# **Supplementary Online Materials**

Ethical free-riding: When honest people find dishonest partners

### **Table of Contents**

1 Materials and methods	2
2 Additional information	15
3 Additional results	22
4 Relationship development	
5 References	
6 All relationships	31

## 1 Materials and methods

All the instructions were presented on the computer screen in English. Before the main task, each participant engaged in an individual die-rolling task to measure baseline honesty. In this task, participants saw a random die roll presented on the computer screen, and were asked to report the outcome of the die roll (see below). Participants engaged in this task five times. At the end of the experiment, one trial was randomly selected for payment. Payoffs were determined by the outcome participants reported, with higher outcomes corresponding to higher payoffs (i.e., reporting 1 = 0.5, 2 = 0.5, 2 = 0.5, 4 = 0.5, 4 = 0.5, 5 = 0.5, 6 = 0.5, 6 = 0.5, 2 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 5 = 0.5, 6 = 0.5, 6 = 0.5, 2 = 0.5, 6 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 5 = 0.5, 6 = 0.5, 6 = 0.5, 2 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 4 = 0.5, 4 = 0.5, 2 = 0.5, 6 = 0.5, 2 = 0.5, 6 = 0.5, 2 = 0.5, 2 = 0.5, 4 = 0.5, 2 = 0.5, 2 = 0.5, 2 = 0.5, 2 = 0.5, 2 = 0.5, 0 = 0.5,

The instructions for the first part (individual die roll) and third part of the experiment (measurement of SVO) were identical for all participants, whereas the instructions for the second part (the sequential dyadic die-rolling paradigm) differed based on the role of the participants (first/second mover) and the condition (Choice/Forced Switch/Forced Stay).

# 1.1 General instructions



Figure S1. General instructions.

1.2 Instructions for the individual die-rolling task



Figure S2. Part 1 instructions.



**Figure S3.** Die-rolling stage. Each participant saw a randomly selected video of a die roll (in this example, the roll of a 4).



**Figure S4.** Report screen. After observing a die roll on the computer screen, each participant was asked to report the observed outcome. Participants engaged in this task for 5 rounds, and at the end of the experiment, one round was randomly selected for payment.

1.3 Instructions for the dyadic die-rolling task paradigm



**Figure S5.** Part 2 instructions (page 1). For second movers, the last sentence read "You are a second mover."

In part II the task goes as follows: 1. The first mover will see a die roll on the computer screen and be asked to report the die roll outcome. 2. After that, the second mover will see the number the first mover reported. Then the second mover will see an independent die roll on the computer screen. Then the second mover will report the die roll outcome. 3. At the end of each round, both the first mover and second mover will see both reported outcome of this round. understood - next page	
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<ol> <li>The first mover will see a die roll on the computer screen and be asked to report the die roll outcome.</li> <li>After that, the second mover will see the number the first mover reported. Then the second mover will see an independent die roll on the computer screen. Then the second mover will report the die roll outcome.</li> <li>At the end of each round, both the first mover and second mover will see both reported outcome of this round.</li> </ol>	In part II the task goes as follows:
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understood - next page	<ol> <li>At the end of each round, both the first mover and second mover will see both reported outcome of this round.</li> </ol>
	understood - next page
(back to previous page)	(back to previous page)

Figure S6. Part 2 instructions (page 2).

Instructions for part II					
Payment for part II	amount of 2 ourse resardings of your re-	nort			
in every found, you will carri a fixed	amount of 5 curos regardless of your rep	port.			
Beyond these 3 euros per round, if y	rou (the first mover) and the second mov	ver report the same number ("a double")	both of you will be paid extra money according	to the following table:	
first mover report	second mover report	first mover receives	second mover receives		
1	1	0.50 euros	0.50 euros		
2	2	1 euros	1 euros		
3	3	1.50 euros	1.50 euros		
4	4	2 euros	2 euros		
5			2.50 euros		
		3 euros	3 euros		
If you (the first mover) and the second mover report two different numbers ("no double") both of you will receive 0 euros extra.					
In total, that means, that you will ear	n:				
(1) a fixed amount of 3 euros per round, that is a total of 9 euros, plus					
(2) the extra money based on your a	ind the second mover reports as explain	ed above.			
		understood - next page			

**Figure S7.** Payoff scheme (page 3). For second movers, the word "you" was followed by "(the second mover)" instead of "(the first mover)."

1.4 Instructions regarding partner-selection conditions



Figure S8. Additional instructions in the Forced Stay condition.

Instructions for part II
After each block of 3 rounds, you will be randomly matched with another second mover. Before re-matching with a new second mover, you will receive a message on the computer screen reminding you that you are matched with a new second mover.
understand - meet page
(back to previous page)

Figure S9. Additional instructions in the Forced Switch condition.

Instructions for part II
After each block of 3 rounds, you and the second mover will be asked whether you want to continue being matched to one another for an
additional block of 3 rounds, or whether you want to be matched with a new person.
If you are the first mover the second mover or both devide that you want to be matched with a new person, both you and the second mover will
in you, as the link intervent, the second intervent, or both decide that you many to a many or the intervent many person, about you many and second intervent will be intervented and another will be intervented and another will be intervented and another personal to the second
be intorned that in the next stourios you will be matched with another person , which will indeed be the case.
(There is a small chance that you or the second mover would ask to be re-matched but that this will not be possible, because nobody else
wants to be re-matched. In that case you will stay with the same second mover in the next block, and you will be informed that this is the case).
understood - next page
(back to previous page)

Figure S10. Additional instructions in the Choice condition.

# 1.5 Instructions about externalities (damage to charity donation)



Figure S11. Charity-damage instructions.

# 1.6 Comprehension questions



**Figure S12.** Comprehension questions. Before starting the task, participants had to answer three questions introducing hypothetical scenarios to make sure they understood the payoff scheme. Participants had to answer all three questions correctly in order to start the task.

# 1.7 Dyadic die-rolling task



Figure S13. Die-roll stage.



Figure S14. Report stage.



**Figure S15.** Information of partner's report (from the second-mover perspective). For second movers, the screen read "The second mover reported a 3."

	Summa block 1, ro	ry und 1		
			earnings for this round (if block is selected for payment)	
	reported outcome	fixed	per reported outcomes	total
First mover (other person)	3	3 euros	0 euros	3 euros
Second mover (me)		3 euros	0 euros	3 euros
	proceed to not	tround		

**Figure S16.** Round summary. After each round, participants saw a summary of theirs and their partner reports and subsequent earnings (if selected for pay). The "fixed" column represents the fixed payment participants got for each round (3 euros), whereas the "per reported outcomes" column represents the extra amount participants earned based on the die-roll outcomes they reported. The "total" is the sum of both "fixed" and "per reported outcomes" payments.

Summary - block 1							
			earnings for this round (If block is selected for payment)				
	round	reported outcome	fixed	per reported outcomes	total		
First mover (me)			3 euros	0 euros	3 euros		
Second mover (other person)			3 euros	0 euros	3 euros		
First mover (me)			3 euros	3 euros	6 euros		
Second mover (other person)			3 euros	3 euros	6 euros		
First mover (me)			3 euros	3 euros	6 euros		
Second mover (other person)			3 euros	3 euros	6 euros		
				Total (me):	15 euros		
				Total (other person):	15 euros		
		proceed to next block					

**Figure S17.** Block summary. After each block (three consecutive rounds), participants saw a summary of theirs and their partners reports and subsequent earnings (if selected for pay).

#### Partner-reassignment procedure

After each block, participants in the Forced Stay condition continued the task with the same partner. In the Forced Switch condition, after each block, participants were reminded they would now engage in the task with a different partner. In the Choice condition, after each block, participants were asked whether they would like to continue the task with the same partner (stay) or do the task with a new partner (switch, Figure S18). If at least one participant in a dyad chose to switch partners (and at least one other participant from another dyad in the session chose to switch), the dyad was resolved and both dyad members were informed that they would be paired to a new partner for the next block. If there were no new partners to switch to, participants were informed they would continue the task with the same partner, informing them that "it was not possible to match you with a new person. You will do the task with the same first mover (second mover) for the next block of three rounds."

The reassignment in the Forced Switch and Choice conditions was implemented in the following way: Each dyad had a fixed number throughout the experiment, numbered from 1 to 10. First movers stayed in their dyad throughout the whole experiment, whereas the second movers moved to the next dyad in a full circle. Thus, in the Forced Switch condition, after the first block,

the second mover of dyad 1 moved to dyad 2, the second mover of dyad 2 moved to dyad 3, and so on. The second mover of dyad 10 moved to dyad 1. Thus, after 30 rounds (9 reassignments), each first mover interacted with each second mover for one block (see Figure 1C in the main manuscript for illustration). The reassignment in the Choice condition was similar, as each second mover that needed a new partner (due to asking to switch, or the partner asking to switch) moved to the next available dyad. For example, if dyads number 4, 5, and 7 were split after a block, the second mover of dyad 4 moved to dyad 5, the second of mover of dyad 5 moved to dyad 7, and the second mover of dyad 7 moved to dyad 4.



Figure S18. Decision screen for the Choice condition.

# 1.8 SVO instructions



**Figure S19.** SVO slider measure. After the main task of the experiment, participants completed the SVO slider measure (15 items).

#### Minimizing reputational concerns and ensuring anonymity

The study was run in the experimental economics lab at CREED (Center for Research in Experimental Economics and political Decision making), Amsterdam. The CREED lab has a strict no-deception policy and ensures anonymity. The CREED subject pool is aware that researchers are not allowed to use any form of deception, and indeed no deception was used in the study. Importantly, participants who took part in the experiment could only be identified by the number they received at the beginning of the study, and were paid according to that number. Participants are aware that the experimenters are not interested in one's individual behavior but rather in the overall emerging trends. We purposefully had no identifiers that could be used to trace back individual behavior to a specific individual (e.g., we did not ask them to provide their student number or e-mail address). We used this approach to reduce possible reputation concerns toward the experimenters that go beyond the experimental situation.

#### Externality of dishonest behavior

Corrupt collaboration often has negative consequences for third parties. Therefore, across all conditions, we implemented a negative externality on a third party for reaping personal benefits from lying. Participants were informed that every time a double was reported in which one or both partners misreported the number they observed, the dyad's earnings from the double would be subtracted from a donation to carbonfund.org—a carbon-footprint-offsetting charity (Figure S11). As such, participants' misreporting led to societal costs by reducing the amount of carbon footprint being offset. Participants learned the planned donation would allow the offsetting of 192 tons of carbon footprint, but they were not informed of the precise monetary cost of doing so (which was equal to  $\in$ 1,821). We did not inform participants about the precise costs to avoid signaling an expected level of dishonesty in the experiment. Because the negative externalities were fixed across all conditions, the externalities cannot explain condition effects, but only the overall magnitude of lying.

# 2 Additional information

#### 2.1 General data analysis

In the Forced Stay condition, each participant was assigned to one other participant to form a dyad and repeatedly interacted with this participant for 30 rounds. Because dyad members could influence each other in their behavior, the average behavior of one dyad was treated as one independent observation.

In the Choice and Forced Switch conditions, participants not only interacted with one partner, but also potentially with all other first (second) movers in their group, due to forced or chosen partner reassignment. In the Choice and Forced Switch condition, we therefore treated one group of 20 participants (one session) as one strictly independent observation. In total, we had 8 groups of 20 participants in the Choice and Forced Switch conditions.

For the statistical models and tests, we either aggregated over non-independent observations (dyads in Forced Stay condition, sessions in Choice and Forced Switch conditions) or accounted

for the non-independence of observations by using mixed-effects regression models. Using standard OLS regression for the non-aggregated data would violate the fundamental assumption that individual data points are randomly drawn from a population. Instead, we should assume behavior across participants (in dyads) and within participants (across rounds) is correlated. The mixed-effects regression model accounts for this dependency by estimating a separate intercept for each individual (i.e., potential autocorrelation of the same individual across rounds) and dyad (potential autocorrelation of two team members in the same team).

Following the recommendations by Gelman and Hill (2006) (see also Kruschke, 2014 or Browne & Draper, 2006), we fitted Bayesian mixed-effects regression models to the data, using non-informative Gaussian priors (m=0, sd=100) for each predictor and non-informative uniform priors (range 0 to 100) for the error terms. Each reported mixed-effects model was fitted in JAGS and R using two parallel chains until the Gelman-Rubin statistic was below 1.05 for all parameters, indicating good mixing of the chains and thus high convergence.

Compared to a maximum likelihood approach, Bayesian mixed-effects models allow us to more reliably estimate the variance parameters in the model. Bayesian models do not provide a p-value in the frequentist tradition. Therefore, we report the 95% Bayesian confidence interval of each estimated parameter. Note that because we used non-informative priors, a 95% CI that only contains negative or positive values can be interpreted as significant at a p = .05 two-sided threshold from the frequentist perspective. Fitting the models using restricted maximum likelihood (REML) as implemented in the lme4 package in R revealed similar estimates and the same statistical inferences. However, models on the individual subject level failed to converge in some cases, and also the censoring in the data (see below) could not be accounted for in these models.

For some models, we accounted for censoring of the dependent variable. For example, die rolls were restricted to lie between 1 and 6 and are hence censored below 1 and above 6. Not accounting for such censoring of the dependent variable can lead to biased estimates and wrong statistical inferences, because the assumption of normally distributed residuals (error variance) across all levels of the dependent variable is violated (violation of homoscedasticity). The censored regression model deviates from a linear regression model in that it accounts for the

censoring in the data. Instead of y, the model assumes a latent variable  $y^*$  (e.g., what participants would have wanted to report if the die outcome had not been restricted to lie between 1 and 6) that linearly depends on the predictors. y is equal to  $y^*$  if y > 0 and y < 6 (in this example of die rolls), but deviates in the upper and lower bound. For  $y = y^*$ , the likelihood function is simply the normal distribution  $N(y|bX, \sigma 2)$  as in a standard linear regression. For  $y \neq y^*$ , the likelihood function is based on the normal cumulative distribution and hence models the probability that  $y^*$  will take a value less than or equal to y (for the left side) and the probability that  $y^*$  will take a value higher than or equal to y (for the right side), given the observed predictors. This approach allows us to derive unbiased estimates for predictors and error variance in the presence of heteroscedasticity.

#### 2.2 Prevalence of types

In the main manuscript, we report the prevalence of five different types, by assigning each subject a label based on their most prevalent behavioral pattern. Specifically, we assigned the following labels (1) *ethical free rider* when a participant was honest (i.e., reported the outcomes observed in all three rounds) and chose to (a) switch when paired with an honest partner and (b) stay when paired with a liar; (2) *Kantian truth-teller* when a participant was honest and chose to (a) switch when paired with a liar and (b) stay when paired with an honest partner; (3) *brazen liar* when a participant lied (misreported the die-roll outcome in at least one out of three rounds) and chose to (a) switch when paired with an honest partner and (b) stay when paired with a liar; (4) tolerant liar when a participant lied and chose to stay when paired with an honest partner; and (5) confused liar when participants lied and chose to switch when paired with a liar.

For each subject, a type was assigned based on the most prevalent exhibited decision pattern (column 1, Table S1). Further, Table S1 shows the average consistency for each type in parentheses. Numbers indicate the average number of blocks (out of nine) in which a type was classified as such (i.e., an average of nine would mean every participant of this type was consistently behaving in accordance with the behavioral pattern of this type across the entire experiment). The assignment of a specific type to each participant assumes participants behave in a consistent manner and do not change their behavior over time. However, in our experiment, participants were free to change their behavior over rounds. Because participants can change their behavior, we test whether the reported results regarding prevalence of types (under the

heading "the prevalence of ethical free-riders" in the results section of the main manuscript) are robust to different classification criteria. Specifically, we looked at the distribution of types, when we only classified participants if they showed one behavioral pattern across (1) at least 60% of the blocks (column 2, Table S1) and (2) at least 80% of all blocks (column 3, Table S1). Participants who did not exhibit a consistent pattern were classified as "unstable." Importantly, across all three classification methods, we find the same rank-order of frequencies. Most participants are classified as dishonest, and among dishonest participants, brazen liars were more frequent than tolerant liars. Additionally, among the honest participants, ethical free-riders were more frequent than Kantian truth-tellers.

Table S1.
Type classification based on (1) most frequently observed behavioral pattern, (2) at least 60%
consistency, and (3) at least 80% consistency.

	Most frequent	60% consistent	80% consistent
ethical free-rider	18.8 (7.2)	16.2	8.1
Kantian truth-teller	10.6 (7.1)	9.4	4.4
prop. of free-rider out of all honest	63.9	63.3	64.8
brazen liar	63.7 (7.7)	52.5	41.9
tolerant liar	7.5 (6.5)	3.8	3.1
confused liar	0.0 (-)	0.0	0.0
prop. of brazen out of all liars	89.5	93.3	93.1
unstable	_	18.1	42.5

*Note.* Numbers in parenthesis show the average consistency across the nine blocks for each type.

### 2.3 Contagiousness of dishonesty

To test for differences across conditions in the contagiousness of dishonesty, we analyzed whether the likelihood of lying increased if the partner misreported the die-roll outcome in the last round (for first movers) or last stage (for second movers). Because we were interested in individual-per-round behavior, we did not aggregate across rounds and instead treated the data as

hierarchically clustered within subjects (over rounds) as well as teams/dyads and fitted a random logistic regression model with two random intercepts (one for each subject and one for each unique team). In the Forced Stay condition, the partner significantly influenced own reporting, by increasing the likelihood of misreporting if the partner misreported, as can be seen in Table S2. We found no significant change under the Choice condition, whereas forcing participants to regularly switch partners significantly decreased the influence of the partner's behavior on the likelihood of lying.

Table S2.	
Logistic random intercept regression modeling lying ( $0 =$ honest report, $1 =$ lie	e)

	estimate	SD	95% CI
intercept (Forced Stay condition)	-0.15	0.380	[-0.849, 0.655]
Forced Switch condition	-0.33	0.433	[-1.192, 0.502]
Choice condition	-0.69	0.435	[-1.563, 0.15]
partner t-1 ( $0 =$ honest report, $1 =$ lie).	0.33	0.179	[0.019, 0.68]
round	0.02	0.004	[0.016, 0.029]
Forced Switch × partner t-1	-0.41	0.201	[-0.808, -0.027]
Choice × partner t-1	-0.01	0.209	[-0.419, 0.392]
random intercept error term (dyad)	0.63	0.068	[0.495, 0.762]
random intercept error term (subject)	2.39	0.118	[2.163, 2.621]

### 2.4 Honest dyads

For each block, we classified participants as honest (H) if they reported honestly in all three rounds and as liars (L) if they misreported the die-roll outcome at least once (out of three). We then categorized each dyad into either being honest (both dyad members reported honestly) or dishonest (one or both dyad member reported dishonestly). For each independent observation (dyad for Forced Stay, and session for Choice and Forced Switch), we calculated the proportion of honest dyads and regressed it on the condition. Because proportions are censored to lie between 0 (0%) and 1 (100%), we fitted a censored regression model to the data (see Table S3).

Note that in this analysis, we aggregated across dependent observations, and hence did not need multilevel modeling.

	estimate	SE	t	р
Intercept (Forced Stay condition)	-0.07	0.044	-1.576	0.115
Forced Switch condition	0.17	0.070	2.416	0.016
Choice condition	0.22	0.070	3.079	0.002

### Table S3.

Censored regression model for the share of HH dyads (censored below zero and above 1).

### 2.5 Effectiveness of lying

To compare the effectiveness of lying across conditions, we calculated the average earnings for each participant across rounds in which they misreported at least on die roll outcome (i.e., were classified as liars). Figures S20 and S21 show the distribution of average earnings across participants and conditions, separate for first movers (Figure S20) and second movers (Figure S21). In particular, first movers profited more from lying in the Forced Stay and Choice conditions, as compared to the Forced Switch condition. Table S1 shows the results of the regression model that was fitted to the data. Because the dependent variable (return on lying) could not go below 0 or above 3, we treated the data as censored. Because data were clustered in dyads (in the Forced Stay condition), or sessions (in the Choice and Forced Switch conditions), we used a random-intercept regression that estimates one intercept for each matching group. As can be seen in Table S4, dishonesty in the Choice condition led to levels of damage to charity similar to those in the Forced Stay condition (random-intercept regression, b = -0.21, 95% CI: [-0.73, 0.31]), whereas the Forced Switch condition led to significantly less damage to charity (random-intercept regression, b = -0.54, 95% CI: [-1.06, -0.02]).



**Figure S20.** Effectiveness of lying. Distribution of average earnings of first movers when misreporting the truth for each partner condition.



**Figure S21.** Effectiveness of lying. Distribution of average earnings of second movers when misreporting the truth for each partner condition.

#### Table S4.

Censored random intercept regression modeling the average return for lying per s	ubject
(censored below zero and above 3).	

	estimate	SD	95% CI
Intercept (Forced Stay condition, first mover)	2.86	0.200	[2.456, 3.244]
Forced Switch condition	-0.54	0.263	[-1.060, -0.020]
Choice condition	-0.21	0.264	[-0.730, 0.312]
Second mover	-0.11	0.246	[-0.589, 0.364]
Forced Switch condition × second mover	0.25	0.282	[-0.308, 0.790]
Choice condition × second mover	0.05	0.283	[-0.514, 0.602]
random intercept error term	0.37	0.117	[0.163, 0.611]
error term y	0.77	0.041	[0.688, 0.848]

# 3 Additional results

### 3.1 Survival of honesty

We further assessed how honesty survived over rounds across conditions. We looked at how long it took for a person reporting honestly in round 1 to start lying. Specifically, we focused on all participants who reported truthfully in the first round, and coded the time point at which the participant switched to misreporting. Because data were clustered on individuals (over time) and groups (dyads in the Forced Stay, and sessions in the Forced Switch and Choice conditions), we fitted a parametric survival regression model clustering on individuals and dyads/session using robust sandwich standard errors as implemented in the survival package in R. The model estimates the hazard rate of honesty  $\lambda$ (round|x) across conditions. Because data points cannot be treated as independent from each other (subjects are potentially influenced by other subjects in each session), and, in particular, errors are not independent, the estimated coefficients are no longer the best linear unbiased estimator, due to potentially different error variance across observations and correlation between errors (both in time and between groups). To eliminate the potential overestimation of effects due to smaller estimated standard errors, the robust sandwich standard errors (Huber–White standard errors or simply robust standard errors), as implemented in the survreg-function, relaxes the assumption of uncorrelated errors and equal variance in errors across observations by estimating the correlation of the residuals between clusters and correcting for it based on the degree of covariance. This approach results in larger standard errors that better reflect the actual uncertainty in the estimates.

Over time, participants' honesty declined. Resonating with the results of the contagiousness of dishonesty, the decline was quicker when participants were forced to stay with their partners than when we forced participants to frequently switch interaction partners (Figure S22, Forced Switch estimate: b = -0.42, SE = 0.20, p = 0.035, Table S5). Importantly, honest participants remained honest longer when they had the choice to switch partners, suggesting partner choice indeed allows honest individuals to stay honest for longer periods of time (Figure S22, Choice estimate: b = -0.42, SE = 0.20, p = 0.034, Table S5). We found no difference between the Choice and Forced Switch conditions in the pace at which honest participants began lying (b = 0.01, SE = 0.11, p = 0.96).



**Figure S22.** Survival of honesty. The proportion of initially honest individuals who remain honest across rounds in each condition (blue = Choice, red = Forced Stay, black = Forced Switch).

 Table S5.

 Survival regression modeling the time until switching to dishonesty per individual.

	estimate	SE	naïve SE	Z	р
Intercept (Forced Stay condition)	3.85	0.192	0.052	20.064	< 0.001
Forced Switch condition	-0.42	0.198	0.055	-2.107	0.035
Choice condition	-0.42	0.199	0.055	-2.114	0.034

Because participants could also observe a 6 by chance in the first round and thereby would have no incentive to misreport the outcome, we also tested the robustness of these findings by controlling for honestly reported 6's in the first round. Table S6 shows that results do not qualitatively change when controlling for honest 6's in the first round. In sum, having the possibility of choosing a partner allows individuals' honesty to survive a little longer (but also leads to ethical free-riding, as discussed in the main manuscript).

#### Table S6.

#### Survival regression model.

Dependent variable: Time (i.e. rounds) until switching to dishonesty per individual, controlling for honestly reported 6'sin the first round.

	estimate	SE	naïve SE	Z	р
Intercept (Forced Stay condition)	3.763	0.191	0.052	19.685	0.000
Forced Switch condition	-0.385	0.193	0.055	-1.992	0.046
Choice condition	-0.407	0.195	0.055	-2.089	0.037
Honest 6 in the first round	0.342	0.151	0.043	2.268	0.023

#### 3.2 Individual vs. dyadic task

Across all conditions, moving from an individual setting to a dyadic setting significantly increased lying (Wilcoxon signed rank test, p < .01). In the individual task, participants misreported the die roll outcomes in 27% of the rounds, whereas they misreported the die roll outcomes in 53% of the rounds in the dyadic setting (Figure S23), replicating earlier findings that working with others can lead to more dishonesty than working alone.



**Figure S23.** Lying rates. Average lying rates in the individual and the dyadic die-rolling task (averaged across conditions).

### 3.3 Social Value Orientation

Social value orientation (SVO) correlated moderately negatively with average lying rates in the dyadic die-rolling task (r = -0.27). However, it did not significantly interact with the condition in predicting the probability of lying (Table S7), suggesting that SVO affects the probability of lying, independent of the partner-selection condition that was implemented.

	estimate	SE	Z	р
Intercept (Forced Stay condition)	-1.325	0.570	-2.322	0.020
SVO type ( $0 = \text{pro-social}, 1 = \text{selfish}$ )	1.780	0.685	2.598	0.009
Forced Switch condition	-0.188	0.654	-0.288	0.774
Choice condition	0.118	0.655	0.181	0.856
round	0.029	0.003	9.768	< 0.01
SVO type × <i>Forced Switch</i> condition	-0.427	0.781	-0.546	0.585
SVO type × <i>Choice</i> condition	-0.982	0.782	-1.256	0.209

Table S7.SVO logistic regression model.Dependent variable: Probability of reporting dishonestly.

### 3.4 Overall magnitude of lying

We compared the lying rate in our experiment with the three closest settings in the literature: (1) the "aligned outcomes" setting, (2) a replication of the aligned outcomes setting, and (3) an "addition" setting, all reported in Weisel and Shalvi (2015). In the "aligned outcomes" setting, participants received payment if they reported a double. Similar to our setting, the higher the double, the more participants earned. Specifically, a 1-1 double corresponded to a payoff of  $\in$ 1 per participant; a 2-2 double corresponded to a payoff of  $\in$ 2 per participant; a 3-3 double corresponded to a payoff of  $\in$ 3 per participant; a 4-4 double corresponded to a payoff of  $\in$ 4 per participant; a 5-5 double corresponded to a payoff of  $\in$ 5 per participant; and a 6-6 double corresponded to a payoff of  $\in$ 6 per participant. In a replication setting, the payoff scheme was identical to the original "aligned outcomes" setting, but the payoff was in British pounds instead of euros. Participants in the "addition" setting further received a fix payment of 2 British pounds for each trial (similar to the  $\in$ 3 in our study). Again, they earned additional payment based on whether they reported a double (using the same payoff scheme as in the replication of the "aligned outcomes" setting).

Unlike in the current study, in Weisel and Shalvi (2015), participants rolled an actual die, whose outcomes were not recorded on the computer. Thus, anonymity was higher in Weisel and Shalvi

(2015). Furthermore, the costs associated with participants' lies in Weisel and Shalvi were paid for from the experimenters' budget, not a charity. In these settings, participants reported to have seen doubles in 81.5% of the trials in the "aligned outcomes" setting, in 74.50% of the trials in the replication, and 56.5% of the trials in the "addition" setting. To assess the estimated proportion of lies, we adopted the approach by Abeler et al. (2018). Specifically, we estimated the proportion of dishonest doubles by dividing the proportion of over-reporting (i.e., the gap between the observed proportion of doubles reported and the expected proportion if participants were honest) by the maximum over-reporting possible (i.e., the gap between the maximum lying possible and expected proportion if participants were honest). Such calculation revealed an estimate of 78% (aligned outcomes), 66% (aligned outcomes; replication), and 48% (addition) of dishonest doubles.

In our current experiment, we classified a double as honest if both participants reported honestly, and as dishonest otherwise. Such classification revealed the proportion of dishonest doubles in the Forced Stay condition (which is the most comparable to the previous settings in the literature) is 76.66%. The proportion of dishonest doubles in the Forced Switch condition is 66.54%, and in the Choice condition, it is 69.95%. We thus conclude that the level of lying in the current experiment is of similar magnitude to those reported in previous work using the dyadic die-rolling paradigm.

Finally, compared to an individual setting, the magnitude of lying in the dyadic setting is higher. A recent meta-analysis assessing over 32,000 participants estimated that, on average, participants only earn 21.6% of the maximum payoff they could have generated by lying brazenly (Abeler et al., 2016). Two additional meta-analyses show a similar magnitude of lying (Gerlach, Teodorescu, & Hertwig, working paper; Kobis, Verschuere, Rand, Bereby-Meyer, & Shalvi, working paper; also see also Capraro, 2017). In the dyadic setting, we estimate lying to be between 48% and 78%, which is more than twice that of an individual setting.

# 4 Relationship development

To illustrate the behavioral patterns in our experiment, Figure S24 shows the dyadic relationships of three honest first movers in our experiment. The first mover depicted in Figure S24A shows a preference for having an honest partner. This person asked to switch every time the second mover matched the first mover's reported outcomes, suggesting (with high probability) the second mover was dishonest. Another first mover, depicted in Figure S24B, attempted to engage in ethical free-riding—intentionally benefiting from others' rule-violating behavior without violating rules oneself—but with little success. This first mover did not choose to switch even though the second mover matched almost any number the first mover had reported. Whereas the honest first mover wanted to stay with the dishonest partner, all second movers asked to switch, arguably seeking a dishonest partner. Finally, the third case shows a successful case of ethical free-riding. In Figure S24C, we observe an honest first mover who does not ask to switch even though the second mover reports the same outcome as the first mover in every round. Here, the second mover does not ask to switch, allowing the first mover to successfully benefit from the second mover's lies, while remaining honest.

In section 6 below, we further include the evolution of each dyadic relationship in the three different conditions. In all figures, the number indicates the reported outcome, the colors represent whether the report was honest or not (blue = honest, red = dishonest), and the frame surrounding the report represents which mover, first (top row) or second (bottom row), asked to switch (in the Choice condition).



**Figure S24.** Exemplary relationship dynamics in the Choice condition. Number indicates the reported outcome, colors represent whether the report was honest (blue) or not (red), and the frame surrounding the report represents which mover, first (top row) or second (bottom row), asked to switch.

# 5 References

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# 6 All relationships

Below we provide all die-roll reports by participants in the three conditions. In the Choice condition, we further include switching requests. Number indicates the reported outcome, and colors represent whether the report was honest (blue) or not (red).

### **Choice condition**

Number indicates the reported outcome, colors represent whether the report was honest (blue) or not (red), and the frame surrounding the report represents which mover, first (top row) or second (bottom row) asked to switch.





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#### **Forced Switch condition**

Number indicates the reported outcome, colors represent whether the report was honest (blue) or not (red).






















































## Forced Stay condition

Number indicates the reported outcome, colors represent whether the report was honest (blue) or not (red).

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