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Essays in Behavioral Economics

A dissertation submitted in partial satisfaction of the requirements for the degree
Doctor of Philosophy

in

Economics

by

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2016
The dissertation of Adam Eric Greenberg is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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2016
DEDICATION

To Buffy Greenberg, for endless support
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This dissertation examines when and why individuals behave prosocially and antisocially in economic environments. The first chapter uses a natural experiment to investigate how people respond simultaneously to two prosocial norms—restaurant tipping and generosity during the holiday season. The second chapter examines the interplay between strong social norms and their fragility by employing a laboratory experiment to see whether strong social norms for honest conduct cause victims of dishonesty to pay forward dishonesty to third parties. Finally, the third chapter investigates the role of shame in honest reporting by asking whether individuals are more truthful when they know their statements will be subject to ex-post disclosure. Taken together, we gain insights into the
motives for altruistic and ethical behavior as well as the roles social norms play in promoting or deterring such behavior. This research both draws from and extends to existing research in psychology and organizational behavior as well as economics.
Chapter 1

On the Complementarity of Prosocial Norms: The Case of Restaurant Tipping During the Holidays
1.1 Introduction

Countless studies in economics and psychology show that *homo economicus* is an unrealistic, yet convenient assumption. Observed economic behavior suggests that while people do act in their own material self-interest, they also gain utility from behaving prosocially toward others. But people also have a preference for conforming to social norms. Sometimes these norms dictate that we act prosocially even when we don’t want to (e.g., when we offer to share our favorite food with a dining partner, or purchase things we don’t want from our colleagues’ children).

Such social expectations abound, and at times, can compete with each other. Consider, for instance, a case in which after a customer purchases a cup of coffee, she must decide whether to leave her change in the tip jar or with the beggar outside the cafe. On the other hand, one might wish to give to her alma mater during the holiday season, which is typically a time one is expected to be generous towards family and friends. Can generosity in one area increase or decrease the marginal utility of generosity in another area?

In the current paper, I examine a similar case of potentially competing (or complementary) social norms: generosity around the Christmas season and tipping in restaurants. I am able to do this by assessing whether an exogenous increase in the marginal utility of generosity in many areas of life (the holiday season) affects one prosocial behavior over time (tipping). Using two years of restaurant tipping data, I find that people tip significantly more during the holidays than they do throughout the rest of the calendar year. Moreover, I find that this jump in tipping during the holidays is driven by the tippers who are already very generous, not those who are relatively stingy to begin with.

The structure is as follows. Section 2 gives a brief background of the literature on norms and prosocial behavior. Section 3 describes in detail the dataset and the empirical methodology employed in the analysis. Section 4 describes the results of the analysis. Section 5 contains a discussion about how prosocial norms interact and what motivates individuals to be prosocial, and briefly concludes.
1.2 Background

There are several reasons why people might have tendencies to be altruistic. Our utility might be a function of what others have, or we might actually gain utility or “warm-glow” from the act of giving to others (Andreoni 1990). While being prosocial is likely to directly affect our utility functions, we do not always give only because we want to.

While individuals might be prosocial and want to give, social pressure can also induce giving. DellaVigna et al. (2012) ran a large-scale field experiment in which research assistants traveled door-to-door to solicit donations on the streets of Chicago. Some of these households were informed in advance that the solicitors would be arriving (and were given the explicit time the solicitor would show up). Other households were given no such advanced notice. Households that first saw the flyer were far less likely to answer their doors when the solicitors arrived. This indicates that we sometimes act prosocially because of social pressure, and not because giving improves our own welfare.

The holiday season (i.e., the time around Christmas) is, according to the conventional wisdom, the season of “giving”. During this time of year, people are expected to help others, give to others, and act prosocially in general. While the holidays might be a nice time of the year, the generosity we extend to one another might not be welfare-improving.¹ To my knowledge, empirical analysis has never been brought to bear on this conventional wisdom.

Tipping represents another ubiquitous prosocial norm. Customers might wish to signal their perceptions of service quality (positively or negatively) to their servers. Yet they might also gain disutility from not adhering to social norms regarding tipping (Azar 2004; Conlin et al. 2003). Some of us might gain utility from tipping per se, although we might not wish to tip as much as the social norm dictates.

The question in the current paper lies in what happens when two (or more) prosocial norms interact. When one prosocial expectation interacts with another,

¹Waldfogel (1993) finds that gift-giving during Christmas is not welfare-increasing, but rather, results in a deadweight loss.
researchers have found that one can potentially crowd out the other. This idea was pioneered by Cain et al. (2005), who coined the term “moral licensing”.\(^2\) This means that when people do things that are “good”, they tend to feel that they have the license to indulge in “bad” behaviors.

A similar phenomenon is presented in Levitt (2006) about an honor system for collecting donations. For several years, an economist ran a bagel-delivery service at various corporate offices and collected donations in a locked box at each location. Thus, donations were private, so large contributions never became noticed and defecting by stealing a bagel was never detected. Levitt found that around the holidays, the amount of money left in these boxes was significantly lower. Perhaps in this setting people felt licensed to not pay for bagels if they were buying expensive gifts for friends, family, and coworkers around the same time.

To determine whether the holiday generosity norm and the tipping norm are substitutes or complements, I employ strong inference (Platt 1964) to determine whether the relationship between the holiday generosity norm and tipping is additive or subadditive. In essence, tipping will either be higher or lower during the holiday season. If tipping is higher, then we know the two norms are complementary; if lower, they are substitutes. Thus the complementarity of two prosocial norms is for empirics to determine.

### 1.3 Data and Methods

#### 1.3.1 Data

Tipping data were collected from 11,766 credit-card receipts for transactions that occurred between June 1, 1999, and June 29, 2001, at a non-chain restaurant in upstate New York. Entrees typically cost between $10 and $15. For each receipt, a transcriber recorded the patron’s first name, waitress’s unique server number\(^3\), date and time of the transaction, last four digits of the credit-card

\(^2\)See also the concept of “self-licensing” in Monin and Miller (2001).

\(^3\)Note that all servers in this restaurant throughout the span of the data were female.
account, the card’s expiration date, card type (e.g., MasterCard), the machine-printed amount of the bill, the customer’s handwritten tip amount, and the customer’s handwritten total (bill amount plus tip amount).

9,376 transactions are usable in the analysis. 474 were eliminated because the customer’s handwritten total on the receipt was not equal to the sum of the machine-printed bill amount and the handwritten tip amount. In 64 of these cases, the customers indicated that a cash tip would be rendered by writing “cash” on the tip line of the receipt. The remaining 410 cases were dropped as a result of errors on the part of the customer or the transcriber. Either the customer made an addition mistake or the transcriber misread the customers’ handwriting. In addition, 1,908 observations had $0 recorded as the tip amount. Servers at the restaurant confirmed that zero-dollar tips (i.e., no tip at all) almost never occur. Thus, I assume that these 1,908 observations of $0 being written as the tip amount on the credit-card receipt were cases in which customers left cash tips.4 Given that we have no way of knowing how large those cash tips were, these 1,908 observations were dropped from the analysis. 6 additional transactions were dropped from the analysis because their date stamps were mistranscribed.

A research assistant used the first name of the customer on the receipt to infer the customer’s gender. But because the gender of many names is ambiguous, gender is missing for 1,493 transactions. Given that this method for inferring gender is by nature imprecise, gender is not used in the analysis below. Moreover, unique server numbers are missing in 25 transactions. As such, regressions containing server number contain fewer observations.5

1.3.2 Sample and Identification

Our goal is to determine whether people respond to the prosocial norm of the holidays by adjusting their tip rates during this period. But when exactly is the “holiday season”? We might consider using the local public school district

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4It is possible that any credit-card tip that we observe was supplemented with a cash tip that we cannot not observe. The servers explained that this was extremely rare, however, so this phenomenon, if it exists, likely has no effect on the results of this paper.

5For more details about the dataset, see Flynn and Greenberg (2012).
calendar to define our “holidays period”. In the two years of data we have, the first day school is not in session for winter break (including weekend days) is December 23 and the last day school is not in session is January 2. Unfortunately, defining the holidays period this way gives weight to only two days before Christmas, of which one is Christmas Eve. One might suggest using a broader period, such as the entire month of December. But this period might capture other regular seasonal variation that has nothing to do with the holidays. Since such a period would be too broad, we would be unable to identify the effect of interest.

We define the holidays period as the week before and after Christmas. This definition gives equal weight to the time before and after the Christmas holiday, and also does not extend beyond New Year’s Day. In addition, the holiday generosity norm is likely most salient directly before and after Christmas compared to early December or in January.

A simple comparison between tip rates during the holidays period and outside the holidays period would not allow us to identify whether individuals tipped more during the holidays. A naive comparison like this would ignore possible selection into the holidays period. The econometrician could not distinguish between a more generous tip during the holidays period or a more generous tipper showing up at the restaurant during the holidays period. To avoid this type of selection, we first restrict the sample to “regular” customers only – in particular, customers we can observe more than once in the dataset. Moreover, we exclude customers that we only observe during the holidays period or outside the holidays period. In this way, we can identify within-customer variation in tipping as a result of the holidays period, assuaging potential concerns about selection into the holidays period (e.g., generous out-of-town visitors).

1.3.3 Empirical Strategy

Table 1.1 reports summary statistics for all variables used in the analysis based on the full available dataset. Table 1.2 reports summary statistics for

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6Using customer name and the last four digits of the customer’s credit card, I construct unique identifiers for customers in the dataset. These unique identifiers are then used to restrict our sample to those customers that we observe two or more times in the data.
the sample used in this analysis. It is important to note that tip rates are slightly higher in the sample (24.31%) used in this analysis compared to the full dataset (21.58%). This is likely driven by the fact that our sample excludes one-time customers. Since regulars have repeated interactions with servers at the restaurant, it is plausible that they would tip more generously overall than one-time customers.

To estimate the effect of the holiday norm on the tipping norm, I use a set of OLS regressions. The dependent variable is the tip percentage associated with check $i$, which is calculated by dividing the tip amount by the bill amount. I use tip percentage as a dependent variable not only because it is standard in the tipping literature, but also because tipping norms are usually reflected in rates (e.g., 15% or 20% of the check) rather than levels (e.g., $10 for all checks). The variable of interest is Holidays$_i$, a dummy that is equal to 1 if check $i$ occurred during the holidays period. The baseline estimating equation is given by:

$$\text{Tip}^\%_i = \beta_0 + \beta_1 \text{Holidays}_i + \beta_2 \text{Number Checks}_i + \beta_3 \text{Other Holidays}_i + \gamma \text{Bill Totals}_i + \delta \text{Weekday Hours}_i + \zeta \text{Customers}_i$$

$$+ \eta_j \text{Server}_{i,j} + \epsilon_i$$

where Number Checks$_i$ counts the number of checks in the data on the same day of transaction $i$; and Bill Totals$_i$, Customers$_i$, and Weekday Hours$_i$ are indicators; and Server$_{i,j} = 1$ when server $j$ was the server for check $i$.\(^7\) Note that Number Checks$_i$ gives us a coarse proxy for how busy the restaurant was on the given day of the check. Since the relationship between tip percentage and bill amount is nonlinear, vigintile dummies control for the size of the check.\(^8\)

Other Holidays$_i$ is a dummy that is equal to 1 if check $i$ occurred on an-

\(^7\)Such hour effects are necessary to capture variation in clientele across the course of the day. In addition to lunchtime and dinnertime norms being different, customer composition and the type of consumption might vary across different hours of the day. It is also likely that nighttime clientele consume more alcohol, which could lead to more generous tipping (Lynn 1988).

\(^8\)For instance, if the bill amount is only a few dollars, customers still might leave $1-2$ tips, which would correspond to very high tip percentages. On the other hand, customers who have very large bills might pay something very close to standard 20% norms since the marginal cost of tipping higher percentages is larger when the bill amount is large.
other holiday not during the holidays period. This variable includes all other minor holidays on the local public school calendar and the weekends that directly precede or succeed them: Labor Day, Yom Kippur, Rosh Hashanah, Columbus Day, Veterans Day, Thanksgiving, Martin Luther King, Jr. Day, Presidents’ Day, and Memorial Day. The only holiday included that does not appear on the school calendar is Independence Day and its associated weekend. In addition, time off from school listed on the calendar that is not associated with a particular holiday (e.g., Superintendent’s Day) is not included in this variable. The purpose of including these other holidays as controls is to rule out the alternative hypothesis that customers are simply rewarding servers for working holiday shifts.

The coefficient of interest in the above specification is $\beta_1$, the effect of the holidays on tip percentage. The purpose of these regressions is to identify the coefficient $\beta_1$, which we can interpret as the independent relationship between the holiday seasons and tip rates controlling for customer- and server-specific heterogeneity, the size of the bill, time of day, and how busy the restaurant is. We can then use the sign and significance of this coefficient to implicitly test two competing hypotheses, which we state here.

**Hypothesis 1** The prosocial norm of holiday generosity and the prosocial norm of tipping are substitutes. During the holiday season, customers tip less generously. So our null hypothesis is $H_0 : \beta_1 < 0$.

**Hypothesis 2** The prosocial norm of holiday generosity and the prosocial norm of tipping are complementary. During the holiday season, customers are more generous tippers. So our null hypothesis is $H_0 : \beta_1 > 0$.

Notice that if one of the aforementioned hypotheses is rejected, we will fail to reject the other. However, the converse is not necessarily true.
1.4 Results

1.4.1 Are the Prosocial Norms Complements or Substitutes?

Table 1.3 reports results from the OLS regression of tip percentage on the holidays period dummy variable and other covariates as in equation (1.1). Regression coefficients are reported with White (1980) standard errors clustered at the customer level. The first column corresponds to the regression of the estimating equation (1.1) without server or customer indicators. The second column includes server indicators, but not customer indicators. The third column includes customer indicators, but not server indicators. Finally, the fourth column includes both server and customer indicators in line with equation (1.1).

Let us first consider the fourth column in which we directly estimate equation (1.1). The coefficient on Holidays, the variable of interest, is both positive and statistically significant at the 5% level. Moreover, the magnitude is of economic significance. During the holidays period, the regression result suggests, customers’ tip rates are approximately 3.7% higher. Compared to a sample mean tip rate of 24.3%, this 3.7% increase represents a notable response to the holiday generosity norm. In this specification, we can reject the hypothesis (H1) that the prosocial norm of holiday generosity and the prosocial norm of tipping are substitutes.

Excluding server indicators does not substantively change the results. In the third column, the coefficient on Holidays is approximately the same, with customers tipping at rates 3.4% higher during the holidays period, which is statistically significant at the 5% level. Including server indicators improves should improve the precision of the point estimates, especially if better (or worse) servers tend to work more shifts during (or outside) the holiday season. That said, we should not expect large differences between the regressions in the third and fourth columns because the relationship between service and tipping is relatively weak (Lynn and McCall 2000).

The magnitudes of the Holidays coefficient tend to be smaller but still statistically significant when excluding customer indicators. In the first and second
columns, we note that tip rates are 2.7% (excluding server indicators) and 2.8% (including server indicators) higher during the holidays period when customer indicators are not included in the regression. However, these specifications do not identify within-customer variation in tipping. Nevertheless the main result does not appear to be sensitive to the exclusion of server or customer indicators.

How do we know that variation in tipping behavior over time is not due to potential income effects? Theory tells us that we should not observe any differences in consumption when there are expected shocks to our income. But many empirical studies find that consumption smoothing does not always hold. Stephens (2003) found that people’s consumption changes after the receipt of Social Security checks. In addition, ? found that consumption routinely falls between paydays and rises again after paydays. Around the holidays, many workers get year-end bonuses, around which time we might expect consumption to increase as well. Thus it is at least plausible that we might see increased generosity as a result of this “income effect”.

We cannot distinguish between generosity in tipping that results from the holiday norm and that which results from such holiday bonus effects, if they in fact exist. However, we can bring the payday hypothesis to the data to test whether individuals tip more around paydays. If we find that they do, then our Holidays coefficient might be biased upward.

Frequency of paydays varies by profession, within an organization, and by state laws. In some states, laws mandate that workers within specific professions must be paid at least once per week. Others can be paid as infrequently as once per month. Nevertheless the two most common payday frequencies in the United States are bi-weekly pay, followed by bi-monthly pay. Employees that get paid bi-weekly can typically expect paychecks every other Friday. If we were to examine differences in tipping under the assumption that workers are paid bi-weekly, we would be unable to distinguish between differences in tipping that result from paydays and those that result from simple weekday effects.9 For this reason, we consider the second most common paycheck frequency: bi-monthly pay. Em-

9There could be a “TGIF” effect that has nothing to do with getting paid.
ployees that get paid bi-monthly can expect paychecks around the 1st and 16th of each month. We proceed by testing whether there are any significant tipping differences across days of the month.

Our null hypothesis is that tipping does not vary significantly across different days of the month (i.e., around the 1st and 16th of the month). In particular, if we find that tip percentages are significantly higher around these days, then the coefficient on Holidays might be biased upward. To test this hypothesis, we include indicators for each of the 31 days of the month in the same four regressions reported above to see whether certain intercepts associated with the bi-monthly payday frequency are significant.\(^1\)

Table 1.4 reports the coefficients for each day of the month for the regressions of tip rate on the holidays period indicator. The 31st of the month is the omitted category in these regressions.\(^1\) Coefficients for the first day of the month are not statistically significant by any conventional measures. The coefficients on the 15th, 16th, and 17th of the month are also insignificant across the specifications. Overall, in all four specifications, no single day-of-the-month coefficient is statistically significant. Therefore, the alternative hypothesis that income shocks could be driving tipping differences finds no support in the data.

Finally, recall that we eliminated concerns about selection into the holidays period by limiting the sample we use to those customers we can observe both inside and outside the holidays period. One potential source of endogeneity, however, is that the beginning of the holidays period coincides with the end of the fall semester at the college campus close to the restaurant. It is possible that customers tip more when the students are not on campus, and that this could be driving the rise in tip rates during the holidays period. If I were to find a similar set of results using another period during which students were away from

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\(^{10}\) It is likely that actual paydays do not coincide with the same day of the month every month. Thus, we do not view this model as a representation of the world. However, we proceed with it simply to have a null hypothesis that can be rejected to determine whether there is any merit to this alternative payday explanation.

\(^{11}\) We cannot draw conclusions from the signs of the day-of-the-month coefficients because they are found relative to the omitted category. What we can determine, however, is whether the coefficients for any of these days are statistically different from zero.
campus, then we might be concerned about endogeneity.

The spring semester does not begin until a few weeks after the New Year. An ideal period to use for this type of placebo test would be around the same time of year, with a similar sample of customers, but not during the holidays period. A natural period to use is the two weeks following the holidays period. I follow the same steps in limiting the sample to this pseudo-treatment for the holidays period, including only those customers that can be observed outside of this period as well. Then I run the same specifications to test whether this period has any effect on tipping. I find that in the specifications with customer indicators, the coefficient on the placebo variable is slightly negative and not significant. In the two regressions that exclude customer indicators, the coefficient on this placebo variable is negative and statistically significant. Since the effect of this pseudo-treatment is not positive and significant, we are not concerned about any endogeneity resulting from the holidays period occurring during the off-semester. I am confident, then, that the results we observe are actually due to the holiday generosity norm.

In the above specifications, we have compelling evidence that we should reject $H_1$ and favor $H_2$.

### 1.4.2 Heterogeneity in How the Norms Interact

The results presented in the above subsection show us that during the holiday season, customers’ tip rates are significantly higher. On average, customers tip more generously during the holidays period than they do outside the holidays period. In light of this difference, one might be interested in knowing whether this represents the entire distribution of tippers. In other words, so we observe higher tip rates during the holidays period because the “good” tippers tip more during the holidays, or because the “bad” tippers become good tippers during the holidays?

Our goal is to characterize the distribution of higher holiday tip rates. We must first consider, then, what it means to be a “good” or “bad” tipper (or some-

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12See the Appendix for the full regression results.
thing in between). I define customer $i$’s “baseline” tip rate as the mean tip rate of all of customer $i$’s checks that did not occur during the holidays period. What this baseline tip rate ultimately captures is how each customer tips, on average, in the absence of the holiday generosity norm. Then I divide customers into bins based on the quantiles of their baseline tip rates. For example, if I use two quantiles, then customers are separated along the median baseline tip rate into the top 50% of tippers and the bottom 50% of tippers. Thus I am able to identify how the bottom and top 50% of tippers respond to the holiday norm by interacting these two categories with the holidays period variable.

Table 1.5 reports results from the OLS regression of tip percentage on the interactions between baseline tip rate quantiles and the holidays period indicator as well as other covariates. I use four specifications that vary only by the number of quantile bins used: 2, 4, 8, and 10. Quantiles are indexed from bottom to top of the baseline tip rate distribution. For instance, the second column reports the regression with four bins. Quantile 1 represents the bottom quartile (0-25th percentile) of tippers while Quantile 4 represents the top quartile (75-100th percentile).

Note that these regression specifications differ from the others used thus far in three ways. First, instead of using a dummy variable for the holidays period, I interact this dummy variable with the quantiles that allow us to identify distributional variation in tip rates during the holidays. Second, I include the indicators for the quantiles themselves in each of the specifications. Finally, I do not include customer-specific indicator variables because they are collinear with the quantiles.\textsuperscript{13}

The first column reports the regression of tip percentage on the interaction between the holidays period indicator and two quantiles. The coefficient on the interaction between Holidays and Quantile 1 (henceforth “Quantile 1”) is about 1.3, which indicates that the bottom 50% of the distribution of tippers tip at rates about 1.3% higher during the holidays period. On the other hand, the coefficient

\textsuperscript{13}It is straightforward to show that these quantiles are simply a convenient way to group customer dummies into fewer categories. However, standard errors are still clustered at the customer level.
on Quantile 2 is 4.79, which indicates that the top of the distribution of tippers tip at rates 4.8% higher during the holidays period. This coarse differentiation along the median suggests that tippers who already tip a lot tip much more during the holidays, while tippers who regularly tip less tip slightly more during the holidays.

The second column reports the regression of tip percentage on the holidays period indicator and four quantile bins. The coefficient on Quantile 1 is 3.47, indicating that the bottom 25% of tippers tip at about 3.5% higher rates during the holidays. The coefficient on Quantile 4 is 6.74, which indicates the top 25% of tippers tip at rates approximately 6.7% higher during the holidays. Similar to the 2-quantile case, the 4-quantile case provides evidence that the increase in tip rates during the holidays is largely driven by the top of the distribution of tippers rather than by the bottom. Interestingly, the size of the coefficient is not monotonically increasing in quantile number.

Consider the specification with 8 quantiles (the third column). The very top of the distribution (Quantile 8) shows a much higher percentage increase during the holidays at more than 9% while the bottom of the distribution (Quantile 1) shows a magnitude that is just moderately higher than in other quantile specifications at about 4%. As we increase the number of quantiles, it becomes quite difficult to discern any patterns in the distribution of the Holidays coefficient across quantiles. It is clear in this exercise that holiday generosity does not linearly affect tip rates. For example, the coefficient on Quantile 2 is 0.86 while the coefficient on Quantile 5 is -0.66.

Finally, consider the 10-quantile specification. The top of the distribution (Quantile 10) has a coefficient of 12.32, while the bottom (Quantile 1) has a coefficient of 4.63. This specification has the same general patterns that exist in the other three specifications. In particular, there are nonlinearities in the distribution of the Holidays coefficient. More importantly, we find, in general, that the top of the distribution of tippers is the main driver of the increase in tip rates during the holidays period. The very bottom of the distribution are also driving this change, albeit to a lesser extent.
1.5 Discussion and Conclusion

I have demonstrated that individuals respond to the holiday generosity norm not by tipping less, but by tipping significantly more. Through strong inference we empirically tested two competing hypotheses: whether tipping and the holiday norm are complements or substitutes. We found no support for the latter. What this indicates is that prosocial acts need not crowd out other prosocial acts. In fact, it seems that one prosocial norm – namely, the norm to be prosocial during the holiday season – can actually complement and enhance other prosocial norms (in this case, restaurant tipping behavior).

This main finding is robust in a number of specifications. First, by controlling for other holidays, I have ruled out the alternative explanation that we observe more generous tipping because people are “off” from work. My findings do not appear to be confounded by the fact that the holidays period coincides with the college’s semester break. Finally, these results cannot be explained by the possibility that people consume – and thus, tip – more after receipt of their paychecks.

This paper also gives insight into why people are motivated to act prosocially. We find that the worst tippers tipped more during the holidays, and the best tippers tipped a lot more. Some norms, such as tipping, are prosocial in nature. Many might tip because of intrinsic motivations; yet more often people tip because it is a social norm (Azar 2007a). There is evidence that people are more likely to be prosocial if the norm is to be prosocial (e.g., Frey and Meier 2004). This is the idea behind the holiday generosity norm: when it is the season of giving and we expect others to be prosocial, we too will act prosocially.

But the tipping norm and the holiday norm differ in one key dimension. In particular, there are social sanctions for not tipping a certain percentage of the bill; yet there are no sanctions for not being “extra-generous” during the holiday season. The fact that those driving the boost in tipping during the holidays period are those who tip very well already tells us that perhaps people tip because they gain some utility from being more generous. There are no sanctions against people who don’t tip more during the holidays if they are already good tippers.
In this way, there is an intrinsic motive to tipping.

The second-place drivers in the boost in tips during the holidays are those who are otherwise bad tippers. Since these people were already bad tippers, they were likely less responsive to the social sanctions for not tipping conventional amounts. It is therefore probable that these people also have an intrinsic motive to tip. Thus this research also shows that while social norms matter in prosocial behavior, there is some intrinsic motive in norms like tipping.\footnote{Azar (2007b) provides a nicely-organized review of the many motives for different kinds of tipping. A large part of the discussion is devoted to social pressure and strategic incentives. I believe that the current paper shows that we should focus also on intrinsic motives, including altruism, the alleviation of guilt, or the desire to reward good service.}

That big tippers become the biggest tippers during the holidays is an interesting result. In fact, it defies what we expect in terms of regression toward the mean. Those who are typically very generous do not need to tip more during the holidays, and those who are otherwise lousy tippers should be giving the most during the holidays. So why does the disparity increase during the holidays? The big tippers must be intrinsically motivated. Tipping is certainly a social norm, but this gives us a compelling reason to believe that people do not simply tip because of social pressure. It is possible that the holiday generosity norm is the most \textit{salient} to those who are already generous. It is a norm that can be largely ignored without social sanctions, so those who respond to it are the people who are likely to be generous in the absence of the holidays.

The absence of social sanctions (extrinsic motives) for generosity during the holidays is the very reason we observe complementarity between the two prosocial norms. Tipping is a prosocial norm with both extrinsic and intrinsic motivation. There is strong evidence that extrinsic incentives can crowd out intrinsic motivation (e.g., Gneezy and Rustichini 2000). If tipping were combined with another prosocial norm for which social sanctions were present, one might be more likely to find that the additional norm crowds out tipping. Consider the example in which a churchgoer, who regularly leaves money in the collection plate during service, is pressured into volunteering for a special church fundraiser. The extent to which this individual is intrinsically motivated to volunteer and leave money
will determine whether volunteering will result in smaller or larger amounts left in the collection plate. Each prosocial act is both intrinsically and extrinsically motivated, so the combination could result in crowding out. However, holiday generosity is purely intrinsic, so we might expect its interaction with tipping to be additive rather than subadditive.

The fact that already-generous tippers increase their tipping most during the holidays is in line with theory. If the holiday norm increases the marginal utility of tipping, then the two prosocial norms are complements. Perhaps generous people can be characterized by low elasticities of substitution between different prosocial behaviors and less generous people could be represented by elasticities that are a bit higher. Then this typology in which generous people increase their tipping most could arise. Economists should consider this type of heterogeneity in research about prosocial behaviors, especially when there are multiple avenues for generosity.

Future research should address the potential for prosocial norms to be complementary. One point which is not addressed in this paper is the possibility that people sort based on prosocial preferences. For example, it is possible that certain types of customers avoid dining out during the holidays because of the social pressure of giving. These individuals might also be more likely to tip less if they were to dine out during the holidays. Andreoni et al. (2011) found that when Salvation Army volunteers explicitly asked for donations at a Boston-area grocery store, customers were more likely to leave from a different door than the one where the volunteer works. The ones that do not “avoid” being asked also tend to be more generous. This type of sorting is not directly testable in this dataset, but could be addressed in future research.

Using credit-card receipt data from one restaurant presents obvious caveats. I cannot tell whether or how much alcohol was consumed, the size of the party, or whether checks had been split. In addition, customer gender can only be inferred using the name on the credit card; ideally we could determine customer gender without bias and also examine the dynamics of gender and the all-female staff of this restaurant. For similar reasons, I cannot determine whether increased tipping rates during the holidays are due to changes in server behavior. It is possible, al-
beit unlikely, that tipping increases during the holiday season because customers are rewarding their servers, who are more generous during the holidays. Similarly, customers may enjoy their experiences more during the holidays when the restaurant is less busy. While it is possible that servers are more upbeat during the holidays, the relationship between service quality and tipping is weak at best. Finally, the analysis employs data from one particular restaurant in a fixed location. This restaurant in a different location or a different type of restaurant might have provided slightly different results.

Chapter 1, in full, is a reprint of the material as it appears in the *Journal of Economic Behavior & Organization*, Vol. 97, January 2014, pp. 103-112. Greenberg, Adam Eric, 2014. The dissertation author was the sole author of this paper.

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15 A meta-analysis conducted by Lynn and McCall (2000) found that there is a positive correlation between reports of customer satisfaction and the size of the tip. However, the magnitude is very small.
Table 1.1: Summary Statistics for Tipping Data, Full Sample

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>S.D.</th>
<th>Min</th>
<th>Max</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tip Percentage</td>
<td>21.58</td>
<td>13.02</td>
<td>0.13</td>
<td>500</td>
<td>9,376</td>
</tr>
<tr>
<td>Holidays</td>
<td>0.027</td>
<td>0.16</td>
<td>0</td>
<td>1</td>
<td>9,376</td>
</tr>
<tr>
<td>Bill Amount</td>
<td>30.87</td>
<td>21.00</td>
<td>2</td>
<td>422</td>
<td>9,376</td>
</tr>
<tr>
<td>Tip Amount</td>
<td>6.14</td>
<td>4.61</td>
<td>0.05</td>
<td>150</td>
<td>9,376</td>
</tr>
<tr>
<td>Number of Checks</td>
<td>15.14</td>
<td>5.14</td>
<td>1</td>
<td>32</td>
<td>9,376</td>
</tr>
<tr>
<td>Other Holidays</td>
<td>0.063</td>
<td>0.242</td>
<td>0</td>
<td>1</td>
<td>9,376</td>
</tr>
</tbody>
</table>

Notes: Data are collected from credit-card receipts for transactions that occurred between June 1, 1999, and June 29, 2001, at a moderately priced, non-chain restaurant in upstate New York. The sample includes two years of customers’ credit-card receipts. Tip Percentage is calculated by the tip amount divided by the bill amount times 100. Holidays is a dummy variable equal to 1 if the transaction occurred one week before and after Christmas. Number of checks counts the amount of checks in the data on the same day of the given transaction, providing a proxy for how busy the establishment was. Other Holidays includes other holidays in which school was not in session (e.g., Labor Day) that are listed on the local public school district calendar. The full sample includes all available transactions from 22 servers and 5,603 customers.
Table 1.2: Summary Statistics for Tipping Data, Holidays Sample

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>S.D.</th>
<th>Min</th>
<th>Max</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tip Percentage</td>
<td>24.31</td>
<td>14.90</td>
<td>0.47</td>
<td>200</td>
<td>849</td>
</tr>
<tr>
<td>Holidays</td>
<td>0.17</td>
<td>0.38</td>
<td>0</td>
<td>1</td>
<td>849</td>
</tr>
<tr>
<td>Bill Amount</td>
<td>31.30</td>
<td>21.00</td>
<td>2</td>
<td>241.70</td>
<td>849</td>
</tr>
<tr>
<td>Tip Amount</td>
<td>6.85</td>
<td>4.65</td>
<td>0.15</td>
<td>48.34</td>
<td>849</td>
</tr>
<tr>
<td>Number of Checks</td>
<td>14.46</td>
<td>4.81</td>
<td>2</td>
<td>32</td>
<td>849</td>
</tr>
<tr>
<td>Other Holidays</td>
<td>0.065</td>
<td>0.246</td>
<td>0</td>
<td>1</td>
<td>849</td>
</tr>
</tbody>
</table>

Notes: Data are collected from credit-card receipts for transactions that occurred between June 1, 1999, and June 29, 2001, at a moderately priced, non-chain restaurant in upstate New York. The sample includes two years of customers’ credit-card receipts. Tip Percentage is calculated by the tip amount divided by the bill amount times 100. Holidays is a dummy variable equal to 1 if the transaction occurred between one week before or after Christmas. Number of checks counts the amount of checks in the data on the same day of the given transaction, providing a proxy for how busy the establishment was. Other Holidays includes other holidays in which school was not in session (e.g., Labor Day) that are listed on the local public school district calendar. The Holidays Sample includes receipts for customers who appear in the data at least twice and excludes those for customers who appear either only in the holidays period or only in the non-holidays period. Transactions come from 22 servers and 123 customers.
<table>
<thead>
<tr>
<th>Variable</th>
<th>(1) Tip %</th>
<th>(2) Tip %</th>
<th>(3) Tip %</th>
<th>(4) Tip %</th>
</tr>
</thead>
<tbody>
<tr>
<td>Holidays</td>
<td>2.680**</td>
<td>2.783**</td>
<td>3.372**</td>
<td>3.671**</td>
</tr>
<tr>
<td></td>
<td>(1.264)</td>
<td>(1.352)</td>
<td>(1.496)</td>
<td>(1.584)</td>
</tr>
<tr>
<td>Number of Checks</td>
<td>-0.0230</td>
<td>0.00186</td>
<td>-0.00216</td>
<td>0.0136</td>
</tr>
<tr>
<td></td>
<td>(0.0883)</td>
<td>(0.0781)</td>
<td>(0.100)</td>
<td>(0.0912)</td>
</tr>
<tr>
<td>Other Holidays</td>
<td>0.761</td>
<td>-0.0878</td>
<td>1.255</td>
<td>0.642</td>
</tr>
<tr>
<td></td>
<td>(1.460)</td>
<td>(1.378)</td>
<td>(0.903)</td>
<td>(0.935)</td>
</tr>
<tr>
<td>Bill Totals</td>
<td>(Included)</td>
<td>(Included)</td>
<td>(Included)</td>
<td>(Included)</td>
</tr>
<tr>
<td>Weekday-Hours</td>
<td>(Included)</td>
<td>(Included)</td>
<td>(Included)</td>
<td>(Included)</td>
</tr>
<tr>
<td>Servers</td>
<td>(Included)</td>
<td></td>
<td>(Included)</td>
<td></td>
</tr>
<tr>
<td>Customers</td>
<td></td>
<td>(Included)</td>
<td>(Included)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>55.54***</td>
<td>39.28***</td>
<td>12.43**</td>
<td>92.56***</td>
</tr>
<tr>
<td></td>
<td>(1.454)</td>
<td>(7.589)</td>
<td>(6.097)</td>
<td>(10.24)</td>
</tr>
<tr>
<td>Observations</td>
<td>842</td>
<td>840</td>
<td>842</td>
<td>840</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.379</td>
<td>0.405</td>
<td>0.640</td>
<td>0.658</td>
</tr>
</tbody>
</table>

Robust standard errors (clustered at customer level) in parentheses

*** p < 0.01, ** p < 0.05, * p < 0.1

Notes: The dependent variable is Tip Percentage. The sample includes two years of customers’ credit-card receipts. Tip Percentage is calculated by the tip amount divided by the bill total times 100. The coefficient of interest is that of Holidays, which is a dummy variable equal to 1 if the transaction occurred one week before or after Christmas. Number of checks counts the amount of checks in the data on the same day of the given transaction, providing a proxy for how busy the establishment was. Other Holidays includes other holidays in which school was not in session (e.g., Labor Day) that are listed on the local public school district calendar. Bill Totals are vigintiles of bill amount. Weekday-Hours, Customers, and Servers are all indicators. The Holidays Sample includes receipts for customers who appear in the data at least twice and excludes those for customers who appear either only in the holidays period or only in the non-holidays period.
### Table 1.4: OLS Regression of Tip Rate on Holidays, Day Coefficients

<table>
<thead>
<tr>
<th>Day of the Month</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>-18.69</td>
<td>-18.25</td>
<td>-16.35</td>
<td>-15.39</td>
</tr>
<tr>
<td>2nd</td>
<td>-16.33</td>
<td>-15.99</td>
<td>-18.10</td>
<td>-17.71</td>
</tr>
<tr>
<td>3rd</td>
<td>-11.01</td>
<td>-10.97</td>
<td>-11.74</td>
<td>-10.77</td>
</tr>
<tr>
<td>4th</td>
<td>-12.78</td>
<td>-12.43</td>
<td>-13.12</td>
<td>-12.35</td>
</tr>
<tr>
<td>5th</td>
<td>-11.54</td>
<td>-11.03</td>
<td>-11.61</td>
<td>-10.28</td>
</tr>
<tr>
<td>6th</td>
<td>-17.54</td>
<td>-16.24</td>
<td>-16.73</td>
<td>-15.22</td>
</tr>
<tr>
<td>7th</td>
<td>-11.22</td>
<td>-9.81</td>
<td>-15.54</td>
<td>-13.22</td>
</tr>
<tr>
<td>8th</td>
<td>-16.06</td>
<td>-14.38</td>
<td>-17.91</td>
<td>-15.38</td>
</tr>
<tr>
<td>9th</td>
<td>-12.81</td>
<td>-12.14</td>
<td>-14.86</td>
<td>-13.75</td>
</tr>
<tr>
<td>10th</td>
<td>-11.94</td>
<td>-10.73</td>
<td>-13.38</td>
<td>-11.53</td>
</tr>
<tr>
<td>11th</td>
<td>-11.89</td>
<td>-12.40</td>
<td>-12.09</td>
<td>-11.67</td>
</tr>
<tr>
<td>12th</td>
<td>-7.10</td>
<td>-5.21</td>
<td>-10.19</td>
<td>-9.15</td>
</tr>
<tr>
<td>14th</td>
<td>-13.35</td>
<td>-12.03</td>
<td>-15.71</td>
<td>-14.42</td>
</tr>
<tr>
<td>16th</td>
<td>-16.08</td>
<td>-16.10</td>
<td>-16.69</td>
<td>-16.70</td>
</tr>
<tr>
<td>17th</td>
<td>-15.62</td>
<td>-14.23</td>
<td>-18.05</td>
<td>-16.54</td>
</tr>
<tr>
<td>18th</td>
<td>-15.14</td>
<td>-14.79</td>
<td>-15.97</td>
<td>-14.77</td>
</tr>
<tr>
<td>19th</td>
<td>-10.64</td>
<td>-9.12</td>
<td>-12.99</td>
<td>-12.19</td>
</tr>
<tr>
<td>20th</td>
<td>-12.81</td>
<td>-11.57</td>
<td>-15.88</td>
<td>-13.89</td>
</tr>
<tr>
<td>21st</td>
<td>-15.37</td>
<td>-14.20</td>
<td>-18.60</td>
<td>-17.59</td>
</tr>
<tr>
<td>22nd</td>
<td>-13.98</td>
<td>-13.05</td>
<td>-14.04</td>
<td>-12.18</td>
</tr>
<tr>
<td>24th</td>
<td>-15.67</td>
<td>-15.74</td>
<td>-16.95</td>
<td>-16.16</td>
</tr>
<tr>
<td>26th</td>
<td>-15.47</td>
<td>-14.00</td>
<td>-15.90</td>
<td>-14.76</td>
</tr>
<tr>
<td>27th</td>
<td>-13.96</td>
<td>-12.43</td>
<td>-15.25</td>
<td>-13.57</td>
</tr>
<tr>
<td>28th</td>
<td>-17.22</td>
<td>-15.42</td>
<td>-18.27</td>
<td>-16.45</td>
</tr>
<tr>
<td>30th</td>
<td>-9.24</td>
<td>-8.81</td>
<td>-12.38</td>
<td>-11.81</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable is Tip Percentage. Regression numbers correspond to specifications in Table 1.3 with the addition of indicators for each day of the calendar month. The 31st of the month is the omitted category.
### Table 1.5: OLS Regression of Tip Rate on Holidays × Baseline Tip Rate

<table>
<thead>
<tr>
<th>Variable</th>
<th>2 bins</th>
<th>4 bins</th>
<th>8 bins</th>
<th>10 bins</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Tip %</td>
<td>Tip %</td>
<td>Tip %</td>
<td>Tip %</td>
</tr>
<tr>
<td>Holidays × Quantile 1</td>
<td>1.336</td>
<td>3.472**</td>
<td>3.965**</td>
<td>4.635**</td>
</tr>
<tr>
<td></td>
<td>(1.302)</td>
<td>(1.520)</td>
<td>(1.551)</td>
<td>(1.917)</td>
</tr>
<tr>
<td>Holidays × Quantile 2</td>
<td>4.790**</td>
<td>-0.518</td>
<td>0.861</td>
<td>0.347</td>
</tr>
<tr>
<td></td>
<td>(1.961)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 3</td>
<td>3.043*</td>
<td>-1.118</td>
<td>2.135</td>
<td>1.762</td>
</tr>
<tr>
<td></td>
<td>(1.828)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 4</td>
<td>7.470*</td>
<td>1.603</td>
<td>1.656</td>
<td>2.573</td>
</tr>
<tr>
<td></td>
<td>(3.552)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 5</td>
<td>-0.661</td>
<td>2.077</td>
<td>(2.061)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 6</td>
<td>4.150*</td>
<td>-0.207</td>
<td>(3.388)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.126)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 7</td>
<td>0.778</td>
<td>2.489</td>
<td>(2.271)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.285)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 8</td>
<td>9.402*</td>
<td>2.354</td>
<td>(3.039)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(5.314)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 9</td>
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<td>(3.651)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holidays × Quantile 10</td>
<td>12.32*</td>
<td></td>
<td>(6.115)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>40.15***</td>
<td>37.08***</td>
<td>38.60***</td>
<td>37.42***</td>
</tr>
<tr>
<td></td>
<td>(7.824)</td>
<td>(7.028)</td>
<td>(6.426)</td>
<td>(7.106)</td>
</tr>
<tr>
<td>Observations</td>
<td>840</td>
<td>840</td>
<td>840</td>
<td>840</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.462</td>
<td>0.524</td>
<td>0.557</td>
<td>0.567</td>
</tr>
</tbody>
</table>

Robust standard errors (clustered at customer level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The dependent variable is Tip Percentage. The sample includes two years of customers’ credit-card receipts. Tip Percentage is calculated by the tip amount divided by the bill total times 100. Baseline Tip Rate is the customer’s mean tip rate not during the holidays period; bins are used to capture nonlinear quantile differences in baseline tip rates. The coefficients of interest are that of Holidays × Baseline Tip Rate bins, which are equal to 1 if the transaction occurred between one week before and after Christmas and the customer’s baseline tip falls into the specified quantile bin. Larger quantiles represent the top of the tipping distribution while smaller ones represent the bottom. Number of Checks, Bill Totals, Servers, Baseline Tip Rates, and Weekday-Hours are included. The Holidays Sample includes receipts for customers who appear in the data at least twice and excludes those for customers who appear either only in the holidays period or only in the non-holidays period.
Chapter 2

The Ripple Effects of Deceptive Reporting
2.1 Introduction

Reliable communication is an essential component in the workings of organizations. Unfortunately, deception and misreporting are commonplace. Dyck et al. (2013) report that the probability of a company engaging in fraud in a given year is 14.5%, costing investors 22% of firm value. Zingales (2015), in his Presidential address, laments that “fraud has become a feature, not a bug” in the financial sector (see also Cohn et al. (2014)). From automobile sales (both of used cars, which served as the motivating example for Akerlof (1970), and new cars, as seen in the Volkswagen case) to professional athletics, we regularly witness destructive acts of dishonesty.

One way organizations have attempted to reinforce honest behavior is through explicit ethical guidelines (e.g., ethics training, honor codes, standards of conduct) which highlight acceptable behavior and strongly discourage deviations from the ethical standard. Much research has been devoted to whether such social norms work (Cialdini and Trost 1998), with the hope that once all or most individuals in an organization follow the desired behavior, this descriptive norm maintains behavior in the future. Yet, there is a paucity of research investigating what happens when individuals are hurt by deviations from these social norms.

The chief concern motivating our research is that there may be a tension between the strength of a social norm and its fragility. Even though social norms can promote good behavior, when everyone expects ethical behavior, a deviation from such a strong norm may be more harmful, thus causing the victim to behave unethically as well. For example, an athlete who uses performance-enhancing drugs will hurt others around her more when there are strict rules forbidding them and when others expect honest behavior. If other teammates were to discover this doping, they would be more likely to consider doping themselves, unless they are particularly committed to honest sportsmanship. In corporations, when one division head is found reporting his performance numbers incorrectly, this may lead other division heads to consider reporting manipulated numbers as well—especially if they believed that the organization has a strong social norm for honest reporting.
In this paper, we study whether and when dishonesty is “paid forward” after being hurt by a dishonest act. We hypothesize that both situational and dispositional factors determine the reaction to being the victim of dishonesty. First, compared to modest ethical norms, strong ethical norms should increase the likelihood that dishonesty is paid forward. Although a norm violation may make morality more salient (in the spirit of Aquino and Reed (2002)), such a violation may cause a negative affective reaction and a loss of self-control. Second, those who are overall more committed to honest behavior and have a stronger internal resistance against conceding the value of honesty should be less likely to pay forward dishonesty.

The existing literature provides enough evidence to consider these hypotheses plausible, but does not provide specific answers to the questions we pose. Gray et al. (2014) and Stanca (2009) provide evidence of paying-it-forward in the area of generosity and its antisocial opposite (greed). Yet these works do not address the critical question regarding how paying-it-forward might vary depending on the expectations people hold (i.e., the social norm), which we manipulate. Moreover, they do not consider how individual attitudes might explain why some pay it forward while others do not. Our paper highlights the importance of both social norms and individual differences in preferences in the propensity to “pay it forward.”

While we focus on how people react to being victims of norm violations, other research has examined how observing others deviate can contagiously erode cooperative or ethical behavior. This theme is present already in the seminal work on social norms by Cialdini et al. (1990). Gino et al. (2009) find that observing a cheating in-group member (“bad apple”) can promote dishonest behavior.1 Similarly, Innes and Mitra (2013) find that expectations of dishonesty are correlated with dishonest behavior. In contrast, the current research studies how those directly hurt by a deviation from the norm react to dishonesty by choosing to pay it forward (or not).2

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1Indirect reciprocity suggests that third parties may punish bad apples (e.g., Fehr and Gächter 2000; Nowak and Sigmund 1998; Wedekind and Milinski 2000).

2Repetition plays no role in our experiment, but can be important in practice (see, e.g., Rand
We conduct laboratory experiments following a paradigm that involves sender-receiver games (Gneezy 2005). Participants play sequential games. Player X decides whether to tell the truth or to lie to Player Y. Thus, we generate exogenous variation in whether Players Y have been lied to or told the truth. To cleanly identify the effects of interest, the setting is fully anonymous and there exists no possibility of rewards or punishments. In the Baseline treatment, Players Y (correctly) know that in a previous round of the experiment with the same structure, 9 out of 10 Players X sent a truthful message (henceforth referred to as the Truthfulness Norm). Our outcome of interest is Player Y’s decision to tell the truth or to lie to an unrelated Player Z in a separate subsequent sender-receiver game that immediately followed (henceforth referred to as No Delay).

We find four primary results. First, in the Baseline treatment (Truthfulness Norm, No Delay), participants who have been lied to are more likely to lie than those who have been told the truth. 74% of Players Y lie to Player Z after receiving a lie from Player X, while 55% of Players Y lie to Players Z following a truthful message from Player X. Second, those who are predicted (via a survey measure) to feel stronger emotional distress when faced with possible violations of honesty not only tell the truth more often on average, but are also significantly less likely to pay forward lies. Third, when Players Y know that in a previous experimental round, 64% of Players X told the truth (henceforth Neutral Norm), receiving a lie has no impact on Player Y behavior. Thus, lying begets lying only when it represents a strong social norm violation. But this also means, conversely, that strong honesty norms can be quite fragile. Finally, we provide evidence which suggests that emotions partially explain why dishonesty is paid forward.

In sum, our paper demonstrates that high ethical standards can have the adverse effect of causing dishonest acts to be paid forward out of a visceral reaction, unless individuals are sufficiently intrinsically motivated.

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e et al. (2009). There can also be a transfer from prior learnings to one-shot games (Peysakhovich and Rand 2016).
2.2 Experimental Design and Procedure

2.2.1 Sequential Sender-Receiver Games

Figure 2.1 presents the sequence of actions taken and information given for the game. We adapt versions of the sender-receiver games used by Gneezy (2005) and Erat and Gneezy (2012). In these games of incomplete information, one player (the “sender”) sends a message to another player (the “receiver”).

Players X participated as senders in a modified version of the cheap-talk sender-receiver game in line with Gneezy (2005) with Players Y as receivers. Players X sent Players Y a message regarding which of two possible options (“Option A” or “Option B”) would earn Players Y more money. Players X knew the payoffs associated with both options and in particular knew that reporting to Player Y that “Option A will earn you more money than Option B” was the true message. Players X were told that they would be paired with receivers (Players Y) and that after the game was finished, the veracity of their message would be revealed to Players Y. Players Y then picked one of the two options. Then, Players Y learned whether the messages sent to them were truthful. They did not learn the payoff possibilities of the game.

Players Y were told a descriptive norm about the proportion of senders in past experiments that had sent true versus false messages at the time they received the messages from Player X. Players X were told that Players Y would be given this information, but Players X did not know the proportion themselves. Both Players X and Y were told that 1 out of 5 pairs would be randomly selected for payment after the experiments were finished.

After learning the true message in the game with Players X, Players Y assumed the role of the sender in a subsequent game with a new set of receivers (Players Z). In the role of senders, Players Y participated in a version of the cheap-talk sender-receiver game in line with Erat and Gneezy (2012). Players Y sent Players Z a message (a number between 1 and 6) regarding the outcome of a roll of a 6-sided die. Players Y knew that stating that the outcome of the die roll was 5 was the true message and knew the payoffs associated with the outcomes (Players...
Players Z knew that the payoffs between sender and receiver were conflicting. Unlike in the game between Players X and Y, the veracity of the message sent by Player Y was not revealed in the game between Players Y and Z. Players Y and Z were told that 1 out of 5 pairs would be randomly selected for payments after the experiments were finished. For the payoff matrix in both games, see Table 2.1.

A number of features of this design merit comments. First, the game between Players X and Y differ from that between Players Y and Z. If the same game had been used, Players Y might have associated the parameters they observed as senders with the unknown parameters in their game with Players X. In addition, we wanted to rule out the possibility that players simply mimic messages they received. By introducing a different game with a new message space, we are able to rule out these potential alternative sources of noise.

Second, the payoffs in the game between Players Y and Z are chosen with two important considerations. One design feature aimed to avoid ceiling effects in truth-telling rates. We expect (and indeed find) that roughly two thirds of Players X report the truth. Truthfulness is made sufficiently costly in our game between Players Y and Z to increase variation in truthful behavior. (In Erat and Gneezy (2012), almost two thirds of participants told the truth if the costs of truthfulness were just $1 (see their treatment [1;−5]).) Moreover, the lowest payoff in the Y-Z game was chosen to be higher than the highest payoff in the X-Y game so that Players Y would not be disappointed about their payoffs they potentially earned as receivers with Players X.

2.2.2 Methods

We first describe the Baseline treatment (Truthfulness Norm, No Delay) and later explain the additional treatments. Participants in all treatments come from the laboratory subject pool at the Rady School of Management at the University of California, San Diego.

At the time Players Y receive messages from Players X, they are also informed about the prevailing descriptive social norm for Player X behavior. An
important aspect of our analysis is the test of the idea that participants would be most likely to pay forward a lie or a truthful message when they strongly expect the truth from Player X. Therefore, we created such a Truthfulness Norm situation as the baseline, and we exposed a different group of participants instead to a setting where expectations regarding Player X behavior were much less clear—the Neutral Norm.

To implement the Truthfulness Norm without deception, we proceeded as follows. We first ran pilot sessions in which Players Y were told that in a recent similar experiment with the same payoff structure, 64% of subjects announced a message that corresponded to the truth (Neutral Norm; based on Gneezy (2005)). In one session of these pilots, 86% of Players X in fact told the truth. Thus, we were able to generate a Truthfulness Norm based on this session. Specifically, in the Truthfulness Norm, Players Y were told that 9 out of every 10 subjects in a previous session announced a message that corresponded to the truth. We also ran actual sessions in which Players Y were informed of the Neutral Norm to test whether paying lies forward was less pronounced when truth-telling was not strongly normative.

While this is not the only experimental procedure one could have chosen, the alternatives would have been undesirable. As one alternative, we could have simply informed Players Y of a purported typical behavior of Players X, although this would have required potentially deceiving subjects.

A second alternative would have been assigning Players’ X true or false messages to Players Y in a proportional fashion—selecting 9 out of every 10 Players Y to receive a truthful message in the Truthfulness Norm treatment, but 6 out of every 10 Players Y to receive a truthful message in the Neutral Norm treatment. While this has the advantage that Player Y expectations would turn out to be correct within each of the two norms, it has the disadvantage that the probability of actually receiving a lie would be confounded with assignment to one of the two norms. Moreover, very few subjects in the Truthfulness Norm would have their expectations violated, which would have been a limitation.

We could have included sophisticated incentivized belief elicitations to determine whether, in fact, Players Y believe the probabilities of receiving a lie that
their respective norms conveyed. A belief elicitation task would have drawn additional attention to receiving a lie which could have introduced experiment demand effects. Moreover, if beliefs were, contrary to our expectations, the same across the Truthfulness Norm and Neutral Norm, we should find no differences across the two norm treatments.

In short, the procedure we chose delivers simple, non-deceiving variation in Player Y expectations about receiving a lie.

We ran additional experiments using a Delay treatment. This is discussed in more detail below.

Thus, we study three groups of Players Y (N = 540): those who went through the Baseline (Truthfulness Norm, No Delay) treatment (N = 205), those who went through the Neutral Norm, No Delay treatment (N = 185), and those who went through the Delay treatment (N = 150). For Players X and Z, the game remained the same throughout.

All Players Y were asked to answer questions about their affect via the standard Positive and Negative Affect Schedule (PANAS), given before the sender-receiver game with Players Z. As shown in Watson et al. (1988), what PANAS measures depends on the precise instructions. When used with short-term instructions (as we do in the experiment by asking participants to “Indicate to what extent you are feeling this way at the immediate moment”), PANAS captures fluctuations in affective state (rather than trait measures).

After sending messages to Players Z, Players Y were asked questions about prosocial concern, protected values for truthfulness, and concern about social image.

To measure prosocial concern, we use participants’ answers regarding the extent to which they believed that announcing the wrong message had negative consequences for Player Z. These answers are coded as Believes lying hurts Z.

We use additional established scales to isolate heterogeneity in decisions regarding dishonesty, as existing theoretical work has highlighted the potential role for psychological and moral lying costs (Kartik 2009). We use two scales for protected values for truthfulness (Gibson et al. 2013; Hanselmann and Tanner 2008), using the terminology in Gibson et al. (2015): One component, PRV
(Protected values for truthfulness - reactions to violations of honesty), captures affective reactions to and emotional consequences of violations of honesty (Tetlock et al. 2000). PRV is designed to pick up variation in the extent to which individuals feel emotional distress when faced with the possibility of violations of honesty, and variation in the extent to which they wish to balance out this distress by acting in line with their intrinsic preferences and by self-regulation. PRV is a central component of the analysis below. A second component, PNT (Protected values for truthfulness - no trade-off), emphasizes the more cognitive notion that some participants may regard truthfulness as priceless and not subject to economic benefit-cost analysis (Baron and Spranca 1997). The order of questions asked was either prosocial concern, PRV, PNT; or PNT, prosocial concern, PRV.

Using the standard Deception Scales of Paulhus (1984), we also measure participants’ tendencies to give deceptive responses. At the very end, participants completed a demographic questionnaire.

### 2.2.3 Descriptive Statistics and Manipulation Check

Table 2.2 reports summary statistics for all variables used in the analysis. The participants are approximately one-half female, one-third economics or management majors (henceforth referred to simply as Economics), and of an average age of 21.\(^3\) PRV, PNT, and Believes lying hurts Z are rescaled to have a mean of zero and a standard deviation of one. The medians of these variables are also essentially zero.

On average, Players Y perceived announcing the true message (that is, the correct number) as significantly more honest \((t(539) = 30.34, p < .001)\), less manipulative \((t(539) = 17.59, p < .001)\), and less associated with personal gains \((t(539) = 20.76, p < .001)\) than announcing a false message (i.e., a different number). In addition, PRV measures are not sensitive to randomly assigned conditions; a regression of PRV on the Truthfulness Norm, Delay treatment, and the

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\(^3\)One participant who erroneously recorded and age of 2 was recoded as 21, the median age given graduation year within the session. Another participant (age 44) was dropped, though including him would not affect the results.
interaction yields no significant coefficients (all $ps > .448$). As expected, this indicates that PRV is a trait measure and unaffected by context.

### 2.3 Results

#### 2.3.1 Main Results in the Baseline Treatment

Our interest is in the decisions made by Players Y. (Players X served to provide exogenous variation in whether Players Y were lied to. Players Z served to make Players’ Y decisions payoff-relevant.)

Visual inspection of the descriptive evidence suggests that in the Baseline treatment prior communication from Player X has an important impact on Player Y’s communication with Player Z. Specifically, Figure 2.2 shows that 26% of Players Y who were told a lie by Players X later told the truth to Players Z; in contrast, 45% of Players Y who were told the truth by Players X later told the truth to Players Z. That is, receiving a false message from Player X results in a higher propensity to lie to Player Z than does a true message from Player X ($t(203) = 2.67$, $p = .008$).

**Result 2.1** When strong social norms for honesty exist, people pay lies forward.

To rigorously test the hypothesis that lies are paid forward by controlling for additional factors that might explain differences in Player Y truthfulness, we run probit regressions with Player Y’s choice to tell the truth as the dependent variable.\(^4\) Heteroskedasticity-consistent standard errors (White 1980) are used to generate robust z-statistics. We primarily consider coefficients, rather than

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\(^4\)Using a logit instead of a probit yields very similar results. Underlying this probit/logit regression is a random utility model, in which the attractiveness of truthfulness relative to lying is affected by (i) whether the agent has just received a lie; (ii) the agent’s intrinsic preferences for truthfulness; (iii) possibly an interaction between (i) and (ii); and (iv) economic incentives (which, however, are held constant in the experiment and, therefore, cannot explain variation in Player Y truthfulness). Note that there is some possibility that senders “lie” by telling the truth if they expect receivers not to follow their message (Sutter 2009). The message space is sufficiently large by construction so that receivers, who have no information about the payoffs in the game, are likely better off by following the messages sent by senders. Indeed, this is empirically verified in our experiment, where 84% of receivers follow senders’ messages.
marginal effects, from the probit regressions in the subsequent analysis. Analyzing coefficients allows us to consider the counterfactual case in which participants would display identical initial probabilities of reporting the truth. (The highest marginal effects are expected to be found in the range of those participants with medium initial probabilities of truthfulness.)

In column (1) of Table 2.3, we find a highly significant negative effect of receiving a lie. We also find that Players Y with high PRV are significantly more likely to tell the truth in general. In column (2), we find that the main result is robust to including age, gender, and undergraduate major. Column (3) suggests that when controlling for PRV the extent to which Player Y believes that lying hurts Player Z offers no additional explanatory power.\(^5\)

### 2.3.2 When and Why Are Lies Paid Forward?

By studying when and by whom lies are paid forward, we gain insights into a few reasons (not mutually exclusive) why lies are paid forward. First, we document differences among individuals. In particular, we show that agents who strongly care about violations of honesty are not only more truthful on average, but also far less likely to pay lies forward. Second, we provide evidence that whether a lie is paid forward depends on what Player Y thought about Player X’s behavior. Finally, we document that lies are not paid forward when Players Y have an opportunity to “cool down” before making their own choices, which is consistent with the notion that paying a lie forward is, in part, an affective-based decision. In line with this interpretation, those receiving a lie experience more negative and less positive affect.

**Differences Among Individuals**

The extant literature emphasizes that agents differ in their preferences for truthfulness (Charness and Dufwenberg 2006; Gibson et al. 2013; Gneezy 2005; Paulhus 1984). In unreported regressions we also find that the results hold when controlling for self-deceit and impression management (Paulhus 1984). These two measures do not significantly determine Player Y’s choice.
Vanberg (2008) and prosocial concern (Fehr and Fischbacher 2002). We hypothesize that these preferences affect not only the level of truthfulness, but also how agents react to experiencing violations of truthfulness. Consistent with this hypothesis, we find striking differences among individuals’ reactions to receiving a false message. Consider Panel A of Figure 2.3, which represents the truth-telling rates of Players Y in the Baseline treatment with low PRV levels (i.e., below the mean) separated by whether they received a lie or a truthful message from Players X. 31% of those with low PRV that received a truthful message subsequently sent truthful messages to Players Z, while 15% of those that received a false message sent truthful messages to Players Z. That is, Players Y with low PRV are half as likely to tell the truth if they received a lie.

In contrast, in Panel B, for those with high PRV (i.e., above the mean), we observe a much smaller difference between those who receive the truth and those who receive a lie (53% versus 46%, respectively).

Result 2.2 Those who have a stronger regard for protecting the value of truthfulness are less likely to pay lies forward.

Table 2.4 shows that these results carry over to the regression analysis. Consider the interaction term between PRV and Received a lie in column (1). The coefficient on this interaction term is positive. This indicates that the more Player Y cares about violations of truthfulness and the more strongly self-regulation is thus predicted to occur, the less she reacts to having received a lie. However, those with low levels of PRV react to receiving a lie by being less truthful with Player Z. Note that since PRV is centered around 0, the coefficient on Received a lie can be interpreted as the effect for the average participant.

It is important to note that certain differences in personal characteristics do not explain heterogeneity in paying lies forward. Column (2) adds the second component of protected values for truthfulness, PNT, which captures the more cognitive notion that Player Y may regard truthfulness as priceless. Column (3) adds prosocial concern. While both trade-off resistance (as measured by PNT) and prosocial concern promote overall truthfulness, neither explains differences in paying lies forward.
These findings on heterogeneous responses among individuals are intuitive. High-PRV Players Y feel emotional distress when faced with possible violations of honesty. The results suggest that for these players, a self-regulatory action is triggered such that they attempt to balance out this distress by acting in line with their intrinsic preferences. Overall, the evidence is consistent with the idea that at least part of the observed tendency to pay lies forward is due to gut reactions, rather than a cognitive consideration.

**Surprising Lies Matter Most**

What happens when receiving a lie does not actually come as a surprise? To address this question, we consider Player Y behavior under the Neutral Norm. Table 2.5 suggests that when agents expect to be lied to with a large enough probability, their reactions to being lied to are subdued; in regressions (1) to (3), which focus on the Neutral Norm sample, receiving a lie has no significant effect on subsequent truth-telling. Column (4) of Table 2.5, which combines both norm samples, shows that paying lies forward differs significantly across the two norms.

**Result 2.3** *In the absence of strong norms against dishonesty, dishonesty is not paid forward.*

This result implies that contagion (Gino et al. 2009; Innes and Mitra 2013) need not occur. Lying begets lying only when it represents a social norm violation. When lying is not a strong violation of social norms, the past experience of receiving a lie has no effect on one’s future behavior. Notably we observe that under the Neutral Norm (see column (2)), even low-PRV types do not react to receiving a lie by paying it forward. Overall, the findings suggest that even though strong social norms can promote honest behavior, unexpected deviations may cause ripple effects.

We also note that these results rule out concerns that Players Y tried to “make up” for lost earnings after having been lied to. If income effects explained why Players Y told the truth less often after receiving a lie, we would observe no differences in behavior between the Truthfulness Norm and Neutral Norm
treatments. Since we observe that lies are paid forward in the Truthfulness Norm and not in the Neutral Norm, income effects cannot explain our findings.

“Hot” Versus “Cool” Decisions: The Role of Emotions in Disclosure

In the Baseline treatment, Players Y sent their messages to Players Z immediately after learning whether Player X had lied to them. In the Delay treatment, Players Y were first given a 10-minute anagrams task directly following the revelation of whether Players’ X messages were true and before they entered their role as sender with Players Z. Similar filler tasks have been used to identify the effects of emotions on behavior (Andrade and Ariely 2009; Gneezy and Imas 2014; Gneezy et al. 2014). Table 2.6 reports regressions results for those assigned to the Delay treatment. When the decision to lie or tell the truth to Player Z is delayed, paying it forward is attenuated. In columns (1) to (3), the coefficients on receiving a lie are about half the size of those in Table 2.3 and are not statistically significant. In columns (4) to (6), we again find that lies are not paid forward in the Neutral Norm—as in the No Delay setting. In short, Players Y do not pay lies forward when delayed regardless of the norm.6

Overall, the findings from comparing the results in the Baseline treatment and Delay treatment suggest that lies are paid forward because of a spontaneous “gut” reaction.

We now investigate emotional responses more directly. To our knowledge, no prior work examines the emotional consequences of being lied to, though some related work suggests that there may be such effects. For example, those from whom a surplus has been “taken” experience more negative emotions and respond by reducing the pie (Bosman and Van Winden 2002). Studying these consequences for Player Y emotions is important because Player Z (in a receiver

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6This evidence is consistent with the general evidence that recent personal experience has a much stronger effect on future behavior than past experience (Haselhuhn et al. 2012). Rand et al. (2012) find that individuals are more likely to be cooperative and prosocial when they have a very short amount of time to decide, but if given time to think, they are more “rational” and selfish. Our experiment does not vary the time participants have for their decisions, but the time that passes between the information received regarding behavior of the previous player and one’s own move.
role) might experience the consequences of Player Y’s “emotional hazard.”

We rely on state affect, as measured through the PANAS. As we are interested in the victims of lies, we focus on those Players Y who followed Player X’s message in the primary analysis. Figure 2.4 illustrates the substantial differences in positive affect due to receiving a lie or being told the truth.

Column (1) of Table 2.7 demonstrates that this result is robust to controls for other factors. Receiving a lie has an overall negative effect on positive affect, reducing it by approximately $1/4$ of a standard deviation. Moreover, after receiving a lie, negative affect is higher in magnitude, although not significantly. If we instead use affect ratio (column (3)), we find that the effect of receiving a lie is -0.384, or about $4/10$ of a standard deviation, a sizable impact. Columns (4) and (5) show that in the No Delay treatment, emotional reactions are more pronounced under the Truthfulness Norm when lying is expected to be relatively rare. In the Delay treatment, the impact of receiving a lie on emotional reactions is less pronounced after delay (columns (6) and (7)).

Finally, we conduct a placebo test for Players Z. For these players—who did not learn whether they had been told the truth or a lie—we do not find any differences in affect among those who received the truth or a lie. Regressions similar to those in Table 2.7 show that receiving a lie has no effect on positive

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7A number of papers document that positive mood in the workplace is associated with more prosocial organizational behavior. Numerous explanations (which are not mutually exclusive) exist for this phenomenon (see, for example, Carlson et al. (1988) and George (1991)). For example, positive moods cause people to perceive stimuli (such as the benefits and costs of acting ethically) and/or potential recipients of help more positively. Alternatively, individuals having received help may value opportunities for helping more favorably or may consider prosocial behavior as a way to maintain their positive moods. There are, however, reasons why one might expect effects in the opposite direction. First, if being lied to brings about an overall negative experience, resilient individuals might subsequently tell the truth to cope or bounce back from the experience (Folkman and Moskowitz 2000; Tugade and Fredrickson 2004). Second, recipients of a lie might feel more empathetic toward the other player (Miller and Eisenberg 1988).

8Recall that the PANAS questions were asked regarding Players’ Y current states, to be evaluated at the immediate moments they were answering the questions.

9The results are particularly strong for some specific aspects of PANAS. For example, consistent with intuition, participants receiving a lie feel particularly “upset” as a result of this act of dishonesty.
affect, negative affect, or the affect ratio (all \( ps > .84 \)).

Overall, these results provide substantial evidence that receiving a lie negatively affects emotions of the recipient of a lie. Our findings complement earlier work that has examined the emotional consequences for those who lie (Gneezy et al. 2014; Ruedy et al. 2013).

**Result 2.4** *Lies are paid forward, in part, due to emotional reactions from being lied to.*

**Player Y Judgment, Justification, and Anticipation**

Finally, we investigate what Player Y states concerning her choices and perceptions in the No Delay treatment. The corresponding questions were asked in later sessions, so sample sizes in this subsection are smaller. Three dimensions are of interest here.

First, after asking the PRV questions, we construct an “Attributed PRV” measure by asking Player Y to judge the behavior of Player X along the same PRV dimensions. We find that when Player Y receives a lie from Player X, he or she judges Player X as significantly less committed to truthfulness (\( t(233) = 12.02, p < .001 \)).

Second, Players Y themselves claim that the message from Player X influenced their decision to send a false or true message to Player Z. Compared with participants who did not pay forward a lie or a truthful message, those who did, in fact, pay forward a lie or a truthful message agree much more strongly (\( t(233) = -2.19, p = .030 \)) with the statement that they chose the message for Player Z because of the message sent by Player X.

Third, Players Y who tell the truth believe their messages will have effects that differ from those who tell a lie. In columns (1) and (2) of Table 2.8, we find that those who sent truthful messages are significantly more likely to believe Players Z will feel good about receiving truthful messages. (See Cain and Dana

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10 Neither the Truthfulness Norm nor the Neutral Norm applies in the placebo test.

11 Haselhuhn et al. (ming) argue that whether people change the perception of another person’s integrity after being deceived by them depends on the implicit theories held by the deceived person. We do not find that Player Y’s PRV significantly explains judgment of Player X.
More importantly, Players Y understand that their messages will be paid forward. The dependent variable in columns (3) and (4) is whether Players Y think Players Z would be more truthful with another, unrelated person. Compared to Players Y who are not truthful, those that are truthful think their truthful messages are more likely to be paid forward. This also suggests that those who lie are less likely to think their lies will have ripple effects.

2.4 Conclusion

This study of whether and why agents “pay lies forward” yields four primary results. We first show that when the truth is expected of senders, receivers are significantly more likely to pay lies forward. However, important predicted heterogeneity among individuals and across situations exists that shows that this generalized reciprocity need not occur—and that thus sheds light on why lies are paid forward. Specifically, our second result is that those who have a weaker emotional regard for protecting the value of truthfulness respond more strongly to whether they have been lied to. Third, when receiving a lie is less surprising in light of existing social norms for the sender, recipients do not react to receiving a lie. Fourth, whether or not the previous player sent the truth or a lie ceases to matter if players wait ten minutes to cool down. Indeed, being told the truth increases positive affect and decreases negative affect.

Our findings suggest that high honesty is a tenuous equilibrium; if honesty becomes the norm in an organizational setting, its members are more sensitive to deviations and certain types will act out when they are violated themselves.\(^\text{12}\) More generally, our findings suggest that strong ethical norms may, in fact, be fragile. Our results offer a first glimpse into the ripple effects of deceptive communication and other unethical behavior. It is plausible that these ripple

\(^{12}\)The idea that high honesty can be tenuous is consistent with Yip and Schweitzer (2015), who suggest that trust may promote deception, because in high-trust environments, senders realize that they have greater chances of successfully misleading followers.
effects may extend through a chain that is longer than one more party.

In this research, we have studied differences in behavior across different descriptive norms. Because descriptive norms are less likely to activate considerations about the appropriateness of paying lies forward, we suspect that violations of injunctive norms may induce even stronger reactions to deviations (see Cialdini et al. (2006), who find that combinations of injunctive and descriptive norms are most effective in governing behavior). Moreover, the results imply that in order to avoid “lying cascades,” visceral factors that affect communication need to be taken into account. This evidence supports the basic thrust of the recent literature on the importance of emotions in economic decisions (Loewenstein 2000), which can even be used strategically (Gneezy and Imas 2014), in the context of social punishment (Hopfensitz and Reuben 2009), and in negotiations (Gaspar and Schweitzer 2013).

One interpretation of our findings is that emotions cause differences in self-control, which in turn have been shown to be related to unethical behavior (Gino et al. 2011). Our results are also of interest because they suggest that while some strongly react to the external impetus of receiving a lie, others are steadfast. Overall, therefore, in settings where truthful choice is important, selecting the right agents and giving agents sufficient time to calmly consider their next move may be beneficial.

Chapter 2, in full, has been submitted for publication of the material. Greenberg, Adam Eric; Wagner, Alexander F. The dissertation author was the primary author of this material.

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13Societies, institutions, and organizations aim to support truthful behavior also by explicit incentives of various forms (e.g., punishment), which are held constant in our experiment. The introduction of punishment might have an ambiguous effect on paying lies forward. On the one hand, the additional cost associated with lying could reduce paying them forward. On the other hand, the strong norm associated with punishment for lying could, ironically, cause victims of lies to pay lies forward.
2.5 Appendix: Experimental Protocol

Note: Items appearing in (RED) are from the Truthfulness Norm treatment.

2.5.1 Instructions for Player X

You will be matched randomly with another participant. Neither of you will know the identity of the other in the pair.

At the end of the study, 1 OUT OF 5 PAIRS will be chosen at random for payment.

[page break]

On the next page, you will be shown the instructions. You will also be asked to answer three questions to assess your understanding of these instructions. You will not be able to advance until you answer all of these questions correctly.

[page break]

INSTRUCTIONS

Two possible monetary payments are available to you and the other participant. The two payment options are:

Option A: $5 to you and $6 to the other participant
Option B: $6 to you and $5 to the other participant

The choice rests with the other participant who will have to choose either option A or option B.
The only information the other participant will have before making this choice is information sent by you in a message. That is, he or she will not know the monetary payments associated with each choice.

We now ask you to choose one of the following two possible messages, which you will send to your counterpart:

Message 1: “Option A will earn you more money than option B.”
Message 2: “Option B will earn you more money than option A.”

Your message will be shown to the other participant, and then he or she will be asked to choose either A or B.

Note that:

- The only information the other participant will have before making his or her choice is information sent by you in this message. Only you will know the monetary payments associated with each choice.
- The other participant will be told that you have been informed about the monetary value connected to each message and that one option earns you more money than the other option.
- However, the other participant will never know what sums were actually offered in the option not chosen.
- Moreover, he or she will never know the sums offered to you by each of the different options.
- The other participant has been informed about behavior of subjects in a recent similar experiment with the same payoff structure. In particular, he or she knows exactly what percentage (proportion) of subjects announced a message that corresponds to the facts.
We will inform the other participant at the end of the experiment (after all payoffs are determined) which message corresponds to the facts.

It is important that you understand the instructions. To assess your understanding, there are three questions. You must answer them correctly before you advance.

Understanding Question #1: Suppose you have sent message 1 to the other participant and he/she chooses A.

What will your payoff be?
What will the other participant’s payoff be?

Understanding Question #2: Suppose you have sent message 2 to the other participant and he/she chooses B.

What will your payoff be?
What will the other participant’s payoff be?

Understanding Question #3: Suppose you have sent message 1 to the other participant and he/she chooses A.

What will your payoff be?
What will the other participant’s payoff be?

The instructions are reproduced below for your convenience.

I will send the following message:

Message 1: “Option A will earn you more money than option B.”
Message 2: “Option B will earn you more money than option A.”

2.5.2 Sample Receiver Instructions for Player Y (Received Lie)

You will be matched randomly with another participant. Neither of you will know the identity of the other in the pair.

At the end of the study, 1 OUT OF 5 PAIRS will be chosen at random for payment.

[page break]

On the next page, you will be shown the instructions. You will also be asked to answer three questions to assess your understanding of these instructions. You will not be able to advance until you answer all of these questions correctly.

[page break]

INSTRUCTIONS

Two possible monetary payments are available to you and the other participant. The payments depend on the option chosen by you. We showed the two payment options to the other participant. The only information you will have before making your choice is the message the other participant sent to you.

The other participant could have sent one of the following two messages:

Message 1: “Option A will earn you more money than option B.”
Message 2: “Option B will earn you more money than option A.”

The message the other participant sent is:
Message 1: “Option A will earn you more money than option B”

Now we ask you to choose between option A and option B. Your choice will determine the payments.

The message you have just received is the only information you will have before making your choice.

- Only the other participant knows what the payoffs are under the two options A and B.
- One of the two options earns the other participant more than the other option.
- At the end of the experiment (after all payoffs are determined), you will be informed about which option corresponds to the facts.
- However, you will never know what sums were actually offered to you in the option not chosen.
- Moreover, you will never know the sums offered to the other participant by each of the two options.

In a recent similar experiment with the same payoff structure, 64% of (9 out of every 10) subjects announced a message that corresponds to the facts. The other participant knows that you know these statistics.

It is important that you understand the instructions. To assess your understanding, there are three questions. You must answer them correctly before you advance.

Understanding Question #1: Correct or incorrect? Regardless of the message sent by the other participant, both of you will be paid according to option A
if you choose option A.

Understanding Question #2: Correct or Incorrect? The other participant knows which option earns him or her more money.

Understanding Question #3: In a recent similar experiment with the same payoff structure, what percentage (proportion) of subjects announced a message that corresponds to the facts? \[17\% \ (17 \text{ out of } 100) \ / \ 50\% \ (1 \text{ out of } 2) \ / \ 64\% \ (9 \text{ out of } 10) \ / \ 80\% \ (\text{None of the above})\]

The instructions are reproduced below for your convenience.

The option I choose is:

Option A
Option B

The message the other participant sent is:

**Message 2: “Option B will earn you more money than option A”**

The message that corresponds to the facts:

**Message 1: “Option A will earn you more money than option B”**

The option you chose was: B
Your payoff is: $5

2.5.3 Sender Instructions for Player Y

[Note: The PANAS is completed before these instructions are seen by Players Y. Players Y assigned to the Delay Treatment completed the PANAS either before or after the Anagrams task. Following these instructions, Players Y answered the questions related to Prosocial Concern, then PRV, then PNT, then Social Desirability; or PNT, Prosocial Concern, PRV, and Social Desirability.]

INSTRUCTIONS

Before starting this experiment, we have rolled a 6-sided die, and obtained the outcome 5. The other participant will not be informed that the outcome of the die roll was 5. However, he or she will be told that you have been informed about the outcome of the die roll.

We will ask you to send a message to the other participant regarding the outcome of the die roll. The message will correspond to a number from 1 to 6. You can send one of the following six messages:

- Message 1: “The outcome from the roll of the 6-sided die is 1”
- Message 2: “The outcome from the roll of the 6-sided die is 2”
- Message 3: “The outcome from the roll of the 6-sided die is 3”
- Message 4: “The outcome from the roll of the 6-sided die is 4”
- Message 5: “The outcome from the roll of the 6-sided die is 5”
- Message 6: “The outcome from the roll of the 6-sided die is 6”

Your message will be shown to the other participant, and then he or she will be asked to choose a number between 1 and 6.
Your payoffs will be determined as follows:

- If the other participant chooses the actual outcome of the die roll (5), then you will receive $7 and the other participant will receive $17.
- If the other participant chooses a number different from the actual outcome (anything except 5), then you will receive $17 and the other participant will receive $7.

Note that:

- The only information the other participant will have before making his or her choice is information sent by you in this message.
- Only you will know the monetary payments associated with each choice.
- The other participant will be told that you have been informed about the monetary value connected to each of his or her choices.
- In addition, the other participant has been told that if he or she chooses a number different from the actual outcome, then you will earn more than if he or she chooses a number that corresponds to the actual outcome.
- However, the other participant will never know what sums were actually offered in the option not chosen.
- Moreover, he or she will never know the sums offered to you by each of the different options.
- Since we have not run this experiment before, we will not inform the other participant about what percentage of subjects have announced a message that corresponds to the facts.

We will never inform the other participant that the actual outcome of the die roll was 5.
It is important that you understand the instructions. To assess your understanding, there are three questions. You must answer them correctly before you advance.

Understanding Question #1: Suppose you have sent message 5 to the other participant and he/she chooses the number 5.

What will your payoff be?
What will the other participant’s payoff be?

Understanding Question #2: Suppose you have sent message 3 to the other participant and he/she chooses the number 3.

What will your payoff be?
What will the other participant’s payoff be?

Understanding Question #3: Suppose you have sent message 4 to the other participant and he/she chooses the number 5.

What will your payoff be?
What will the other participant’s payoff be?

The instructions are reproduced below for your convenience.

I will send the following message:

Message 1: “The outcome from the roll of the 6-sided die is 1”
Message 2: “The outcome from the roll of the 6-sided die is 2”
Message 3: “The outcome from the roll of the 6-sided die is 3”
Message 4: “The outcome from the roll of the 6-sided die is 4”
Message 5: “The outcome from the roll of the 6-sided die is 5”
Message 6: “The outcome from the roll of the 6-sided die is 6”

2.5.4 Sample Instructions for Player Z

You will be matched randomly with another participant. Neither of you will know the identity of the other in the pair.

At the end of the study, 1 OUT OF 5 PAIRS will be chosen at random for payment.

[page break]

On the next page, you will be shown the instructions. You will also be asked to answer three questions to assess your understanding of these instructions. You will not be able to advance until you answer all of these questions correctly.

[page break]

INSTRUCTIONS

Before starting this experiment, we have rolled a 6-sided die. We told the other participant the outcome of this die roll.

You will never know the actual outcome of the die roll.

After being informed of the roll of the die, the other participant has sent a message to you. The message corresponds to a number from 1 to 6. The other participant could have sent one of the following six messages:

Message 1: “The outcome from the roll of the 6-sided die is 1”
Message 2: “The outcome from the roll of the 6-sided die is 2”
Message 3: “The outcome from the roll of the 6-sided die is 3”
Message 4: “The outcome from the roll of the 6-sided die is 4”
Message 5: “The outcome from the roll of the 6-sided die is 5”
Message 6: “The outcome from the roll of the 6-sided die is 6”

The message the other participant sent is:

**Message 3: “The outcome from the roll of the 6-sided die is 3”**

Now we ask you to choose a number between 1 and 6. Your choice of a number will determine the payments.

The message you have just received is the only information you will have regarding the roll of the die.

Before sending you the message, the other participant knew that payments will be made according to what choice you make.

If you choose the number that corresponds to the actual outcome of the die roll, both of you will be paid according to option A. If you choose a number different from the actual outcome, you will both be paid according to option B.

- Only the other participant knows what the payoffs are under the two options A and B.
- One of the two options earns the other participant more than the other option.
- If you choose a number different from the actual outcome, the other participant earns more than if you choose a number that corresponds to the actual outcome.
• You will never know what sums were actually offered to you in the option not chosen.

• Moreover, you will never know the sums offered to the other participant by each of the two options.

It is important that you understand the instructions. To assess your understanding, there are three questions. You must answer them correctly before you advance.

Understanding Question #1: Correct or incorrect? Regardless of the message sent by the other participant, both of you will be paid according to option A if you choose a number different from the actual outcome of the die role.

Understanding Question #2: Correct or Incorrect? The other participant knows which option earns him or her more money.

Understanding Question #3: Suppose the other participant has sent a message that corresponds to an outcome that is different from the actual outcome of the die roll and you choose a number that corresponds to the actual outcome of the die roll. Both of you will be paid according to: [Option A / Option B]

The instructions are reproduced below for your convenience.

The number I choose is: [1 / 2 / 3 / 4 / 5 / 6]
2.6 Appendix: Assessment of Prosocial Concern

[Note: Participants were asked to evaluate each point below on a 7-point Likert scale. For the main analysis, to measure prosocial concern, we use participants’ answers regarding the extent to which they believed that announcing the wrong message hurts Player Z, that is, the question marked with [*] below. The scales were not labeled for participants.]

Prosocial Concern:
To what extent do you judge the decision to send something other than “Message 5: The outcome from the roll of the 6-sided die is 5” as:

[1: Very dishonest ... 7: Very honest]
[1: Very manipulative ... 7: Not at all manipulative]
[1: Strongly associated with personal gains ... 7: Not at all associated with personal gains]
[*] [1: Strongly hurting the other participant ... 7: Not at all hurting the other participant]
[1: Not at all acceptable ... 7: Very acceptable]

Alternative to Prosocial Concern:
To what extent do you judge the decision to send “Message 5: The outcome from the roll of the 6-sided die is 5” as:

[1: Very dishonest ... 7: Very honest]
[1: Very manipulative ... 7: Not at all manipulative]
[1: Strongly associated with personal gains ... 7: Not at all associated with personal gains]
[1: Strongly hurting the other participant ... 7: Not at all hurting the other participant]
[1: Not at all acceptable ... 7: Very acceptable]
2.7 Appendix: Protected Values for Truthfulness

[Note: Participants were asked to evaluate each point below on a 7-point Likert scale. The answers were recoded such that higher values correspond to higher protected values. To calculate the two protected values measures, we take the average of the recoded answers. The scales were not labeled for participants. Items appearing in (BLUE) were added in later sessions.]

Protected Values - Reactions to Violations (PRV):

To what extent do you judge the decision to send something other than “Message 5: The outcome from the roll of the 6-sided die is 5” as:

[1: Very immoral ... 7: Very moral]
[1: Not at all praiseworthy ... 7: Very praiseworthy]
[1: Not at all blameworthy ... 7: Very blameworthy]
[1: Not at all outrageous ... 7: Very outrageous]
[1: Not at all acceptable ... 7: Very acceptable]

Protected Values - No Trade (PNT):

Individuals have an opportunity to choose which information they provide to their audience, and they can modify information they obtain before passing this information on to others. Some view such modification as a violation of truthfulness; others regard it as acceptable protection of personal interests. What do you think about the value of truthfulness in such a situation?

Truthfulness is about something...

... that one should not sacrifice, no matter what the (material or other) benefits. [1: Strongly Disagree ... 7: Strongly Agree]

... for which I think it is right to make a cost-benefit analysis. [1: Strongly
Disagree ... 7: Strongly Agree]

... that cannot be measured in monetary terms. [1: Strongly Disagree ... 7: Strongly Agree]

... about which I can be flexible if the situation demands it. [1: Strongly Disagree ... 7: Strongly Agree]

[Note: Following two questions in random order and automatically depending on the message Player Y had received from Player X and what Player Y had done]

I chose to send Message 5 (something other than Message 5) because I received a true (false) message myself. [1: Strongly Disagree ... 7: Strongly Agree]

Recall that the participant who sent you a message sent a message that corresponded to the facts (a message that did not correspond to the facts). To what extent do you judge the decision of that participant as:

[1: Very immoral ... 7: Very moral]
[1: Not at all praiseworthy ... 7: Very praiseworthy]
[1: Not at all blameworthy ... 7: Very blameworthy]
[1: Not at all outrageous ... 7: Very outrageous]
[1: Not at all acceptable ... 7: Very acceptable]

If the other participant who receives your message were to find out whether the message you sent corresponds to the facts, the other participant would feel...
[1: Very Bad ... 7: Very Good]

Imagine that the other participant who receives your message would also send a message to a different participant. If, before sending his/her message, the other participant who receives your message were to find out whether the message you sent corresponds to the facts, your message would make him/her feel...
[1: Less Truthful ... 7: More Truthful]
Table 2.1: Payoff Matrices for Sequential Sender-Receiver Games

<table>
<thead>
<tr>
<th>Sender Action</th>
<th>X-Y Game</th>
<th>Y-Z Game</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sender</td>
<td>Receiver</td>
</tr>
<tr>
<td>Report truth</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>Report lie</td>
<td>6</td>
<td>5</td>
</tr>
</tbody>
</table>

Notes: Payoffs to the sender and receiver when the receiver follows the message in cheap-talk sender-receiver games are reported in US$. Senders in each game send a message to a receiver, who then chooses whether to follow the sender. The receiver’s choice determines the payoffs. The payoffs shown are realized if the receiver follows the sender’s message. If the receiver does not follow the sender’s message, the payoffs in the row of the opposite message are realized. See the text for a summary of the message space and the information players had about payoffs.
### Table 2.2: Summary Statistics for Players Y

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>S.D.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sends truthful message</td>
<td>0.375</td>
<td>0.485</td>
<td>541</td>
</tr>
<tr>
<td>Received a lie</td>
<td>0.323</td>
<td>0.468</td>
<td>541</td>
</tr>
<tr>
<td>Followed message</td>
<td>0.845</td>
<td>0.362</td>
<td>541</td>
</tr>
<tr>
<td>PRV</td>
<td>0</td>
<td>1</td>
<td>541</td>
</tr>
<tr>
<td>PNT</td>
<td>0</td>
<td>1</td>
<td>541</td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td>0.008</td>
<td>1.001</td>
<td>541</td>
</tr>
<tr>
<td>Age</td>
<td>21.144</td>
<td>2.06</td>
<td>541</td>
</tr>
<tr>
<td>Female</td>
<td>0.49</td>
<td>0.5</td>
<td>541</td>
</tr>
<tr>
<td>Economics</td>
<td>0.305</td>
<td>0.461</td>
<td>541</td>
</tr>
<tr>
<td>Positive affect</td>
<td>24.684</td>
<td>8.206</td>
<td>541</td>
</tr>
<tr>
<td>Negative affect</td>
<td>14.815</td>
<td>5.404</td>
<td>541</td>
</tr>
<tr>
<td>Affect ratio</td>
<td>1.783</td>
<td>0.673</td>
<td>541</td>
</tr>
</tbody>
</table>

Notes: This table presents descriptive statistics for the full sample of Players Y. 205 participants were in the Baseline treatment; 185 participants were in the Neutral Norm, No Delay treatment; and 150 participants were in the Delay treatment. Sends truthful message is a binary indicator variable which is equal to 1 when Player Y sent a message to Player Z that represented the true outcome of a die roll and equal to 0 otherwise. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Followed message is a binary indicator variable which is equal to 1 when Player Y followed the message from Player X and equal to 0 otherwise. Protected values - reactions to violations of honesty (PRV) and Protected values - no trade-off (PNT) are proxies for intrinsic costs of lying, described in the text. PRV and PNT are standardized to have means of zero and standard deviations of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity. Age represents age in integer years. Female is a binary indicator variable which is equal to 1 if Player Y is female and equal to 0 otherwise. Economics is a binary indicator variable which is equal 1 if Player Y is an economics or management major and equal to 0 otherwise. Positive/Negative affect represents the positive/negative portion of Player Y’s responses to the Positive and Negative Affect Scale (PANAS). Affect ratio is Positive Affect divided by Negative Affect. The affect variables are standardized to have means of zero and standard deviations of unity.
Table 2.3: Probit Regressions of Player Y Truth-telling in Baseline Treatment

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received a lie</td>
<td>-0.609***</td>
<td>-0.603***</td>
<td>-0.608***</td>
</tr>
<tr>
<td></td>
<td>(-3.16)</td>
<td>(-3.13)</td>
<td>(-3.13)</td>
</tr>
<tr>
<td>PRV</td>
<td>0.227*</td>
<td>0.225**</td>
<td>0.168</td>
</tr>
<tr>
<td></td>
<td>(2.26)</td>
<td>(2.24)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td></td>
<td></td>
<td>0.092</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.84)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.016</td>
<td>-0.016</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.23)</td>
<td>(-0.24)</td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.047</td>
<td>0.047</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.25)</td>
<td>(0.26)</td>
<td></td>
</tr>
<tr>
<td>Economics</td>
<td>-0.010</td>
<td>-0.020</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.05)</td>
<td>(-0.10)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.096</td>
<td>0.210</td>
<td>0.216</td>
</tr>
<tr>
<td></td>
<td>(-0.86)</td>
<td>(0.15)</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Observations</td>
<td>205</td>
<td>205</td>
<td>205</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.0483</td>
<td>0.0489</td>
<td>0.0514</td>
</tr>
<tr>
<td>Wald test statistic ($\chi^2$, p-value)</td>
<td>13.44 (.001)</td>
<td>13.38 (.020)</td>
<td>13.75 (.033)</td>
</tr>
</tbody>
</table>

Robust z-statistics in parentheses

** p<0.01, * p<0.05, * p<0.1

Notes: This table presents probit regressions. The sample comprises participants in the Baseline treatment (Truthfulness Norm, No Delay). The dependent variable, Sends truthful message, is a binary indicator variable which is equal to 1 when Player Y sent a message to Player Z that represented the true outcome of a die roll and equal to 0 otherwise. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Protected values - reactions to violations to honesty (PRV) is a proxy for intrinsic costs of lying, described in the text. PRV is standardized to have a mean of zero and a standard deviation of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
**Table 2.4:** Probit Regressions of Player Y Truth-telling in Baseline Treatment, Individual Heterogeneity

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Received a lie</td>
<td>-0.741***</td>
<td>-0.678***</td>
<td>-0.569***</td>
</tr>
<tr>
<td></td>
<td>(-3.27)</td>
<td>(-3.13)</td>
<td>(-2.84)</td>
</tr>
<tr>
<td>PRV</td>
<td>0.044</td>
<td>(0.33)</td>
<td></td>
</tr>
<tr>
<td>PRV x Received a lie</td>
<td>0.541**</td>
<td>(2.20)</td>
<td></td>
</tr>
<tr>
<td>PNT</td>
<td></td>
<td>0.435***</td>
<td></td>
</tr>
<tr>
<td>PNT x Received a lie</td>
<td>0.185</td>
<td>(0.82)</td>
<td></td>
</tr>
<tr>
<td>Believes lying hurts Z (BLHZ)</td>
<td>0.097</td>
<td>0.142</td>
<td>0.181*</td>
</tr>
<tr>
<td></td>
<td>(0.86)</td>
<td>(1.53)</td>
<td>(1.72)</td>
</tr>
<tr>
<td>BLHZ x Received a lie</td>
<td></td>
<td></td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Age</td>
<td>0.013</td>
<td>-0.006</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(-0.08)</td>
<td>(-0.26)</td>
</tr>
<tr>
<td>Female</td>
<td>0.053</td>
<td>-0.109</td>
<td>0.061</td>
</tr>
<tr>
<td></td>
<td>(0.28)</td>
<td>(-0.55)</td>
<td>(0.33)</td>
</tr>
<tr>
<td>Economics</td>
<td>0.017</td>
<td>-0.025</td>
<td>-0.041</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(-0.12)</td>
<td>(-0.21)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.415</td>
<td>0.113</td>
<td>0.232</td>
</tr>
<tr>
<td></td>
<td>(-0.28)</td>
<td>(0.08)</td>
<td>(0.16)</td>
</tr>
<tr>
<td>Observations</td>
<td>205</td>
<td>205</td>
<td>205</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.0704</td>
<td>0.147</td>
<td>0.0437</td>
</tr>
<tr>
<td>Wald test statistic ($\chi^2$, p-value)</td>
<td>14.76 (.039)</td>
<td>31.14 (&lt;.001)</td>
<td>11.37 (.078)</td>
</tr>
</tbody>
</table>

Robust z-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents probit regressions. The sample comprises participants in the Baseline treatment (Truthfulness Norm, No Delay). The dependent variable, Sends truthful message, is a binary indicator variable which is equal to 1 when Player Y sent a message to Player Z that represented the true outcome of a die roll and equal to 0 otherwise. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Protected values - reactions to violations (PRV) and Protected values - no trade (PNT) are proxies for intrinsic costs of lying, described in the text. PRV and PNT are standardized to have means of zero and standard deviations of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
Table 2.5: Probit Regressions of Player Y Truth-telling in Baseline Treatment, Heterogeneity Across Norms

<table>
<thead>
<tr>
<th></th>
<th>Neutral Norm</th>
<th>Both Norms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Received a lie</td>
<td>0.153</td>
<td>0.144</td>
</tr>
<tr>
<td></td>
<td>(0.71)</td>
<td>(0.65)</td>
</tr>
<tr>
<td>Truthfulness Norm</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Truthfulness Norm x Received a lie</td>
<td>0.824***</td>
<td>(-2.90)</td>
</tr>
<tr>
<td>PRV</td>
<td>0.526***</td>
<td>0.474***</td>
</tr>
<tr>
<td></td>
<td>(4.10)</td>
<td>(3.36)</td>
</tr>
<tr>
<td>PRV x Received a lie</td>
<td>0.182</td>
<td>(0.70)</td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td>-0.006</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(-0.05)</td>
<td>(-0.09)</td>
</tr>
<tr>
<td>Believes lying hurts Z x Received a lie</td>
<td>-0.002</td>
<td>(-0.01)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.048</td>
<td>-0.048</td>
</tr>
<tr>
<td></td>
<td>(-0.88)</td>
<td>(-0.88)</td>
</tr>
<tr>
<td>Female</td>
<td>-0.020</td>
<td>-0.022</td>
</tr>
<tr>
<td></td>
<td>(-0.10)</td>
<td>(-0.11)</td>
</tr>
<tr>
<td>Economics</td>
<td>-0.133</td>
<td>-0.123</td>
</tr>
<tr>
<td></td>
<td>(-0.61)</td>
<td>(-0.56)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.832</td>
<td>0.823</td>
</tr>
<tr>
<td></td>
<td>(0.69)</td>
<td>(0.70)</td>
</tr>
<tr>
<td>Observations</td>
<td>185</td>
<td>185</td>
</tr>
<tr>
<td>Robust z-statistics in parentheses</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wald test statistic ($\chi^2$, p-value)</td>
<td>27.62 (.&lt;.001)</td>
<td>26.73 (.&lt;.001)</td>
</tr>
</tbody>
</table>

Notes: This table presents probit regressions. In columns (1)-(3) the sample comprises participants in the No Delay treatment for only the Neutral Norm. In column (4), the sample comprises participants in the No Delay treatment for both the Truthfulness Norm and the Neutral Norm. The dependent variable, Sends truthful message, is a binary indicator variable which is equal to 1 when Player Y sent a message to Player Z that represented the true outcome of a die roll and equal to 0 otherwise. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Protected values - reactions to violations (PRV) is a proxy for intrinsic costs of lying, described in the text. PRV is standardized to have a mean of zero and a standard deviation of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
## Table 2.6: Probit Regressions of Player Y Truth-telling, Delay

<table>
<thead>
<tr>
<th></th>
<th>Truthfulness Norm</th>
<th>Neutral Norm</th>
<th>Neutral Norm</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Received a lie</td>
<td>-0.304 (-0.96)</td>
<td>-0.348 (-1.09)</td>
<td>-0.349 (-1.09)</td>
</tr>
<tr>
<td>PRV</td>
<td>0.392*** (2.64)</td>
<td>0.393*** (2.60)</td>
<td>0.399* (1.95)</td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td>-0.009 (-0.05)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age</td>
<td>-0.062 (-0.68)</td>
<td>-0.062 (-0.68)</td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.031 (0.11)</td>
<td>0.030 (0.11)</td>
<td></td>
</tr>
<tr>
<td>Economics</td>
<td>0.138 (0.44)</td>
<td>0.140 (0.44)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.247 (-1.49)</td>
<td>1.013 (0.52)</td>
<td>1.005 (0.52)</td>
</tr>
<tr>
<td>Observations</td>
<td>87</td>
<td>87</td>
<td>87</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.0612</td>
<td>0.0661</td>
<td>0.0661</td>
</tr>
<tr>
<td>Wald test statistic ($\chi^2$, p-value)</td>
<td>7.63 (.022)</td>
<td>8.20 (.146)</td>
<td>8.30 (.217)</td>
</tr>
</tbody>
</table>

Robust z-statistics in parentheses

** * ** p < 0.01, ** p < 0.05, * p < 0.1

Notes: This table presents probit regressions. The sample comprises Players Y in the Delay treatment. The dependent variable, Sends truthful message, is a binary indicator variable which is equal to 1 when Player Y sent a message to Player Z that represented the true outcome of a die roll and equal to 0 otherwise. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Protected values - reactions to violations (PRV) is a proxy for intrinsic costs of lying, described in the text. PRV is standardized to have a mean of zero and a standard deviation of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
Table 2.7: OLS Regressions of Player Y Affect Among Followers

<table>
<thead>
<tr>
<th></th>
<th>Both Norms</th>
<th>Truthfulness Norm No Delay</th>
<th>Neutral Norm No Delay</th>
<th>Truthfulness Norm Delay</th>
<th>Pre</th>
<th>Post</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Received a lie</td>
<td>-0.232*</td>
<td>0.118</td>
<td>-0.384***</td>
<td>-0.308**</td>
<td>-0.161</td>
<td>-0.264*</td>
</tr>
<tr>
<td></td>
<td>( -2.02)</td>
<td>( 1.09)</td>
<td>( -3.24)</td>
<td>( -2.24)</td>
<td>( -0.84)</td>
<td>(-1.97)</td>
</tr>
<tr>
<td>PRV</td>
<td>-0.062</td>
<td>0.011</td>
<td>-0.081</td>
<td>-0.088</td>
<td>-0.028</td>
<td>-0.076</td>
</tr>
<tr>
<td></td>
<td>( -0.87)</td>
<td>( 0.21)</td>
<td>( -1.16)</td>
<td>( -0.95)</td>
<td>( -0.25)</td>
<td>(-0.83)</td>
</tr>
<tr>
<td>Truthfulness Norm</td>
<td>0.111</td>
<td>0.077</td>
<td>0.093</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>( 1.01)</td>
<td>( 0.73)</td>
<td>( 0.81)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td>-0.049</td>
<td>0.003</td>
<td>0.001</td>
<td>-0.001</td>
<td>-0.102</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>( -0.75)</td>
<td>( 0.04)</td>
<td>( 0.02)</td>
<td>( -0.01)</td>
<td>( -1.01)</td>
<td>(-0.04)</td>
</tr>
<tr>
<td>Age</td>
<td>0.025</td>
<td>0.047</td>
<td>-0.012</td>
<td>0.008</td>
<td>0.049</td>
<td>0.019</td>
</tr>
<tr>
<td></td>
<td>( 0.76)</td>
<td>( 1.17)</td>
<td>( -0.35)</td>
<td>( 0.15)</td>
<td>( 1.21)</td>
<td>( 0.39)</td>
</tr>
<tr>
<td>Female</td>
<td>-0.344***</td>
<td>-0.011</td>
<td>-0.250**</td>
<td>-0.476***</td>
<td>-0.179</td>
<td>-0.398***</td>
</tr>
<tr>
<td></td>
<td>( -3.16)</td>
<td>( -0.10)</td>
<td>( -2.06)</td>
<td>( -3.60)</td>
<td>( -1.00)</td>
<td>(-3.17)</td>
</tr>
<tr>
<td>Economics</td>
<td>0.099</td>
<td>0.158</td>
<td>-0.004</td>
<td>-0.043</td>
<td>0.321</td>
<td>0.126</td>
</tr>
<tr>
<td></td>
<td>( 0.83)</td>
<td>( 1.24)</td>
<td>( -0.03)</td>
<td>( -0.31)</td>
<td>( 1.53)</td>
<td>( 0.95)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.322</td>
<td>-1.135</td>
<td>0.523</td>
<td>0.279</td>
<td>-0.998</td>
<td>-0.033</td>
</tr>
<tr>
<td></td>
<td>( -0.46)</td>
<td>( -1.33)</td>
<td>( 0.67)</td>
<td>( 0.25)</td>
<td>( -1.09)</td>
<td>(-0.03)</td>
</tr>
<tr>
<td>Observations</td>
<td>331</td>
<td>331</td>
<td>331</td>
<td>179</td>
<td>152</td>
<td>219</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.065</td>
<td>0.015</td>
<td>0.055</td>
<td>0.109</td>
<td>0.056</td>
<td>0.075</td>
</tr>
</tbody>
</table>

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents OLS regressions. The sample comprises Players Y who followed the messages sent by Players X. 331 Players Y followed Players X in the No Delay treatment, of which 179 were assigned the Truthfulness Norm and 152 were assigned the Neutral Norm. 250 Players Y followed Players X in the Delay treatment, of which 219 completed the PANAS before the filler task and 31 completed it after the filler task. The dependent variables across the columns are Positive Affect, Negative Affect, and Affect Ratio (Positive divided by Negative Affect)-all of which are standardized to have a mean of zero and a standard deviation of unity. Received a lie is a binary indicator variable which is equal to 1 when Player Y received a dishonest message from Player X and equal to 0 otherwise. Protected values - reactions to violations (PRV) is a proxy for intrinsic costs of lying, described in the text. PRV is standardized to have a mean of zero and a standard deviation of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
Table 2.8: Ordered Probit Regressions of Player Y’s Beliefs About Emotional Consequences of Actions

<table>
<thead>
<tr>
<th></th>
<th>Truth Makes Z Feel Good</th>
<th>Truth Induces Z’s Truthfulness</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Sent truthful message</td>
<td>0.849***</td>
<td>0.789***</td>
</tr>
<tr>
<td></td>
<td>(5.78)</td>
<td>(5.27)</td>
</tr>
<tr>
<td>PRV</td>
<td>0.231***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.38)</td>
<td></td>
</tr>
<tr>
<td>Believes lying hurts Z</td>
<td></td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-0.21)</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.16)</td>
</tr>
<tr>
<td>Female</td>
<td></td>
<td>-0.254*</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-1.85)</td>
</tr>
<tr>
<td>Economics</td>
<td></td>
<td>0.239</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.36)</td>
</tr>
<tr>
<td>Observations</td>
<td>235</td>
<td>235</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.040</td>
<td>0.056</td>
</tr>
<tr>
<td>Wald test statistic ($\chi^2$, p-value)</td>
<td>33.37 (&lt;.001)</td>
<td>53.52 (&lt;.001)</td>
</tr>
</tbody>
</table>

Robust z-statistics in parentheses

Notes: This table presents ordered probit regressions. The sample includes participants in the No Delay treatment (only later sessions including these variables). The dependent variables are whether Players Y believe Players Z will feel good about receiving a true message or whether Players Y believe Players Z would pay forward a truthful message. Protected values - reactions to violations (PRV) is a proxy for intrinsic costs of lying, described in the text. PRV is standardized to have a mean of zero and a standard deviation of unity. Believes lying hurts Z represents Player Y’s response to the question about how much sending a false message hurts Player Z on a 7-point Likert scale, standardized to have a mean of zero and a standard deviation of unity.
Player X / Player Y play sender-receiver game

Player Y learns whether Player X’s message was true or false

Player Y completes PANAS

Player Y / Player Z play sender-receiver game

Attitudes and demographic questions

Figure 2.1: Design Overview for the Baseline Treatment
Figure 2.2: Truth-telling Rates in the Baseline Treatment
Figure 2.3: Truth-telling Rates in the Baseline Treatment, by PRV
Figure 2.4: Positive Affect in No Delay Treatment Among Followers
Chapter 3

Promoting Truthful Communication Through Ex-post Disclosure
3.1 Introduction

Truthful communication is an essential component of economic and political activity. When truthful communication fails, often severe negative economic and legal consequences ensue. Despite the existence of strong norms and laws that promote ethical behavior, economists and legal scholars have devoted a great deal of attention to understanding the incentives for dishonest acts, such as tax fraud (Andreoni et al. 1998), perjury (Wolfram 1977), lack of disclosure of conflicts of interest (Mehran and Stulz 2007), and governmental corruption (Treisman 2000). Such dishonest acts undermine efficiency in market settings. For example, financial advice to clients is often biased, resulting in substantially lower investment returns (Bergstresser et al. 2009; Hackethal et al. 2012). Similarly, car mechanics have been shown to provide unnecessary services to customers (Beck et al. 2014). Dishonesty has also extended into the medical field, in which health care professionals have financial conflicts of interest (Dana and Loewenstein 2003; Engelberg et al. 2013). So, what motivates people to communicate truthfully when it is in their self-interests to lie?

Incentives like punishment of bad behavior and rewards for good behavior can be an effective way to promote truthful communication. Beginning with the seminal work of Becker (1968), there is a vast literature in law and economics on the economic incentives for ethical behavior (Abbink et al. (2002); Charness et al. 2013; DeAngelo and Charness 2012; Engel et al. 2015; Fisman and Miguel 2007; Khadjavi 2015a; Witte 1980). Yet, non-financial incentives might also promote honest communication. We hypothesize that truthful communication can be increased through ex-post disclosure, even if disclosure has no monetary or reputational consequences. We propose that anticipation of negative emotions from disclosed dishonesty can have powerful effects on reducing deception. For example, a financial advisor who provides advice could anticipate negative emotions, like shame, when facing clients who discover the advice was dishonest. Ex-post disclosure is crucial, but often absent in the world of finance and beyond.

One possible channel through which ex-post disclosure could decrease deception is through shame aversion. Colloquially, feeling shame (or ashamed) has
a number of different meanings. Yet in the psychology literature, shame is a widely studied emotion encompassing concern about others’ (negative) perceptions of the self. As noted by (Lewis, 1971, p. 39), “shame may be experienced in private or it may be evoked by an actual encounter with a specific or ill-defined ‘other’.” Our paper thereby builds on a recent stream of literature in economics that explains prosocial and antisocial behavior with emotion-based motives (Cappelen et al. 2013; Hopfensitz and Reuben 2009; Khadjavi 2015a; Khadjavi 2015b; Miettinen and Suetens 2008; Reuben and Van Winden 2010; Tadelis 2011).

The advantage of using a controlled experiment is that we can rule out reputation concerns by creating a setting in which individuals are fully anonymous. Moreover, we have control over the relevant information available to individuals, which is impossible in the field. To test for the effect of ex-post disclosure on truthful communication, we present results of a laboratory experiment, which isolates the effect of ex-post disclosure.

We study a cheap-talk sender-receiver game in which the payoffs are conflicting (similar to Gneezy (2005)). The sender accurately or inaccurately reports the state of the world (a six-sided die roll) to the receiver. Importantly, the sender has a financial incentive to lie. In the basic game, the receiver neither detects whether the sender lies nor learns about the possible payoffs in the game. Our experiment identifies the effect of ex-post disclosure by introducing a treatment in which the sender and the receiver know that the receiver will be informed about the actual outcome of the die roll after all decisions will have been made. The receiver can thus detect whether the sender lied to her, which could evoke shame for a deceiving sender.

Our results show that senders send the truthful message approximately twice as often in the treatment in which receivers can detect whether the senders lied compared to the treatment in which they cannot. This is particularly interesting, as the ex-post disclosure of the actual outcome has no payoff consequences for either player.

These results fit into a wider literature which documents that many people have a preference for truth-telling at the expense of their own monetary payoffs (Fischbacher and Föllmi-Heusi 2013, 2013; Gibson et al. 2013; Gneezy 2005; Mazar
et al. 2008). Our results on ex-post disclosure are consistent with psychological game theory (Geanakoplos et al. 1989) in the sense that individuals anticipate negative emotions from being detected.

Previous evidence on the potential role of ex-post disclosure in the promotion of prosocial behavior is mixed. In dictator-game and trust-game settings, giving is largely driven by a desire to appear fair (Cappelen et al. 2013; Dana et al. 2006; Dana et al. 2007; Tadelis 2011). A few papers have examined the effect of image motivation on truthful communication. Behnk et al. (2014) obtain a puzzling finding that ex-post exposure of deception is more effective when it occurs with 50% probability than with certainty. While our study differs in several ways, a chief difference is that we use a between-subjects design rather than a within-subjects design, which provides a more conservative and robust test (Charness et al. 2012). Van de Ven and Villeval (2015) find that revealing the sender’s identity to a third party has no effect on dishonesty. In our study, the identity is revealed to the receiver who is hurt by the lie, rather than to a third party. Therefore, the effect of interest in our study is that of disclosure for the relevant party involved. Because senders might believe that receivers would expect to earn more in a setting with ex-post disclosure, we anchor senders’ and receivers’ expectations about receivers’ payoffs. Therefore, this design feature aims to alleviate concerns that senders are more truthful in settings with ex-post disclosure simply to avoid letting down receivers’ expectations about payoffs (e.g., Battigalli and Dufwenberg 2007).

Several studies have focused on the effects of social image by revealing the identities of participants (e.g., Andreoni and Bernheim 2009; Ariely et al. 2009). They find that individuals behave more prosocially when their actions are public. We show that image concerns are so deeply rooted in human behavior that they even reduce deception when individuals are fully anonymous.

---

1Other relevant papers on image motivation and social pressure include Bateson et al. (2006); Bénabou and Tirole (2006); Ellingsen and Johannesson (2008) Glazer and Konrad (1996); Ismayilov and Potters (2013); and Jiang (2013).
3.2 Experiment

3.2.1 The Modified Deception Game

To test for the influence of ex-post disclosure on truth-telling, we use a modified version of the cheap-talk sender-receiver game (similar to Gneezy (2005)). At the beginning of the experiment, each subject is randomly assigned one of two roles: “sender” or “receiver.” The sender and receiver play an anonymous one-shot game.

Before the start of the game, we communicate the outcome of a six-sided die roll to the sender, but not to the receiver. Then, we ask the sender to communicate two messages to the receiver. The six possible messages are: “The outcome from the roll of the die is $i$,“ where $i \in \{1, 2, 3, 4, 5, 6\}$. The sender is free to send two identical or two different messages and can send the true number to the receiver or a wrong number. A screenshot of the choices faced by the sender appears in the appendix.

To investigate the effect of monetary incentives on deception, we introduce two sets of payoff states. Both payoff states can occur with equal probability. We tell senders that both players are paid according to payoff Options $A^H$ and $B^H$ if nature decided on the “High” state, or that they are paid according to payoff Options $A^L$ and $B^L$ if nature decided on the “Low” state, with each state occurring with 50% probability. The receiver does not know that the sender is making multiple choices using the strategy method (see Brandts and Charness (2011) and Selten (1967)). Table 3.1 summarizes these possible payoffs.

Because the sender does not know ex ante which state will be realized, she chooses a message for each state using the strategy method. Then nature draws one of the states with 50% probability each and the receiver gets the message chosen by the sender for the relevant state.

After the receiver observes the one message that is randomly determined to become payoff-relevant, she is asked to choose a number between 1 and 6. If the receiver chooses the actual outcome of the die roll, payoff Option A will be implemented; otherwise, payoff Option B will be implemented. That is, in the case
of the High state, either payoff Option $A^H$ or $B^H$ will be implemented while in the case of the Low state, either payoff Option $A^L$ or $B^L$ will be implemented. Only the sender knows the payoffs associated with each option, and this is common knowledge.

In summary, except for the fact that receivers do not know that senders make two choices, the rules of the game are common knowledge. Senders are informed about the actual die roll and the possible payoffs in the game; receivers are not. Receivers only observe the payoff-relevant message sent by senders and their own payoffs.

We refer to the game just described as the Baseline game. We will build on this game by adding ex-post disclosure of the outcome.

### 3.2.2 Ex-post Disclosure

We introduce two treatments: the Disclosure treatment and the No Disclosure treatment. The only difference between the treatments is that in the Disclosure treatment, we inform all players at the beginning of the experiment that receivers will learn the actual outcome of the die roll after all choices will have been made, which is not the case in the No Disclosure treatment. In both treatments, we anchor the beliefs of both senders and receivers about receivers’ payoff expectations (see the following section for details).

### 3.2.3 Senders’ Beliefs

Previous work has shown that deception is influenced by beliefs (Battigalli et al. 2013; Battigalli and Dufwenberg 2007; Peeters et al. 2015). This account would suggest that any effect of ex-post disclosure on truthful communication is driven, in part, by the fact that senders are attempting to fulfill receivers’ payoff expectations. In theory, senders might act on their beliefs about what receivers believe they will earn. To help alleviate concerns about this alternative account, we anchor senders’ and receivers’ beliefs in both the Disclosure treatment and No Disclosure treatment on a focal average payoff that receivers should expect.
Specifically, we inform both senders and receivers that receivers in a similar previous experiment earned, on average, approximately €10. The amount we communicate is the only source of information participants receive about the payoffs receivers can expect in the game. This anchor contains a strong norm so that a reference-based account of our hypothesized result is largely ruled out in the High payoff state.

For the Baseline, Disclosure treatment, and No Disclosure treatment, we elicited senders’ beliefs about the fraction of receivers that will follow the messages they receive. If the sender correctly guesses the number, she receives an additional €2. If her guess deviates from the actual number by 1, she receives €1 in addition; larger deviations are not rewarded.

### 3.2.4 Procedure

All sessions were run at the University of Cologne in June 2013 with z-Tree (Fischbacher 2007). Senders and receivers were paired anonymously and seated in separate rooms. We recruited 234 student participants (59% female) from a large subject pool. Subjects earned on average €12.74 (approximately $16.68 given conversion rates at the time of the experiment) including earnings from the belief elicitation task. There was no show-up fee. The experiment took about 45 minutes.

### 3.3 Results

Figure 3.1 summarizes our main findings. A tabular form can be found in the appendix. We identify the effect of ex-post disclosure by comparing the truth-telling rate in the Disclosure treatment and the No Disclosure treatment.

Remarkably, in the High stakes choice, 42.5% of participants send a truthful message when the true outcome will be disclosed while 22.5% of participants tell

---

2The actual earnings for receivers in the Baseline were €9.1, which is €10 when rounded up to a whole euro. This is in line with the experimental instructions. The Baseline was conducted before the Disclosure treatment and the No Disclosure treatment.
the truth when the outcome will not be disclosed (Mann-Whitney test, $p = .058$).

**Result 3.1** *Ex-post disclosure significantly promotes truthful communication.*

In the Low stakes choice, 55% of participants send a truthful message if the outcome of the die roll will be revealed while 30% of participants send a truthful message when the outcome will not be revealed (Mann-Whitney test, $p = .025$).

**Result 3.2** *The effect of ex-post disclosure on truthful communication is largely independent of stake size.*

One alternative explanation for the effects of disclosure is that senders expect receivers not to follow their messages. Sutter (2009) demonstrates that individuals can also “lie” by telling the truth if they expect others not to follow their message. The higher truth-telling we observe in the Disclosure treatment than in the No Disclosure treatment could be due to the fact that senders in this treatment believe that receivers are less likely to follow their messages. However, the senders’ beliefs are almost identical across the different conditions. A Kruskal-Wallis test confirms that there are no significant differences in beliefs between the Baseline, the Disclosure treatment, and the No Disclosure treatment ($p = .729$).³ Note that senders believe receivers will follow their messages 58%, 59% and 62% of the time in the Baseline, the Disclosure treatment and the No Disclosure treatment, respectively. Thus, our results on disclosure cannot be explained by differences in beliefs about following rates.⁴

We next investigate the effect of ex-post disclosure on deception when controlling for beliefs about following rates. Table 3.2 shows a linear probability model⁵ for the High stakes choice in which the dependent variable takes a value of 1 if the subject told the truth and 0 otherwise. Columns (1) and (2) show

---

³Binary comparisons of beliefs (using Mann-Whitney tests) between pairs of the three treatments yield similar results (each $p > .49$).

⁴Note that the inclusion of the anchor does not affect truth-telling rates. Senders’ choices in the Baseline and the No Disclosure treatment do not significantly differ (Mann-Whitney test, High: $p = .473$; Low: $p = .980$).

⁵A probit model yields substantively similar results.
that the effect of disclosure is robust to controlling for the senders’ beliefs about the likelihood that receivers will follow their messages. Ex-post disclosure significantly increases truth-telling by 20 percentage points ($p = .057$) and 19 percentage points ($p = .062$), respectively, for the specification without and with controlling for beliefs about following rates.

Table 3.3 documents analogous results for the Low stakes choice. Similar to the High stakes choice, the effect of ex-post disclosure on truth-telling persists when controlling for senders’ beliefs. Ex-post disclosure significantly increases truth-telling by 24-25 percentage points ($p = .024$, and $p = .026$).

Senders who believe that the chance the receivers will follow their messages is larger are significantly more likely to tell the truth. Specifically, in the High stakes choice, a sender who expects that all receivers will follow is 49 percent more likely to tell the truth than a sender who expects that no receivers will follow ($p = .005$). Similarly, she is 44 percent more likely to tell the truth in the Low stakes choice ($p = .021$). This highlights that senders do not tell the truth because they think that the receiver will not follow the message, which Sutter (2009) calls “lying by telling the truth.” If senders were, in fact, lying by telling the truth, we would find that believing that more receivers will follow decreases rather than increase truthful communication.

Figure 3.2 also shows strong gender differences in the effect of ex-post disclosure.6 Whereas male subjects are substantially more honest with ex-post disclosure, female subjects are unaffected by ex-post disclosure. The linear probability models confirm this pattern. In the Low choice, female senders do not respond significantly to ex-post disclosure. The interaction effect of -0.55 and -0.58 in specification three and four of Table 3.3 respectively are large and statistically significant ($p = .011$ and $p = .005$). In the High choice (Table 3.2), the interaction effect is also negative, but not significant ($p = .215$ and $p = .146$).

The finding that males are more sensitive than females to ex-post disclosure is particularly interesting in light of previous findings. Dreber and Johannesson (2008) find that males lie more than females overall in a deception game. Croson

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6Among senders we have 51% females in the Baseline, 50% females in the No Disclosure treatment, and 47.5% females in the Disclosure treatment.
and Gneezy (2009) report that the prosocial behavior of females appears to be more situation-specific than that of males, yet our results indicate that this may not hold in cases regarding deception.

3.4 Conclusion

This paper uses an experiment to show that ex-post disclosure increases truthful communication. Our experiment is fully anonymous and thereby rules out reputation concerns, which are often present in the field. Our results illustrate that non-monetary incentives can decrease deception even without direct social image concerns. In our study, truthful communication of males appears to be more sensitive to ex-post disclosure than that of females.

The strength of the effects of ex-post disclosure might be highly situation-dependent. It could be stronger in the field, where reputation is at stake. Research Papers in Economics (RePEc) maintains a website in which it exposes acts of plagiarism in the field of economics. Diederik Stapel, a famous social psychologist in the Netherlands, was exposed as a fraud when his colleagues and students discovered he had spent his career fabricating data in many of his well-published journal articles, ultimately bringing him infamy in the international press. His academic dishonesty had profound effects on the scientific community, and more direct negative effects on his collaborators, many of whom were his graduate students whose papers have now been debunked. In his autobiographic book, he admits that the worst part of the experience was the shame he felt for himself and his family (Stapel 2012). Moreover, low-cost communication on social media platforms is already widely used for shaming individuals who do “wrong.” Future research could investigate the impact of ex-post disclosure on deception in the field.

Previous research has found that people have a preference for keeping promises (e.g., Charness and Dufwenberg 2006; Sánchez-Pagés and Vorsatz 2007; Serra-Garcia et al. 2013; Vanberg 2008). We show that ex-post disclosure promotes truthful communication even without an explicit promise. Future research could investigate whether the effects of ex-post disclosure are stronger if someone does
not live up to a promise.

Chapter 3, in full, has been submitted for publication of the material. Greenberg, Adam Eric; Smeets, Paul; Zhurakhovska, Lilia.
### Appendix: Table of Truth-telling Rates by Treatment and Gender

<table>
<thead>
<tr>
<th>Disclosure</th>
<th>All</th>
<th>Male</th>
<th>Female</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>42.5%</td>
<td>42.8%</td>
<td>42.1%</td>
</tr>
<tr>
<td>Low</td>
<td>55.5%</td>
<td>71.4%</td>
<td>36.8%</td>
</tr>
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<td>Observations</td>
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<th>Female</th>
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<td>High</td>
<td>22.5%</td>
<td>10.0%</td>
<td>35.0%</td>
</tr>
<tr>
<td>Low</td>
<td>30.0%</td>
<td>20.0%</td>
<td>40.0%</td>
</tr>
<tr>
<td>Observations</td>
<td>40</td>
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<table>
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<th>Baseline</th>
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<th>Female</th>
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<tbody>
<tr>
<td>High</td>
<td>29.7%</td>
<td>33.3%</td>
<td>26.3%</td>
</tr>
<tr>
<td>Low</td>
<td>29.7%</td>
<td>27.7%</td>
<td>31.5%</td>
</tr>
<tr>
<td>Observations</td>
<td>37</td>
<td>18</td>
<td>19</td>
</tr>
</tbody>
</table>
3.6 Appendix: Experimental Protocol

Note: Items appearing in (BLACK) are seen by participants in the Baseline, the No Disclosure treatment, and the Disclosure treatment. Items appearing in (BLUE) are from the No Disclosure treatment and the Disclosure treatment. Items appearing in (RED) are from the Disclosure treatment.

3.6.1 Instructions for Senders

Welcome to this economics experiment!

This experiment is anonymous. Nobody will find out with which other participant he or she interacts. The experiment is not repeated–every decision in this experiment is made only once. The experiment analysis is also conducted anonymously. The money earned by you in this experiment is paid to you in cash at the end of the experiment. Please read these instructions carefully.

In this experiment, you are the sender of two messages. A receiver is assigned to you at random.

A computer version of a six-sided die is about to roll a number. This number will not be the same for all pairs of participants. The result of the roll of the die will be told to you right away on your screen. The receiver is not informed during the experiment what the result of the die roll is. However, he or she is indeed informed that you have been informed about the result of the die roll.

We would now ask you please to decide for each of the two payoff cases, described on the following page, which message–with regard to the die roll–you wish to send to the receiver. In each case, you can send one of the following six messages:

Message 1: “The result of the roll of the six-sided die is 1”
Message 2: “The result of the roll of the six-sided die is 2”
Message 3: “The result of the roll of the six-sided die is 3”
Message 4: “The result of the roll of the six-sided die is 4”
Message 5: “The result of the roll of the six-sided die is 5”
Message 6: “The result of the roll of the six-sided die is 6”

Your payoffs are determined in part by the choice of receiver and partly by a draw. A computer version of a coin toss will determine whether you and the receiver will be paid according to “payoff case 1” or “payoff case 2.” You and the receiver may be paid according to “payoff case 1” with a chance of 50% and according to “payoff case 2” with a chance of 50%. The payoff cases are distinguished as follows:

Payoff case 1:
- If the receiver chooses the actual result of the die roll, then you receive €10 and the receiver gets €10.
- If the receiver chooses a result different from that of the die roll, then you receive €15 and the receiver gets €5.

Payoff case 2:
- If the receiver chooses the actual result of the die roll, then you receive €15 and the receiver gets €15.
- If the receiver chooses a result different from that of the die roll, then you receive €20 and the receiver gets €10.

The coin toss determines whether “payoff case 1” or “payoff case 2” becomes payoff-relevant, before your message to the receiver is transmitted. This means the receiver only receives the one message that you send for the payoff-relevant “payoff case.” Following this, he or she chooses a number between 1 and 6. In doing so, the receiver can either choose the number you sent him or her, or else choose another number between 1 and 6.
Only you are informed of the payoff figures described above and associated with the receiver’s respective number choice. The receiver is not informed of these payoff figures. However, he or she is informed that you are informed of the payoff figures associated with his/her number choice. In addition, once the experiment is over, he or she is informed of the actual result of the die roll (once all decisions in the experiment have been made and all payoffs have been determined, as described above).

Only at the end of the experiment will you find out whether the payoff was determined by “payoff case 1” or “payoff case 2.” Please decide on the message you wish to send in case the payoffs are determined by “payoff case 1.” And please decide on the message you wish to send in case the payoffs are determined by “payoff case 2.” Please make your choices for both cases carefully, since each of the two cases could be payoff-relevant both for you and for the receiver.

Please note that a group of participants in this laboratory recently took part in a decision task with an identical set of rules to the one presented to you here (i.e., the same choice possibilities for the participants, the same participant allocation, and the same respective payoff cases). The participants in the receiver role earned on average approximately €10 (rounded to full euro figures). Please note further that the receiver was also informed that participants recently earned on average €10 in the receiver role.

Here is a summary of the experiment proceedings:

1. A die roll determines a number (1, 2, 3, 4, 5, or 6).

2. You are informed (in the instructions distributed here) of the payoff figures associated with the respective choice of a number by the receiver.

3. You are informed about the die roll.

4. You send a message, for each of the two payoff cases, on the number determined by the die roll (1, 2, 3, 4, 5, or 6).
5. The coin toss determines whether “payoff case 1” or “payoff case 2” becomes payoff-relevant.

6. The receiver gets your message on the payoff-relevant payoff case.

7. The receiver chooses a number (1, 2, 3, 4, 5, or 6) that determines your payoff and the receiver’s payoff.

8. The receiver is informed about the number determined by the die roll (1, 2, 3, 4, 5, or 6).
3.6.2 Decision Screen for Senders

The result of the die throw is: xy.

Please send your message to the receiver now. In case...

- the coin toss determines that the payoffs will be paid in accordance with "payoff case 1", I wish to send the following message:
  
The result of the throw of the six-sided die is:

- the coin toss determines that the payoffs will be paid in accordance with "payoff case 2", I wish to send the following message:
  
The result of the throw of the six-sided die is:
3.6.3 Belief Elicitation for Senders

We have an additional question for the experiment. Your answer to this question will never be passed on to the receiver.

Today, [number auto-populated] participants in the receiver role took part in the experiment. Please estimate how many participants in this role, when choosing the number, decided to follow the message they received from the participant in your role. If your estimate is correct, then you will earn an extra €2, on top of your current earnings. If your estimate deviates by ± 1, you will earn an extra €1. It is therefore in your own interest to give your actual estimate.

Please give your estimate here:

3.6.4 Instructions for Receivers

Welcome to this economics experiment!

This experiment is anonymous. Nobody will find out with which other participant he or she interacts. The experiment is not repeated–every decision in this experiment is made only once. The experiment analysis is also conducted anonymously. The money earned by you in this experiment is paid to you in cash at the end of the experiment. Please read these instructions carefully.

In this experiment, you are the receiver of a message. You are randomly assigned a sender.

At the beginning of the experiment, a computer version of a six-sided die rolled a number. This number was not the same for all pairs of participants. You are not informed during the experiment what the result of the roll of the die is. Once the experiment is over (once all decisions in the experiment have been made by all participants), you are informed of the actual result of the die roll. The sender knows that you are informed of the actual result of the die roll at
the end of the experiment. The sender was told the result of the die roll at the beginning of the experiment (before he or she made any decisions in the experiment).

After the sender has been informed about the die number that has been thrown, he or she sent you a message. The sender was able to choose from the following messages:

Message 1: “The result of the roll of the six-sided die is 1”
Message 2: “The result of the roll of the six-sided die is 2”
Message 3: “The result of the roll of the six-sided die is 3”
Message 4: “The result of the roll of the six-sided die is 4”
Message 5: “The result of the roll of the six-sided die is 5”
Message 6: “The result of the roll of the six-sided die is 6”

The message the sender has sent to you will be shown to you on your screen in a few moments.

We would ask you please to choose a number between 1 and 6. The message you receive is the only information you receive, in the course of the experiment, about the result of the die roll. Your number choice determines both your own payoff and the sender’s payoff.

If you choose the number that corresponds to the actual result of the die roll, both of you will be paid according to Option A. If you choose a number that does not correspond to the actual result of the die roll, both of you will be paid according to Option B. These payoff options are known only to the sender.

Please note that a group of participants in this laboratory recently took part in a decision task with an identical set of rules to the one presented here to you (i.e., the same choice possibilities for the participants, the same participant allocation and the same respective payoff cases). The participants in your role earned on average approximately €10 (rounded to full euro figures). Please note
further that the sender was also informed that participants recently earned on average €10 in your role.

1. A die roll determines a number (1, 2, 3, 4, 5, or 6).

2. The sender is informed of the payoff options associated with your respective choice of a number.

3. The sender is informed about the die roll.

4. The sender sends you a message about the number determined by the die roll (1, 2, 3, 4, 5, or 6).

5. You choose a number (1, 2, 3, 4, 5, or 6) that determines your payoff and the payoffs of the participant allocated to you.

6. You are informed about the number determined by the die roll (1, 2, 3, 4, 5, or 6).
Table 3.1: Payoff Matrix for Modified Deception Game

<table>
<thead>
<tr>
<th></th>
<th>Option A</th>
<th>Option B</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sender</td>
<td>Receiver</td>
</tr>
<tr>
<td>High</td>
<td>15</td>
<td>15</td>
</tr>
<tr>
<td>Low</td>
<td>10</td>
<td>10</td>
</tr>
</tbody>
</table>

Notes: Senders in a cheap-talk sender-receiver game choose messages to send to receivers based on two payoff possibilities (High and Low), each of which occurs with probability 50%. Receivers, who have no knowledge of the payoff possibilities, make a choice based on the message they receive. Receivers’ choices dictate whether both players are paid according to Option A or Option B.
Table 3.2: Linear Probability Models of Truth-telling, High Choice

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
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<td>0.19*</td>
<td>0.33**</td>
<td>0.34***</td>
</tr>
<tr>
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<td>(0.10)</td>
<td>(0.10)</td>
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<td>(0.12)</td>
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<td>Belief</td>
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<td>0.50***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td></td>
<td>(0.17)</td>
<td></td>
</tr>
<tr>
<td>Female</td>
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<td></td>
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<td>0.25*</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
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<td>(0.13)</td>
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<tr>
<td>Disclosure x Female</td>
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<tr>
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<td>(0.20)</td>
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<td></td>
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<tr>
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<td>-0.19*</td>
</tr>
<tr>
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<td>(0.07)</td>
<td>(0.11)</td>
<td>(0.07)</td>
<td>(0.10)</td>
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<tr>
<td>Observations</td>
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<td>80</td>
</tr>
<tr>
<td>R-squared</td>
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<td>0.118</td>
<td>0.081</td>
<td>0.154</td>
</tr>
</tbody>
</table>

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table reports linear probability model regressions of truth-telling in the High choice. The sample includes subjects in the Disclosure treatment and No Disclosure treatment. The Disclosure dummy equals 1 for all observations of the Disclosure treatment and 0 otherwise; Belief controls for how likely the sender thinks it is that receivers will follow messages; Female dummy equals 1 for all observations of female senders and 0 otherwise.
Table 3.3: Linear Probability Models of Truth-telling, Low Choice

<table>
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<tr>
<th>Variable</th>
<th>(1)</th>
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<th>(3)</th>
<th>(4)</th>
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<td>0.51***</td>
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<td>Female</td>
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<td>Observations</td>
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<td>80</td>
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<tr>
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<td>0.115</td>
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</table>

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table reports linear probability model regressions of truth-telling in the Low choice. The sample includes subjects in the Disclosure treatment and No Disclosure treatment. The Disclosure dummy equals 1 for all observations of the Disclosure treatment and 0 otherwise; Belief controls for how likely the sender thinks it is that receivers will follow messages; Female dummy equals 1 for all observations of female senders and 0 otherwise.
Figure 3.1: Truth-telling Rates by Treatment
Figure 3.2: Truth-telling Rates by Treatment and Gender
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Cain, D. and J. Dana (Forthcoming). Paying people to look at the consequences of their actions. Management Science.


