TITLE

Longitudinal Evidence that Experience with Anonymous Interactions Reduces Intuitive Cooperation

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Why would people pay costs to deliver benefits to anonymous strangers in one-shot interactions? The Social Heuristics Hypothesis (SHH) claims that cooperation is intuitive because it is positively reinforced in everyday life, where behavior typically has reputational consequences^[1, 2]. Consequently, participants will cooperate in anonymous laboratory settings unless they either reflect on the one-shot nature of the interaction or learn through experience with such settings that cooperation does not promote self-interest. Experiments reveal that cognitive-processing manipulations (which increase reliance on either intuition or deliberation) indeed affect cooperation^[3, 4], but may also introduce confounds^[5, 6]. Here, we elide the interpretation issues created by between-subjects designs in showing that people are less cooperative over time in laboratory paradigms in which cooperation cannot promote self-interest, but are just as cooperative over time in paradigms that have the potential to promote self-interest. Contrary to previous findings^[7-9], we find that cooperation is equally intuitive for men and women: Unilateral giving did not differ across gender at the first study session, and decreased equally for both genders across sessions.

Researchers testing the SHH typically measure cooperation using economic game paradigms. Two of the most prominent economic game measures are the Dictator Game^[10] (DG) and Trust Game^[11] (TG). The DG involves two participants—a “Dictator” and a “Recipient.” The Dictator is given a monetary endowment, and decides whether to send money to the Recipient, who passively receives the Dictator’s monetary transfer. The game is typically played anonymously and for one round only. Although many researchers have used the DG to test the SHH,^[12, 13] there is some controversy over its suitability for this purpose. Rand, for his part, did not include

the DG in his meta-analysis of experiments testing the SHH because sharing money cannot maximize long-term, material self-interest in the DG even if the game is played with the same interaction partner on a repeated basis^[3]. Rand and colleagues instead regard the DG as a measure of altruism (i.e., unilaterally providing resources to others), and posit that altruism is intuitive only for women because, they argue, gender norms encourage women but not men to engage in self-sacrifice^[7]. However, some evidence indicates that participants interpret the DG as a situation in which they must decide whether to fairly divide a windfall with other people with whom they could conceivably interact in the future^[14-16]. Participants may therefore believe that their decisions in the DG have consequences to the extent that they may interact with the Recipient again *in some other context besides another DG*. If so, then participants may apply heuristics to the DG that dictate behaving fairly in anticipation of future interactions in which they do not have total control over the distribution of resources^[17, 18]. Under this supposition, the SHH would predict that both men and women should transfer less in the DG after learning through experience that decisions in the game have no reputational consequences.

The TG also involves two participants—a “Truster” and a “Trustee,” but both participants start the game with endowments of the same amount. The Truster can transfer money to the Trustee, and any money transferred is multiplied by a constant before the Trustee receives it (we dub this decision TGsend). After that, the Trustee can send none, some, or all of the money received to send back to the Truster (we dub this decision TGreturn). The TG is also typically played just once with anonymous partners. The SHH does not predict that reflection on or experience with the TGsend will reduce cooperation because Trusters can benefit from transferring money even when the game is played just once (provided that the Trustee returns more money than the

Truster sent). In contrast, the SHH does predict that reflection on or experience with the TGreturn will reduce cooperation because in one-shot TGs choosing to keep all of the money received from the Truster always maximizes the Trustee's income.

A few studies have tested predictions of the SHH across time and gender. For instance, researchers have found that previous experience playing economic games attenuates the effect of cognitive-processing manipulations on cooperation^[19, 20], indicating that people learn not to act on habit in one-shot settings. Also, in support of assertions that DG transfers should be intuitive only for women, studies suggest that women typically transfer less in the DG^[9], and that the positive effect of time pressure on DG transfers are statistically significant only for women^[7].

However, to show definitively that game transfers differ across time and/or gender, researchers must first demonstrate that economic game measures are measurement invariant (MI) across time or gender. MI refers to an isomorphism in how a given construct relates to a measure of that construct across different circumstances^[21]. Violations of MI invalidate attempts to compare behaviour across the parameter of interest, including how scores change over time or across gender. Establishing MI requires showing that (a) the construct has the same factor structure, and is (b) interpreted and (c) responded to in the same way across the categories of interest (e.g., gender, time, etc.). Violations of MI in economic games could occur for any number of reasons, including different levels of game comprehension or different motivations underlying decision-making across groups or over time. Testing for MI requires fitting a latent variable using a structural equation model or item response theory model, and it takes at least three statistically independent measurements to generate unique parameters estimates for a latent variable. A likely

reason why researchers have yet to establish that economic games exhibit MI across time and gender is that they typically have participants play only one or two rounds of a given game.

In the present study, 276 participants (191 women; $M_{\text{age}} = 21.67$, $SD_{\text{age}} = 7.47$) played six rounds of the DG, TGsend, and TGreturn. Each round of each game was played with a different, anonymous interaction partner. After participants learned how much they earned from playing all 18 games, they watched videos made by Oxfam, Feeding America, and International Rescue Committee in random order. After watching each video, participants decided how much their game earnings to donate to the charity that made the video. 200 participants returned to complete the same tasks at a second study session ($M = 3.89$ weeks later, $SD = 5.03$ weeks later).

We first sought to ensure that our measures possessed sufficiently strong psychometric properties to test predictions of the SHH. We conducted a series of Welch's independent samples *t*-tests to examine whether average DG, TGsend, TGreturn, and charity transfers at the first session differed as a function of whether participants attended a second session. None of these tests attained statistical significance, suggesting that attendees of the second session did not seem to represent a biased subset of the Time 1 participants: DG, $t(142) = .25$, $p = .788$, $d = .03$; TGsend, $t(131) = .24$, $p = .810$, $d = .03$; TGreturn, $t(126) = 1.10$, $p = .271$, $d = .15$; Charity, $t(121) = 1.16$, $p = .247$, $d = .16$. Next, we confirmed that the games possessed high internal consistency (alphas $\geq .91$ at both sessions, see Table 1). Next, we established measurement invariance across time and gender (see Methods and Table 3), allowing us to make valid comparisons of mean economic game transfers. Finally, we created a structural equation model in which DG, TGsend, TGreturn, and charitable giving during the first session predicted their second session counterparts. The

results confirmed that the rank-order of transfers in the games and charity measure were stable across study sessions ($r_s = .60$ -.71, $p_s < .001$; see Table 2).

Table 1. *Descriptive Statistics of Behavioral and Verbal Measures*

	<i>N</i>	Mean	SD	Alpha
DG- Time 1	276	.36	.24	.93
DG- Time 2	200	.29	.24	.95
TGsend-Time 1	271	.50	.32	.97
TGsend- Time 2	194	.49	.34	.96
TGreturn- Time	269	.42	.23	.92
TGreturn- Time	194	.36	.25	.94
Charity- Time 1	270	.54	.40	--
Charity- Time 2	194	.38	.40	--

Notes: *N* reflects the number of participants that had at least partial data for the measure. The means and standard deviations for the behavioral measures are percentages.

Table 2. *Test-Retest Reliability of Economic Games and Charitable Giving.*

	b	SE	Z	p	r
DG Time 1	.74	.09	8.13	<.001	.60
TGsend Time 1	1.00	.10	9.55	<.001	.71
TGreturn Time 1	.80	.11	7.54	<.001	.63
Charity Time 1	.64	.05	11.91	<.001	.64

Note: Regression results are from a structural equation model. Charitable giving and the week gap between sessions were manifest variables, while the economic games were latent variables. Each Time 1 variable is predicting its Time 2 analog. Model Fit: $\chi^2(651) = 1166.702$, $p < .001$; SRMR = .089; RMSEA = .063, 90% CI [.057, .069]; CFI = .940.

Table 3. Invariance testing across gender and time for each economic game outcome.

Invariance Comparison	Invariance Model	$\chi^2(df), p$	CFI	RMSEA [90% CI]
<i>Dictator Game</i>				
Gender Time 1	Configural	55.628(18), $p < .001$	0.97	0.124 [0.088, 0.161]
	Metric	Δ 11.04 (6), $p = 0.087$	0.966	0.114 [0.082, 0.147]
	Scalar	Δ 3.092 (5), $p = 0.686$	0.967	0.101 [0.071, 0.132]
Gender Time 2	Configural	134.042(18), $p < .001$	0.933	0.258 [0.218, 0.3]
	Metric	Δ 5.549 (6), $p = 0.476$	0.933	0.223 [0.188, 0.259]
	Scalar	Δ 5.575 (5), $p = 0.35$	0.933	0.203 [0.171, 0.237]
Longitudinal	Configural	161.084(53), $p < .001$	0.963	0.086 [0.071, 0.101]
	Metric	Δ 7.664 (6), $p = 0.264$	0.962	0.082 [0.068, 0.097]
	Scalar	Δ 4.133 (5), $p = 0.53$	0.963	0.079 [0.065, 0.093]
<i>Trust Game Send</i>				
Gender Time 1	Configural	70.651(18), $p < .001$	0.974	0.147 [0.112, 0.184]
	Metric	Δ 7.449 (6), $p = 0.281$	0.973	0.129 [0.098, 0.162]
	Scalar	Δ 5.096 (5), $p = 0.404$	0.973	0.118 [0.089, 0.148]
Gender Time 2	Configural	99.51(18), $p < .001$	0.959	0.218 [0.177, 0.26]
	Metric	Δ 6.377 (6), $p = 0.382$	0.958	0.189 [0.153, 0.227]
	Scalar	Δ 4.295 (5), $p = 0.508$	0.959	0.171 [0.138, 0.206]
Longitudinal	Configural	161.457(53), $p < .001$	0.974	0.086 [0.071, 0.102]
	Metric	Δ 3.303 (6), $p = 0.77$	0.975	0.081 [0.066, 0.095]
	Scalar	Δ 5.591 (5), $p = 0.348$	0.975	0.078 [0.064, 0.092]
<i>Trust Game Return</i>				
Gender Time 1	Configural	36.013(18), $p = 0.007$	0.981	0.087 [0.044, 0.128]
	Metric	Δ 9.574 (6), $p = 0.144$	0.978	0.082 [0.044, 0.118]
	Scalar	Δ 4.088 (5), $p = 0.537$	0.979	0.073 [0.036, 0.107]
Gender Time 2	Configural	53.405(18), $p < .001$	0.962	0.144 [0.1, 0.189]
	Metric	Δ 11.765 (6), $p = 0.067$	0.956	0.134 [0.096, 0.174]
	Scalar	Δ 6.348 (5), $p = 0.274$	0.954	0.124 [0.088, 0.16]
Longitudinal	Configural	105.588(53), $p < .001$	0.973	0.06 [0.043, 0.077]
	Metric	Δ 6.361 (6), $p = 0.384$	0.972	0.057 [0.041, 0.073]
	Scalar	Δ 3.702 (5), $p = 0.593$	0.973	0.054 [0.038, 0.07]

To test predictions of the SHH, we ran separate confirmatory factor models for each economic game (see Table 4). Latent means for women at Time 1 were constrained to zero and all other latent means (for women at Time 2 and men at both times) were free to vary. For the DG, men did not differ from women in the amount sent at Time 1, $b = .11$, $se = .14$, $p = .409$. Both men (b

= -.42, $se = .12$, $p < .001$) and women ($b = -.24$, $se = .08$, $p = .003$) sent less at Time 2 than at Time 1. There was no difference across genders in the reduction in DG transfers over time, $b = .18$, $se = 0.15$, $p = 0.204$.

Table 4. Estimates of Latent Mean Differences Across Gender and Time.

		Baseline		Change	
		Mean (SE)	<i>p</i>	Mean (SE)	<i>p</i>
Dictator Game	Female	0.00 (REF)	NA	-0.235 (.078)*	0.003
	Male	0.112 (.136)	0.409	-0.419 (0.123)*	0.001
	Gender Difference in Change: $b = 0.184$, $se = 0.145$, $p = 0.204$				
Trust Game Send	Female	0.00 (REF)	NA	-0.009 (.072)*	0.902
	Male	0.414 (.134)	0.002	-0.045 (0.099)	0.296
	Gender Difference in Change: $b = 0.036$, $se = 0.123$, $p = 0.269$				
Trust Game Return	Female	0.00 (REF)	NA	-0.217 (.068)*	0.001
	Male	0.031 (.138)	0.823	-0.262 (0.140)*	0.061
	Gender Difference in Change: $b = -0.045$, $se = 0.155$, $p = 0.772$				

Men sent more than women in the TGsend at Time 1, $b = 0.41$, $se = 0.13$, $p = .002$. Men also sent more in the TGsend at Time 1 than did women at Time 1, $b = .37$, $se = 0.15$, $p = .011$. This finding indicates that men sent more than women in the TGsend at both timepoints, as women did not send less money at Time 2 than at Time 1, $b = -.01$, $se = 0.07$, $p = 0.902$. Men also did not send less money in the TGsend at Time 2 than at Time 1, $b = -.05$, $se = .10$, $p = .296$, indicating that neither gender reduced their level of cooperation at the second session.

Men's transfer decisions in the TGreturn did not significantly differ from women's transfers decisions at Time 1, $b = 0.03$, $se = 0.14$, $p = 0.823$. Men transferred marginally less money at Time 2 compared to Time 1, $b = -0.26$, $se = 0.14$, $p = 0.061$. Women had a clear, significant reduction in transfers Time 2 compared to Time 1, $b = -.22$, $se = 0.07$, $p < 0.001$. The reduction

in transfers at the second session did not differ across genders, $b = -.05$, $se = 0.16$, $p = 0.772$. The only marginally significant decrease in males' returns was likely a result of the smaller sample size for men.

To see whether our results would extend beyond economic games, we examined whether charitable giving differed across gender or study session. Charitable giving is relevant to the SHH for the same type of reason that the DG is relevant: People may intuitively help others in need because in everyday life they can attract social partners by demonstrating a willingness to act selflessly^[22]. The three indicators of charitable giving were not independent because participants were told in advance that they would use their game earnings to make decisions about whether to donate to three different charities. Because we could not factor analyze the charity decisions, we instead, we examined the (logit-transformed) percentage of total earnings that participants donated to all three charities combined. To test whether gender predicted the degree to which charitable giving changed over time, we regressed participants' difference scores (charitable giving during second session – charitable giving during first session) on gender (0 = Men; 1 = Women). The significant intercept indicated that men gave less during the second session ($M = 29\%$, $SD = 38\%$) than during the first session ($M = 50\%$, $SD = 43\%$), $b = -1.39$, $se = 0.30$, $p < .001$. The non-significant effect of gender indicated that women also gave less during the second session ($M = 42\%$, $SD = 41\%$) than during the first session ($M = 56\%$, $SD = 39\%$), and that their reduction over time did not differ from men's reduction over time, $b = 0.52$, $se = 0.35$, $p = 0.137$. However, we counsel caution in interpreting these results until the measurement invariance of the charitable giving measure can be demonstrated.

Overall, the present findings strongly support the SHH: Familiarity with one-shot social interactions decreased cooperation, but only in paradigms in which cooperation cannot possibly maximize personal material welfare (i.e., the DG, TGreturn, and the charitable giving task, but not the TGsend). However, we emphasize that our conceptualization of what paradigms are relevant to the SHH differ from some other authors. The SHH asserts that people internalize a disposition to cooperate because factors common to everyday life, such as large benefits from reciprocity relationships and threats of ostracism for noncooperation, make cooperativeness a safer bet in most situations^[1, 2]. Curiously, however, some versions of SHH do not regard tasks in which participants make unilateral transfers in one-shot settings as directly relevant because cooperation would not yield *material* benefits even if the game occurred on a repeated basis.^[3] Instead, researchers explain cooperation in paradigms such as the DG by positing self-sacrifice norms that differentially apply to women who adhere to traditional gender roles.^[7] We believe that the SHH should predict that both men and women would intuitively make unilateral transfers in the DG and charitable giving tasks because in everyday life people try to avoid appearing unfair or callous^[15, 23]. This conceptualization of the SHH was supported here: Experience with one-shot social interactions decreased cooperation not only in the TGreturn—a paradigm in which cooperation could maximize personal material welfare if the interaction occurred repeatedly with the same partner—but also in the DG and the charitable giving task. These mean differences across sessions were not qualified by gender. More generally, the only clear gender difference we observed was that women transferred less in the TGsend.

Much research on the SHH has measured cooperation using the public goods game (PGG), an economic games in which participants increase the size of a public monetary fund at a net cost

when they spend money from their own individual endowments^[1, 20]. The PGG loads on the same latent variable as the DG, TGsend, and TGreturn in factor analysis^[24] and participants cannot maximize personal welfare when the game is played for just one round. Thus, testing whether PGG contributions decrease across study sessions would provide an important robustness check on our results.

Experimental research on the SHH assigns participants to either a “time-pressure” condition in which participants must make their decisions within a short time window (thereby increasing reliance on intuition), or to a “reflection” condition in which they must wait to make their decision (thereby increasing reliance on deliberation)^[3]. Although we believe that the preponderance of experimental evidence supports the SHH^[4, 25], we also think that our results illustrate the complementary benefits of longitudinal designs. As researchers have noted, a large proportion of participants do not comply with the time-pressure manipulation, and including noncompliant participants in analyses typically eliminates condition differences in cooperation^[6]. It is possible that excluding noncompliant participants creates spurious group differences because the time pressure group has more participants who are willing to comply with difficult demands (who also happen to be more cooperative), or because the time pressure group has more fast responders (who are more cooperative in most studies). In contrast, our effects were not likely affected by traits that increase compliance with experimenters’ wishes: Following through on the commitment to attend a second session was not related to cooperative behaviour at the first session. Between-subjects differences in response time also cannot explain our results, as our results revealed a within-subjects effect of experience on cooperation.

This latter point speaks to a general advantage of our longitudinal design: SHH makes a claim about how cognitive processes *within* a person affects his or her social behaviour. Within-subjects designs are necessary to verify that the processes that distinguish cooperators from noncooperators are the same processes that distinguish cooperative decisions from uncooperative decisions within the same person^[26]. To illustrate this point, consider the between-subjects finding that previous experience with laboratory social dilemmas attenuates the effect of cognitive-processing manipulations on cooperation^[19, 20]. It is possible that experienced participants differ from unexperienced participants in some other variable that affects cooperation. For example, perhaps more experienced participants are more dependent on psychology studies for their income, giving them a greater incentive to behave selfishly. In contrast, our study compared participants to their less experienced selves from a few weeks or months prior. Although events in between sessions could in principle increase selfishness at the second session, it is difficult to conceive of a plausible historical variable that would have consistently affected participants in the same way over the 2.5 year over which our data were collected. Overall, even though our longitudinal design does not provide direct causal evidence in the favor of the SHH, the pattern of findings provides novel confirmatory evidence that does not have any other obviously equally compelling explanation.

To sum up, our design found evidence for the SHH by providing participants an opportunity to learn how to advance self-interest in an anonymous laboratory setting. Specifically, the experience of collecting payment after the first study session gave participants clear evidence that there were neither monetary nor reputational benefits to cooperating in the DG, TGreturn, and charitable giving task. Moreover, participants had weeks to reflect on this knowledge before

bringing it to bear on their decisions during the second study session. The results were consistent with the hypothesis that increased appreciation for the anonymous nature of one-shot settings reduces unilateral transfers in both men and women because participants have adjusted their decision-making criteria to match a setting in which noncooperation (a) increases material welfare in the long run, and (b) does not incur negative social evaluation.

Methods

Recruitment

276 participants were recruited from a middle-sized southeastern U.S. university ($n = 221$) and in several locations around the county in which the university is located ($n = 55$). Participants were offered a \$10 show-up payment for each session they attended, as well as opportunities to earn more money during the study sessions. Although all 276 participants were signed up for a second session, 200 participants actually returned to the laboratory for a second session in which they completed the behavioral measures again (i.e., the DG, TGsend, TGreturn, and charitable donations). By collecting 200 participants at both sessions, we achieved our planned sample size and had 80% power to detect small mean differences ($d = .20$) across sessions. We tried to schedule the second session at least two weeks after the first session ($M = 3.89$ weeks, $SD = 5.03$), but seven participants could attend a second session only if scheduled within the week following the first session. Readers can view the preregistration of our planned sample size and protocol at <https://osf.io/siqyz/>.

Procedure

There were 55 sessions with between four to fourteen participants ($M = 8.75$, $SD = 2.26$).

Participants were each brought to a private carrel with a computer. They wore headphones to

hear audio recordings of the instructions, which were simultaneously displayed in text on the computer screen. The protocol was run entirely through a computer script using E-Prime^[27]. See Table 1 for descriptive statistics of all measures described below.

Tasks

As the computer script began, participants were told that they were going to play some games with the other participants, and that the games would provide an opportunity to earn real money. Furthermore, they were told that all game decisions would be anonymous and that they would play any given game with a specific partner only once (i.e., they played a series of one-shot economic games). Participants were then given instructions about how to play the DG (which was described as “Game 1” to the participants). Participants were told that they would play multiple rounds of the DG as Decision-Maker (though they were not told how many). They also learned that they act as Recipients for other participants, although they would not observe the outcome of these rounds. Finally, participants were told that Recipients would not receive feedback on how much money they received after each round, but instead, would learn how much they had earned over the course of all the games after they had completed the session. Participants then played six rounds of the DG. In these games, the Dictator had an endowment of \$0.50 in the first, second, and fifth rounds, and \$1.00 in the third, fourth, and sixth rounds. Transfer options ranged between \$0.00 and the endowment amount in \$0.05 increments.

Next, participants learned how to play the TGsend (referred to as “Game 2” in the experiment), which involved a “First Mover” (the Truster) and a “Second Mover” (the Trustee), both of whom received equal endowments (see below). Participants were told that they would play multiple rounds of the game (though not how many) as the First Mover. Participants were also told that

Second Movers would be allowed to transfer only money that they received, not money from their original endowment. Because knowledge of an upcoming opportunity to play as the Trustee influences Truster's decisions^[28], we did not tell participants that they would later act as Second Movers in response to decisions that others made as First Movers. Participants played six rounds of the TGsend. The Truster had an endowment of \$0.40 in the first round, \$0.80 in the second round, \$0.60 in the third round, \$0.30 in the fourth round, \$0.70 in the fifth round, and \$0.50 in the sixth round. Transfer options ranged between \$0.00 and the endowment amount in \$0.05 increments. Transfers from the Truster were multiplied by three before being put in the Trustee's account.

After participants had made six decisions as First Mover, they moved onto the TGreturn. Specifically, participants learned that they would serve as Second Movers by responding to the transfer decisions of six different First Movers. Participants then proceeded to complete each of those six rounds as Second Mover. In cases where the First Mover had not transferred any money to them, the screen merely reported this outcome and participants moved onto the next round. The raw scores for the TGreturn are not comparable across participants because different participants started with different amounts of money. Thus, we transformed scores to percentages. A Shapiro-Wilks test revealed that the TGreturn percentage distribution departed from normality, but not severely, $W = .981, p < .001$. A logit-transformation did not improve normality, so we analyzed untransformed percentages instead.

We took several steps to ensure that the economic game data were of high quality and met a few conventions of the field of experimental economics. First, there was no deception involved in the

economic games. During the 10 sessions in which fewer than seven participants were present, we told participants either that some of their interactions would be repeated (but that they would not know which ones), or that some of their interactions would be with a research assistant who was making random decisions (although that they would not know which ones those were). Second, to prevent participants from clicking through the instructions without paying attention, the computer program did not allow participants to continue through the instructions until the audio recording of the instructions had ended. Third, participants watched an example round of each game after they had read the instructions for that game. Last, to minimize error variance from accidental clicks or failures of comprehension, the program did not automatically enact participants' transfer decisions. Instead, participants were shown the monetary consequences of their transfer decisions, and were then forced to confirm that they indeed wanted the presented outcomes for themselves and the interaction partner.

After completing the economic games and self-report measures that will be described in another paper, participants learned how much they had earned from the economic games. They were then told that they would watch videos from three different charities (Oxfam, Feeding America, and International Rescue Committee), and that after each video they would have the opportunity to donate as much money from their game earnings as they wished (in \$0.05 increments) to each of the three charities. Participants were not allowed to use their show-up payment to donate to the charities. Charity videos were each about two minutes long and were presented in random order. Because the amount that participants had available to donate was not independent across charity decisions (this lack of independence was made known to participants up front), we do not regard the three charitable donation decisions as statistically independent. Instead, we regarded each

participant's charitable giving score as the percentage of their total earnings they donated to all three charities in total. We logit-transformed these charity percentage scores, which improved their normality.

Participants alerted a research assistant when they completed the study. Then, they were clandestinely awarded the amount of earned money they had chosen not to donate to the three charities, rounded up to the nearest dollar, plus the \$10 show-up payment. We donated the money that participants had allotted to each charity in lump sums after the study was completed.

Measurement Invariance

We used Mplus to assess measurement invariance and differences across sessions and gender.^[29]

We used multi-group confirmatory factor analysis to assess whether the Time 1 game data exhibited MI across gender and time. We used full information maximum likelihood estimation to handle missing data (the only causes of missing data within laboratory sessions were computer crashes). Testing for latent group mean differences is valid when the measure is scalar invariant (i.e., the model has the same factor structure, factor loadings, and item intercept values across groups). We assessed invariance by observing change in model fit after sequentially constraining factor loadings and then item intercepts equal across groups.

We created multi-group Confirmatory Factor Analysis (CFA) models separately for each of the economic games with gender as the group factor. Absolute model fit was adequate, albeit with high RMSEA values, and fit did not get worse upon sequentially constraining factor loadings and intercepts across gender (see Table 3). Thus, the DG, TGsend, and TGreturn all evince scalar invariance across gender, which enabled us to compare latent means. Next, we assessed

longitudinal invariance for each economic game. We first fit unidimensional models to Time 1 and Time 2 data separately to confirm configural invariance. We then constrained Time 1 and Time 2 factor loadings and intercepts equal in a multi-group CFA to test for metric and scalar invariance, respectively. The DG, TGsend, and TGreturn models all exhibited scalar invariance.

Code Availability. All syntax used for performing the analyses are available at

<https://osf.io/inkv3/>.

Data Availability. All data and computer program scripts are available at <https://osf.io/inkv3/>.

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AUTHOR CONTRIBUTIONS

M.E.M., W.H.B.M., E.J.P., and D.E.F conceived of the study. E.J.P., D.E.F, and W.H.B.M. programmed the study, and W.H.B.M. managed data collection. D.E.F. and W.H.B.M. analyzed the data and interpreted results. W.H.B.M. drafted the manuscript. W.H.B.M., D.E.F, E.J.P., and M.E.M. revised the manuscript.

ADDITIONAL INFORMATION

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COMPETING INTERESTS

The authors declare no competing interests.