror

these forecasts. A one-way ANOVA revealed a marginal effect of *Condition, $F(2,141) = 2.37$, $p*

$= .097$, $\eta_p^2 = .03$, but unlike the forecasts, participants in Study 1a reported greater relationship

improvement in the *Honesty* condition than in the *Control* condition ($t(84) = 2.15$, $p = .03$, $d =$

.49), and there was no difference between the *Honesty* condition and *Kindness* conditions ($p =$

.16).

Forecasters (i.e., participants in Study 1b) did not mispredict the relational improvements

associated with *Honesty* ($p = .21$). However, they overestimated the relational improvement

associated with the *Control* condition ($t(264) = 5.63$, $p < .001$, $d = 0.85$) and the *Kindness*

condition ($t(279) = 4.18$, $p < .001$, $d = 0.59$).

Long-term relational harm.

Predicted long-term relational harm. A one-way ANOVA revealed a significant effect of

Condition on predicted long-term relational harm, $F(2,422) = 138.39$, $p < .001$, $\eta_p^2 = .39$.

Forecasters (i.e., participants in Study 1b) expected to harm their relationships more in the

Honesty condition than in the *Kindness* and *Control* conditions (ts > 13.38, ps $< .001$, ds > 1.23).

Actual long-term relational harm. Actual relational improvement did not mirror these

forecasts. A one-way ANOVA revealed no main effect of *Condition* on actual relational harm,

$F(2,141) = 1.57$, $p = .21$, $\eta_p^2 = .02$.

Forecasters (i.e., participants in Study 1b) expected that *Honesty* would cause greater

relational harm than participants in Study 1a actually experienced ($t(264) = 6.49$, $p < .001$, $d =$

1.01). Participants also overestimated relational harm in the *Control* condition ($t(264) = 2.08$, $p =$

$.039$, $d = .32$), but they did not mispredict relational harm in the *Kindness* condition ($p = .68$).

Long-term eudaimonic well-being.

Predicted long-term eudaimonic well-being. A one-way ANOVA revealed a significant

effect of *Condition* on predicted long-term meaning, $F(2,422) = 15.54$, $p < .001$, $\eta_p^2 = .07$.

Participants in the *Honesty* condition of Study 1b expected to derive less long-term meaning than

in the *Kindness* and *Control* conditions (ts > 2.24, ps $\leq .025$, ds $> .15$).

Actual long-term eudaimonic well-being. A one-way ANOVA revealed no main effect of

Condition, $F(2,141) = 1.45$, $p = .24$, $\eta_p^2 = .02$.

Participants did not mispredict the long-term meaning they would derive from *Honesty* (*p* = .90) or *Kindness* (*p* = .22). Participants overestimated the long-term meaning they would derive from the *Control* condition ($t(264) = 5.05, p < .001, d = .78$).

Appreciation of the experience.

Predicted appreciation of the experience. A one-way ANOVA revealed a significant effect of *Condition, F*(2,422) = 33.84, $p < .001$, $\eta_p^2 = .13$. Participants in Study 1b expected to appreciate their experience less in the *Honesty* condition than in the *Control* and the *Kindness* conditions ($ts > 6.43, ps < .001, ds > 0.52$).

Actual appreciation for the experience. Actual appreciation did not mirror these forecasts. A one-way ANOVA revealed no main effect of *Condition, F*(2,141) = 1.71, $p = .19$, $\eta_p^2 = .02$.

Importantly, Forecasters (i.e., participants in Study 1b) underestimated how much Experiencers (i.e., participants in Study 1a) actually appreciated the experience of *Honesty* ($t(264) = 3.38, p < .001, d = 0.62$). Participants in Study 1b did not mispredict their appreciation of *Kindness* ($p = .66$) and they marginally overestimated their appreciation of the *Control* condition ($t(264) = 1.84, p = .067, d = 0.30$).

Choice. Using the Study 1b data, we assessed participants' choices for which condition to participate in by conducting a chi-square goodness of fit test against the null hypothesis that there were no significant differences in preferences across the three conditions (i.e., expected proportion of 33.3% for each of the three conditions). Participants were not equally likely to choose to participate in all three conditions; $\chi^2(2) = 15.75, p < .001$. Specifically, they were less likely to choose the *Honesty* condition (21.1%) compared to the *Kindness* (37.2%) and *Control* conditions (41.7%).

The drivers of choice versus appreciation. We conducted regression analyses using the data from Studies 1a and 1b to examine what predictions and experiences underlie participants' a priori decisions to avoid honesty and their post-hoc appreciation for honesty. Specifically, a logistic regression on the choice *not* to engage in honesty in Study 1b (1 = chose kindness or control, 0 = chose honesty), using forecasted enjoyment, meaning, and social connection (within the honesty condition) as independent variables revealed that the choice to avoid honesty was associated with predicted enjoyment ($b = -.53$, $SE = .21$, $p = .014$, $OR = .59$), but not predicted meaning ($b = -.45$, $SE = .28$, $p = .11$, $OR = .64$) and social connection ($b = -.19$, $SE = .22$, $p = .39$, $OR = .83$). These results suggest that mispredictions of the *hedonic* consequences of honesty may be the strongest deterrent for being honest.

On the other hand, a regression on appreciation of the experience (in the honesty condition) in Study 1a, using judgments of enjoyment, meaning, and social connection as independent variables revealed that reported *meaning* significantly predicted participants' appreciation of honesty ($b = 1.13$, $SE = .44$, $p = .014$), but enjoyment ($b = .13$, $SE = .30$, $p = .65$) and social connection ($b = -.49$, $SE = .47$, $p = .30$) did not. In other words, individuals' appreciation of an experience is driven primarily by the meaning associated with it, but their choice of which experience to engage in is driven by their predictions of enjoyment.

Study 1a and 1b Discussion

Mispredicting the consequences of honesty. Study 1 provides evidence consistent with our hypotheses. Importantly, we find that honesty is significantly more enjoyable (during the experiment and two weeks later) than people expect. Furthermore, honesty yielded significantly greater social connection during the experiment, and caused less relational harm two weeks later than participants expected. We find marginal evidence that people underestimate the immediate

eudaimonic consequences of honesty, and no evidence that they mispredict the long-term eudaimonic consequences of honesty.

Interestingly, we also find that individuals mispredict their appreciation of honesty. Forecasters (i.e., participants in Study 1b) predicted they would be much less likely to recommend or want to repeat the experience of honesty than Experiencers (i.e., participants in Study 1a) indicated two weeks after completing the study. Participants' level of experienced meaning drove this appreciation for honesty. However, similar to our Pilot Study, participants' level of (anticipated) enjoyment, *not meaning,* predicted their choice not to participate in the honesty condition (Study 1b). Taken together, these results suggest that individuals may be making a mistake. Individuals' *miscalibrated* hedonic expectations lead them to avoid a behavior that they appreciate post hoc.

The consequences of honesty. A secondary, and more exploratory, goal of this study was to examine the consequences of honesty, broadly. We find that focusing on honesty often has some positive consequences relative to simply being conscious of your communication (our control condition), but, in many ways, does not differ from focusing on kindness. For example, participants in our honesty condition experienced greater meaning during the three-day experiment and greater relational improvements two-weeks later than participants in our control condition. However, participants in our honesty condition did not experience significantly different consequences than participants in our kindness condition on any measure. These results replicate the results of our Pilot Study, in which we also find evidence that honesty yields some benefits relative to our control condition, but is no different than kindness. These results suggest that being intentional in one's communication, and focusing on any moral value – whether it is honesty *or* kindness – may yield psychological and relational benefits, relative to one's usual

communication. Furthermore, these results provide further evidence that individuals may be making a mistake when they choose to communicate as usual, rather than communicating honestly.

Study 1c: Text analysis

We analyzed the text from the two-week reflection surveys that the participants in Study 1a provided. These data provide us with insight into participants' actual thought processes and conversations, and thus are useful for understanding the mechanisms underlying our effects. We note, however, that our analyses of these data are limited by the retrospective self-report nature of participants' responses. Thus, we interpret them as primarily illustrative and inductive, rather than as providing us with conclusive evidence of the nature of our participants' experiences.

Survey questions. Two weeks after the experiment ended, participants completed a survey that included several open-ended questions about participants' experiences, whether the three-day experiment caused participants to implement changes in their life, whether their experience surprised them, and what they learned during the experiment (see Appendix C for exact questions). We coded participants' responses from both Study 1 and the Pilot Study according to three meta-categories: What happened during the experiment, How the experience differed from expectations, and Long-term impact of the experiment. We coded each participant's responses to our focal questions as a single transcript, and coded the transcript for the presence or absence of 14 categories (7 categories about *What happened during the experiment*, 4 categories about *How the experience different from expectations*, and 3 *Long-term impact* categories). Descriptions of our coding procedures, all coding categories, examples of participant responses, and the frequency with which the categories occurred in each experimental condition are provided in Table 3, along with inter-rater reliability statistics.

---Table 3 about here---

What happened during the experiment. Participants' two-week reflections indicate that they took the experiment seriously and followed the instructions of each condition. The qualitative coding revealed significant differences in the extent to which participants expressed themselves honestly, overall $\chi^2(2) = 45.26$, $p < .001$, which we consider to be a manipulation check. Specifically, participants were significantly more likely to express themselves honestly in the *Honesty* condition (48.8%) than in the *Kindness* (3.4%, $\chi^2(1) = 18.93$, $p < .001$) and *Control* conditions (2.4%, $\chi^2(1) = 23.90$, $p < .001$).

Interestingly, we also found that participants were more likely to discuss difficult topics in the *Honesty* condition (11.6%) than in the *Control* condition (0.0%; $\chi^2(1) = 5.19$, $p = .023$), but not compared to the *Kindness* condition (6.9%, $\chi^2(1) = .68$, $p = .41$), overall $\chi^2(2) = 4.93$, $p = .085$. Participants were also more likely to generate conflict in the *Honesty* condition (11.6%) than in the *Kindness* (0.0%; $\chi^2(1) = 7.10$, $p = .008$) and *Control* conditions (0.0%; $\chi^2(1) = 5.07$, $p = .024$), overall $\chi^2(2) = 11.93$, $p = .003$.

Importantly, we did *not* find that the experiment caused people to avoid social interactions. This point is important because one possible explanation of our results could be that individuals expect to engage in difficult conversations (particularly in the *Honesty* condition), but avoid those conversations during the actual experience. We do not find support for this idea. Relatively few people reported avoiding social interactions or limiting their conversations, and this did not differ significantly across conditions (*Honesty* = 2.3% vs. *Kindness* = 3.4% vs. *Control* = 4.8%; overall $\chi^2(2) = 0.37$, $p = .83$).

To illustrate the experiences participants had in Study 1, consider the quotes below. These reflections from participants in the *Honesty* condition highlight the nature of their experiences and how it affected their lives.

Participant 94 (*Honesty* condition): *I remember one of the three days I was asked to be honest, I ended up lashing out at my coworker. This caused a little bit of tension between us that I feel is still there. Nonetheless, I feel it was worth it. What's the point of pretending when someone asks how you are feeling. Additionally, being able to ask whatever question I desired and to answer in a truthful manner allowed me and the other person I was speaking to to be more open and comfortable. I feel that generally, being honest allows for better relationships and more trust. Since that experience, I have been trying to be more honest in my daily life.*

Participant 128 (*Honesty* condition): *I felt less fake when I didn't have to lie about what I thought about some things or say I felt fine when I didn't. Tension was building up with my roommate because couldn't bring myself to tell her the things she was doing that were annoying and while doing the study I told her all those things I had been avoiding to tell her for a while and it felt kind of liberating. Some of the things were kind of awkward but others felt good and it helped ease some of the tension. I learned that it feels better to say those kind of things instead of keeping them inside until it explodes.*

How the experience differed from expectations. We explored four possible ways in which participants' experiences may have differed from their expectations (and thus could have caused them to mispredict the consequences of honesty): the experience was easier than they expected, the experience led to better social interactions than they expected, the experience was harder than they expected, and the experience led to worse social interactions than they expected. Our coding suggests that one mechanism underlying participants' mispredictions could be that honesty caused them to have better social interactions than they expected, overall $\chi^2(2) = 8.93$, $p = .01$. Participants were more likely to indicate that they had better interactions with others than they expected in the *Honesty* condition (18.6%) than in the *Control* condition (0.0%; $\chi^2(1) = 8.63$, $p = .003$), but not the *Kindness* condition (8.6%, $\chi^2(1) = .2.20$, $p = .14$). In other words, participants expected their conversational and relational partners to react more poorly to their honesty than they actually did. The following quotes from participants in the *Honesty* condition illustrate this sentiment:b

Participant 34 (*Honesty* condition): *I was particularly surprised when being honest got me further in my position in an organization because voicing my honest opinion made others think about the situation more and come to the conclusion that I was thinking as well.*

Participant 49 (*Honesty* condition): *People reacted differently than what I thought. They liked and appreciated the honesty and honestly I did not believe that would happen. It was refreshing meaning that I was happy to talk what was on my mind and not worry about what was said.*

We do not find evidence that participants were more likely to believe that the experience was easier than they expected, harder than they expected, or lead to worse social interactions than they expected in the honesty condition relative to our other conditions ($ps > .19$).

Long-term impact of the experiment. Many (but not all) participants across the conditions believed they learned about the importance of communication (*Honesty* = 32.6% vs. *Kindness* = 31.0% vs. *Control* = 35.7%), that the experiment positively impacted their lives (*Honesty* = 11.6% vs. *Kindness* = 5.5% vs. *Control* = 4.8%), and that the experiment prompted them to change their life in the future (*Honesty* = 39.5% vs. *Kindness* = 41.4% vs. *Control* = 45.2%). None of these categories significantly differed across experimental conditions.

Study 1c Discussion. Our analysis of participants' two-week reflections reveals several key insights. First, as illustrated by the quotes, and supported by the qualitative coding of the open-ended responses, participants appeared fully committed to the study and followed the instructions associated with each condition. Accordingly, participants in the *Honesty* condition shared information that they would have otherwise left unsaid, which lead to more difficult conversations, and increased conflict. Importantly, participants in the *Honesty* condition did not simply avoid social interactions or limit their conversations. Instead, they challenged themselves to pursue the goals of the study, and ultimately found their experience rewarding despite whatever difficulties they may have faced. Finally, our results highlight an important potential

mechanism underlying individuals' mispredictions of honesty: participants' relational partners reacted more positively to their honest communication than participants expected, which may have influenced both enjoyment and social connection during the study.

Study 1 Post-test

Although we carefully crafted our manipulation in the field experiment to parallel our theoretical definition of honesty (*speaking in accordance with one's own beliefs, thoughts and feelings*), we recognize that we also explicitly mentioned the trade-off between honesty and kindness in the study instructions. Thus, upon reading our instructions, participants may have associated honesty with harshness, but upon engaging in honesty, realized that they did not actually need to speak harshly. In other words, participants' subjective meaning of honesty may have shifted during different phases of the experiment. We ran a post-test to examine this possibility.

Specifically, we presented the instructions from our honesty condition in Study 1a to a new set of participants from the same laboratory pool used in Study 1 ($N = 144$). We asked them to identify the primary goal of the study (by selecting one option from a selection of six possible choices: "To encourage participants to speak bluntly"; "To encourage participants to speak harshly"; "To encourage participants to share their private opinions"; "To encourage participants to be truthful"; "To encourage participants to engage in self disclosure"; "To encourage participants to avoid lying"). We found that the majority of participants (68.1%) believed that the instructions were designed to encourage participants to be truthful. Respectively, 18.8%, 7.6%, 3.5%, and 2.1% of participants believed that the primary goal of the study was to encourage participants to engage in self-disclosure, to avoid lying, to speak bluntly, and to share their

private opinions. No participants believed that the primary goal of the study was to encourage participants to speak harshly.

We also had participants rate their agreement with each of the following statements (1= *strongly disagree*, 7 = *strongly agree*): "These instructions are asking participants to…speak bluntly, to speak harshly, to share private opinions, to be truthful, to engage in self-disclosure, to avoid lying." Participants generally agreed that the instructions asked participants to be truthful ($M = 6.45$, $SD = .75$), to avoid lying ($M = 6.06$, $SD = 1.12$), and to engage in self-disclosure ($M = 6.45$, $SD = .75$), consistent with our theoretical conceptualization of honesty (all means were significantly above the scale midpoint, $ps < .001$). To some extent, participants also believed that the instructions were asking them to share their private opinions ($M = 4.53$, $SD = 1.55$), and to speak bluntly ($M = 4.48$, $SD = 1.57$; these means were also significantly above the scale midpoint, but significantly below the means of the prior three items, $ps < .001$). Participants generally disagreed that the instructions were asking them to speak harshly ($M = 2.72$, $SD = 1.23$; significantly below the scale midpoint, $ps < .001$). These results suggest that participants seem to have interpreted our instructions as intended: they associated honesty with truthfulness and self-disclosure.

Study 2: A deeper look into the consequences of honesty

Study 1 examined the consequences of focusing on honesty in one's everyday life across one's social interactions. Our qualitative coding and our post-test of Study 1 suggest that our three-day honesty intervention caused individuals to engage in greater self-disclosure and initiate more difficult conversations than they would have otherwise. In Studies 2 and 3, we examine the consequences of these honest conversations (involving self-disclosure and difficult topics) in greater detail, using well-controlled laboratory paradigms. In doing so, we gain greater insight

into the mechanisms underlying our findings and rule out several potential confounds present in the field experiments.

First, we address the possibility that forecasters mispredicted the *types* of conversations that would actually occur during the three-day field study, rather than the *experience of engaging* in those conversations. Just as individuals imagine that they would have the courage to confront racism but rarely do (Kawakami, Dunn, Karmali, & Dovidio, 2014), it is possible that when considering the consequences of honesty, participants imagined that they would have the courage to state their most ardent criticisms or most embarrassing secrets, but perhaps did not do so during the actual three-day experiment. Although our qualitative analyses in Study 1c suggest that focusing on honesty did prompt more difficult conversations, and our Study 1 Post-test suggests that participants did not necessarily associate honesty with harshness, we do not yet fully understand whether individuals expected the content and delivery of their conversations (rather than the experience of these conversations) to be much different than they were. Furthermore, given that participants were not directly monitored during the field experiment, we cannot be positive that their self-reports (e.g., of how honest they were) were not biased by experimental demand.

In Study 2, we had forecasters and experiencers consider the exact same conversational topics (a series of questions that promote interpersonal evaluation and self-disclosure), thereby ruling out differences in the topics of predicted conversations as the key driver of our results. Furthermore, we monitor participants' behavior in Study 2 to address the possibility that participants in Study 1 did not fully commit to the experiment, and that their self-reports simply reflect experimental demand. In Study 2, research assistants watched participants engage in

honest conversations and reminded them to follow the experimental instructions if they were not fully engaged.

In Study 2, we also expand our investigation by examining how honest communication influences both communicators and targets. Although Study 1 suggests that communicating honestly can create meaning for communicators (relative to our control condition), and does not harm enjoyment or social connection, it is not clear that honesty will influence targets (i.e., those on the receiving end of the honesty) in the same way. In fact, recent research demonstrates that relational partners often resent painful honesty (Levine & Schweitzer, 2015). Investigating targets' reactions allows us to provide further insight into the mechanism we identified in the qualitative analyses: that individuals expect targets to react more poorly to honesty than they actually do. In Study 2, we also randomly assign participants to Forecaster and Experiencer roles, rather than comparing them across studies.

Method

Sample. We ran Study 2 in the fall of 2016 in Chicago, Illinois. We aimed to recruit 50 pairs of Experiencers and 50 pairs of Forecasters and ended up with a final sample of 50 Experiencer-pairs and 51 Forecaster-pairs (Askers: 41.3% female, mean age = 29; Responders: 41.0% female, mean age = 28). We preregistered all aspects of this study at AsPredicted.org (http://aspredicted.org/blind.php?x=jqfjrp). A total of 31.2% of dyads were female-female pairs, 32.1% were mixed-sex pairs, and 36.7% were male-male pairs.

A total of 62.4% of participants participated in the study with someone they considered to be a friend, 12.8% with a roommate, 16.5% with a non-married romantic partner, 7.3% with a colleague or teammate, 4.6% with a spouse, 2.8% with a relative, and 6.4% with a neighbor.

Note that these categories were not mutually exclusive, so participants could have participated with someone they considered to be both a friend and a roommate.

Recruitment. We advertised Study 2 to a panel of adults in downtown Chicago (using a laboratory listserv and Craigslist) and students at a Chicago-area university (using a laboratory listserv and flyers). The recruitment materials informed potential participants that they would be required to come to the laboratory with a close relational partner (such as a roommate, romantic partner, close friend, or colleague) and that they would either be asked to have (or imagine having) a conversation in which they would discuss personal information and sensitive topics with this person. Participants knew they would earn $8-$10 for participation and that they would have the chance to win a $100 bonus. Participants who were assigned to the *Experiencer* condition ended up earning $10 for 40 minutes of participation, whereas participants who were assigned to the *Forecaster* condition ended up earning $8 for 20 minutes of participation.

Assignment to condition. Four research assistants, who were blind to our hypotheses, were responsible for running the study. Participants were randomly assigned to be either Forecasters or Experiencers when they arrived to the lab, based on a predetermined randomization schedule. Each pair of participants was seated in a private room at computers that were separated by a divider.

Experiencers. Experiencers began the study by completing a consent form on the computer. In their consent form, Experiencers could opt in to having their conversation filmed for research purposes (79% of experiencer-pairs were video-taped).

After participants completed their consent form, they read the study instructions on a new page of the survey, which the research assistant also read aloud. Specifically, participants learned that they would be required to have a 20-minute conversation, in which they would discuss 20

pre-specified questions with the partner they signed up with (the other person in the room).

Furthermore, participants learned that one person would be randomly assigned to the role of

Asker and one person would be randomly assigned to the role of Responder. The Asker would

ask questions, and the Responder would answer them.

Then, participants saw the list of 20 questions that they would ask or respond to. The first

10 questions were inspired by the topics of conversation that arose in Study 1 (e.g., "How is

school/work? Are you having any issues?", "How long do you expect our relationship to last?",

"Do you have any (positive or negative) opinions about me that you have been hesitant to share?

What are they?"). The second group of 10 questions were adapted from Aron, Melinat, Aron,

Vallone, and Bator's (1997) Interpersonal Closeness Task (e.g., "When did you last cry in front

of another person?"). Taken together, the questions were selected to increase both self-disclosure

(sharing thoughts, feelings, and opinions about the self) and interpersonal feedback (sharing

thoughts, feelings and opinions about one's relational partner). We expected that both sets of

questions could elicit difficult conversations, similar to those that were prompted by the honesty

intervention in Study 1. We provide the full question list in Appendix D. Participants were

instructed to discuss every question on the list.

Next, the research assistant randomly assigned participants to the role of Asker or

Responder. Participants then indicated their role in the computerized survey, and were directed

to role-specific instructions.

The Asker read:

> *You will ask the RESPONDER questions. Your goal is to listen to the RESPONDER's responses and respond as if you are having a natural conversation. You can respond to follow-up questions the RESPONDER may ask you. So, ask a question, listen to and process the RESPONDER's response carefully, and have a discussion about the response as needed. Then, move on to the next question.*

The Responder read:

> *The ASKER will ask you the questions on the list you received. Your goal in answering all questions is to be as honest as possible. Think carefully about your answers and focus on sharing your completely honest opinions, feelings, and reactions. This means that when the ASKER asks you a basic question like, "How are you?" you should openly share your feelings and speak authentically. TRY TO BE AS HONEST AS POSSIBLE. Please focus on being entirely honest, but do not do anything you are uncomfortable with.*

These role assignments allowed us to examine the consequences of honesty for both communicators (Responders, who were explicitly asked to be honest) and targets (Askers, who simply listened to the Responder's honesty).

When both participants were finished reading their instructions, the research assistant directed them to sit at a table, facing each other, and begin their 20-minute conversation. The research assistant then set up the video-camera (if both individuals had consented to it) and left the room. The research assistant checked on the participants and if they did not seem engaged in the study reminded them to follow the instructions and take the task seriously. The research assistant returned after 20 minutes to instruct them to end their conversation and complete their exit survey.

Forecasters. Forecasters followed a similar protocol (however, their consent form did not include a section on being video-taped). Forecasters' instructions specified that participants would *imagine having* a 20-minute conversation, and *imagine* being the Asker or Responder. Forecasters did not actually engage in a 20-minute conversation. Rather, they imagined the experience for at least two minutes, and then completed the exit survey.

Exit survey. After completing [imagining] their conversation, participants rated the conversation on enjoyment (Asker: $\alpha = .90$, Responder: $\alpha = .85$), social connection (Asker: $r = .63$, Responder: $r = .60$), and meaning (Asker: $\alpha = .86$, Responder: $\alpha = .82$) using the same scales

we used in Study 1. Then we asked participants to briefly explain their answers (free response).

Next, participants answered three questions about how the experiment influenced their

relationship: "This experience deepened [would deepen] my relationship with my conversational

partner" (reverse-scored), "This experience strained [would strain] my relationship with my

conversational partner," and "This experience caused [would cause] relational conflict with my

conversational partner." We combined these three items into a single measure of relational harm

(Asker: $\alpha = .81$, Responder: $\alpha = .73$). Participants were then asked to recall [imagine] what they

[their partner] had [would have] answered in response to each of the 20 questions.

Experiencers also answered three additional exploratory questions about how the

experience differed from their expectations: "Was your conversation more or less enjoyable than

you expected?" (1 = *Much less enjoyable than I expected*, 4 = *As enjoyable as I expected,* 7 =

Much more enjoyable than I expected), "Was your conversation more or less meaningful than

you expected?" (1 = *Much less meaningful than I expected*, 4 = *As meaningful as I expected,* 7 =

Much more meaningful than I expected), and "Did your conversation have a better or worse

effect on your relationship than you expected?" (1 = *Much worse for my relationship than I*

expected, 4 = *As good for my relationship as I expected*, 7 = *Much better for my relationship*

than I expected).

At the end of the survey, all participants answered a series of open-ended questions about

their partner and their experience: "Who was your conversational partner (e.g., friend, roommate,

significant other)?", "How long have you known each other?", "What was [do you think would

be] the most surprising thing revealed during your conversation?" and "Do you have any other

reactions to, or thoughts about your conversation [the conversation you imagined]?"

Participants then provided demographic information and were informed that they could earn a $100 bonus for completing a follow-up survey. Participants provided their email address if they were willing to complete the follow-up survey.

Experiencer follow-up survey. One week after Experiencers completed the study, they were emailed a follow-up survey. A total of 55% of the initial Experiencers ($n = 55$) completed the follow-up survey. The follow-up survey contained questions measuring Long-term hedonic well-being ($\alpha = .82$), Long-term eudaimonic well-being ($\alpha = .96$), Long-term relational harm ($\alpha = .81$), and Appreciation for the experience ($\alpha = .88$) using similar items to those we used in Study 1a. Before exiting the survey, participants indicated whether they would prefer to receive their $100 payment (if they were the randomly selected winner) via PayPal, Venmo or by receiving an Amazon.com gift card.

After the study closed, we also sent Forecasters a follow-up survey, so that they could also be entered into the $100 raffle. The forecaster follow-up survey simply asked them to confirm their email address and to indicate whether they would prefer to receive their $100 payment (if they were the randomly selected winner) via PayPal, Venmo or by receiving an Amazon.com gift card. We randomly selected one participant in the study to win the $100 bonus, and we compensated the participant within one week of the survey closing.

Results

We conducted mixed within-between subjects ANOVAs at the dyad level on our main dependent variables (enjoyment, social connection, relational harm, and meaning) using *Perspective* (Experiencer vs. Forecaster) as a between-subjects factor and *Role* (Asker vs.

Responder) as a within-subjects factor.[4] We provide descriptive statistics associated with our

main dependent variables in Table 4.

In addition to these analyses, for Experiencers, we descriptively examined judgments of

how the honest conversation differed from their expectations, and we examined the long-term

consequences of the honest conversation. Specifically, we conducted a repeated-measures

ANOVA at the dyad level using *Role* (i.e., Asker vs Responder) as a within-subjects factor

(among Experiencers only).

---Table 4 about here---

Forecasters versus Experiencers.

Enjoyment. There was a significant effect of *Perspective, $F(1, 99) = 26.91, p < .001, \eta_p^2$*

$= .21$; Forecasters expected their honest conversation to be less enjoyable than Experiencers

actually experienced it to be. There was also an effect of *Role*, $F(1,99) = 4.66, p = .033, \eta_p^2 =$

$.045$; Askers found the conversation to be somewhat more enjoyable than Responders did.

Importantly, there was no significant *Perspective* x *Role* interaction, $F(1,99) = .38, p = .54, \eta_p^2 =$

$.004$. We depict these results in Figure 4, Panel A.

Social connection. There was a significant effect of *Perspective, $F(1, 99) = 13.29, p <$*

$.001, \eta_p^2 = .11$; Forecasters expected their honest conversation to be less socially connecting than

Experiencers actually experienced it to be. There was also a significant main effect of *Role*,

$F(1,99) = 5.39, p = .022, \eta_p^2 = .05$; Askers found the conversation to be more socially connecting

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[4] We preregistered between-subjects analyses to compare Communicators to Targets. However, given that Communicators and Targets are nested within dyads, and observations within dyad are not independent, we decided that within-subjects analyses (with dyad representing the subject) would be more appropriate. Our results are unchanged if we use between-subjects ANOVAs per our preregistration. We follow the same procedure for reporting results in Study 3.

than Responders. There was no significant *Perspective* x *Role* interaction, $F(1,99) = .61$, $p = .44$, $\eta_p^2 = .006$. We depict these results in Figure 4, Panel B.

Relational harm. There was also a significant effect of *Perspective* on judgments of relational harm, $F(1, 99) = 11.22$, $p = .001$, $\eta_p^2 = .102$; Forecasters expected their honest conversation to cause more relational harm than Experiencers actually experienced. There was no main effect of *Role*, $F(1,99) = .94$, $p = .34$, $\eta_p^2 = .009$, nor was there a significant *Perspective* x *Role* interaction, $F(1,99) = .57$, $p = .45$, $\eta_p^2 = .006$.

Meaning. There was a significant effect of *Perspective*, $F(1, 99) = 5.40$, $p = .022$, $\eta_p^2 = .052$; Forecasters expected their honest conversation to be less meaningful than Experiencers actually experienced it to be. There was no main effect of *Role*, $F(1,99) = 1.15$, $p = .29$, $\eta_p^2 = .011$, nor was there a significant *Perspective* x *Role* interaction, $F(1,99) = .31$, $p = .58$, $\eta_p^2 = .003$. We depict these results in Figure 4, Panel C.

Although participants significantly underestimated the meaning associated with honesty in this study, participants underestimated enjoyment to a greater degree (consistent with Study 1). Indeed, a mixed within-between subjects ANOVA using *Perspective* as a between-subjects factor and *Measure* (enjoyment vs. social connection vs. relational harm vs. meaning) and *Role* as within-subjects factors, revealed a significant *Measure x Perspective* interaction, $F(3, 297) = 15.81$, $p < .001$, $\eta_p^2 = .14$, such that the meaning misprediction was significantly smaller in magnitude relative to the enjoyment and social connection mispredictions, and significantly different in both magnitude and direction relative to the relational harm misprediction. We find no evidence of a significant *Measure x Perspective x Role* interaction, suggesting that Askers and Responders did not misforecast meaning (relative to other measures) to different degrees.

---Figure 4 about here---

Experiencers only: Experience versus expectations and subjective long-term impact.

Experience versus expectations. A repeated-measures ANOVA with *Role* as the within-subjects factor revealed no differences between Askers and Responders ($Fs < .74$, $ps > .39$). Across roles, Experiencers indicated that their honest conversations were significantly more enjoyable ($M_{Asker} = 4.67$, $SD = 1.58$; $M_{Responder} = 4.67$, $SD = 1.58$), meaningful ($M_{Asker} = 4.67$, $SD = 1.58$; $M_{Responder} = 4.67$, $SD = 1.58$), and better for their relationship ($M_{Asker} = 4.67$, $SD = 1.58$; $M_{Responder} = 4.67$, $SD = 1.58$) than they expected (all means are significantly above 4, the midpoint of the scale, $ps < .001$).

Subjective long-term impact. A repeated-measures ANOVA with *Role* as the within-subjects factor revealed no differences between Askers and Responders ($Fs < 3.14$, $ps > .09$). In their one-week follow-up survey, Experiencers indicated that their honest conversations had significant positive effects for their long-term eudaimonic well-being ($M_{Asker} = 4.63$, $SD = 1.79$, marginally above 4, the midpoint of the scale, $p = .08$; $M_{Responder} = 4.74$, $SD = 1.38$; significantly above 4, the midpoint of the scale, $p < .001$), that their conversations did not cause relational harm ($M_{Asker} = 2.21$, $SD = 1.11$; $M_{Responder} = 2.61$, $SD = 1.36$; significantly below 4, the midpoint of the scale, $ps < .001$), but also did not significantly strengthen their relationships ($M_{Asker} = 2.96$, $SD = 1.77$; $M_{Responder} = 3.03$, $SD = 1.40$; significantly below 4, the midpoint of the scale, $ps < .01$). However, Experiencers indicated that they had high appreciation for the experience ($M_{Asker} = 5.69$, $SD = 1.82$; $M_{Responder} = 5.43$, $SD = 1.31$; significantly above 4, the midpoint of the scale, $ps < .001$). Conversations did not have a significant effect on long-term hedonic well-being ($M_{Asker} = 3.57$, $SD = 1.37$; $M_{Responder} = 4.02$, $SD = 0.74$; $ps > .13$ compared to 4, the midpoint of the scale).

Discussion

Even when we guided the content of participants' honest conversations via discussion prompts and monitored their conversations, we found that individuals mispredicted the consequences of honesty. Thus, we can be reasonably confident that individuals' expectations about the topics that they would discuss when being completely honest are not the sole driver of the forecasting error that we document in this research. Participants' open-ended responses suggest that individuals' mispredictions are driven, at least in part, by their failure to understand others' reactions to honesty during difficult conversations. Take for example, the following quotes provided by Forecasters in Study 2 when explaining their predictions:

> Participant 108: *I feel that, the questions pertaining to my darker past and my weaknesses may be harder to talk about with my significant other because I would not want her to alienate me.*

> Participant 124: *These questions ask me to reveal much about myself that I've held back. I hold back some of these things for good reason: They will harm other people, they will result in me being judged and socially isolated.*

Several participants shared similar sentiments, worrying that their honesty would burden their partner, hurt their feelings, and cause them to be aggressive. In reality, however, most Experiencers felt that the conversation, though difficult, helped them achieve mutual understanding with their relational partners and brought them closer together. The following quotes from Experiencers illustrate this idea:

> Participant 50: *the asker got to know more about me; more about how I think/feel, and how I feel about him. So while this exercise was difficult and unpleasant for me at times,I think perhaps we strenghten our friendship, at least I hope we did. / It was also kinda liberating to be able to say exactly what I think about him and our friendship. I think he learned more about me, probably more than he wanted, but I also learned about him as well, from the responses to my answers. So while this experience was a stressful, difficult and somewhat unpleasant it was also a positive one.*

Participant 25: *It felt good to tell her what I had been holding in, and I think it really helped to strengthen our bond, for me to open up to her about something in my past that I had always kept separate from my day to day life. There was a little emotional distress, but I focused mostly on not allowing myself to cry, because I know she would cry too.*

Finally, one striking finding from Study 2 is how long-lasting the benefits of a single honest conversation can be. We found that a single honest conversation led to significant gains in eudaimonic well-being one week later, and did not significantly harm people's relationships. Moreover, participants indicated that they were grateful for and would want to repeat the experience. Although our interpretation of the results from the follow-up survey is limited by attrition and potential self-selection issues—only 55% of participants completed the follow-up survey—when the results of Study 2 are combined with Study 1 (as well as our Pilot Study), they provide converging evidence that a small dose of honesty injected into one's relationships may have long-lasting and unexpected positive effects.

Study 3

Study 3 extends our investigation in four ways. First, we directly measure our proposed mechanism. Specifically, we measure whether individuals mispredict how others will react to their honesty, and we examine whether that is associated with mispredictions of enjoyment and social connection. Second, we disentangle mispredictions about the content of one's honest conversation from mispredictions about others' *reactions* to one's conversation. In Study 2, we control for the topics of conversation, but it is still possible that forecasters expected to say different responses to the questions than experiencers actually did. In Study 3, we had all participants write down exactly what they would say, make a forecast before reading their statement aloud to their partner, and finally, make judgments after reading their statement aloud. We also conducted Study 3 within-subjects, rather than between-subjects, which gave us greater control over the content participants shared (imagined sharing) across experiences and forecasts.

Method

Sample. We ran Study 3 in the spring of 2018 in Chicago, Illinois. We intended to recruit 110 dyads. To determine our sample size, we conducted a power analysis, based on our results from Study 2, in which we obtained an effect size (*d*) of .35 for our smallest focal effect: the difference between Experiencers' and Forecasters' judgments of meaning. We used G*Power to calculate the required sample size for a within-subjects design and 80% power, which yielded a recommendation of *N* = 109. We preregistered all aspects of this study at AsPredicted.org (http://aspredicted.org/blind.php?x=6hg6pj).

We ultimately ended up with 106 dyads[5] who completed our entire experiment (Communicator: 47.2% female, mean age = 26; Target: 40.6% female, mean age = 26). A total of 27.4% of dyads were female-female pairs, 38.7% were mixed-sex pairs, and 34.0% were male-male pairs. A total of 67.9% of participants participated in the study with someone they considered to be a friend, 16.0% with a roommate, 22.6% with a non-married romantic partner, 4.7% with a colleague or teammate, 2.8% with a spouse, 4.7% with a relative, and 0.9% with a neighbor. Note that these categories were not mutually exclusive, so participants could have participated with someone they considered to be both a friend and a roommate.

Recruitment. We advertised Study 3 to a panel of adults in downtown Chicago (using a laboratory listserv and Craigslist) and students at a Chicago-area university (using a laboratory listserv and flyers). The recruitment materials informed potential participants that this study examined interpersonal communication and that they would be required to come to the

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[5] When we ended data collection, we believed that 110 dyads had completed the study. However, when we began to analyze the data, we realized that there had been an error in assigning dyad numbers to each pair of participants; four numbers were skipped. Thus, we only had 106 dyads complete the study.

laboratory with a close relational partner (such as a roommate, romantic partner, close friend, or colleague). Participants knew that they would be asked to discuss a sensitive topic with this person. Participants earned $15 for participation in exchange for a 30-minute study.

Sixty-one dyads were recruited in this manner. However, recruitment began to slow after roughly one month. We recruited the remaining dyads using a slightly altered recruitment protocol. Our laboratory hosted a dinner-and-a-study event, for which we recruited pairs of close relational partners to come to the lab for 40 minutes one evening, in exchange for $6 and free dinner. We find no differences in results based on recruitment type; therefore, we do not discuss this feature of our design in our results.

Assignment to condition. Research assistants, who were blind to our hypotheses, were responsible for running the study. For regular recruitment, each pair of participants who arrived to the lab was seated in a private room at computers that were separated by a divider.

For event recruitment (those recruited during the "dinner-and-a-study" event), all pairs of participants arrived to the event at the same time and were seated next to their partner in a large classroom. Participants completed the survey on their phones or tablets. Participants completed the initial portion of the survey in the classroom, and then, each pair found a private space in the building in which to have their honest conversation. Following their conversation, each pair returned to the classroom to complete their surveys.

All participants began by completing a consent form. Then, we randomly assigned one participant in each pair to the roles of Communicator and Target. Communicators learned that they would have to deliver negative feedback to the Target. Specifically, we instructed them to, "Provide one piece of critical feedback to the person you came to the lab with today (the "Target" in this study). Specifically, you will share your honest opinions, feelings, and reactions

about *one thing you think this person should do differently, change about themselves, or improve upon."* Communicators knew that they would have to read their message aloud to the Target and engage in a conversation with the Target about it.

Targets learned that the Communicator would deliver a personal message to them, but they did not know the nature of that message when they began their survey. We instructed targets to listen to the Communicator's message and respond as if they were having a natural conversation.

Forecast judgments. Before Communicators read their message aloud to the Target, we asked Communicators to forecast the experience. Specifically, we asked Communicators to predict their enjoyment ($\alpha = .84$), social connection ($r = .39$), relational harm ($\alpha = .65$), and Meaning ($\alpha = .55$), using nearly identical scales to those we used in Study 2.[6] The only change was that we replaced "conversational partner" with "target" in the Relational Harm items. As in Study 2, we consider relational harm as another dependent variable in addition to enjoyment, social connection, and meaning.

Next, Communicators answered a series of questions about how they expected the Target to react to the conversation. Specifically, participants rated their agreement (1 = *strongly disagree*, 7 = *strongly agree*) with the following eight items, which we combined into a single measure of "target's negative reaction" ($\alpha = .75$): "This conversation will offend the target," "This conversation will make the target defensive," "The target will be receptive to this conversation (reverse-scored)," "This conversation will cause the target to be judgmental," "The target will appreciate this conversation (reverse-scored)," "This conversation will make the target

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[6] We note the relatively low internal consistency reliability coefficients for some of our measures in this study. This was unexpected given that we used the same items as we did in the previous studies. We discuss this limitation of our study in the General Discussion.

relieved (reverse-scored)," "This conversation will be awkward," "What I say during this conversation will surprise the target." Whereas relational harm captures participants' beliefs about how a conversation will impact the relatively long-term status of their relationship, target's negative reaction captures participants' beliefs about their partners' moment-to-moment response to the conversation itself.

Experience judgments. Communicators read their message aloud to the Target. The Communicator's message was piped back to them on their computer (phone, tablet) screen and Communicators were instructed to allow the Targets to see the message. This procedure helped us to ensure that Communicators read exactly what they had anticipated saying to the Targets. We informed participants that the Target could respond to the Communicator's message and the two parties could engage in a conversation as they saw fit. Communicators then judged the experience on all of the same measures they forecasted (αs for enjoyment, meaning, and relational harm > .56, *r* for social connection = .69).

Target judgments. After the conversation, targets also rated their experience using the same enjoyment (α = .82), social connection (*r* = .55), relational harm (α = .62), meaning (α = .81), and target's negative reaction (α = .70) scales that Communicators used.[7]

At the end of the survey, Communicators and Targets answered a series of open-ended questions about their partner and their experience: "Who was your conversational partner (e.g., friend, roommate, significant other)?", "How long have you known each other?", "What was the most surprising thing revealed during your conversation?" and "Do you have any other reactions

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[7] We note that in our preregistration, we only indicated that we would measure "Target's Negative Reaction" from the target perspective. However, after filing our preregistration, but before collecting data, we decided to add measures of enjoyment, social connection, meaning, and relational harm from the target perspective.

to, or thoughts about your conversation?" All participants provided demographic information at the end of the study, and then received payment.

Results

Consistent with our preregistration, we conducted two sets of analyses. First, we used repeated-measure ANOVAs to examine how prediction and experience judgments differed among Communicators. Specifically, we used *Perspective* (Forecast vs. Experience) as our within-subjects factor and each of our measures as dependent variables. We followed this analysis with within-subjects mediation analyses to test whether Communicators' mispredictions of enjoyment, social connection, relational harm, and meaning were driven by their mispredictions of the targets' negative reactions to their conversations.

Second, we conducted two within-subjects ANOVAs to examine 1) how Communicators' *forecasts* of targets' negative reactions differed from Targets' ratings of their own negative reactions, and 2) how Communicators' *experiences* differed from Targets' experiences. In the first analysis we used Communicators' forecasts vs. Targets' experiences as with the within-subjects variable, and in the second analysis, we used Communicators' experiences vs. Targets' experiences as the within-subjects variable. We depict all means and standard deviations in Table 5.

---Table 5 about here ---

Communicators' predictions versus experiences.

Enjoyment. There was a significant effect of *Perspective, F*(1, 105) = 87.46, $p < .001$, η_p^2 = .45; such that Communicators expected their conversations to be less enjoyable than they actually were. We depict these results in Figure 5, Panel A.

Social connection. There was a significant effect of *Perspective, F*(1, 105) = 40.02, *p* < .001, η_p^2 = .28; such that Communicators expected their conversations to be less socially connecting than they actually were. We depict these results in Figure 5, Panel B.

Relational harm. There was a significant effect of *Perspective, F*(1, 105) = 21.71, *p* < .001, η_p^2 = .17; such that Communicators expected their conversations to harm their relationship more than it actually did.

Meaning. There was a significant effect of *Perspective, F*(1, 105) = 13.49, *p* < .001, η_p^2 = .11; such that Communicators expected their conversations to be less meaningful than they actually were. We depict these results in Figure 5, Panel C.

Although we found that Communicators did mispredict the meaning associated with honesty, consistent with our prior studies, we found that the magnitude of this misprediction was smaller than the misprediction of either enjoyment or social connection. Specifically, a within-subjects ANOVA using perspective (Forecast vs. Experience) and measure (enjoyment vs. social connection vs. relational harm vs. meaning) as the factors revealed a significant *Perspective* x *Measure* interaction, *F*(3, 315) = 44.31, *p* < .001, η_p^2 = .29, such that the meaning misprediction was significantly smaller in magnitude relative to the enjoyment and social connection mispredictions, and significantly different in both magnitude and direction relative to the relational harm misprediction.

--- Figure 5 about here---

Targets' Negative Reactions. There was a significant effect of *Perspective, F*(1, 105) = 58.50, *p* < .001, η_p^2 = .36; such that Communicators expected the Target to react more poorly to the conversation than they actually did.

Mediation analyses. To explore whether Communicators' mispredictions of their own experiences were driven by their mispredictions of Targets' reactions to their conversations, we ran several mediation analyses. We used the MEMORE macro for within-subjects mediation using SPSS (Montoya & Hayes, 2017), in which the independent variable is represented by the repeated measurements of the mediator variable and the dependent variable (in this case, Forecaster vs. Experiencer judgments). In each model, we examined whether the Forecaster/Experiencer difference in judgments of targets' negative reactions mediates the Forecaster/Experiencer difference in enjoyment (model 1), social connection (model 2), relational harm (model 3), and meaning (model 4). As indicated in our preregistration, we predicted significant mediation in Models 1-3, but not in Model 4. However, we found significant evidence of mediation in all four models (see Table 6). The confidence interval around the indirect effect of target's negative reaction excluded zero in every model, suggesting that Communicators' mispredictions of their relational partners' reactions to honest feedback at least partially accounts for mispredictions of the enjoyment, social connection, and meaning they derive from the conversation, as well as the relational harm caused by the conversation.

--- Table 6 about here---

Communicators vs. Targets.

Communicators' predictions of Targets' negative reactions vs. Target's negative reactions. There was a significant effect of *Role, F*(1, 102) = 106.09, *p* < .001, η_p^2 = .51; such that Communicators expected Targets to react more negatively to their honest conversations than Targets reported.

Communicators' experiences of Targets' negative reactions vs. Target's negative reactions. After the conversation took place, Communicators continued to overestimate how

negatively Targets had reacted compared to what the Targets reported, $F(1, 102) = 9.27$, $p = .003$, $\eta_p^2 = .08$, albeit this misprediction was smaller than before the conversation took place.

Communicators' experiences vs. Targets' experiences. There was a marginal effect of *Role, F*$(1, 102) = 3.46$, $p = .066$, $\eta_p^2 = .033$, on enjoyment such that Communicators enjoyed the honest conversation somewhat *less* than Targets did. There were no effects of *Role* on judgments of social connection*, F*$(1, 102) = 1.00$, $p = .32$, $\eta_p^2 = .01$, relational harm, $F(1, 102) = .04$, $p = .85$, $\eta_p^2 < .01$, or meaning*, F*$(1, 102) = .43$, $p = .51$, $\eta_p^2 = .004$.

Discussion

Study 3 reveals several key insights. First, Study 3 demonstrates that communicators mispredict the consequences of honest, difficult conversations even when they know the exact information they will communicate ahead of time. This result is compelling because it suggests that communicators misunderstand the experience of being honest with others, rather than (or perhaps in addition to) the information that they will share when they focus on being honest. Study 3 also provides further evidence that communicators mispredict the affective and social consequences associated with honesty during difficult conversations more than they mispredict its meaning.

Third, Study 3 provides further insight into targets' reactions to honesty. Honest conversations – in this case, conversations involving interpersonal criticism – did not yield significantly different levels of enjoyment, social connection, or meaning for targets relative to communicators. However, communicators and targets do have different impressions of how negatively targets react to these conversations. Communicators are particularly likely to overestimate their relational partners' reactions to honesty *before* a conversation has occurred. In Study 3, we find that communicators expected their relational partners to react more negatively

to their interpersonal critique before having the conversation than participants indicated after having the conversation, and that this misprediction at least partially accounts for mispredictions of enjoyment, social connection, meaning, and relational harm.

Interestingly, communicators in Study 3 overestimated the negativity of their relational partner's reactions, relative to the reports made by their relational partners, both before the conversation and after the conversation. This result suggests that communicators may not receive accurate feedback from their relational partners, or they may not believe the feedback they receive, after engaging in difficult conversations. For example, it is possible that targets do not directly express their appreciation for constructive criticism, and therefore, communicators fail to fully learn targets' true reactions. On the other hand, it is possible that targets do express their appreciation, but communicators assume that targets are just trying to be polite. Communicators' failure to fully understand targets' experiences likely perpetuates the avoidance of honesty.

General Discussion

In this research, we break new ground by exploring how honesty, one of the most basic moral principles and facets of human communication, influences – and is expected to influence – psychological well-being. We accomplished this by conducting an intensive three-day field experiment in which individuals had to be honest (versus kind or conscious of their communication) in all of their social interactions, one laboratory experiment in which individuals had to be honest with a close relational partner while answering personal and potentially difficult discussion questions, and a final laboratory experiment in which individuals had to provide honest, critical feedback to a close relational partner.

Our findings make several important contributions to our understanding of morality, affective forecasting, and human communication. First, we provide insight into why people

avoid being honest with others. Our results suggest that individuals' aversion to honesty is driven by a forecasting failure: Individuals expect honesty to be less pleasant and less socially connecting than it is. Furthermore, our studies suggest this is driven by individuals' misguided fear of social rejection. Whereas prior work on mispredictions of social interactions has primarily examined how individuals misunderstand others or their preferences for interaction, the present research examines how individuals misunderstand others' reactions to honest disclosure of thoughts and feelings, and how this shapes social communication.

Second, this research documents the broader consequences of being honest. Individuals' predictions that honest communication would be less enjoyable and socially connecting than kind communication or one's baseline communication were generally wrong. In the field experiment (Study 1a), participants in the honesty condition either felt similar *or higher* levels of social connection relative to participants in the kindness and control conditions. Participants in the honesty condition also derived greater long-term hedonic well-being and greater relational improvements relative to participants in the control condition. Furthermore, participants in Study 2 reported increased meaning in their life one week after engaging in their brief, but intense, honest conversation. Scholars have long claimed that morality promotes well-being, but to our knowledge, this is the first research to document how enacting specific moral principles promote different types of well-being.

Taken together, these findings suggest that individuals' avoidance of honesty may be a mistake. By avoiding honesty, individuals miss out on opportunities that they appreciate in the long-run, and that they would want to *repeat.* Individuals' choices about how to behave – in this case, whether or not to communicate honestly – seem to be driven primarily by expectations of enjoyment, but appreciation for these behaviors is driven by the experience of meaning. We

encourage future research to further examine how affective forecasting failures may prevent individuals from finding meaning in their lives.

Practically, our research has implications for the delivery of performance appraisals in organizations, and the establishment of honest work cultures. Prior work has documented that managers are often overly positive when attempting to deliver critical feedback, leading subordinates to hold misperceptions about how their work is actually evaluated by their supervisors (Schaerer, Kern, Berger, Medvec, & Swaab, 2018). A suggestion from our work is that one factor underlying this phenomenon might be miscalibrated expectations about the difficulty of giving honest feedback. Like the participants in our studies, we suspect that many managers have erroneous expectations about the pain associated with providing honest feedback to direct reports. We hope our findings shine a light on this potential inaccuracy, and provide an impetus for those tasked with providing performance appraisals to do so with greater honesty and clarity.

Our findings also suggest that prioritizing honesty may indeed benefit companies and their employees, though future research is needed to fully understand the nature and boundaries of these benefits. Recently, several organizations – such as Netflix (McCord, 2017) and Bridgewater Associates (Dalio, 2017) – and public figures have celebrated the practice of "radical candor" (Scott, 2017) and "radical honesty" (Blanton, 2005), but there have been no systematic empirical investigations of the consequences of such practices for personal or organizational relationships.[8] Many lay people believe that radical honesty in organizations is a terrible idea—something that is likely to be painful to experience and a recipe for destructive

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[8] However, there has been at least one entertaining report from a single person who experimented with radical honesty in his own life (Jacobs, 2007).

conflict among employees. Our research suggests that such beliefs may be misguided. Nonetheless, we caution against organizations endorsing radical honesty as a general practice given that other research suggests that, at least in some circumstances, people prefer those who speak "prosocial lies" to those who offer painful truths (Levine & Schweitzer, 2014, 2015). There is important work to be done to examine the relational and organizational consequences of complete honesty, particularly over time and within work contexts.

Finally, the current research has implications for understanding barriers to successful conflict management. In the presence of disagreement, honest communication affects whether conflict will escalate into unpleasant, negative spirals, or whether it will de-escalate into more reflective and productive conversations (Weingart et al., 2015). To the extent that the parties are reluctant to communicate honestly because of the faulty assumptions highlighted in the current research, they are unlikely to communicate their disagreements effectively, which, of course, puts them at risk for destructive conflict escalation.

Limitations and Future Directions

Many questions remain about how individuals and their conversational partners react to honesty. Although Studies 2 and 3 demonstrate that individuals underestimate the benefits of honesty even when the topics and content of conversation are held constant, we still do not know precisely which *types* of honest conversations yield the greatest benefits. For example, does self-disclosing personal information or does sharing long-hidden critiques yield greater enjoyment, social connection, and meaning for the two parties? And, how do these outcomes compare with people's expectations of these conversations? We also do not yet know what types of conversational strategies yield the greatest benefits. For example, does avoiding explicit lies or does unburdening oneself of secrets (e.g., Slepian, Chun, & Mason, 2017) yield greater personal

benefits? We encourage future researchers to more deeply explore the conversational topics and tactics that are most beneficial for communicators, and most misunderstood. We also encourage future researchers to examine our proposed mechanism in greater detail. We find that individuals misunderstand others' reactions to the information they share during honest conversations, but other potential mechanisms may also contribute to mispredictions of honesty. Furthermore, mispredictions of others' reactions to specific information may also contribute to forecasting errors in many other conversational domains, such as story-telling and expressing gratitude (see Cooney, Gilbert, & Wilson, 2017; Kumar & Epley, 2018).

It will also be important for future work to examine the types of relationships that most benefit from (or are harmed by) honesty. Although we sampled from many different types of relationships across our studies, we did not have sufficient power to explore how honesty influenced different relationships differently. One possibility is that honesty harmed some relationships and benefited others. For example, perhaps honesty within insecure or dysfunctional relationships leads to the dissolution of the relationship, whereas honesty within secure and functional relationships makes the relationship even stronger. And perhaps, individuals only focus on the former possibility when making forecasts, when in reality, both processes occur. We conducted exploratory analyses to examine this idea. In particular, with the data from the field experiments (the Pilot Study and Study 1), we examined whether there was greater variance in social connection and relational improvement and harm in the honesty condition than in the kindness or control conditions. If this were the case, it would suggest that honesty leads to more extreme relational outcomes than other communication tactics. However, we find no evidence of greater variance in the honesty condition (all $ps > .167$ for Levene's test

for equality of variance). Despite these null results, we encourage future researchers to test this idea more thoroughly and systematically.

Our research also suggests that there is interesting work to be done on the nature of individuals' mispredictions, and in particular, the specific outcomes that individuals do and do not have insight into. Existing research has focused almost entirely on *affective* forecasting, concluding that individuals often lack insight into the affective consequences of future experiences (e.g., Dunn et al., 2007; Gilbert, Driver-Linn, & Wilson, 2002; Van Boven, Loewenstein, Dunning & Nordgren, 2013; Wilson & Gilbert, 2005). Our findings are consistent with this body of research. Interestingly, we also find that individuals are often more accurate when predicting the eudaimonic consequences of future experiences—individuals were relatively more accurate when predicting the meaning (compared to the enjoyment, social connection, and relational harm) associated with the communication interventions. These results suggest that individuals may generally be more accurate when making more cognitive predictions, like predictions of personal meaning, than affective or social predictions. The forecasting literature has not explored this possibility, so our results suggest an interesting area for future research to explore. Perhaps individuals who experience human suffering – through breakups, death, and defeat (Gilbert et al., 1998) – do recognize that with hardship comes meaning. We encourage future scholars to pursue this idea and to employ more reliable scales to capture hedonic, eudaimonic and social well-being. We note that one limitation of the current work was the relatively low internal consistency for some of the measures in some of the studies. We used the same or highly similar measures across studies, so it is unclear why internal consistency coefficients differed across studies, with strong evidence of reliability in some and more modest reliability in others.

It will also be important to more deeply explore targets' reactions to honesty. In Study 2, we find that targets, like communicators, underestimate the benefits of honesty. Research on targets' positive reactions towards prosocial lies (Levine & Schweitzer, 2014, 2015), however, suggests that targets and observers often have negative reactions to hurtful truths. In our studies, unlike Levine and Schweitzer's economic games, the costs and benefits of honesty were ambiguous. In the context of difficult conversations, people expect honesty to do more harm (e.g., cause relational strain, offend another person) than it actually does, and in fact, this contributes to communicators' misguided forecasts of enjoyment and social connection. We expect that in circumstances in which honesty does indeed cause harm, targets would judge truth-tellers negatively (as in Levine and Schweitzer, 2014, 2015). Thus, the present research extends existing research by demonstrating how forecasts of harm might diverge from reality and by highlighting the importance of perspective (communicator versus target) when studying judgments of honesty and deception. Future research is needed to fully understand targets' reactions towards honesty, kindness, and prosocial lies, particularly in naturalistic settings.

Finally, it would be worthwhile for future research to more carefully disentangle the consequences of focusing on honesty, relative to the consequences of engaging in difficult conversations. An additional experiment reported in the online supplement suggests that honesty per se does not drive our effects. Specifically, in Study S1, individuals misunderstood the consequences of delivering negative feedback regardless of whether individuals were specifically prompted to be completely honest when delivering it. This suggests that individuals misunderstand the consequences of honesty because they misunderstand the consequences of difficult conversations, not because of the associations they have with the word honesty. More research, however, is needed to understand the full range of difficult conversations that

individuals misunderstand. In the present research, we directly examined self-disclosure and negative feedback. Would other difficult conversations, such as delivering bad news, confronting another person, or proposing a bold new idea be similarly misunderstood? Do people overestimate the risks and interpersonal costs of any conversation that could lead to negative interpersonal judgment? We encourage future scholars to answer these questions.

Conclusion

Individuals often shy away from communicating honestly during difficult conversations, fearing the hedonic and social costs of honesty. Our findings suggest these fears are misguided. Honesty is not as unpleasant or isolating as it seems, and in fact, may promote meaning and long-term growth. People can handle the truth (more than you think).

Context

This paper is based on a chapter of Emma Levine's dissertation. Levine's research focuses primarily on how individuals navigate the tension between honesty and kindness. In previous studies, she has found that targets often prefer kindness to honesty and thus reward prosocial lies. In the current paper, she sought to extend her prior work by exploring how communicators react to honesty and kindness, and whether they make accurate predictions about these experiences.

Taya Cohen served as a member of Levine's dissertation committee and actively collaborated with her on the current project. Cohen's research focuses primarily on understanding the antecedents of moral behavior. In previous studies, she has found that some people are reliably and predictably more honest than others across different situations they encounter in their lives, and that such people can be identified via standardized personality assessments and behavior-based interview questions. In the current paper, she sought to extend her prior work by exploring the consequences of an honesty intervention on people's daily lives, and to test whether making people act in a more honest way is beneficial for their well-being and social relationships.

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Tables

Table 1. Enrollment and attrition across conditions (Study 1a)

	Assignment to condition	Day 1	Day 2	Day 3	Completed any daily surveys	Completed any daily survey and the two-week follow-up
Honesty	50	49	45	45	49	43
		98.00%	90.00%	90.00%	98.00%	86.00%
Kindness	59	57	55	55	58	58
		96.61%	93.22%	93.22%	98.31%	98.31%
Control	48	47	44	44	47	43
		97.92%	91.67%	91.67%	97.92%	89.58%
Total	157	153	144	144	154	144
		97.45%	91.72%	91.72%	98.09%	91.72%

Note. Percentages reflect the proportion of individuals assigned to condition that completed surveys at each subsequent time-point.

Table 2. The anticipated and experienced effects of honesty and kindness on communicators (Study 1)

		Experience			Manipulation checks		Subjective Impact two-weeks later						
		Enjoyment	Social Connection	Meaning	Daily Honesty	Daily Kindness	Long-term honesty	Long-term kindness	Long-term hedonic well-being	Long-term relational Improvement	Long-term relational harm	Long-term eudaimonic well-being	Appreciation for the experience
Forecasts (Study 1b)													
Honesty	M	2.41	2.83	3.57			5.59	3.66	3.62	4.52	4.82	4.73	4.39
	SD	1.01	1.09	0.88	N/A	N/A	1.26	1.51	1.41	1.37	1.30	1.08	1.52
	n	222	222	222			222	222	222	222	222	222	222
Kindness	M	3.57	4.22	3.72			3.62	6.15	4.75	5.19	2.98	4.91	5.15
	SD	1.00	0.84	0.90	N/A	N/A	1.65	1.01	1.35	1.22	1.57	1.21	1.23
	n	222	222	222			222	222	222	222	222	222	222
Control	M	3.21	3.45	3.60			4.79	4.85	4.09	5.14	3.27	5.19	5.09
	SD	0.86	0.81	0.81	N/A	N/A	1.26	1.26	1.16	0.99	1.21	0.95	1.17
	n	222	222	222			222	222	222	222	222	222	222
Experiencers (Study 1a)													
Honesty	M	3.46	3.71	3.79	5.92	5.02	5.44	3.70	4.13	4.80	3.34	4.74	5.20
	SD	0.82	0.69	0.59	1.13	1.24	1.44	1.50	1.24	1.34	1.58	1.18	1.11
	n	49	49	49	49	49	43	43	43	43	43	43	43
Kindness	M	3.59	3.88	3.63	4.55	5.80	3.45	5.51	4.26	4.41	2.90	4.69	5.07
	SD	0.71	0.66	0.64	1.47	0.86	1.40	1.34	1.29	1.46	1.34	1.08	1.36
	n	58	58	58	58	58	58	58	58	58	58	58	58
Control	M	3.27	3.47	3.34	5.05	4.81	4.37	4.47	3.62	4.16	2.85	4.36	4.73
	SD	0.56	0.64	0.54	1.16	1.03	1.29	1.49	1.10	1.29	1.41	1.16	1.20
	n	47	47	47	47	47	43	43	43	43	43	43	43

Note. Experience ratings were made on a five-point rating scale. Subjective impact ratings were made on seven-point rating scale.

Table 3. Two-week Reflections: Qualitative coding of responses from Study 1a/c

	Coding category	Definition	Example	Kappa	Total	Honesty	Kindness	Control	*p*
What happened during the experiment	**Avoided people or spoke less**	Participant explicitly discusses avoiding social interaction (either by talking less or avoiding people) to cope with experiment.	*-In order to deal with the demands of this experiment, I basically did not communicate nearly as much as I normally do.*	0.75	3.5%	2.3%	3.4%	4.8%	.83
	Expressed myself more honestly (manipulation check)	Participant discusses how they opened up to others more, filtered themselves less, expressed their true feelings or opinions. This is mutually exclusive with "Avoided people or spoke less."	*-I lowered how much I filtered in the way I speak to others*	0.73	16.8%	48.8%	3.4%	2.4%	<.01
	Expressed myself less honestly	Participant discusses how they opened up to other less, filtered themselves more, expressed their true feelings or opinions less, bit their tongue, etc. This is mutually exclusive with "Avoided people or spoke less."	*-I felt like I was being restrained in communicating how I truly felt*	0.91	17.5%	4.7%	27.6%	16.7%	.01
	Discussed difficult topics	Participant mentions that they engaged in challenging or difficult conversations, or mention topics of conversations that seem difficult or challenging.	*-It effected my relationship with my boyfriend. I told him the truth about how i felt sometimes, which lead to our break up*	0.88	6.3%	11.6%	6.9%	0.0%	.09
	Engaged in self-reflection	Participant discusses consulting their own feelings, looking inwards, thinking about their own feelings, desires and relationships, or learning about themselves during the experiment.	*-…enabled me to recognize things about myself and my interactions that I hadn't before*	0.9	27.3%	20.9%	20.7%	42.8%	.03
	Generated conflict	Participant mentions disagreements, arguments, or upsetting/offending others as a result of their behavior in the experiment.	*-…she felt like she was being attacked when it was anything but*	0.8	3.5%	11.6%	0.0%	0.0%	<..01

Table continued…

	Coding category	Definition	Example	Kappa	Total	Honesty	Kindness	Control	*p*
How the experience compared to expectations	**Easier for self**	Participant mentions that they were already honest [kind], that they did not find themselves in situations or discussing topics that required them to alter their behavior compared to their normal, or that this experience was in general, not as difficult as they expected.	*-It wasn't hard because I did not come across any uncomfortable questions that would have made me uneasy if I had to be honest about*	0.75	5.6%	7.0%	5.2%	4.8%	.89
	Better interaction with others	Participant mentions that others reacted more positively to their honesty [kindness] than they expected, discusses feeling surprised when someone reacted positively, or that the experiment caused less conflict than expected.	*-Was surprised how little anyone cared about my honest feedback; was well received*	0.83	9.1%	18.6%	8.6%	0.0%	.01
	Harder for self	Participant mentions that they were surprisingly dishonest [unkind] before the experiment, that they found themselves in situations or discussing topics that required them to alter their behavior more than they expected, or that this experience was in general, more difficult than they expected.	*- I thought it would have been easier than it was.*	0.76	18.2%	14.0%	20.7%	19.0%	.68
	Worse interaction with others	Participant mentions that others reacted more negatively to their honesty [kindness] than they expected, or discusses feeling surprised when someone reacted negatively, or that the experiment caused more conflict than expected.	*-I was surprised to find that one of my friends got detached from the conversation*	1	4.2%	9.3%	3.4%	0.0%	.19
Long-term Impact	**Learned about importance of communication**	Participant says they have become more conscious of their communication as a result of the experiment	*- help me learn to be more aware of how I interact with people and the tone I set by interacting a certain way and the choices I make*	0.62	32.9%	32.6%	31.0%	35.7%	.89
	Experiment had positive life impact	Participant says that the experiment caused a positive change in their life	*-I have smiled more and felt much happier than before. Also, I have had more conversations with people that I didn't regularly talk to.*	0.82	11.2%	11.6%	15.5%	4.8%	.24
	Experiment caused them to seek out life changes in future	Participant says that they learned new things about how they want to behave/change in the future	*-I have told myself to be more open and honest with people because it's the best way to do things with the most tangible results from what Ive seen.*	0.80	42.0%	39.5%	41.4%	45.2%	.86

Note. In the two-week reflections, participants answered 5 open-ended questions about how they behaved during the study, what surprised them, and how the study impacted them. We used an iterative coding procedure (Strauss & Corbin, 1990) to code participants' responses. For coding, we combined the data from Study 1 with the data from our pilot study (see online supplemental materials). We first developed an initial coding scheme, based on our hypotheses and our knowledge of participants' responses. Then, we had two research assistants, blind to the study hypotheses, independently code 15 participants' responses. Then we met to discuss the codes and made edits to the guide to clarify the categories and resolve disagreement. We repeated this procedure three times; in round one, the research assistants coded 15 responses, in round 2, they coded 30, and in round three, they coded 50. In each round, we revised the guide and the research assistants' previous codes were deleted. After round three, we developed the final coding guide. The research assistants used the final guide to code 100 responses independently, which we used to test for agreement. The research assistants then coded the remaining participant responses independently and resolved disagreement through discussion. In the table, *kappa* reflects the level of agreement between the two coders' independent coding of the 100 participant transcripts for each category. Kappa values above .81 reflect excellent agreement; Kappa values above .61 reflect substantial agreement (Landis & Koch, 1977). Frequencies in columns labeled *Total, Honesty, Kindness, and Control* reflect the percentage of participants who mentioned each coding category within each condition (in each study). "P" reflects the p-value associated with a chi-square test of proportions of the null hypothesis that each code appeared in equal frequency across the three experimental conditions.

Table 4. The anticipated and experienced effects of honesty on communicators and targets (Study 2)

	Askers				Responders			
	Enjoyment	Social Connection	Relational Harm	Meaning	Enjoyment	Social Connection	Relational Harm	Meaning
Forecasts								
M	3.3	4.1	2.77	3.95	3.12	3.75	3.05	3.89
SD	1.01	0.9	1.41	0.78	1	0.93	1.22	0.81
n	51	51	51	51	51	51	51	51
Experiencers								
M	4.17	4.48	2.23	4.28	3.85	4.31	2.26	4.11
SD	0.9	0.71	1.21	0.78	0.96	0.85	1.27	0.78
n	50	50	50	50	50	50	50	50

Table 5. The anticipated and experienced effects of honesty on communicators and targets (Study 3)

		Enjoyment	Social Connection	Relational Harm	Meaning	Target's Reaction
Communicators' Forecast	*M*	2.91	3.63	3.08	3.67	3.73
	SD	1	0.88	1.15	0.65	0.95
	n	106	106	106	106	106
Communicators' Experience	*M*	3.87	4.17	2.47	3.97	2.91
	SD	1.02	0.9	1.21	0.93	1.06
	n	106	106	106	106	106
Targets' Experience	*M*	4.07	4.29	2.46	3.92	2.63
	SD	0.9	0.81	1.21	0.83	0.95
	n	103	103	103	103	103

Table 6. Mediation results: Indirect effect of Target's reaction, mediating the effect of Perspective (Forecast vs. Experience) on DV (Study 3)

	Model 1	Model 2	Model 3	Model 4
	Enjoyment	Social Connection	Relational Harm	Meaning
DV:				
Lower CI	0.2136	0.1426	-.7259	0.0844
Upper CI	0.6769	0.5112	-.3373	0.3727
Estimate	0.4173	0.3100	-.5314	0.2178

Note. Results depict bootstrapped confidence intervals and point estimates of the indirect effect of Target's reaction as a mediator for the within-subjects difference between each DV at each time-point (forecast before the conversation and experience after the conversation). We used the MEMORE macro for within-subjects mediation using SPSS (Montoya & Hayes, 2017), in which the independent variable is represented by the repeated measurements of the mediator variable and the dependent variable.

Figures

Figure 1. The anticipated and experienced effects of honesty and kindness on hedonic well-being during the 3-day experience (Study 1).
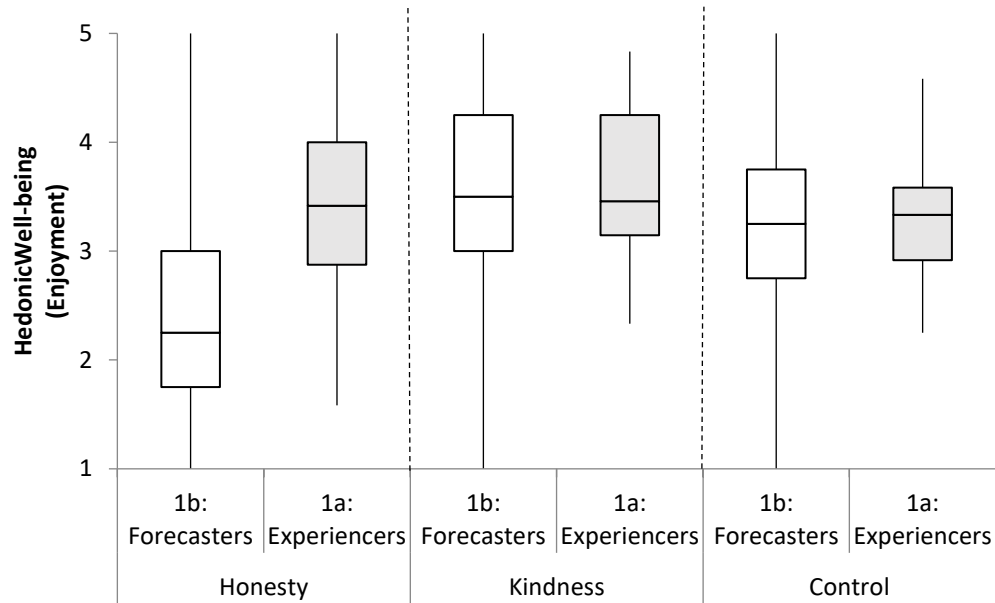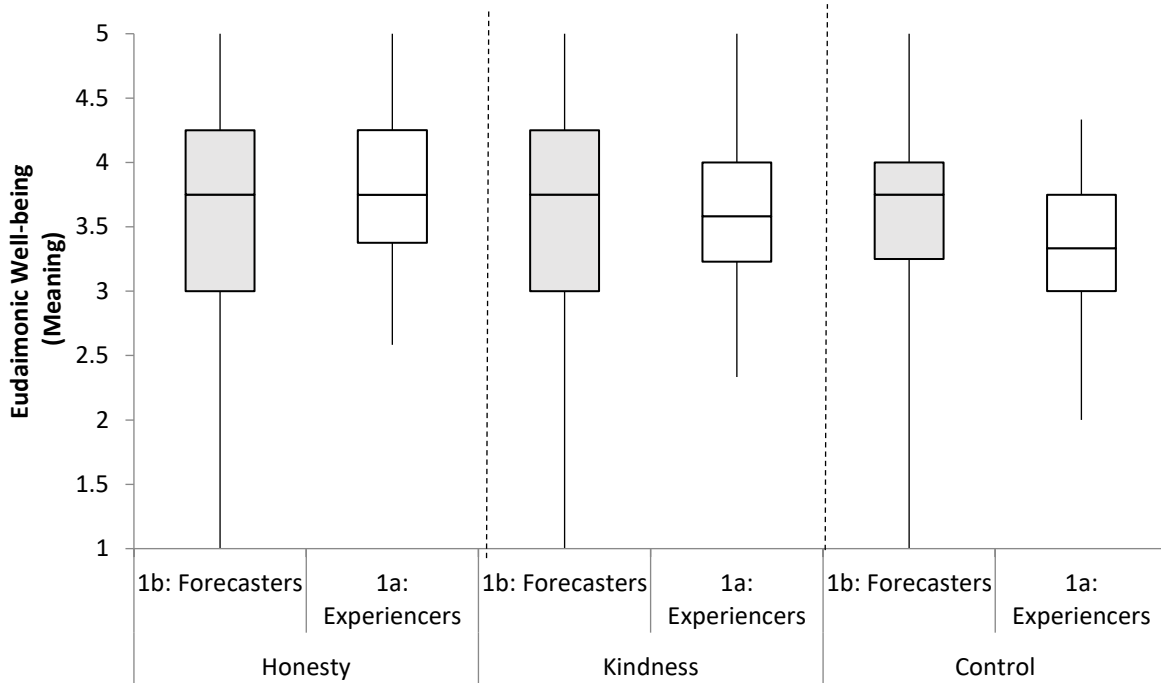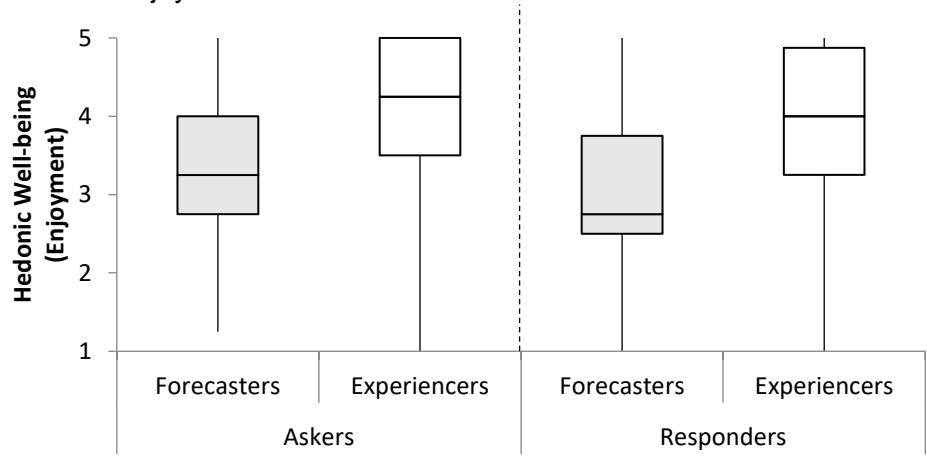

Note. Ratings were made on five-point rating scales. Box plots depict the lower quartile, median, and upper quartile of each set of ratings. Whiskers end at minimum and maximum ratings (Spear style). Forecasters (Study 1b) significantly underestimated Experiencers' (Study 1a) enjoyment within the Honesty condition ($p < .001$), but not the Kindness or Control conditions.

Figure 2. The anticipated and experienced effects of honesty and kindness on social connection during the 3-day experience (Study 1).
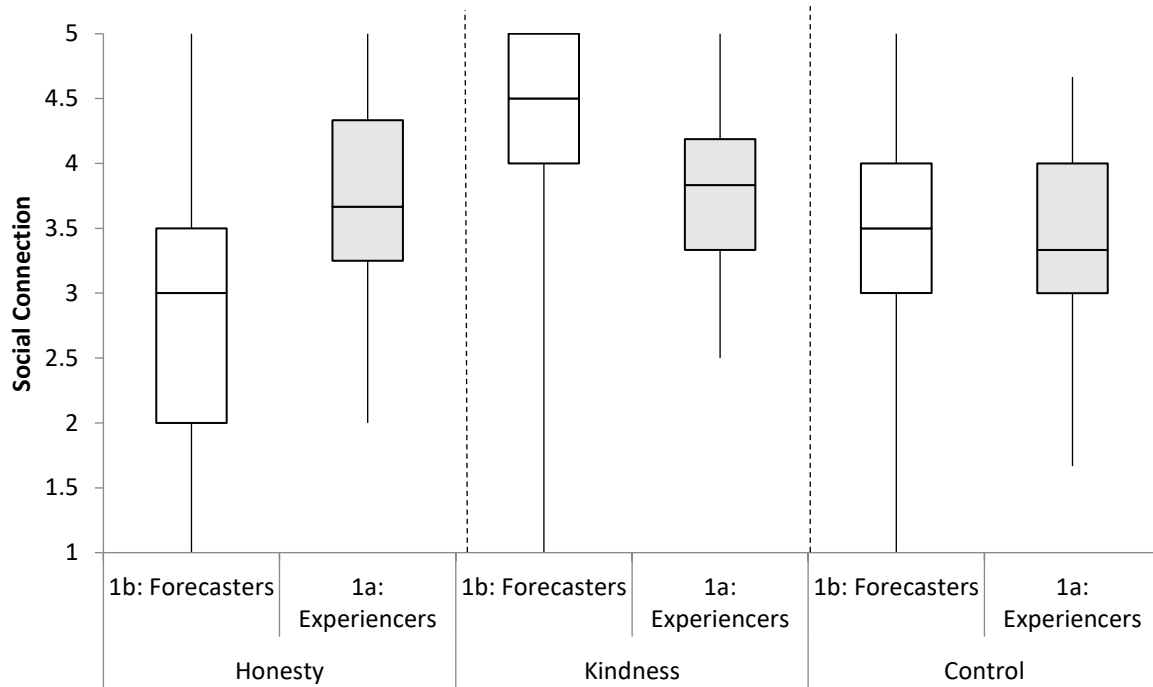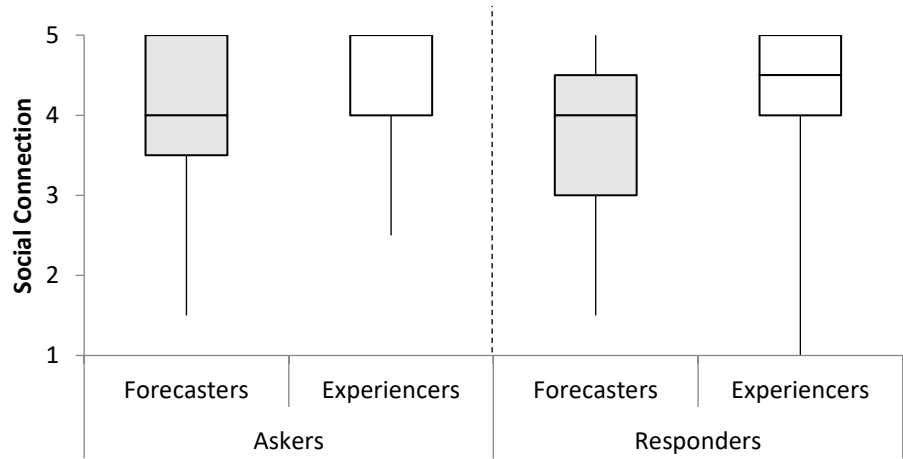

Note. Ratings were made on five-point rating scales. Box plots depict the lower quartile, median, and upper quartile of each set of ratings. Whiskers end at minimum and maximum ratings (Spear style). Forecasters (Study 1b) significantly underestimated Experiencers' (Study 1a) social connection within the Honesty condition ($p < .001$), but not the Kindness or the Control conditions.

Figure 3. The anticipated and experienced effects of honesty and kindness on eudaimonic well-being during the 3-day experience (Study 1).

Note. Ratings were made on five-point rating scales. Box plots depict the lower quartile, median, and upper quartile of each set of ratings. Whiskers end at minimum and maximum ratings (Spear style). Forecasters (Study 1b) marginally underestimated Experiencers' (Study 1a) meaning associated with Honesty condition (*p*= .088), but not the Kindness or the Control conditions.

Figure 4. The anticipated and experienced effects of honest conversations among communicators (Responders) and targets (Askers) (Study 2).

Panel A. Enjoyment
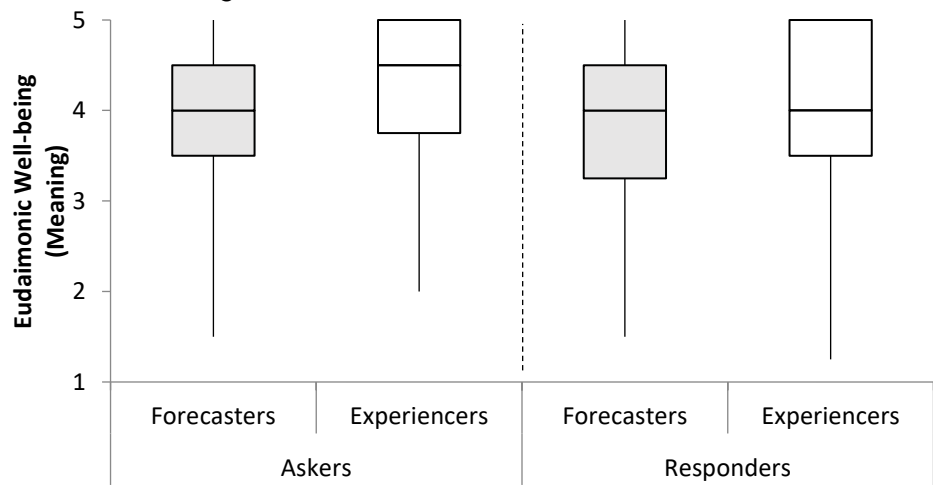

Panel B. Social Connection
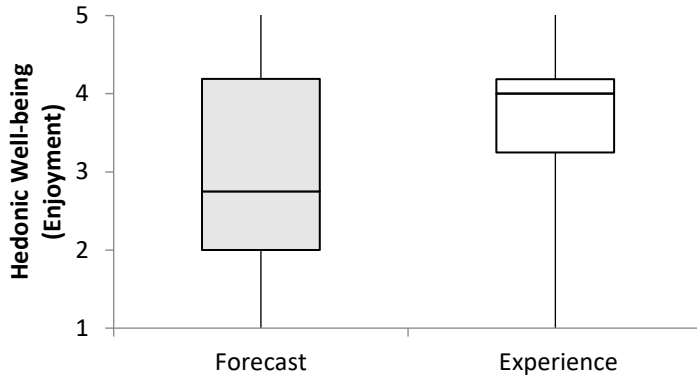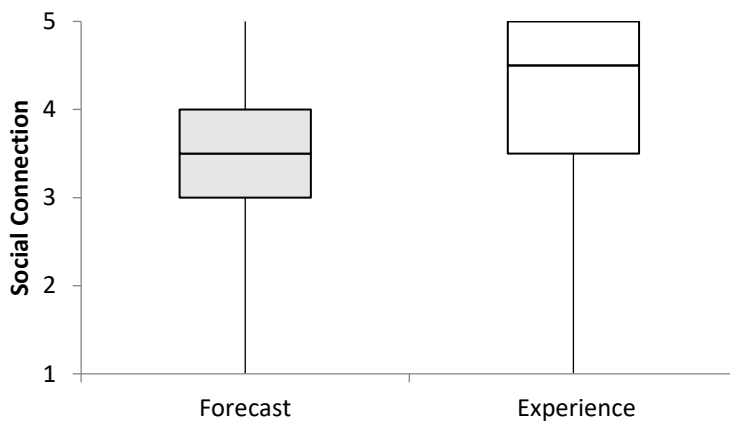

Panel C. Meaning

Figure 5. The anticipated and experienced effects of honest conversations among communicators (Study 3).
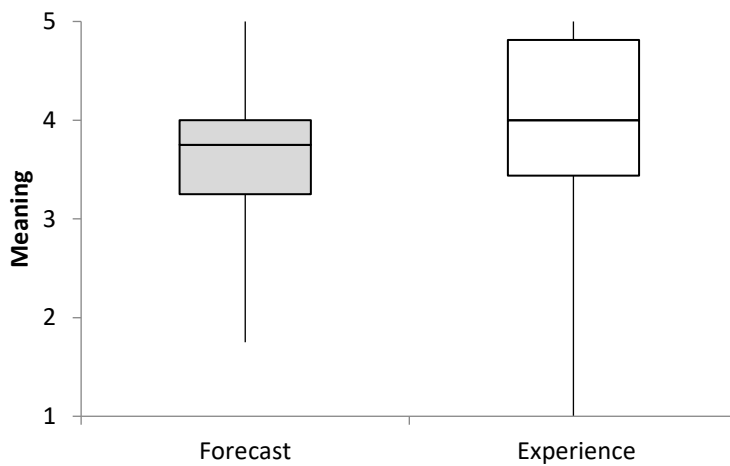
Panel A. Enjoyment

Panel B. Social Connection

Panel C. Meaning

Note. Ratings were made on five-point rating scales. Box plots depict the lower quartile, median, and upper quartile of each set of ratings. Whiskers end at minimum and maximum ratings (Spear style). For every measure, the difference between Experiencers and Forecasters is significant, *ps* < .001. That is, Communicators underestimated the enjoyment, social connection, and meaning associated with honesty.

Appendix A. Verbal Instructions for Recruitment and Three-Day Communication Intervention in Study 1a

Please listen carefully.

The study is about communication in everyday life. In this study, you will be asked to be very conscious of your interpersonal communication. We expect that as a result of participating in this study, you will learn about the way they communicate with and relate to others. However, you may be asked to communicate in ways that could cause discomfort. You should only participate if you are truly willing to be thoughtful about your communication and are open to communicating in different ways.

To participate, you must show up to the Wharton Behavioral Lab and take an introductory survey, which will take roughly 5 minutes. Then, you will learn more about the study. In order to participate in the full study, you will have to follow detailed for three days, which will specify how you should communicate with others in your daily interactions. You will also have to complete 3 surveys (on Thurs night, Friday night, and Saturday afternoon), which will each take about five minutes. You will take one survey about your experience two weeks later. You will receive each survey via email.

In exchange for participating in this study, you will earn a $5 show-up fee during the lab session, and $20 and the chance to win an iPad mini after you have completed the entire study (all nightly surveys and 2-week follow up). Your $20 payment will be paid either directly to you by the experimenter, through PayPal, Venmo or you can choose to receive a $20 amazon e-gift card instead.

In addition to the payment of $20, we will run a lottery for an iPad mini. Thus, you will also have a chance to win an iPad mini in exchange for your participation.

If you do not want to join this study, you can check out of the lab at this time.

Please take a moment to think about your decision. You are in no way obligated to participate in this research and you can choose to leave the study at any time. You can head to check out if you do not want to participate.

[wait before proceeding to study instructions]

All conditions:

In this study, you will be asked to reflect upon your social communication. Often, speaking with others requires balancing honesty and kindness. Being completely open and honest about our thoughts, feelings, and opinions, can sometimes upset others and be unkind. Alternatively, being kind, considerate, and helpful towards others sometimes means not being 100% honest.

Control:

Throughout the next three days – that means today, tomorrow, and the following day - please be conscious of the way you communicate with others. Please act as you normally would throughout the length of this study. You should not change your behavior, but you should be conscious of it.

You should act as you normally would with your closest relational partners. However, you should NOT tell them, or anyone else, any specific information about this study. They can only know that you were asked to pay special attention to your interpersonal communication. After the study has ended, you can share any information you'd like about this study.

Please think about what it means to be conscious of your communication. Feel free to raise your hand if you have questions. [field questions, wait for a moment] Is everyone ready to continue? If so, you can complete the next link on your computer.

Honesty:

Throughout the next three days – that means today, tomorrow, and the following day - be honest in every conversation you have with every person you talk to. Really try to be completely candid and open when you are sharing your thoughts, feelings, and opinions with others. You should be honest in every conversation you have, in every interaction, with every person in your life. Even though this may be difficult, try your best to be honest.

Being authentic, honest, and true to oneself are important virtues. Embrace these virtues every day for the next three days. When someone asks you how you feel, tell them the truth. That means saying you feel happy only when you feel happy and saying you feel sad when you feel sad. When you are giving your opinion, be completely honest. You should provide positive opinions only when you truly feel positive, and you should provide negative opinions when you feel negative.

You should be particularly honest with your closest relational partners. However, you should NOT tell them, or anyone else, any specific information about these instructions. They can only know that you were asked to pay special attention to your interpersonal communication. After the study has ended, you can share any information you'd like about this study.

Please think about what it means to be completely honest. Feel free to raise your hand if you have questions. [field questions, wait for a moment] Is everyone ready to continue? If so, you can complete the next link on your computer.

Kindness:

Throughout the next three days – that means today, tomorrow, and the following day - please strive to be kind in every conversation you have with every person you talk to. Really try to be caring and considerate when you are sharing your thoughts, feelings, and opinions. You should be kind in every conversation you have, in every interaction, with every person in your life. Even though this may be difficult, you should do your absolute best to be kind.

Being kind and helpful, and avoiding harming others are important virtues. Embrace these virtues every day for the next three days. When someone asks you how you feel, give a kind answer. That means taking their feelings and state of mind into consideration. When you are giving your opinion, be kind. You should provide opinions kindly and focus on the needs and feelings of those around you.

You should be particularly honest with your closest relational partners. However, you should NOT tell them, or anyone else, any specific information about these instructions. They can only know that you were asked to pay special attention to your interpersonal communication. After the study has ended, you can share any information you'd like about this study.

Please think about what it means to be kind. Feel free to raise your hand if you have questions. [field questions, wait for a moment] Is everyone ready to continue? If so, you can complete the next link on your computer.

Appendix B. Open-ended reflection Questions in the Survey Administered Two Weeks after the Three-Day Communication Intervention (Study 1a/c)

1. Please think about your experience in this study. How did focusing on communicating kindly [communicating honestly, being conscious of your communication] change the way you thought, felt, behaved, and communicated with others? What were the key difficulties you experienced during the study? What have you learned?

   Please describe your experience and insights in as much detail as possible, using the space below.

2. Did your experience in the study cause you to implement any changes in your life? What have you done since the study to enact kindness in your daily life? What, if any, long-term changes have you made?

   Please describe how your communication has or has not changed as a result of this study, using the space below.

3. Did your experience in the study surprise you in any way? Think back to your expectations of this experience. Was there anything particularly interesting or unexpected that occurred during your participation?

4. What did you learn in this study - either about yourself, others, or the way you communicate?

5. Is there anything else you would like to share with the research team, about your experience in the study?

Appendix C. Measures in the Reflection Survey Administered Two Weeks after the Three-Day Communication Intervention in Study 1a

Participants rated their agreement with the following statements (1 = *Strongly disagree*, 7 = *Strongly agree*):

Long-term honesty
- This experience made me communicate more honestly.
- This experience made me communicate more directly.

Long-term kindness
- This experience made me communicate more kindly.
- This experience made me communicate more nicely.

Long-term hedonic well-being
- This experience made me less stressed.
- This experience made me happier.
- This experience made my life more pleasurable.

Long-term eudaimonic well-being
- This experience gave me greater meaning.
- This experience made me more thoughtful.
- This experience brought me self-awareness.
- This experience led to personal growth.
- This experience brought me self-improvement.
- This experience created purpose in my life.
- This experience made me a better person.
- This experience made me a better communicator.

Long-term relational improvement
- This experience improved how I interact with others.
- This experience deepened my relationships.

Long-term relational harm
- This experience strained my relationships.
- This experience caused relational conflict.

Appreciation for the experience
- I am grateful for the experience
- I would want to repeat the experience
- I would recommend the experience to others
- I am glad I participated in the study

Appendix D. Questions asked in Study 2

1. How are you?
2. What's new in your life?
3. How is school/work? Are you having any issues?
4. Are you having any issues in any of your relationships?
5. Is there anything you need my advice on? What is it?
6. What is your least favorite thing about me? / What behavior of mine bothers you most?
7. Are you happy in our relationship? Is there anything you would like to change?
8. How long do you expect our relationship to last?
9. Have you ever been frustrated with something I did but not told me about it? What is it?
10. Do you have any (positive or negative) opinions about me that you have been hesitant to share? What are they?
11. Is there anything you have never told me about yourself or anything you have been hiding from me? What is it?
12. What is your most terrible memory?
13. If you knew that in one year you would die suddenly, would you change anything about the way you are now living? Why?
14. What does friendship mean to you?
15. What roles do love and affection play in your life?
16. How close and warm is your family? Do you feel your childhood was happier than most other people's?
17. How do you feel about your relationship with your mother?
18. What is your most embarrassing moment?
19. When did you last cry in front of another person? By yourself?
20. If you were to die this evening with no opportunity to communicate with anyone, what would you most regret not having told someone? Why haven't you told them yet?