A Within-Subject Analysis of Other-Regarding Preferences*

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Abstract

In this paper we assess the predictive power of inequity aversion. We run four different experiments (an ultimatum game, a dictator game, a sequential prisoner's dilemma and a public-good game) with the same cohort of experimental subjects. This allows us to make intra-personal comparisons across the four games. We use the responder data from the ultimatum game in order to estimate a parameter of negative inequity aversion, and we take data from a modified dictator game to estimate a parameter of positive inequity aversion. We then use this joint distribution to test several hypotheses about individual behavior in the other games. Our results show that the inequity aversion model has considerable predictive power at the aggregate level but fails almost entirely at the individual level.

JEL Classification numbers: F13, L13, C92.

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1 Introduction

Behavioral economists are currently investing substantial efforts into developing models of social or other-regarding preferences. The starting point of this literature is that data from, for example, the ultimatum game (Güth et al., 1982), the dictator game (Kahneman et al., 1986; Forsythe et al., 1994) and the trust game (Berg et al., 1995) are by and large inconsistent with the economics paradigm of selfish utility maximization. By relaxing the assumptions of the standard model and allowing for other-regarding motives, the behavioral models attempt to better explain these experimental results.

Models of other-regarding preferences have to take into account that behavior can be quite heterogenous among participants. A robust finding of various experimental studies is that not all participants behave alike. Not everybody behaves perfectly selfish as the traditional model asserts but not all subjects behave perfectly, say, altruistic either. The heterogeneity of behavior has implications on how hypotheses are set up in models of other-regarding preferences. A typical prediction is to claim how the distribution of behavior is expected to vary (or stay constant) between two or more games. An example would be the hypothesis that "subjects should be more likely to behave altruistically (in some well-defined way) in game A compared to game B". The experimental evidence in favor or against a behavioral model depends on whether or not such comparisons of heterogenous behavior across games are in line with the model's predictions.

The starting point of this paper is that predictions of this kind can be tested in two ways. The existing literature has, according to our knowledge, almost exclusively relied on *aggregate-level* or unrelated-sample experimental data. In the above example, the hypothesis is simply tested by comparing the share of altruistically behaving subjects in game A and B. The data of the two games come from different cohorts. If there is a higher share of altruistically behaving subjects in A, the hypothesis is confirmed. However, the same hypothesis can also be tested with *individual-level* or related-sample data. In a related-sample of game A and game B decisions, the experimenter can analyze each individual's behavior in the two games with respect to the hypothesis. The test would be whether, as predicted, each single individual behaves more altruistically in game A.

We believe that such individual-level data tests of theories of other-regarding preferences are crucial regarding the behavioral validity of the models. All such models we are aware of explicitly make individual-level predictions. They are emphatically models of individual behavior since they follow the general microeconomic approach of individual utility maximization. Sure enough, theories of other-regarding preferences also make predictions about the population as a whole but these predictions only result from predictions on individual behavior. The other-regarding preference models are generally considered to by and large correctly predict aggregate outcomes across several games. While this constitutes remarkable progress in the interpretation of recent experimental findings, we believe this predictive success is incomplete without individual-level data support.[1]

The lack of a comprehensive individual-level data study of other-regarding preference theories is indeed somewhat surprising. Two prominent papers argue in favor of it. Fehr and Schmidt (1999, p.847) explicitly welcome this approach as "one of the most interesting tests of our theory". Similarly, Andreoni et al. (2003) argue that the comparison of aggregate and individual-level data "gives a new and interesting dimension to the analysis of experimental data". What is more, individual-level experimental data can easily be produced. In fact, any related-sample design allows the analysis of individual behavioral patterns. To test an individual theory with aggregate-level data is a plausible way of proceeding when individual-level data are not available or not reliable (for example, data on voting). This is not the case with, for example, bargaining experiments.

The main novelty of this paper is to provide a first[2] systematic individual-level data test of a behavioral theory. The behavioral theory we analyze is a model of inequity aversion. This model was first proposed by

[1] Upfront, we should concede that, while we believe the kind of test we suggest is crucial in assessing the validity of behaviroral theories, we also think that it is a tough test. A model's performance at the individual level could easily turn out to be less impressive than that at the aggregate level. To see this, suppose that a theory accurately predicts at the aggregate level that, say, subjects who exploit the first mover in the trust game will not contribute in a public-good game. At the individual level, surely, some violations will occur. It seems unlikely that literally all subjects who exploit the first mover in the trust game will not contribute to the public-good—if only for the reason that behavior in experiments is always noisy to some extent. In our data analysis below, we will therefore impose the much more modest requirement that a theory predicts individual behavior in the right direction. That is, subjects who exploit in the trust game should simply be less likely to contribute to the public-good. In our view, this minimum requirement should be met for a theory to claim validity on an individual level.

[2] Andreoni et al. (2003) do conduct an individual-level comparison in their paper. However, they analyze only one game (a modified ultimatum game where subjects play both the proposer and the responder role), and the individual-level comparison of decisions is only an aside of the analysis. Charness and Rabin (2002) conduct a related-sample experiment where subjects

Bolton (1991) and was later refined by Fehr and Schmidt (1999), Bolton and Ockenfels (2000). Basically, inequity aversion stipulates that individuals do not only care about their own material payoff but also about the payoffs of others. In particular, individuals dislike both having a lower as well as a higher payoff as others, and so, all else equal, an equal distribution of payoff maximizes their utility. The inequity model is very popular among experimental economists (judging from citation figures) and also apparently successful. Even though the model fails in some experiments, the more recent extended behavioral models that aim at explaining results in these experiments nevertheless include to different degrees some concerns for equity.[3]

In the main part of paper, we will test the model of inequity aversion by Fehr and Schmidt (1999, henceforth F&S). Their model has the advantage of a straightforward parametrization that can be easily estimated. Furthermore, F&S has been quite successful in rationalizing the behavior in many classical games. We run four different experiments (an ultimatum game, a modified dictator game, a sequential prisoner's dilemma and a public-good game) with the same cohort of experimental subjects. We then use the responder data from the ultimatum game in order to estimate a parameter of aversion towards disadvantageous inequity, and we take data from a modified dictator game[4] to estimate a parameter of aversion towards advantageous inequity. Because our paper is the first to pursue an appropriate related-sample data set, it is also the first to report a joint distribution of individual inequity parameters. We then use this distribution to derive seven explicit hypotheses about individual behavior in the other games.

Our results show that the model has considerable predictive power at the aggregate level but fails almost entirely at the individual level. With one exception, the model does not predict the right correlation of decisions. That is, whether or not an individual exhibits strong aversion against inequity has very little explanatory power in other games.

had to play two or four different games. They explicitly mention that they did not conduct an individual-level data analysis (see page ?).

[3] For example, Rabin [1993]; Levine [1998]; Dufwenberg and Kirchsteiger [2004]; Falk and Fischbacher [forthcoming]; Cox, Friedman and Gjerstad [2004]).

[4] The original dictator game cannot be used to obtain a point estimate of the positive inequiry parameter. See Fehr and Schmidt (1999, p. ?). Our modification (see below), by contrast, does yield a point estimate.

2 Experimental design

We ran four different two-person games of roughly similar complexity with the same cohort of experimental subjects. We kept the initial total surplus at £20 across all games. Each game was played once by each subject. In two of our games, there is more than one decision node. In these cases, we asked subjects for decisions at all nodes by employing the so-called strategy elicitation method. In total, every subject made seven decisions.

Each section of the experiment, consisting of one of the games, was presented separately. Instructions were distributed and were also read aloud in each of the four parts by the experimenter and participants had the chance to ask questions. Once the experimenter was convinced everyone had understood the game, the corresponding computer screen was displayed and subjects submitted their decisions. Only when all the participants in the session had made their decisions in one game were the instruction for the following game distributed.

Subjects did not receive any feedback or payment until the end of the experimental session. All decisions were to be done without any information on other subjects' choices and without any communication. At the end of the experiment, one game was chosen randomly and subjects were randomly matched in pairs. In all games except for the public good game (which is symmetric), the roles in the game were determined randomly between the two subjects of each pair. The payment to the subjects was determined by the single decision pair in the one randomly chosen game only. Subjects knew this procedure in advance and the computer screen at the end of the experiment informed them about all the random draws of the computer and also about the decisive pair of decisions. We believe that our design is appropriate to minimize confounding effects between games and to avoid that subjects average their earnings across games.[5] In this respect, our design is identical to the one in Charness and Rabin (2002). See their paper for a further discussion of issues arising due to the related-sample design.

When selecting the games for our experiment, we wanted to make sure that we include the ones most relevant in the other-regarding preferences literature. Therefore, we chose the ultimatum game, the dictator

[5] Alternatively, we could have invited subjets to four separate sessions, with some time gap between the sessions. With such a design, inevitably, some subjects do not show up for some sessions causing the data to be incomplete. More importantly, the emotional state on each of these separate sessions may differ and confound the data.

game, and the public-good game. The trust game is also rather frequently discussed in the literature. Conducting a standard trust game would either require to give feedback, which would violate our design approach or to use the strategy method for second movers, which would make the game rather complex. We hence used instead a sequential prisoner's dilemma. This is essentially a simplified trust game reduced to two decisions for each players. It has the same essential qualitative properties as the trust game (in terms of trust and trustworthiness) but is much simpler and therefore more suitable for our purposes.

We also had to decide on the number of games to be played. Four seemed a reasonable compromise to us between generating a rich data set and maintaining salient incentives (Smith, 2002). With a higher number of games and decisions, we might have risked that subjects did not care about each individual decision any more. Coincidently, Charness and Rabin (2002) also ran a maximum of four games per subjects in their experiments.

We now introduce the four games as implemented in our experiments. The ultimatum game (henceforth UG) (Güth et al., 1982) is a sequential two-stage game. Given a pie of $\pounds 20$, the proposer has to make an offer (£z) to the responder, keeping £20− £z to himself. The responder can accept or reject the offer. In the case of a rejection both players earn zero. If the responder accepts, players get £20− £z and £z, the outcome proposed, respectively. As mentioned, we let subjects decide as both proposers and responders. This requires that the responder decisions can only be made based on a menu of hypothetical offers (this is the aforementioned strategy elicitation method). That is, when deciding as the responder, subjects had to accept or reject a complete list of every possible distribution of the pie, starting from £20-£0, £19-£1, £18-£2, ... all the way to £0-£20. That is, there were 21 different distributions to decide upon. If the ultimatum game was selected as the game relevant for the final payment to subjects, the proposer's actual offer was compared to the responder's decision on this offer and payments were finalized according to the rules of the ultimatum game.

In the traditional dictator game (Kahneman et al., 1986; Forsythe et al., 1994), the dictator unilaterally determines how to divide a fixed amount of money ($\pounds 20$ in our case) between himself and the recipient. The distribution chosen by the dictator is final. As mention above, the standard dictator is not suitable to yield a point prediction of the parameter measuring aversion against advantageous in equality. Therefore, we modified the dictator game (henceforth MDG) in the following way. The dictator has to decide about how much of the initial pie of £20 (if any) he is at most willing to sacrifice in order to achieve an equal distribution of payoffs. More specifically, subjects were given a list of 21 pairs of payoff vectors, and they had to choose one of the two payoff vectors in all 21 cases. The left payoff vector was always £20-£0, that is, if the left column was chosen, the dictator would receive £20 and the recipient nothing. The right payoff vector contained equal payoffs of £x-£x where $x \in \{0, 1..., 20\}$.[6] (Table ? in the instructions, see Appendix, indicate how this was visualized in the experiment.) If the ultimatum game was randomly selected at the end, one of the 21 payoff vector pairs was randomly chosen and then the dictator's decision determined the payments.

The sequential prisoner's dilemma (henceforth SPD) (Clark et al., 2001) is a prisoner's dilemma where one player moves first, the other player second. The first mover can cooperate or defect. After observing this action, the second mover responds either with cooperation or defection. If both defect, both player receive a payoff of £10. If both cooperate, they get £14 each. If one defects and the other player cooperates, players earn £17 and £7, respectively. As in the ultimatum game, subjects had to play both roles. They had to make two second-mover decisions, one if the first mover decides to defect and one for the case of cooperation. When the SPD was selected as the game relevant for the final payment to subjects, one player was randomly made the first mover and the other subject the second mover. The computer took their SPD decisions and determined payoffs accordingly.

Finally, the public-good game (henceforth PGG) we used was a simple two-player voluntary contribution mechanism (see Ledyard, 1995, for a survey). The two players are endowed with £10 each. They simultaneously decide how much (if any) money from the endowment to contribute to a public good. Each monetary unit that the individual keeps for himself raises his payoff by exactly that amount. Both subjects

[6] In this modified dictator game, a purely selfish individual would always choose £20-£0 over £x-£x where $x \in \{0, 1..., 19\}$ and would be indifferent between £20-£0 and £20-£20. A dictator strongly disliking advantegoeus inequity, would choose the right column, no matter how small x. Subjects between these two extremes would be expected to switch from choosing the left column to the right column.

The reason why subjects with monotone preferences should switch at some some point x^* from choosing the left colums to the right column (if at all), is that the egalitarian outcome is "cheaper" for all decisions $x > x^*$. Hence, if somebody prefers £x^*-£x^* to £$20-$ £0, she should also prefer £$x+1$ - £$x+1$ to £$20-$ £0.

receive $m = £0.7$ for each £ contributed to the public good. Note that, when restricting actions to the extreme choices of zero and full contribution, the payoffs are the same as in the SPD. If the public-good game was chosen for the final payoffs, the computer simply calculated payoffs according to the contributions of each randomly paired player.

We implemented two different sequences in which the games were played. There are several reasons for the restriction to two sequences. First, because of the similarity of the games, we wanted to avoid that ultimatum game and dictator are played in a row. Second, because of length of the instructions and a control questionnaire, we wanted to do the public-good to be the last game. This leaves two possible sequencing variants with either the ultimatum game coming first and the dictator game coming third, or vice versa. The sequential prisoner's dilemma would be played as the second game, and the public-good would be last. This is only a small subset of the 24 possible sequencing variants, but to run sufficiently many repetitions of all variants does not appear to be feasible.[7] We did not find any significant differences between the two sequences and therefore we pool the data and refrain from further references to the sequences in the results section.

We ran six sessions with 12 subjects in each session. All 72 subjects were non-economists.[8] Sessions lasted about 50 minutes and the average earning was £11.

In the data analysis below, we discarded 11 out of those 72 subjects from the data set. The reason is that these subjects do not have a unique switching point in the MDG or no unique rejection threshold in the UG. Therefore, we cannot calculate their inequity parameters (see below) and decided to drop them from the analysis. Henceforth, we will deal with a total of 61 subjects.[9]

[7] Given that the ultimatum game and the sequential PD can be played in two and three sequences, respectively, we would even have to 144 different variants into account. See also Charness and Rabin (2002) who do not control for sequencing effects. Andreoni et al. (2002) do not control for alternative sequences either.

[8] See Fehr, Naef, and Schmidt (2006) and Engelmann and Strobel (2006) for a discussion whether economics majors may behave differently in distribution experiments.

[9] Holt and Laury (2002) elicit risk preferences with sets of binary choices similar to our UG responder decisions and our MDG. They also needed to drop several subjects from their analysis due to non-unique switching points.

3 Instrument check

In this section, we want to check whether the games we analyze below generate results similar to those of previous experiments. Such an instrument check[10] is essential for the significance of the main part of our analysis.

In the UG, proposers' mean offer is 40% of the pie. Roughly half of the proposers (48%) offer the equal split which is also the modal and median offer. Only 10% of the offers are consistent with the subgame perfect equilibrium offer (which is to either offer nothing or £1). These results are remarkably similar to the results under the standard UG design as reported in Roth (1995), Camerer (2003) and the meta study of Oosterbeek et al. (2004). Regarding responder decisions, all subjects were willing to accept the equal split. About 20% of our subjects accepted only more than 4/9 of the pie. Another 16% had acceptance thresholds between 1/3 and 4/9. About 33% had their acceptance level between 1/4 and 1/3 and the remaining 31% accepted less than 1/4. These figures differ only in the top segment from the ones in F&S which were derived from data in Roth (1995). They report 10% who are willing to accept only more than 4/9 and 30% for the other three segments. We will see below that our distribution does not differ significantly from the one in F&S.

In the MDG, subjects the average switching point was at £9-£9. The modal switching point was £10-£10 (with a frequency of 13%) and 26 subjects (43%) switched to the egalitarian outcome in the range of £0-£0 to £9-£9. There are 18% who either switch to the egalitarian outcome only when it is costless (at £20-£20) or do not switch at all (that is, they even prefer £20-£0 over £20-£20). Two subjects prefer £0-£0 over £20-£0. Because we modified the dictator game, our version has no precedent in the literature and the results cannot be directly compared to those reported for standard DG experiments. There are some parallels to the standard dictator game, however. For example, Forsythe et al. (1994) show that 20% of the dictators chose to pass nothing to the other player, a figure which is in line with the number of players who choose the egalitarian outcome only when it is costless or do not switch at all. We observe 44% subjects in our data set who have a switching point larger than 10. This figure is roughly consistent with the 60% of the subjects in Forsythe et al. (1994) who give less than the equal split.

[10] Our terminology follows Andreoni et al. (2002) here.

In the SPD, 21 subjects (34%) cooperated as the first mover. In the role of second mover, 23 (38%) reciprocated first mover's cooperation. Given the first-mover defection, all but four subjects (93%) defected as well. Our results are very similar to the ones obtained by Clark et al. (2001) in their SPD. The figures they obtained (baseline treatment, last round)[11] are 32.5% cooperation of first movers, 37% second mover cooperation given first mover cooperation, and 96% defection given first mover defection.

In our PGG, the average contribution was £4.7 (recall the endowment was £10). Less than half of the endowment was contributed by 25 subjects (41%) including 17 (28%) who contributed nothing. This was also the modal behavior. More than half the endowment was contributed by 27 subjects (44%), including 11 subjects (18%) who contributed the entire endowment. Holt, Goeree and Laury (2002) report on one-shot PGGs. They have one treatment with two players where the marginal per capita return is similar to our's ($m = 0.8$).[12] The average contribution in that treatment is 50%, very similar to the average we observe (47%). Roughly 47% gave less than half the endowment and 53% gave more than half the endowment. Considering that the equal split was not possible in Holt, Goeree and Laury (2002) since the endowment was 25 tokens, again, the results are remarkably similar to those we observed in the PGG.

We conclude that our results successfully replicate those of other experiments (and even in the subgames of the ultimatum game and the SPD) despite our related-sample design. Therefore, our design should be suitable for the individual-level test of the inequity model.

4 Test of the inequity aversion model

4.1 Model and estimation of inequity parameters

In Fehr and Schmidt's (1999) outcome-based theory, fairness is modelled as self-centered inequity aversion. This means that players are not only concerned about their own material payoff but also about the difference

[11] Clark et al. (2001) repeat their SPD and report cooperation rates in the first and the last round. We consider the last round of their data more relevant for comparison to our one-shot setting. Moreover, the percentage gain from exploting compared to reciprocating cooperation is 21% in our game which compares to the 20% gain in Clark et al. (2001).

[12] Most of the treatments in Holt, Goeree and Laury (2002) distinguish between an internal and an external return factor. We refer to the treatment ("2 4 4") where both factors are equal as in our experiments and in the standard PGG.

between their payoff and the other players' payoff. For two-player games, a F&S utility function is given by

$$U_i(x_i, x_j) = \begin{cases} x_i - \alpha_i(x_j - x_i); \text{ if } x_i \leq x_j \\ x_i - \beta_i(x_i - x_j); \text{ if } x_i > x_j \end{cases} ; i \neq j. \quad (1)$$

F&S impose two apriori assumptions. First, they assume $\beta_i \leq \alpha_i$, meaning that individuals suffer more from disadvantageous inequality than from advantageous inequality. Second, they impose $0 \leq \beta_i < 1$, which rules out individuals who enjoy being better off than others and individuals who will burn money in order to reduce their advantage with respect to other players.

We follows F&S in deriving the distribution of the parameter for aversion towards disadvantageous inequality, α, from the UG responder decisions. Since we employ the strategy elicitation method, the rejection levels in the ultimatum game give us (near) point estimates of α_i for each individual. To see this, suppose s'_i is the lowest offer responder i is willing to accept, and, consequently $s'_i - 1$ is the highest offer i rejected (recall that choices had to be integers). We conclude that this responder is indifferent between accepting some offer $s_i \in [s'_i - 1, s'_i]$ and getting a zero payoff from a rejection. Therefore, we have $U_i(x_i, x_j) = s_i - \alpha_i(20 - s_i - s_i) = 0$ (assuming the proposer does not offer more than half of the pie). Thus, the point estimate of the parameter of disadvantageous inequity is

$$\alpha_i = \frac{s_i}{2(10 - s_i)}. \quad (2)$$

In our data, we set $s_i = s'_i - 0.5$. This is somewhat arbitrary but it does in no way affect our results. A rational F&S player will always accept the equal split in the UG and hence have $s'_i < 10$, so, division by zero cannot occur by assumption here. For a subject with $s'_i = 0$, we observe no rejected offer and we cannot infer the indifference point s_i. Therefore, we set $\alpha_i = 0$ for participants with $s'_i = 0$ but theoretically, it could be that these subjects have $\alpha_i < 0$, that is, they could positively value the payoff of another player who is better off.

Let us now turn to the parameter of advantageous inequity aversion, β. F&S (1999) derive the distribution of this parameter from offers in the UG. There are various problems with this. First, proposers' offers depend on their beliefs about the other players' minimum acceptance threshold in the UG. F&S assume that proposers know the empirical distribution of α. While this is a plausible way of proceeding, their conclusions on the β distribution hinge on the assumption that beliefs are correct. Second, F&S derive the β distribution assuming risk neutrality—which may not hold for all proposers. Risk averse proposers may propose the equal split even if they do not care about inequity. Third, even a relatively small number of responders who reject anything but the equal split can imply that the optimal decision of a purely selfish proposer is to offer half of the endowment (this is the case in our data; see below). In that case, no β distribution can be derived. Fourth and most importantly, with the method F&S use, it is only possible to derive three relatively coarse intervals of the β parameter. The intervals are $\beta < 0.235$, $\beta \in (0.235, 1/2)$, and $\beta > 1/2$ (see F&S, p. 844).[13]

We prefer to derive (nearly) exact point estimates for β analogously to the way the α were derived. In the UG, α_i is defined by the offer that makes a responder indifferent between accepting and rejecting the offer. In our modified dictator game, we can get a point estimate for β_i by finding the egalitarian allocation, $£x_i - £x_i$, such that the dictator is indifferent between keeping the entire endowment (the £20-£0 outcome) and $£x_i - £x_i$. Suppose an individual switches to the egalitarian outcome at a payoff x_i'. That is, he prefers $£20 - £0$ over $(£x_i' - 1) - (£x_i' - 1)$ but $£x_i' - £x_i'$ over $£20 - £0$. We conclude that he is indifferent between the $£20 - £0$ distribution and the $£x_i - £x_i$ egalitarian distribution where $x_i \in [x_i' - 1, x_i']$ and $x_i' \in \{0, ..., 20\}$. From (1) we get $U_i(20, 0) = U_i(x_i, x_i)$ if and only if $20 - 20\beta_i = x_i$. This yields

$$\beta_i = 1 - \frac{x_i}{20}. \tag{3}$$

For our data analysis, we use $x_i = x_i' - 0.5$ (which does not affect our results), but this does not work at the boundaries. Subjects with $x_i' = 0$ even prefer $£0 - £0$ over $£20 - £0$, so they are possibly willing to sacrifice more than £1 in order to reduce the inequity by £1. Therefore, these subjects might have $\beta_i > 1$. However, since we do not observe a switching point for these subjects, we cautiously assign $\beta_i = 1$ to them in the data. Similarly, subjects who prefer $£20 - £0$ over $£20 - £20$ are possibly willing to spend money in order to increase inequity. These subjects might have $\beta_i < 0$ but, again, we do not observe a switching

[13] On this point, see Shaked (2005).

point for them and therefore we set $\beta_i = 0$ for such subjects in our data.[14]

We will now report on the joint distribution of α and β parameters. The α distribution does not differ significantly from the one in F&S (Kolmogorov-Smirnov, $D = 0.197$, $p > 0.1$). This is hardly surprising given that, as seen above, our responder behavior is very similar to the one in F&S. One difference to F&S is that there are subjects in our data set with $\alpha_i \geq 9.5$. These are the subjects who reject any offer in the UG below the equal split. In F&S, there is a comparable number of subjects with a high α_i but F&S assign them only $\alpha_i = 4$.

The β distribution differs significantly from the one in F&S (Kolmogorov-Smirnov, $D = 0.393$, $p \leq 0.001$). The reason for this result is that, in the F&S, the β density function only has mass at three points, $\beta = 0$ (30%), $\beta = 0.25$ (30%), and $\beta = 0.6$ (40%). Unless one interprets the mass points literally, one has to use a degree of freedom to interpret how a more heterogenous data set with more mass points would be distributed. In any event, we find that 30% have $0 \leq \beta_i < 0.25$, 31% have $0.25 \leq \beta_i \leq 0.60$, and 39% have $\beta_i > 0.6$. While these proportions are strikingly similar, the comparison also suggest an upward shift of the distribution.

A key advantage of our data set is that we can estimate the joint distribution of α and β. Previous research, including F&S, did not have access to the joint distribution because related-sample data were not collected. Figure 1 shows this joint distribution. As expected, both parameters turn out to be widely distributed in the population. It is apparent that the α_i and β_i are not significantly correlated, and a test for correlation confirms this (Spearman, two tailed, $rho = -0.03$, $p = 0.820$). We find that 23 subjects violate the F&S assumption that $\alpha_i < \beta_i$. They can be found to the left of the 45° line in the figure. Fifteen subjects do not exhibit inequity aversion in one of the two directions. Nine subjects have $\alpha_i = 0$ and six subjects with $\beta_i = 0$ but no participant has $\alpha_i = \beta_i = 0$. Nine subjects are highly inequity averse in either direction. Eight subjects have $\alpha_i \geq 9.5$ and one subject has $\beta_i = 1$.

[14] F&S (p. 824) acknowledge that subjects with $\beta_i < 0$ may exist. Indeed such subjects have been found in the experiments of Huck et al. (2001).

4.2 Offers in the ultimatum game

We now move on to the test of the F&S model. Given the distribution of parameters of inequity aversion, we will analyze the results from each game in two steps. First, we will assess the predictive power at the aggregate level and second at the individual level. A typical prediction of the F&S model would be that subjects with, say, β_i larger than some threshold $\widetilde{\beta}$ should contribute in the PGG. Our data would support the model at the aggregate level if the proportion of subjects with $\beta_i > \widetilde{\beta}$ is similar to the proportion of contributors to the PGG. This is how previous test of the F&S model have proceeded. We can go beyond that point by analyzing individual-level data. That is, we can check whether it is actually the subjects with $\beta_i > \widetilde{\beta}$ who contribute or not.

We will analyze the games one by one by testing hypotheses about the F&S prediction first on an aggregate and then on an individual level.

Hypothesis 1 *(i) Subjects with* $\beta_i > 0.5$ *should offer* $s_i = 10$ *in the Ultimatum Game. (ii) Subjects with* $\beta_i < 0.5$ *may, depending on their beliefs, offer both* $s_i = 10$ *and* $s_i < 10$ *in the Ultimatum Game.*

We take a look at the aggregate level first and compare predictions and data as if they came from different data sets and without taking the intra-personal information we have available into account. In the data, we have 33 subjects with $\beta_i > 0.5$ and 26 subjects with $\beta_i < 0.5$.[15] In the UG, we observe 29 subjects who offer $s = 10$. The aggregate outcome of $s = 10$ offers can be interpreted as support of F&S since subjects with $\beta < 0.5$ should offer $s < 10$ for some beliefs. The deviation of actual from the predicted $s = 10$ observations is $(33 - 29)/33 = 12.1\%$ which seems a small enough deviation in order to accept the relevance of the F&S theory.[16]

[15] There are two subjects in the sample who offer $s > 10$. These subjects are not consistent with F&S regardless of their β parameter. Therefore, we cannot interpret their behavior within the inequity model and so we discard them from the analysis. Further, note that for no subject in our sample $\beta_i = 0.5$.

[16] More formally, a chi-quare test cannot reject that the share of subjects with $\beta > 0.5$ is different from the share of subjects offering $s = 10$ ($\chi^2 = 0.82$, $d.f. = 1$, $p > 0.10$).

At the individual level, the data do not support the F&S model. Among the 33 subjects with $\beta_i > 0.5$, 18 chose $s = 10$, and among the 26 subjects with $\beta_i < 0.5$, 11 chose $s = 10$. A chi-square test does not indicate significant differences between these proportions ($\chi^2 = 0.79$, $d.f. = 1$, $p > 0.10$). That is, subjects with $\beta_i > 0.5$ are not significantly more likely to offer the equal split than their $\beta_i < 0.5$ counterparts. Robustness checks with thresholds $\beta \in [0.3, 0.7]$ reveal that the insignificance result does not depend on the particular value of $\beta = 0.5$. Therefore, we reject Hypothesis 1 (i).

We move on to part (ii). Among the 27 subjects with $\beta_i < 0.5$,[17] 16 chose $s < 10$. This behavior is consistent with Hypothesis 1 (ii) if subjects holds heterogenous beliefs. However, if we claim that the behavior of the $\beta < 0.5$ subsample is consistent, the aggregate outcome does not support F&S any more. The reason is that, if the $s = 10$ choices of $\beta < 0.5$ subjects are to be F&S rational, we should observe 45 $s = 10$ choices in total which is significantly different from the 29 actual observations. In other words, the degree of freedom arising due to arbitrary beliefs about responder behavior can only be used to either rationalize the outcome at the aggregate level or the behavior of the $\beta < 0.5$ subsample.

The previous argument highlights the role of beliefs for proposer behavior. It is possible to make a prediction for arbitrary proposer beliefs.

Hypothesis 2 *If subjects are uncertain about the acceptance thresholds of the responders, offers in the Ultimatum Game should be positively correlated with* β_i.

This hypothesis cannot be tested at the aggregate level, so, we take a look at the individual level. UG offers and βs should be positively correlated. This is, however, not the case. A Spearman test shows no significant correlation (two-tailed, $rho = 0.133$, $p = 0.350$). We conclude that the β data has no explanatory power regarding the UG offers.

As an aside, it is straightforward to see that independent of β the optimal offer given our α distribution is $s = 10$.[18] This implies that all proposers in our data should offer $s = 10$, regardless of their beta

[17] Note that in this part we do not discard the subjects with $s_i > 10$ so that we have 27 subjects and not 26 as above.

[18] The main reason for this is that we have 8 subjects in the data with an acceptance threshold of $s = 10$. Therefore, even completely selfish proposers ($\beta = 0$) should offer the equal split.

parameters. Deviations from the optimal offer may therefore be better explained by risk attitudes rather than inequity aversion. Andreoni et al. (2002) also emphasize the importance of risk attitudes in analyzing proposer behavior.

4.3 Behavior in the SPD

By backward induction, we start by analyzing second mover behavior in the SPD.

Hypothesis 3 *(i) Given first-mover cooperation, second movers in the SPD should defect if and only if* $\beta < 0.3$. *(ii) Given first-mover defection, second movers in the SPD should defect.*

Consider the aggregate level first. Regarding part (i) of the hypothesis, we have 20 subjects with $\beta < 0.3$ in the data but we have 38 subjects who defect given first mover cooperation. Prediction and experimental data differ by a substantial $(38 - 20)/20 = 90\%$.[19] While such a large deviation is hard to reconcile with F&S, we note that this failure to predict at the aggregate level does depend on the critical β threshold. For example, we have 37 subjects with $\beta_i < 0.6$ in the data which matches the share of (second move) defectors almost perfectly. As for part (ii), subjects should defect given first-mover defection and indeed 57 out of 61 subjects did so. While this strongly supports F&S, we note that F&S makes the same prediction here as the standard theory of rational payoff maximization.

Interestingly, even though F&S fails to explain choices at the aggregate level, the individual β parameters have some predictive power regarding second mover decisions when first movers cooperate. When $\beta < 0.3$, 16 out of 20 subjects defect whereas, when $\beta > 0.3$, "only" 22 out of 41 defect. This difference is significant ($\chi^2 = 3.97$, $d.f. = 1$, $p < 0.05$).[20] Nevertheless, it should be emphasized that, taken literally, F&S predict that 100% of the subjects with $\beta > 0.3$ cooperate but we find that not even half of these subjects cooperate.

[19] A chi square test rejects the hypothesis that the proprtion of $\beta < 0.3$ players is identical to the proportion of defectors ($\chi^2 = 10.65$, $d.f. = 1$, $p < 0.01$).

[20] A probit regression with the "cooperate" decisions as dependent variable and a dummy which is equal to 1 if and only if $\beta_i > 0.3$ may seems more appropriate here but yields the same result. A Spearman test correlating βs and "cooperate" decisions is significant (two tailed, $rho = 0.293$, $p = 0.025$).

Of course, part (ii) of the hypothesis is strongly supported at the individual level as virtually all subjects decided according to the F&S theory.

Hypothesis 4 *If subjects know the probability of second-mover defection in the data, first-movers in the SPD should cooperate if and only if* $\alpha_i < 0.520$*.*

In the data, we have 30 subjects with $\alpha_i < 0.52$, and we observe 21 subjects who cooperate as first movers. Based on the prediction made from the distribution of α_i, the deviation is $(30 - 21)/30 = 30\%$.[21] For two reasons, this test might be considered too strict. First, we assume that subjects hold correct beliefs here but of course this may not be realistic. That is, some subjects may have underestimated the share of cooperating second movers and therefore they may have defected themselves even though, based on the true behavior of second movers, cooperation would have maximized expected payoffs. Second, whether or not the theory predicts correctly here depends on the realization of the α threshold used in the derivation. With only minor changes in the threshold, the F&S theory would perform better at the aggregate level. For example, the proportion of $\alpha_i < 0.40$ and first-mover cooperation choices are virtually identical. We conclude that F&S is not strongly supported at the aggregate level but not rejected either.

At the individual level, the performance is not impressive. Among the 30 subjects with $\alpha_i < 0.52$, only 10 cooperate (rather than all 30). And among the 31 subjects with $\alpha_i > 0.52$, we observe 11 cooperate choices (rather than zero). The theory clearly fails and of course there are no significant differences in proportions ($\chi^2 = 0.031$, $d.f. = 1$, $p > 0.10$). Further, we note that this result does not depend on the particular threshold of α which is also apparent from the next result.

Hypothesis 5 *If subjects hold identical beliefs in the SPD, first-mover cooperation decisions and* α_i *should be negatively correlated.*

The hypothesis can only be tested at the individual level. The Spearman correlation test on individual i's α_i and the first-mover "cooperate" decision exhibits the right sign but is far from being significantly

[21] A chi-square test only marginally rejects that the proportion of $\alpha_i < 0.52$ and first-mover cooperation choices are identical at the 10% level ($\chi^2 = 2.729$, $d.f. = 1$, $p = 0.10$).

different from zero (two tailed, $rho = -0.027$, $p = 0.838$). It appears that disadvantageous inequality aversion does not have explanatory power regarding first-mover behavior.

Concluding this section, we note that the share of cooperative choices for first and second movers (given first mover cooperation) is practically identical. As first movers, 40 subjects defect and, as second movers given first-mover cooperation, 38 subjects defect. We conclude that a large fraction of the subjects exploit first-mover cooperation.

4.4 Contributions to the public good

Hypothesis 6 *(i) Subjects with $\beta_i < 0.3$ should choose $y_i = 0$ in the PGG. (ii) Subjects with $\beta > 0.3$ should contribute $y_i = \overline{y} \in [0, 20]$ in the PGG if they believe the other player also chooses $y_j = \overline{y}$ and $y_i = 0$ otherwise.*

Note that players with $\beta > 0.3$ face two coordination problems here. One, they will only contribute if the other player contributes and, two, even if they want to contribute, they face a coordination problem how much ($\overline{y} \in [0, 20]$) to contribute.

We consider data at the aggregate level and take into account both merely positive contributions ($y_i \geq 1$) and contributions of at least half the endowment [$y_i \geq 5$]. There are *41* subjects with $\beta_i > 0.3$ and we observe 44 [36] subjects who contribute a positive amount [contribute $y_i \geq 5$]. The data at the aggregate level are consistent with F&S if we assume that all 41 subjects with $\beta > 0.3$ believe the other player will contribute as well.

At the individual level, among the 20 subjects with $\beta_i < 0.3$, 13 chose $y_i > 0$ [10 chose $y_i \geq 5$]. This is not consistent with F&S. Among the other subjects, 31 out of 41 chose $y_i > 0$ [26 chose $y_i \geq 5$]. This outcome is consistent with F&S. However, the difference to the subjects with $\beta_i < 0.3$ is not significant either when considering $y_i > 0$ choices ($\chi^2 = 0.75$, $d.f. = 1$, $p > 0.10$) or $y_i \geq 5$ choices ($\chi^2 = 1.00$, $d.f. = 1$, $p > 0.10$). This implies that the beta parameter does not have any explanatory power regarding the contributions to the PG. As robustness checks, we analyzed various levels of contributions to the PG and various threshold of β. None suggested a significant explanatory power of the F&S theory.[22]

Hypothesis 7 *If subjects are uncertain about the contributions of the other player in the PGG, contributions of subjects with* $\beta_i > 0.3$ *should be negatively correlated with* α_i *and positively correlated with* β_i.

The intuition behind the hypothesis is that, the higher α_i, the more subject i suffers from being exploited in the PGG. Hence, if a subject is uncertain about the y_j of the other subject, a higher α_i makes the subject contribute less or even nothing. The opposite holds for the advantageous inequity parameter, β_i. However, we cannot find support for the hypothesis. A Spearman correlation test indicates that the correlations have the right sign but neither the correlation between α_i and contributions (two tailed, $rho = -0.177$, $p = 0.268$) nor between β_i and contributions (two tailed, $rho = 0.103$, $p = 0.520$) are significant for the $\beta_i > 0.3$ subjects.[23]

4.5 Categorization of types

There are 13 subjects who cooperate in all three games/stages (first-move SPD, second-move SPD given cooperation, and PG) and 14 subjects who defect at all points. Among these 27 subjects, the β have (very limited) explanatory power. A one-sided MWU shows that cooperators have higher β ($p = 0.1$) but there are no differences in the α. Moreover, if we test either the strong cooperators or the strong defectors against the rest of the subject pool, there are no significant differences.

5 Discussion

We now discuss several possible explanations for our findings.

[22] For example, the share of subjects who contribute the full amount ($y_i = 10$) the the PG is virtually identical for the $\beta_i \gtrless 0.3$ subpopulations, 3 out of 20 and 8 out of of 41 respectively.

[23] Simple reduced-form probits with the decisions to contribute $y_i \geq 1$ or contribute at least half the endowment ($y_i \geq 5$) as dependent variable and with α or β as explanatory variables do not yield significant results either. Further, the results do not change (indeed are often even weaker) when we include also the subjects with $\beta_i < 0.3$, or when we include only $\beta_i > \widetilde{\beta} \in [0.3, 0.6]$ subjects.

5.1 Is behavior correlated at all across games?

Given that the inequity parameters have so little explanatory power, one may simple conclude that individual behavior is not correlated at all across games. Perhaps, subjects play entirely erratic? A reason for this could be that participants are confused by the multi-game setting and just play random choices. Or they might feel an irrational need to vary their choices, behaving fair or cooperatively in one game and then behaving selfishly in the next. In any event, if individual behavior turned out to be completely random across decisions, the inequity model could hardly be blamed for failing to predict individual decisions well.

This is, however, not the case. We now check whether individual behavior is correlated across the decisions of our experiments. The correlations we analyze are straightforward even though they are arguably a little ad hoc in that we are not looking for theories predicting them. We found the following significant effects.

In the UG, it seems plausible that offers and acceptance thresholds should be positively correlated. One would expect a subject who rejects even moderately unequal offers also to offer more herself. Similarly, a subject offering very little might also be expected to have a lower acceptance threshold. This is indeed the case. In the UG, s and acceptance levels are positively and highly significantly correlated (Spearman, two tailed, $rho = 0.398$, $p = 0.001$). Andreoni et al. (2002, section 7) made the same comparison and found the same result. See also Gueth et al. (1982).[24]

The first move in the SPD and contribution to the PG are very similar decisions. The only difference is that the second movers in the SPD know what the first mover has chosen whereas in the PG both players decide not knowing what the other player is choosing. Therefore, one would expect similar cooperation levels for these decisions.[25] Among the 17 subjects who do not contribute to the PG, all but two also defect

[24] This is in remarkable contrast to our result that α and β are not correlated. This suggests that the UG offers are in fact not driven by inequity aversion. We suspect that they are primarily driven by the expectation concerning the rejection probability and that players with a higher rejection threshold also expect a higher probability of offers being rejected. This is consistent with an effect that is well-established in the social psychology literature, but mistakenly labeled "false consensus effect" (see Engelmann and Strobel, 2004b).

[25] We note the possibility here that a rational and selfish player would not contribute in the PGG but he might cooperate in the SPG if he expects the second mover to reciprocate.

as the first mover. By contrast, among the 44 subjects who do contribute to the PG, 19 also cooperate as the first mover — a higher share, and the difference is significant ($\chi^2 = 5.36$, $d.f. = 1$, $p \leq 0.025$). The results are virtually identical for $y_i \geq 5$ contributions.

The same point can be made about the second mover in the SPD (given first-mover cooperation) and PGG contributions. Among the 17 subject who give $y_i = 0$ to the PG, as above, all but two also defect as the second mover. But among the 44 subjects who contribute a positive amount, 21 cooperate as the second mover. The difference is significant again ($\chi^2 = 6.75$, $d.f. = 1$, $p \leq 0.01$). The results are virtually identical for $y_i \geq 5$ contributions.

Given that both first- and second-mover behavior are positively correlated with PGG contributions, unsurprisingly, also first and second mover choices in the SPD are correlated. There are 40 subject who defect as first movers and, among those, 31 defect as second movers. Among the 21 subjects who cooperate as first movers, 14 cooperate as second movers. A chi-square test indicates that this difference is significant ($\chi^2 = 11.44$, $d.f. = 1$, $p \leq 0.001$). Note that this is not predicted by the inequity model.[26][27]

Finally, we note that there is a correlation between offers in the UG and second-mover behavior (given first-mover cooperation) in the SPD, which is consistent with the inequity model. In any event, we conclude that individual behavior is not random across game. The failure of inequity aversion to predict individual decision across games must have other reasons.

[26] The higher β, the more likely the second mover is to cooperate and the higher α, the less likely the first mover is to cooperate. While the basic inequity model does not assume that α and β are correlated, and hence predicts no correlation between first- and second-mover behavior, Fehr and Schmidt (1999) need to assume that α and β are positively correlated to rationalize results in public good games with punishment (p.864). This would then suggest that second movers who cooperate are less likely to cooperate as first movers, in contrast to our results. The correlation between first and second mover SPD suggests that, as in the UG, beliefs play a vital role. A player who would not exploit cooperation might well expect a lower chance of exploitation than those who would exploit themselves. As a result, a rational first mover is more likely to defect if he is defecting as second mover.

[27] Furthermore, in clear contrast to the inequity model, α is positively correlated with the probability to fully contribute in the PGG. Aversion to disadvantageous inequality would imply that one gives less as this reduces the probability to end up behind.

5.2 The role of intentions, efficiency concerns and responsibility

It is by now accepted that inequity aversion cannot explain all games. Several papers have shown that, among other motives, intentions (see e.g. Falk, Fehr, and Fischbacher, 2003) and efficiency (see e.g. Charness and Rabin, 2002, Engelmann and Strobel, 2004a) play an important role in explaining some experimental results. In our experiments, we focussed on games that could well be rationalized by inequity aversion, and have indeed partly inspired the inequity model. Nevertheless, we have to consider whether other motives might partly drive our results.

Intentions are clearly irrelevant in the MDG. In contrast, rejections in the UG could be driven by negative reciprocity (and the evidence in Falk, Fehr, and Fischbacher, 2003, for mini-ultimatum games suggests that they are to a large extent). As a result, our estimates of α might be biased. Nevertheless, if the inequity model is supposed to work as an "as if" model in a large class of games by capturing both literal inequity version as well as negative reciprocity, our estimates of individual α's should still have predictive power. This is true unless negative reciprocity and inequity aversion are unrelated within subjects. We get back to this issue below. Similarly, second mover SPD behavior can be influenced by positive (after first mover cooperation) and negative (after first mover defection) reciprocity.

Concerns for efficiency may play a role in all four games. In the UG, rejecting an offer not only decreases the inequity between proposer and responder it also burns the entire £20 pie. Therefore, a subject who gains from overall efficiency may be less inclined to reject an offer. This implies that our measure of α would be biased downwards if subjects care for efficiency. In the SPD and in the PGG, cooperation is not individually profitable but even unilateral cooperation increases the sum of payoffs of the two players. Therefore, participants with a preference for efficiency should, all else equal, cooperate more. In these three games, efficiency concerns are partly in conflict with individual profit maximization and with inequity aversion. This does not, however, affect our hypotheses with respect to the correlations. Moreover, efficiency concerns have been invoked in distribution experiments where the inequity model failed to capture choices that increase the payoff of players that already are better off. In principle, this could be captured by allowing for $\alpha < 0$, so that a generalized inequity model[28] could capture this motivation as

[28] Such a model would be a linear version of the altruism model of Cox and Sadiraj (2005).

well.

In the MDG, efficiency concerns might also play a role. When a player chooses the egalitarian outcome below a level of 10, efficiency is reduced. By contrast, choosing the egalitarian outcome at any point above 10 increases efficiency. If players have efficiency concerns, their choice should be biased upwards whenever their unbiased switching point is below 10 and biased downwards if their "true" switching point is above 10. Note, however, that as long as efficiency concerns are not in some systematic way correlated with inequity concerns, the resulting bias in the estimate of β would not affect the expected correlations with other behavior.

The behavior in our games might also be influenced by the degree of responsibility that the other player has for his outcome. Camerer (2003, p.56)[29] argues

> "I suspect that Proposers behave strategically in ultimatum games because they expect Responders to stick up for themselves, whereas they behave more fairly- mindedly in dictator games because Recipients cannot stick up for themselves. This behavior could be codified in a theory of reciprocal fairness that includes responsibility. Define the last- moving player who affects player i's payoff as the one 'responsible' for i. If that responsible player is not i then she must take some care to treat i fairly; otherwise, she can treat i neutrally and expect i to be responsible for herself."

Our results regarding UG offers are consistent with this interpretation. Subjects with $\beta_i > 0.5$ have a switching point smaller than 10 in the MDG. This decision to switch below 10 costs these subjects more money than the other player receives. In other words, they are willing to pay a price higher than one for the equal distribution. In the UG, every monetary unit the responder receives costs the proposer exactly one. Hence, one would expect the subjects with a switching point below 10 to offer $s = 10$ in the UG. As seen above, this is largely not the case, and there is no correlation between β_i and the offer in the UG. What seems to be happening is that the subjects who violate the hypothesis (18 out of 33) are more generous in the MDG compared to the UG where they face the risk of rejection. While this finding is at odds with

[29] For a similar argument, see Charness and Rabin (2002).

most interpretations of behavior in the UG and MDG, we believe it is the key understanding to some of our results.

What we observe in our data is that a large share of the observed behavior is correlated across games. What does not seem to be correlated is behavior when subjects make final decisions over payoff distributions (UG responder, or α; MDG, or β) and behavior in strategic situations (SPD first move, PGG, UG proposer). The exception is the second move in the SPD. Here the player makes a final decision over outcomes, so the correlation with the MDG is no coincidence. The probability to cooperate after first-mover cooperation is, however, also correlated to UG offers and PGG contributions, which is both consistent with the inequity model, but also with the UG rejection threshold and first-mover SPD, which is both not predicted by the inequity model.[30]

We conclude that it appears that other considerations can dominate purely distributional concerns in strategic games and, moreover, that these considerations are not correlated in a systematic way with distributional concerns. As a result, many subjects are consistent with respect to one motivation that the inequity model can capture within one class of decisions. In different classes of decisions, however, different motives matter, which are not systematically related and hence subjects are not consistent with the inequity model across classes of decisions.

6 Conclusions

In this paper we assess the predictive power of inequity aversion. We run four different experiments (an ultimatum game, a modified dictator game, a sequential prisoner's dilemma and a public-good game) with the same cohort of experimental subjects. This allows us to make intra-personal comparisons across the four games. We use the responder data from the ultimatum game in order to estimate the parameter of aversion towards disadvantageous inequity of the Fehr-Schmidt model, and we take data from a modified dictator game to estimate the parameter of aversion towards advantageous inequity. We then use this joint distribution to test several hypotheses about individual behavior in the other games. Our results show

[30] See our discussion above.

that the inequity aversion model has considerable predictive power at the aggregate level but fails almost entirely at the individual level.

Our main conclusion is that aggregate support of a theory, if remarkable, should not be equated to individual validity of the theory. This seem particularly relevant for behavioral models of other-regarding preferences. Second, our results and discussion suggest that an inequity model calibrated on distributional decisions has little predictive power in strategic situations.[31] It appears that the success of the inequity model at an aggregate level is largely based on its ability to qualitatively capture different important motives in different games, including altruism in the dictator game, (negative) reciprocity in the ultimatum game, (positive) reciprocity in the sequential prisoner's dilemma game. The low predictive power of the model at an individual level then seems to be driven by the low correlation of these motives within subjects. Thus it appears to be both the strength and the weakness of the inequity model that it can capture different motives in one functional form. On the one hand, this permits to rationalize several apparently disparate results in one simple model. On the other hand, an individual's behavior is not well captured by this same model, since different motives drive behavior in different situations and these seem to have little correlation within subjects. The inequity model can hence serve as a relatively elegant "as if" model in several situations, but it does not appear to accurately and consistently reflect the motives of individuals. As a result, it can be problematic to use the model to predict behavior in novel situations, in particular in those where different motives, that can be captured by the inequity model in standard experiments imply opposing decisions (see Engelmann and Strobel, 2005, for an experiment and a discussion why the model can fail if negative reciprocity is in conflict with inequity aversion).

[31] One might argue that one should hence calibrate the inequity model on strategic decisions. This, however, is virtually impossible, as it requires specific assumptions about the beliefs (see also Shaked, 2005, on the issue of calibration of the model).

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Appendix: Proofs

Here, we formally derive the hypotheses of the results section. Most proofs can also be found in F&S.

Hypothesis 1 *(i) Subjects with* $\beta_i > 0.5$ *should offer* $s = 10$ *in the Ultimatum Game. (ii) Subjects with* $\beta_i < 0.5$ *should offer* $s_i = 10$ *in the Ultimatum Game if they believe the responder has* $x_j = 10$ *and* $s_i < 10$ *otherwise.*

Proof. An offer of $s = 10$ will surely be accepted by all responders and thus gives the proposer a utility of $U_i(10, 10) = 10$. Offering $s < 10$ either gives zero utility to the proposer if the offer is rejected or $U_i(20 - s, s) = 20 - s - \beta_i(20 - 2s)$ if it is accepted. When $\beta_i > 0.5$, we have $20 - s - \beta_i(20 - 2s) < 10$, hence, these subjects will choose $s = 10$. When $\beta_i < 0.5$, by contrast, $20 - s - \beta_i(20 - 2s) > 10$ and the proposer gains from offering $s < 10$ if the offer is accepted. Now, a responder with $x_j = 10$ is indifferent between accepting and rejecting $s = 9$ whereas all other responders accept. ■

Hypothesis 2 *If subjects are uncertain about the acceptance thresholds of the responders, offers in the Ultimatum Game should be positively correlated with* β_i*.*

Proof. Suppose player i believes that an offer s is accepted with probability $d_i(s)$, where $d_i(s) \leq d_i(s+1)$, $d_i(0) = 0$ and $d_i(10) = 1$. Offering s yields an expected utility of

$$d_i(s)\left(20 - s - \beta_i(20 - 2s)\right).$$

All else equal, the higher β_i, the (weakly) higher the offer that maximizes expected utility. ■

Hypothesis 3 *(i) Given first-mover cooperation, second movers in the SPD should defect if and only if* $\beta < 0.3$*. (ii) Given first-mover defection, second movers in the SPD should defect.*

Proof. If the first mover cooperates, player i prefers to defect if and only if $U_i(14, 14) < U_i(17, 7)$, that is, if and only if $\beta_i < 0.3$. If the first mover defects, player i is better off defecting regardless of the inequality parameters since $U_i(10, 10) = 10 > U_i(7, 17) = 7 - 10\alpha_i$. ■

Hypothesis 4 *If subjects know the probability of second-mover defection in the data, first-movers in the SPD should cooperate if and only if* $\alpha_i < 0.520$.

Proof. If first mover defects, the second mover will also defect (regardless of α_j and β_j) and both players get $U_i(10, 10) = 10$. Suppose the first mover's belief for the second mover to cooperate is p. Then the expected payoff from cooperating is $pU_i(14, 14) + (1 - p)U_i(7, 17)$, and cooperating yields an expected payoff higher than defecting if and only if

$$\alpha_i < \frac{7p - 3}{10(1 - p)}.$$

The true value for p in our data is $41/61 = 0.672$. Therefore, first-movers should cooperate if and only if $\alpha_i < 0.520$. ■

Hypothesis 5 *If subjects hold identical beliefs in the SPG, first-mover cooperation decisions and* α_i *should be negatively correlated.*

Proof. As seen above, cooperating yields an expected payoff higher than defecting if and only if $\alpha_i < (7p - 3) / (10(1 - p))$. Therefore, if first-movers hold identical (random) beliefs p, first-mover cooperation decisions and α_i are negatively correlated. ■

Hypothesis 6 *(i) Subjects with* $\beta < 0.3$ *should choose* $y_i = 0$ *in the PGG. (ii) Subjects with* $\beta > 0.3$ *should contribute* $y_i = \overline{y} \in [0, 20]$ *in the PGG if they believe the other player also chooses* $y_j = \overline{y}$ *and* $y_i = 0$ *otherwise.*

Proof. Suppose player i believes that player j will contribute $\overline{y} \in [0, 20]$ so that the payoff for player i is $20 - y_i + 0.7(y_i + \overline{y}) = 20 + 0.7\overline{y} - 0.3y_i$ and the payoff of player j is $20 + 0.7y_i - 0.3\overline{y}$. If player i also contributes $\overline{y}$, she gets a utility of $20 + 0.4\overline{y}$. If player i contributes $y_i < \overline{y}$, this yields a utility of $20+0.3(\overline{y}-y_i)+0.4\overline{y}-\beta_i(\overline{y}-y_i)$ which is larger than $20+0.4\overline{y}$ if and only if $\beta < 0.3$. If player i contributes $y_i > \overline{y}$, this yields a utility of $20 - 0.3(y_i - \overline{y}) + 0.4\overline{y} - \alpha_i(y_i - \overline{y}) < 20 + 0.4\overline{y}$. Hence, player i will never contribute more than $\overline{y}$, will contribute $\overline{y} \in [0, 20]$ if $\beta > 0.3$, and will contribute $y_i = 0$ if $\beta < 0.3$. ■

Hypothesis 7 *If subjects are uncertain about the contributions of the other player in the PGG, contributions of subjects with* $\beta_i > 0.3$ *should be negatively correlated with* α_i *and positively correlated with* β_i*.*

Proof. Suppose player i believes contribution level y_j will be chosen will probability $d_i(y_j) \in [0,1]$, where $\sum_{y_j=0}^{20} d_i(y_j) = 1$. Using the expressions derived above, i's expected utility from contributing y_i is

$$\begin{aligned}
& \sum_{y_j=0}^{20} d_i(y_j) U_i(y_i, y_j) \\
= \quad & \sum_{y_j=0}^{y_i-1} d_i(y_j)[20 + 0.3(y_j - y_i) + 0.4y_j - \beta_i(y_j - y_i)] + d_i(y_i)[20 + 0.4y_i] \\
& + \sum_{y_j=y_i+1}^{20} d_i(y_j)[20 - 0.3(y_i - y_j) + 0.4y_j - \alpha_i(y_i - y_j)] \\
= \quad & \sum_{y_j=0}^{20} d_i(y_j)(20 + 0.4y_j) + \sum_{y_j=0}^{y_i-1} d_i(y_j)(0.3 - \beta_i)(y_j - y_i) - \sum_{y_j=y_i+1}^{20} d_i(y_j)(0.3 + \alpha_i)(y_i - y_j).
\end{aligned}$$

This expressions shows that only subjects with $\beta_i > 0.3$ will contribute anything (as already seen above). For subjects with $\beta_i > 0.3$, some $y_i^* \in [0, 20]$ exists that maximizes i's expected utility, and y_i^* increases in β_i and decreases in α_i, as follows from the signs of $\beta_i y_i$ and $\alpha_i y_i$. ■