Complicity without Connection or Communication♣

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Abstract

We use a novel laboratory experiment involving a die rolling task embedded within a coordination game to investigate whether complicity can emerge when decision-making is simultaneous, the potential accomplices are strangers and neither communication nor signaling is possible. Then, by comparing the behavior observed in this original game to that in a variant in which die-roll reporting players are paired with passive players instead of other die-roll reporters, while everything else is held constant, we isolate the effect of having a potential accomplice on the likelihood of an individual acting immorally. We find that complicity can emerge between strangers in the absence of opportunities to communicate or signal and that having a potential accomplice increases the likelihood of an individual acting immorally.

KEYWORDS: complicity, lying, die under the cup task

JEL classifications: C90, C91, C92, D83

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1. Introduction

Complicity "the fact or condition of being involved with others in an activity that is unlawful or morally wrong" (Oxford English Dictionary), is difficult to study in the field. Successful complicity, by definition, is never observed and the few cases of apparent complicity that have been brought to light offer little foundation for generalizable insights. In many cases, complicity appears likely but cannot be proven, owing to a lack of evidence of directly relevant communication between the likely accomplices, but is suspected for two reasons. First, the accomplices are in decision-making contexts in which coordination is advantageous and there are opportunities to reciprocate either by directly assisting one another in an immoral act, by lying to protect one another's and/or their collective reputations, or by turning a blind eye upon each other's wrongdoing. In short, one likely accomplice is helping the other, knowing or anticipating that the other is reciprocating. Second, the likely accomplices share social ties, i.e., they are associates or colleagues and possibly also friends. However, we cannot infer from the existing case studies whether both of these features are necessary pre-conditions for the emergence of complicity. Put another way, the case studies do not lead to insights about precisely what "being involved with others" means in such contexts. In addition, in some cases, the likely accomplices appear to be types of individual that, *a priori*, we would not expect to engage in activities that are "unlawful or morally wrong" and this begs the question does the "being involved with others" increase the likelihood of an individual engaging in an activity that is "unlawful or morally wrong". Consider, for example, the inquiry in the 1990s which revealed that many babies had died after heart surgery at the Bristol Royal Infirmary because medical professionals had not been applying appropriate standards of safety and had remained collectively silent about the issue for half a decade. *A priori*, one would not expect individuals who have selected into caring professions in medicine to behave immorally. So why did they? Was it because of their shared social ties and opportunities to communicate? Or was it owing to something less obvious, but arguably more fundamental, about the decision-making context they found themselves in?

In this paper, first, we investigate whether individuals are willing to engage in complicit acts when they neither know nor are able to communicate with their potential accomplices. More specifically, we look at whether individuals are prepared to lie in order to coordinate with a stranger with whom they are unable to communicate in any way. Second, we investigate whether, *ceteris paribus*, having a potential accomplice – specifically, one who is a stranger with whom one cannot communicate – increases the likelihood of an individual acting immorally, i.e., whether there is a pure *potential accomplice effect*.

If social ties are a necessary pre-condition for complicity, interventions that moderate social tie formation and maintenance between colleagues, such as staff rotation (Abbink 2004), might be sufficient to combat complicity. And if communication between accomplices is necessary for success in complicity, monitoring communication between potential accomplices could provide the basis for an effective deterrent. However, if social ties and opportunities to communicate are not necessary, i.e., if the mere existence of someone who is facing the same moral dilemma and individual and collective incentives and with whom it would be advantageous to coordinate increases the likelihood of an individual behaving immorally, other forms of intervention will be necessary.

To investigate whether complicity can emerge between strangers in the absence of communication of any kind, we designed and invited individuals to play the Complicity Game (CG thereafter), which combines the die-under-cup paradigm of Fischbacher and Föllmi-Heusi (2013) with a coordination game. Specifically, in the CG two anonymous players are randomly paired. Simultaneously, each is asked to roll a die in private and report the outcome. The report of each player determines the monetary payoff received by the other. In addition, each player receives a bonus if both reports are 5 and a higher bonus if both reports are 6.[2] In this game, the distribution of die roll reports will deviate from the uniform distribution of fair die rolls if the value players place on ensuring high monetary payoffs for themselves and others and on coordinating with others facing the same choice outweighs any psychological discomfort they experience when lying. Then, to isolate the *potential accomplice effect*, we designed a variant of the game in which there is no potential accomplice, while everything else, including any altruistic motivation for lying and the subjective distributions of anticipated monetary payoffs conditional on own die roll reports, remains unchanged compared to the CG.

Finally, in another variant of the game, we removed the moral dilemma, while holding everything else, this time including the presence of an active playing partner, constant. In this variant each player reports a number between 1 and 6 without, first, rolling a die.

In the absence of any moral dilemma, 97 percent of players reported a 6 – they tried and usually succeeded to coordinate on the monetary payoff dominant equilibrium. A significantly lower 59 percent of the players participating in the CG reported a 6 indicating that the moral dilemma had a bearing on their decision-making. Finally, a significantly lower again, 41 percent of the players participating in the 'no potential accomplice' variant of the game reported a 6. These results indicate that a significant proportion of people are willing to behave immorally with the aim of coordinating to achieve a higher payoff, i.e., they are willing to engage in complicity, and having a potential accomplice increases individual willingness to behave immorally even when that accomplice is a stranger and communication is not possible.

Our findings contribute to the growing behavioral and experimental literature on immoral behavior. In this literature, behaving immorally is associated with an intrinsic, psychological cost (Hurkens and

[2] This bonus structure is explained in detail in section 3 of the paper.

Kartik, 2009; Abeler, Becker and Falk, 2014; Gneezy, Kajackaite and Sobel, 2016). However, this cost appears to be context specific. For example, people behave more honestly when they have been religiously or morally primed (Mazar, Amir and Ariely 2008), when they have to report their immoral intentions before they act (Jiang 2013), when deviating from honesty might reduce their own earnings by suppressing others' effort (Ederer and Fehr 2007), and when immoral actions harm others (Gneezy, 2005; Fischbacher and Föllmi-Heusi, 2013).

Closely related to our study is that of Weisel and Shalvi (2015), who found that, in a sequential two-player game in which both must lie for each to secure a positive monetary payoff, when the first mover lied, the second mover reciprocated by also lying. Also related is the study of Kocher, Schudy, and Spantig (2016), who found, first, that people were considerably more inclined to lie when in groups within which communication was possible compared to when they were acting alone and, second, that people were marginally more inclined to lie in groups within which communication was possible when they had to coordinate on a lie with their co-group members in order to receive a positive payoff. However, in both of these studies some form of communication was possible. In the latter, the subjects in the group treatments could "chat" on-line before making their decisions in private, while in the former the first mover could signal intent through choice of action and the second mover observed this signal prior to making their choice. Further, neither of these studies endeavored to isolate the effect of having a potential accomplice while holding all other aspects of the decision-making environment constant. In Weisel and Shalvi (2015), the game always involved strategic complementarities and, while Kocher, Schudy, and Spantig (2016) compared the decisions made by people when acting alone to the decisions they made when in groups, many other aspects of the decision-making environment, including expected individual conditional (on own action) payoffs and the ability to communicate, varied between the individual and group treatments.

The prior study that comes closest to identifying the potential accomplice effect is that of Conrads, Irlenbusch, Rilke and Walkowitz (2013). They investigated people's willingness to lie in two contexts. In one (context A), the individual had to share her returns from lying about a die roll equally with another who had the same option to lie and must also share equally. In the other context (context B), the individual, again, had to share her returns from lying about a die roll equally with another and that other's payoff was determined by both players' die roll reports, but her own payoff was determined by her own die roll report and the outcome of a fair die roll rather than the die roll report of the other.[3] They found that people lied significantly more in context A, which is consistent with the existence of a potential accomplice effect. However, note that there two decision-making contexts differ in not one

[3] Specifically, in context A, player i's payoff equals 1/2 ($p_i + p_j$), where p_i is player i's die roll report, p_j is player j's die roll report, and player j's payoff is the same. In context B, player i's payoff equals 1/2 ($p_i + p_c$), where p_c is the outcome of a fair die roll, while player j's payoff continues to equal 1/2 ($p_i + p_j$).

but two ways. First, in context A the individual has a potential accomplice, while in context B she does not. Second, while in context A the individual is guaranteed to get the same payoff as her playing partner, in context B she is not; in context B, if she expects her playing partner to lie, she should also expect to end up with a lower payoff than her playing partner. The first of these differences is the difference we are interested in. However, given the second difference, the greater lying in context A cannot, unequivocally, be attributed to a potential accomplice effect. The potential inequality in payoffs owing to the other player lying in context B could, for example, temporarily increase the degree to which the individual perceives lying as immoral and, thereby, reduce the individual's inclination to lie.

Our experiment is similar to that of Conrads et al (2013) in that the playing partner in the 'no potential accomplice' variant of the CG remains present and continues to benefit from the decision-maker's lying, while the decision-maker's payoff can no longer be increased by the playing partner lying. However, instead of replacing the die roll report of the playing partner in the decision-maker's payoff function with a fair die roll, we replace it with a random draw from the set of all die roll reports made in a prior session of the CG and the decision-makers knew that this was the case. Thus, we aim to hold expected payoffs, conditional on own reported die roll, constant across treatments, while nevertheless varying whether the decision-maker's payoff depends on coordinating with another, i.e., with a potential accomplice, or aligning with a pre-determined payoff distribution. Further, to establish that we have succeeded in holding expected payoffs, conditional on own reported die roll, constant across treatments, we elicit and compare beliefs across the CG and the 'no potential accomplice' variant.

The remainder of the paper is organized as follows. In section 2 we introduce a theoretical framework within which we explore the motivations that could be affecting behavior in the CG. In section 3 we describe our experimental design and procedures. In section 4 we present the results of two checks pertaining to the internal validity of the experiment. In section 5 we present the main results and in section 6 we conclude.

2. Theoretical Framework

In this section, we set out a simple *ad hoc* theoretical framework and use it to explore the various motivations that could affect behavior within the CG.

First, consider the version of the game in which there is no die rolling, i.e., in which each of the two players *chooses* a number between 1 and 6 to report, each player's choice directly determines the other's monetary payoff, and then a bonus of one or two is added if both choose 5 or 6 respectively. This game is represented by the matrix below.

Player A's report: \ Player B's report:	**1**	**2**	**3**	**4**	**5**	**6**
1	1 1	1 2	1 3	1 4	1 5	1 6
2	2 1	2 2	2 3	2 4	2 5	2 6
3	3 1	3 2	3 3	3 4	3 5	3 6
4	4 1	4 2	4 3	4 4	4 5	4 6
5	5 1	5 2	5 3	5 4	6 6	5 6
6	6 1	6 2	6 3	6 4	6 5	8 8

This game has 18 pure-strategy Nash equilibria[4] and the monetary payoff dominant equilibrium is both players report a 6. In this game, we expect players to report 6s for two reasons. First, while multiplicity of equilibria can lead to coordination failures, Van Huyck, Battalio and Beil, (1990) have shown that, if there is a unique monetary payoff-dominant equilibrium, this is what players tend to focus on and select. Second, Mehta, Starmer and Sugden, (1994) have shown that, when there are multiple equilibria but one is visually and/or intuitively salient, players tend to coordinate on that. Owing to its position and monetary payoff-dominance in the game above, (6, 6) meets these criteria.

Now, we introduce the die rolling, i.e., we move to the Complicity Game (CG), and consider the various possible psychological costs and benefits that accrue to subjects depending on their reports and how they relate to their die rolls. To describe formally the complete payoff function for players playing the CG, we define $\Delta = \{1, 2, 3, 4, 5, 6\}$. Players i ($i =$ A, B) individually observe a message sent by Nature $O_i \in \Delta$ and, having made the observation, report an element of Δ, $R_i \in \Delta$, which may or may not equal O_i. The messages the two players observe are uncorrelated. The strategy space Δ^2 of each player has 36 elements (O_i, R_i) which are all the possible combinations of observation and report. Also, we assume that each player's belief about the message sent by nature that the other player observes, $\overline{O_\iota}$, is defined by a degenerate distribution, $\overline{O_\iota} \in \Delta$.[5]

[4] The Nash equilibria of this game are: (1, 1), (2, 1), (3, 1), (4, 1), (1, 2), (2, 2), (3, 2), (4, 2), (1, 3), (2, 3), (3, 3), (4, 3), (1, 4), (2, 4), (3, 4), (4, 4), (5, 5), (6, 6)

[5] We could reformulate everything in terms of expected utility using a generic probability distribution over the set of possible beliefs, but the gain in generality would not justify the added complexity.

Let $U_i : \Delta^2 \times \Delta^2 \rightarrow \mathbb{R}$ be the utility function of Player A (Player B's utility function is symmetric). We assume this is given by:

$$U_A[(O_A, R_A), (\bar{O}_B, R_B)] = [R_B + \delta(R_A, R_B)] + \alpha_A(R_A) - s_A(R_A)$$
$$-g_A(|R_A - O_A|) + \tau_A \kappa(R_A, O_A, R_B, \bar{O}_B) \quad (UF1)$$

The first component of UF1, $R_B + \delta(R_A, R_B)$ is the *monetary gain* for Player A, where R_B equals the report of Player B and $\delta : \Delta^2 \rightarrow \{0,1,2\}$ is a function that indicates whether the reports of the two players are both either 5 or 6 given by the following:

$$\delta = \begin{cases} 1 \; if \; R_A = R_B = 5 \\ 2 \; if \; R_A = R_B = 6 \\ 0 \; otherwise \end{cases}$$

The second component, $\alpha_A(R_A)$, captures *altruism*. Following the literature on altruistic white lies (Erat and Gneezy, 2011; Rosaz and Villeval, 2011), we assume that Player A may derive utility from securing Player B a higher monetary payoff. We assume that $\alpha_A(R_A)$ is strictly monotonically increasing in the co-player's monetary payoff.[6]

The third component, $s_A(R_A)$, captures the non-monetary cost associated with *internal shame*. Motivated by Greenberg, Smeets and Zhurakhovska (2015), we define internal shame as the personal discomfort individuals experience when they imagine another player suspecting them of lying. $s_A(R_A)$ depends only on Player A's report and is monotonically increasing in that report.

The fourth component, $g_A(|R_A - O_A|)$, captures the feeling of discomfort an individual experiences when making an untrue report, i.e., one that does not match nature's message. We will refer to this component as *guilt*, although, conceptually, it is similar to Abeler, Becker, and Falk's (2014) moral cost of dishonesty, Kartik, Tercieux and Holden's (2014) preference for honesty, and Mazar, Amir, and Ariely's (2008) notion of self-concept maintenance. With reference to Mazar, Amir, and Ariely (2008), who argued that lying to a small extent does not necessarily require someone to change their self-image as an honest person, we assume that g_A is monotonically increasing. An individual feels guiltier or less able to think of themselves as an honest person the greater the distance between the message she observes and her report.

[6] Note that this element would also be relevant when dice are not rolled and reports are chosen.

Finally, the last component $\tau_A \kappa(R_A, O_A, R_B, \bar{O}_B)$ captures what we described above as the *potential accomplice effect.* We define a potential accomplice as someone with whom it would be advantageous to coordinate and who is exposed to and is expected to react in a similar way to the same incentives and moral dilemma. We assume that this mutual exposure and expected reaction to the same moral dilemma increases an individual's utility by τ_A under the conditions specified by the indicator function $\kappa :$ $\Delta^2 \times \Delta^2 \rightarrow \{0, 1\}$ and we specify κ as follows:

$$\kappa = \begin{cases} 1 \ if \ R_A > \ O_A \ and \ R_B > \bar{O}_B \\ 0 \ if \ R_A \leq O_A \ or \ R_A > O_A \ and \ \ R_B \leq \bar{O}_B \end{cases}$$

According to this component, Player A gains additional utility, τ_A, from reporting a higher number than that which she observes (implied by $R_A > \ O_A$) if she expects that Player B will respond in a similar way to the dilemma (implied by $R_B > \bar{O}_B$). This component could be interpreted in several different ways. It could be capturing a reduction in *internal shame*, i.e., it could be that the discomfort that Player A experiences when she imagines another player suspecting her of lying is reduced if she, likewise, suspects that other player of lying. Alternatively or as well, it could be capturing a reduction in the feeling of *guilt* that Player A experiences on reporting a higher die roll than she observes if she suspects that the other player is similarly miss-reporting. Alternatively or as well again, it could be capturing additional utility derived from reciprocating an anticipated kind action by Player B, as described in models of intention based reciprocity. Although the general definition of this concept implies that reciprocators receive a signal regarding the intentions of the reciprocity receiver, formal models of intention based reciprocity (Rabin, 1993; Dufwenberg and Kirchsteiger, 1998; Falk and Fischbacher, 1999) incorporate beliefs regarding such intentions. Applied to the CG, a Player A, who believes that Player B will report a higher than observed die roll, perceives that Player B is kind, reciprocates by also reporting a higher than observed die roll and, thereby, further increases her own utility.

According to UF1, players for whom *guilt* and *internal shame* are important components of utility when they lie will be disinclined to play the CG in the same way that they play the game without die rolling; specifically, they will be less inclined to report 6 with the aim of coordinating. However, any such feelings of *guilt* and *shame* will be off-set in players who are altruistic and/or who are susceptible to the *potential accomplice effect*.

Our empirical objective is to establish whether the potential accomplice effect is positive, that is whether $\tau_A > 0$. We do this by comparing subjects' behavior in the CG to subjects' behavior in the no accomplice (NAc) variant of the game in which Player B is present but passive and, hence, not exposed to the moral dilemma, not open to suspicion, and not able to be kind. This is equivalent to a situation where $R_B \leq \bar{O}_B$ and, hence, $\kappa = 0$. The no accomplice variant is designed such that all the other monetary and psychological stimuli are identical to those in the CG.[7] Consequently, if subjects exhibit higher levels of dishonesty in the CG compared to the NAc variant, this can be attributed to the existence of a potential accomplice, i.e., to $\tau_A > 0$.

The behavioral economics literature offers many other elements that we could have built into the utility function. For example, Battigalli and Dufwenberg's (2007) *belief based guilt,* which is increasing in the extent to which a player thinks her actions will disappoint her co-player, could be included as a sub-component of the potential accomplice effect or as a sub-component of internal shame. However, our objective is not to test theory but to identify an effect of decision-making context on willingness to lie and, this being the case, a relatively simple utility function such as UF1 is preferred.

3. Experimental Design

3.1.1. The Complicity Game (CG)

Subjects are randomly matched into pairs. In each pair there is a Player A and a Player B. The players cannot identify or communicate with each other. Each player is asked to roll a fair six sided die once in private and report the outcome to the experimenter by writing it on a slip of paper. Each player's report determines the monetary payoff of the other player in the pair. There are two exceptions to this; if both players report a 5, each gets £5 plus a bonus of £1 and, if both players report a 6, each gets £6 plus a bonus of £2. Play is one-shot and simultaneous.

We included the bonuses for two reasons. First, in many real world examples of complicit lying, the benefits appear to be not only reciprocal but also collective. Second, the bonuses enhance the dominance and focality of (6, 6) and, thereby, increase the ease with which players could coordinate. We included a smaller bonus for (5, 5) because including a bonus for (6, 6) only could have had undesirable effects. For example, rendering (6, 6) too attractive could have reduced the likelihood of observing any cross-treatment difference in behavior. Or, recalling that Fischbacher and Föllmi-Heusi (2013) and Gächter and Schulz (2016) found that many people tend to lie but not maximally, rendering (6, 6) too attractive could have deterred individuals interested in avoiding suspicion and, thereby, rendered coordination harder to achieve.

[7] We check that the design works in this regard in section 4.

3.1.2. The No Moral Dilemma (NMD) treatment (a design check)

The NMD treatment is identical to the CG except that each player simply chooses a number between 1 and 6 and reports his or her choice to the experimenter.

3.1.3. The No Accomplice (NAc) variant

In the NAc variant, as in the CG, subjects are randomly paired, in each pair there is a Player A and a Player B, the players cannot identify or communicate with each other, Player A's task is to roll a fair six sided die once in private and report the outcome to the experimenter, and Player A's report determines the monetary payoff of Player B. The difference between the CG and the NAc is that Player B is passive in the game and Player A's monetary payoff is determined as follows. After Player A reports her die roll, she is asked to randomly draw a report from the set of all reports made during a prior session in which other subjects played the CG. Player A knows this and the details of the CG prior to rolling the die and reporting the outcome. Player A's draw determines her own monetary payoff and, if her report and her draw are both 5 or both 6, she and Player B get a bonus of £1 or £2 respectively.

In this variant of the game, while Player B remains present, Player A has no potential accomplice because Player B is passive and this, essentially, "turns off" the last component in utility function UF1.

In addition, this variant was designed to hold one other factor constant relative to the CG. That factor was the Player As' *ex ante* subjective distributions of anticipated monetary payoffs conditional on their own die roll reports.[8] Describing the CG in full and then inviting the Player As to draw a report from the set of all reports made during a CG session is the natural way to do this. However, it has a drawback, the Player As might think of the players in that prior CG session as accomplices. While this is a concern, note that it could not drive a significant result in support of a positive potential accomplice effect; if anything, it would reduce the chances of us observing such an effect.

3.2. Elicitation of active players' beliefs about the distributions of decisions that would determine their monetary payoffs

The internal validity of this experimental design depends critically on the *ex-ante* subjective distributions of the active players' anticipated monetary payoffs, conditional on those players' own die roll reports, being constant across the CG and the NAc variant. To check that this was the case, at the end of each session of the CG and the NAc variant, we invited players to participate in an un-incentivized belief elicitation exercise in which they were asked to guess how many out of 30

[8] In this regard, our experiment is similar to Bohnet and Zeckhauser's (2004) and Bohnet, Greig, Herrman, and Zeckhauser's (2008) experiments designed to isolate and measure betrayal (of trust) aversion.

participants in the CG would make a report of 1, 2, 3, 4, 5, and 6. If the beliefs of active players under the CG and NAc variant are statistically indistinguishable, we can infer that so too were their *ex-ante* subjective distributions of anticipated monetary payoffs, conditional on their own decisions.

3.3. Experimental Procedures

The experiment was conducted at the CeDEx laboratory, University of Nottingham, in May 2015. In total, 294 students, recruited through ORSEE (Greiner 2004), participated in the CG, NAc variant, and NMD treatment. Of these, 63% were females. The large majority, 80%, were aged between 19 and 22 years, 13% older than 22, and 7% younger than 19. Across treatments the samples were balanced with respect to gender and age. We ran 3 sessions of the CG, 6 sessions of the NAc variant, and 1 session of the NDM (design check) treatment. Each session lasted approximately 40 minutes and each participant earned between £1 and £8 plus a show up fee of £2. The smallest session was run with 24 subjects and the largest with 30 subjects. We started by running two sessions of the CG, one of which generated the reports used to determine the monetary payoffs for the Player As in the NAc sessions,[9] then we randomized the treatments across the remaining sessions.

The games were conducted using die, cups, pens, paper and envelopes. Participants were paired before they made their reports and anonymity was maintained throughout. The neutrally framed instructions were presented to the participants both verbally and in writing. The participants' understanding was tested prior to them proceeding to the game.[10]

4. Internal Validity Checks

In this section, we exploit two features of the experimental design to establish the experiment's internal validity.

4.1. Establishing a Coordination Benchmark

The internal validity of our experimental design depends on subjects trying to coordinate on (6, 6) in the absence of any moral considerations. This needs to be checked empirically because, while (6, 6) is the monetary payoff dominant equilibrium, the CG, assuming no moral considerations, has multiple

[9] 30 die roll reports had to be generated by subjects participating in an early session of the CG before we could start running NAc variant sessions. Anticipating that turnout for the first session of the CG could be less than 30 subjects, we scheduled two consecutive sessions of the CG before scheduling any NAc variant session. In fact, both of the first two CG sessions had a turnout of 30 players. We tossed a coin to determine that the second session of the CG would be used for the draw in the subsequent NAc sessions.

[10] If a participant gave one or more wrong answers in the test, a research assistant went through the instructions with them again and, then, the participant retook the test. Between 1 and 3 participants retook the test in each session. The retake rate was statistically indistinguishable across the CG and NAc according to a t-test (p-value= 0.9477).

equilibria. We included the NDM treatment in our design to facilitate such a check. The data from the NDM treatment is graphed in Figure 1. It indicates that, in the absence of any moral considerations, 97 percent of the time subjects choose 6.[11]

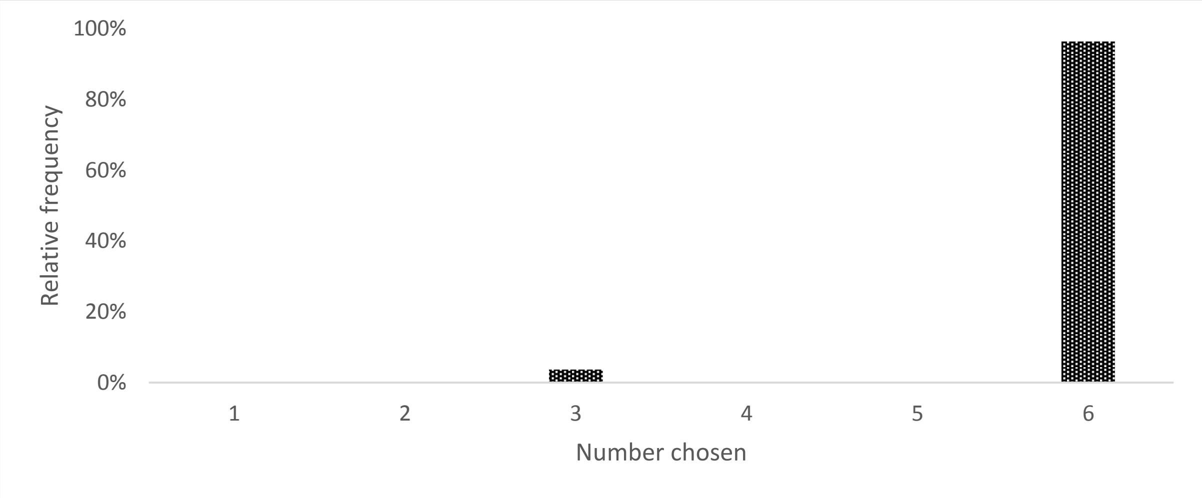

Figure 1: Relative frequencies of numbers chosen in the No Moral Dilemma treatment (n=30)

4.2. Consistency of beliefs across treatments

The internal validity of our experimental design also depends on the active players under the CG and NAc variant having indistinguishable subjective beliefs about the likelihood of others making each of the possible reports and, hence, indistinguishable *ex-ante* subjective distributions of their own monetary payoffs, conditional on their own decisions.

[11] Exactly 29 out of 30 players reported a 6.

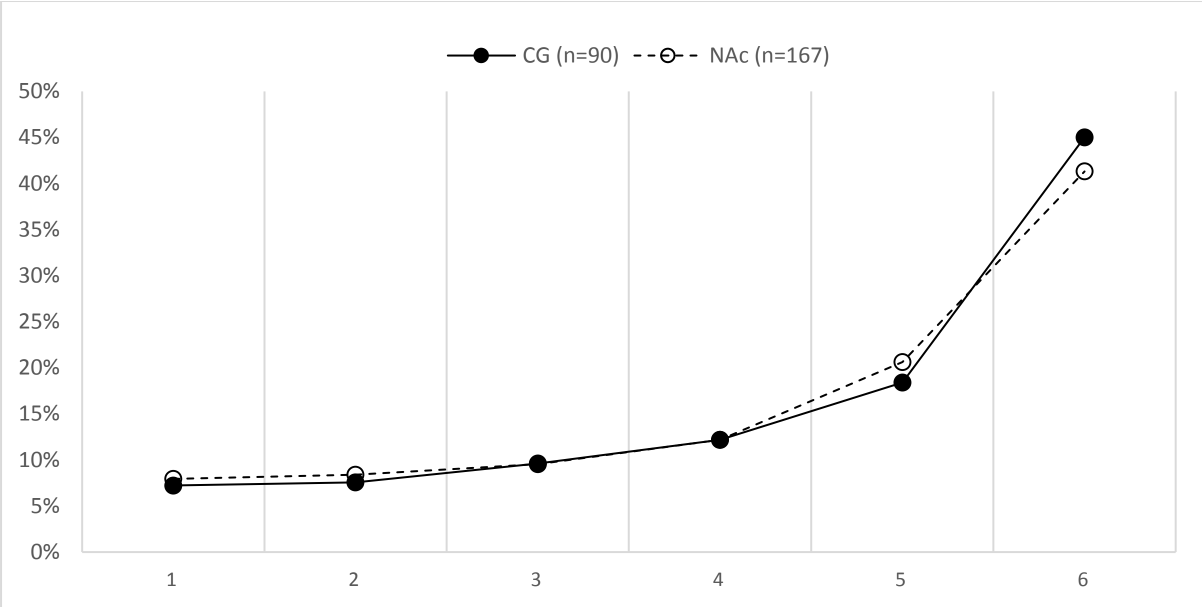

Figure 2: Elicited beliefs about the distribution of reports under the CG and the NAc variant

Figure 2 presents the elicited belief distributions averaged across, in the case of the solid line, all the participants in the CG and, in the case of the dotted line, all the participants in the NAc variant.[12] The figure suggests that there is no difference in players' beliefs across the two treatments. According to a multivariate analysis-of-variance we cannot reject the null hypothesis that the distribution of beliefs is identical across the two treatments (p-value 0.338).[13]

5. Main Results

Having established our experiment's internal validity, we turn to our main results. First, by comparing the distribution of die roll reports in the CG to the expected distribution for a fair die, we address the issue of whether strangers try to coordinate in the absence of communication when coordination requires that they lie. Second, we use the die roll reports made in the CG and NAc variant to test the hypothesis that people are more willing to lie when they have a potential accomplice with whom it would be beneficial to coordinate and who faces the same incentives and is exposed to the same moral dilemma, i.e., that $\tau_A > 0$.

Figure 3 graphs the relative frequencies of the die roll reports made in the CG and the NAc variant. The figure also presents the expected distribution of fair die rolls, which is uniform, with each number

[12] Under the NAc variant, beliefs were elicited from both the active Player As (n=87) and the passive Player Bs (n=87). Of these, 7 did not respond to belief elicitation questions. In the figure, we graph the mean beliefs of the active and the passive players in the NAc variant pooled together. If the beliefs of the passive players in the NAc are excluded, the graph is almost identical.

[13] If the beliefs of the passive players in the NAc are excluded from the multivariate analysis-of-variance, the conclusion is the same (p-value 0.3833).

expected to arise in approximately 16.7 percent of rolls (dotted line). Table 1 presents the percentages of each die roll report under each treatment.

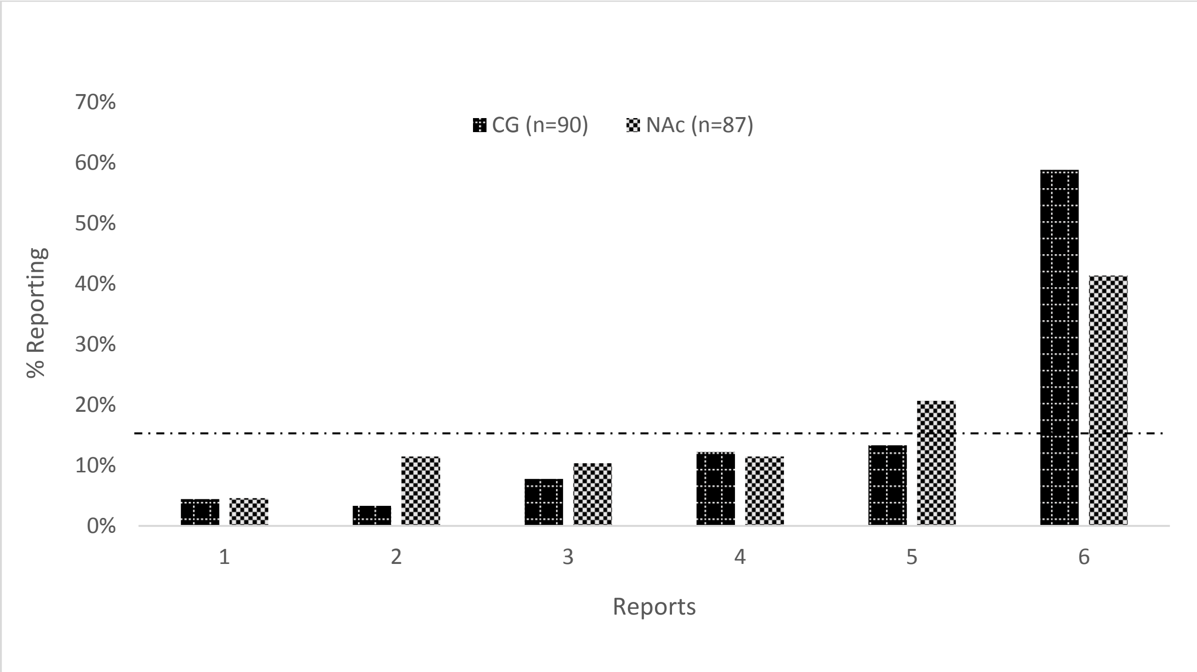

Figure 3: Die roll reports in the CG and the NAc variant

5.1. Do strangers try to coordinate in the absence of communication when coordination requires that they lie?

Figure 3 and Table 1 indicate that 59 percent of the reports made in the CG were 6. This is significantly higher than the 16.7 percent expected in the absence of lying; according to a two sided binomial test, adjusted to account for the fact that we perform six such tests (one for each possible report value) by applying Benjamini and Hochberg's (1995) method,[14] we can reject the null hypothesis that the percentage of reports of 6 in the CG is consistent with no lying at the 1 percent level. The table also indicates that, in the CG, the frequencies of low-value die rolls were significantly below the 16.7 percent expected in the absence of lying. Further, according to a Kolmogorov-Smirnov test, we can reject the null hypothesis that the distribution of reports made in the CG, as a whole, is consistent with no lying at the 1 percent level (p-value<0.01).

[14] The Benjamini-Hochberg method involves: sorting the test p-values in ascending rank; multiplying each by the number of separate tests being performed (in our case six); then, dividing each by its rank. Thus, the greatest adjustment is made to smallest p-value.

Table 1: Die roll reports in the CG and NAc variant

	(1) **CG** (n=90)	(2) **NAc** (n=87)	(3) **Difference** (percentage points)
Panel A			
Die roll report			
1	4.44% ǂǂǂ	4.60% ǂǂǂ	-0.16
2	3.33% ǂǂǂ	11.49%	-8.16 *
3	7.78% ǂǂ	10.34%	-2.56
4	12.22%	11.49%	0.73
5	13.33%	20.69%	7.36
6	58.89% ǂǂǂ	41.38% ǂǂǂ	17.51 ***
Panel B			
H_0: Report distribution same as for a fair die			
K-S test	p-value <0.01	p-value <0.01	
Panel C			
H_0: Mean reports in CG and NAc the same			
t-test, two-sided			p-value = 0.041
Wilcoxon rank-sum			p-value = 0.023
Panel D			
H_0: Report distributions in CG and NAc the same			
K-S test			p-value = 0.100

Notes: In Panel A, columns (1) and (2) report relative frequencies and column (3) reports differences in those relative frequencies; in columns (1) and (2), the significance of the difference between the reported relative frequency and that expected for a fair die according to two-sided binomial tests, adjusted to account for the fact that we perform six tests per distribution (Benjamini and Hochberg, 1995), is also indicated (ǂǂǂ and ǂǂ indicate significance at 0.01 and 0.05 respectively); in column (3), the significance of the difference between the treatments according to t-tests, adjusted to account for the fact that we perform six tests, is also indicated (*** and * indicate significance at 0.01 and 0.10 respectively); Panels B, C, and D describe tests of other hypotheses relating to the die roll distributions and report the results; in the second and fourth panel, K-S indicates Kolmogorov-Smirnov.

Note also that the 59 percent of reports of 6 in the CG is significantly lower than the 97 percent of subjects who chose a 6 in the NMD treatment (p-values from unadjusted t-test < 0.001).

These results indicate that a significant proportion of people try to coordinate with strangers in the absence of communication when coordination requires that they lie.

5.2. Is there a potential accomplice effect?

Figure 3 and Table 1 also indicate that reports of 6 were more frequent in the CG, at 59 percent, compared to the NAc variant, at 41 percent. According to a two-sided t-test, adjusted to account for the fact that we perform six such tests, one for each possible report value, we can reject the null hypothesis that the proportion of reports of 6 in the CG and the NAc are the same at the 1 percent level. Further, according to a t-test and a Wilcoxon rank-sum test, we can reject the null hypothesis that the mean reports made in the CG and the NAc are the same at the 5 percent level (p-values 0.041 and 0.023 respectively). Only one test, the Kolmorogorov-Smirnov test of the null hypothesis that the distributions of reports made in the CG and the NAc are the same, yields a result on the borderline of significance at the 0.10 level.

These findings indicate that, *ceteris paribus*, subjects are more inclined to lie when they have a potential accomplice.

6. Conclusion

The objective of this study is to investigate whether strangers try to coordinate in the absence of communication when coordination requires that they lie and whether people are more willing to lie when, in so doing, they are trying to coordinate with a potential accomplice, i.e., another person facing the same potential individual and mutual benefits and the same moral dilemma, even when that individual is a stranger and communication is not possible. We conducted an experiment focused on a game, the Complicity Game (CG), in which the two players could maximize their individual and collective earnings only if they were willing to coordinate by lying.

By comparing the distribution of die roll reports made in the CG to the expected distribution for a fair die, we found that a significant proportion of strangers are indeed able and willing to coordinate in the absence of communication when coordination requires that they lie. Then, to isolate the effect of having such a potential accomplice in the CG, we also designed and conducted a variant of the game, the NAc variant, in which the one active player had no potential accomplice, but everything else was the same. By comparing the distributions of die roll reports made in the CG and the NAc variant, we found that subjects were more inclined to lie when they had a potential accomplice.

These results indicate that complicity does not require social ties or communication and that, to an extent at least, the emergence of complicity is owing to a desire to coordinate with others facing similar incentives even when coordination requires immoral behavior. These findings are relevant to both the public and the private sector. They suggest that interventions designed to moderate social tie formation and maintenance or monitor communication between colleagues may not be sufficient to eliminate complicit wrongdoing. Measures designed to inculcate moral values within individuals may be the answer, but further research is required. As a next step, it would be interesting to establish whether coordinating with others in such contexts reduces feelings of guilt or internal shame or increases feelings of satisfaction associated with reciprocating the anticipated kindness of others.

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