The Rhetoric of Inequity Aversion – A Reply*

Ernst Fehr [a)]
University of Zurich

Klaus M. Schmidt [b)]
University of Munich

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Abstract: In a recent "pamphlet" Shaked (2005) harshly criticizes two of our papers, Fehr and Schmidt (1999, 2003). This reply shows that Shaked's charges are not substantiated in any way. It points out several logical flaws in his arguments and shows that he grossly misquotes and misinterprets our papers.

* This note is a reply to A. Shaked, "The Rhetoric of Inequity Aversion", dated March 1, 2005.

a) Ernst Fehr, Institute for Empirical Research in Economics, University of Zurich, Bluemlisalpstrasse 10, CH-8006 Zurich, Switzerland, email: efehr@iew.unizh.ch (corresponding author).
b) Klaus M. Schmidt, Department of Economics, University of Munich, Ludwigstrasse 28, D-80539 Muenchen, Germany, email: klaus.schmidt@Lrz.uni-muenchen.de.

1. Introduction

The concept of Inequity Aversion that we introduced in a QJE paper in 1999 has stimulated a lively debate in several fields of economics. Experimental economists applied it to different experimental games and tested it against other notions of fairness or reciprocity that have been suggested in the literature. Economic theorists generalized the idea and put it on an axiomatic foundation. Applied economists used our concept to better understand real world incentive schemes, incomplete contracts, or ownership structures. We enjoyed this debate, even though several of these papers have been critical, claiming that inequity aversion is inconsistent with the observations in some experimental games (see e.g. Engelmann and Strobel, 2004, and our reply (2004a)) or that other notions of fairness or reciprocity are more convincing and do a better job in explaining experimental data (see e.g. Charness and Rabin, 2002). This debate is very important. After all, our paper intended to introduce a simple model that captures concerns for fairness in a tractable way and that can be applied and tested in different experimental set-ups.

A. Shaked's (2005) contribution, however, differs fundamentally from the papers mentioned above.[2] He is not interested in *"how useful the theory may be"*, and neither wants to *"confirm it, nor prove it false"* (p.3). Shaked's *"pamphlet"* is meant as a *"literary study"*, he wants to examine the "*structure and quality of (our) arguments*", our *"rhetorical devices"*, our *"choice of words and the way (we) use them"* (p.3). He describes our work as a *"subculture that apparently coexists parallel to main-line economics and in which different rules of logic and different laws of proof apply. In this sphere it seems to be permitted to misquote one's own theorems, to place crucial information into appendices and footnotes, to treat data in a casual and nonchalant way, and it is allowed to inflate results when citing them in subsequent papers."* (p.2) These are *"grave charges"* and Shaked quickly reassures his readers that they *"ought to be, and will be, meticulously substantiated in this pamphlet"* (p.3). As we will demonstrate in this reply, however, his charges are not "substantiated" in any way.

In contrast our reply is not a literary study, so we will stick to the facts and ignore the rhetorical devices Shaked uses to misrepresent what we did or why we did it. Thus, our reply

[2] In this note we respond to A. Shaked's pamphlet "The Rhetoric of Inequity Aversion" of March 1, 2005, available at http://ssrn.com/abstract=675227 To avoid endless discussions and revisions we respond exclusively to this version of Shaked's pamphlet.

is much shorter than Shaked's pamphlet and focuses on the few "hard" charges that he comes up with:

1. Shaked claims that we *"misquote (our) own theorems"* (p.2). This charge is "substantiated" in Section 3.5.1 of Shaked (2005) where he argues that our interpretation of Proposition 4 is *"false"* and that our *"seemingly minor mistake is crucial for the analysis of the data"*.

2. Shaked claims we failed to calibrate our model by using data from Ultimatum Games and to explain the experimental observations in four other games using this calibration.

3. Shaked claims we failed to establish that our model is consistent with the experimental evidence in many market games, in which the (rather unfair) competitive equilibrium outcome is often observed.

As we show below, none of these charges bears scrutiny. In the following, we use Shaked's abbreviations, so Fehr and Schmidt (1999) is referred to as "QJE", Fehr and Schmidt (2003) is referred to as "INV", and we refer to the pamphlet of Shaked (2005) as "PAM".

2. Misquoting Theorems

We start with this point because it seems to be the most serious charge against our work. The claim is not just that we exaggerated what we did, but that we made a claim that is plainly false. We will show below that all our claims are in fact correct. What is all the excitement about?

In Section IV of QJE, we used the model of inequity aversion to analyse linear public good games. Proposition 4b specifies a sufficient condition for cooperation to break down. This condition says that if the number of selfish individuals is sufficiently large [$k > a(n-1)/2$], then there is a unique equilibrium in which nobody contributes anything to the public good. We later conclude (QJE, p. 845) from this proposition that cooperation can only be sustained *if* the number of selfish players is sufficiently small [$k < a(n-1)/2$]. Shaked points out that the second statement does not follow from the first one, because it would have required a weak inequality [$k \geq a(n-1)/2$] in the statement of the proposition.

Furthermore, Shaked claims that "*(t)his statement is false since for the case* $k = a(n-1)/2$ *(for some* α, β *s) there exists an equilibrium with contributions, as well as an equilibrium with no contributions. This false statement is used by F&S in the analysis of the data. … Three of the experiments in the table (by Andreoni) satisfy the above condition with equality, hence F&S' analysis does not apply to them. The three experiments by Andreoni constitute about 22% of the observations in Table II.*" (p. 11).

Given all this excitement, the simple truth is disappointing in its banality: there is a fairly obvious typo in the statement of Proposition 4b in QJE (p. 839). It should have said that there is a unique equilibrium with zero contributions if $k \geq a(n-1)/2$. A short look at the proof of Proposition 4b (QJE, p. 861-62, in particular equation A18) immediately shows that the proof establishes our result not just for the strong but also for the weak inequality. Thus, the structure of our argument is in fact perfectly correct. This implies that Shaked's claim that there are multiple equilibria for the case where $k = a(n-1)/2$ is definitely false. It is not for us to say why Shaked did not look at the proof, but had he made any effort to understand the argument behind Proposition 4b, he would have quickly discovered the typo and not made a false accusation.

The typo in Proposition 4b is embarrassing and we apologize to our readers. However, we find some comfort in the fact that even A. Shaked is not immune to similar mistakes. When he summarizes our model he states our parameter restrictions as $0 < \beta_i < 1$ and $\beta_i < \alpha_i$ (PAM, p. 4), while the correct statement would have been $0 \leq \beta_i < 1$ and $\beta_i \leq \alpha_i$ (see QJE, p. 822). This seemingly minor mistake is in fact quite important. After all, our theory explicitly allows for the possibility that a significant share of the population is self-interested, i.e. $\alpha_i = \beta_i = 0$, and we emphasize constantly that the interaction between self-interested and inequity averse players drives out results.

3. Predictions across games

We derive a distribution of α and β in section V of QJE that is consistent with the experimental data of the ultimatum game. We then show that this distribution is also consistent with the experimental data in four other games of competition and cooperation. As we said in QJE (p. 843): "*The objective is … to offer a first test for whether there is a chance*

that our theory is consistent with the quantitative evidence from different games. Admittedly this test is rather crude." Shaked claims that we fail to pass this test (PAM, p.3). We will first explain our exact procedure briefly, and then evaluate Shaked's charges.

We started out by looking for a distribution of α and β that is consistent with the experimental data from the ultimatum game. We never said that the data from the ultimatum game can be used to identify the distribution of preferences uniquely. On the contrary, we say: "*Table III suggests a* [emphasis added] *simple discrete distribution of* α_i *and* β_i*. We have chosen* [emphasis added] *this distribution because it is consistent with the large experimental evidence we have on the ultimatum game …"* We then explain in detail that the parameters we have chosen have reasonable implications for the ultimatum game (see QJE, p. 843-844). In particular equation (14) on p. 844 makes it very clear that it is impossible to pin down the exact value of β_i of a proposer who offered an equal split of the pie. The only conclusion that can be drawn for such a player is that $\beta_i \in [\frac{1}{2}, 1)$. Shaked claims that "*any value (of* β*)* $\geq 1/2$ *could have been chosen*" (PAM, p. 9). This statement is false and misleading because it ignores the restriction $\beta_i < 1$. This restriction is very important to understand what we did. In QJE (p. 824) we explain this restriction by looking at a two player game in which player i has a higher monetary payoff than player j: *"In this case* $\beta_i = 0.5$ *implies that player* i *is just indifferent between keeping one dollar to himself and giving this dollar to player* j*. If* $\beta_i = 1$*, then player* i *is prepared to throw away one dollar in order to reduce his advantage relative to player* j *which seems very implausible. This is why we do not consider the case* $\beta_i \geq 1$*."* In fact, even a value of $\beta_i = 0.8$ is quite extreme. It implies that a player is prepared to reduce his own monetary payoff by one dollar even if this increases the payoff of his opponent by only 25 cent. We picked the value of $\beta_i = 0.6$ because this implies that such a player is willing to give away one dollar if this increases his opponent's payoff by at least 66 cents, which seemed more plausible to us.

After fixing the distribution of α and β we looked at the implications for four other games. Shaked argues that the exact value of $\beta_i \geq 0.5$ plays a role in two of these games. The first of these games is the market game with responder competition. Shaked claims: "*F&S prove in Proposition 3 (QJE p. 832) that in order to obtain an outcome close to the competitive equilibrium two individuals need to be sufficiently selfish: the proposer AND the*

least inequity averse responder" (PAM, p. 9). This statement is false. The first sentence of Proposition 3 establishes that if the proposer's preferences satisfy $\beta_1 < (n-1)/n$ then there always exists a subgame perfect equilibrium in which all responders accept any $s \geq 0$, and the proposer offers $s = 0$. This result is independent of the preferences of the responders. Thus only <u>one</u> individual (the proposer) needs to be sufficiently selfish. The second sentence of Proposition 3 further states that if in addition one of the responders is sufficiently selfish, this equilibrium is unique. Shaked ignores the subtleties of existence and uniqueness and claims: *"When they come to describe this result, F&S ignore the condition on the proposer's selfishness, they misquote the proposition, and claim that a single player (a responder) can induce a competitive outcome (QJE, p. 819)."* Again, this claim is false. On page 819 (the introduction of our QJE paper) we said: "… under certain conditions a single purely selfish player can induce a large number of extremely inequity averse players to behave in a completely selfish manner, too." We do <u>not</u> refer to the responder as Shaked makes the reader believe, nor do we misquote our proposition.

However, it is true that we do say in Section V of QJE (p. 845) that we need at least one responder who is sufficiently selfish in order to obtain a <u>unique</u> equilibrium outcome. There we ignore the additional condition on the proposer's preferences. The reason is that this condition is automatically satisfied given the distribution of Table III that we assumed to hold throughout this section. Shaked points out that if we had assumed $\beta_i = 0.84$ rather than $\beta_i = 0.6$ in Table III, then only 60 percent (rather than 100 percent) of the proposers would have satisfied the condition $\beta_1 < (n-1)/n$. While this argument is correct, strictly speaking, it is also quite silly. As we pointed out above, a value of $\beta \geq 0.8$ implies an extreme degree of inequity aversion. If Shaked wants a more realistic distribution, then he should assume that there is some continuous distribution of $\beta \in [0,1)$ and not put 40 percent probability mass on such an extreme value as $\beta = 0.84$. But even under the most extreme assumption that all inequity averse proposers have $\beta > 0.84$, we still get a (unique) competitive equilibrium outcome in 50 (rather than 80) percent of all cases.

The exact value of β_i could also play a role for the analysis of the public good game with punishment. In Section IV, Proposition 5 (QJE, p. 841), we describe one equilibrium of this game in which a small group *n'* of "conditionally cooperative enforcers" can sustain full cooperation. These conditionally cooperative enforcers must satisfy $a + \beta_i \geq 1$, where *a* is the

marginal return of a contribution to the public good to each member of the group. The value *a=0.4* is used in Fehr and Gächter (2000). Thus, the condition of Proposition 5 requires $\beta_i \geq 0.6$. We had picked the highest possible value of β_i to be $\beta_i = 0.6$ in Table III, which is just sufficient, but very tight. We did not try to hide this fact from the reader by changing the value of β in Table III ex post (any value $\beta \leq 0.83$ would have been fine with all the other games). Another problem with the public good game with punishment is that we had to make an assumption about the correlation between α_i and β_i (which we did not have to do in the other games we consider). It seems plausible that somebody who cares a lot about inequality to his advantage also cares a lot about inequality to his disadvantage, so we assumed for simplicity that α_i and β_i are perfectly correlated. Further problems of this game are that there are always multiple equilibria - one of which has to be selected - and that the total amount of available data from the public good game with punishment in Fehr and Gächter (2000) is much smaller than for the public good game without punishment or the ultimatum game. As we said in Footnote 22: *"Future experiments will have to show whether the Fehr-Gächter results are the rule in the punishment game or whether they exhibit unusually high cooperation rates."* All of these problems are discussed openly in our paper.

Shaked argues that this game cannot be used as evidence for our model. We agree that the evidence this game provides may not be very strong for the reasons given above (and in QJE already). But, the experimental data from this game are consistent with our model and the distribution assumed in Table III. Furthermore, we find it interesting to see that there is a large range of parameters under which both observations - full cooperation in the public good game with punishment and no cooperation in the game without punishment - are consistent with our model. And, last but not least, we repeatedly stated in QJE that these calculations were meant as a rough first test of our model. As we said in QJE on p. 846, right after the discussion of the public good game with punishment: *"Clearly the above computations provide only rough evidence in favour of our model. To rigorously test the model, additional experiments have to be run. We would like to suggest a few variants of the experiments discussed so far that would be particularly interesting: …"* Shaked never mentions that we suggested five other experiments as a more rigorous test for our model in QJE, nor does he mention that we conducted quite a few additional experiments in the meantime, most (but not all) of which confirm our theory. These papers are freely available on our websites.

We fully agree with Shaked when he says: *"It is unfair to criticize a crude and rough calculation which is meant to be only a first test"* (PAM, p.6). However, he goes on and complains bitterly that we exaggerate and inflate our results when citing them in subsequent papers. In particular, he criticizes the fact that we call our humble exercise a "calibration" in INV. What is a calibration? According to Shaked (PAM, p. 13): *"Leaping from the empirical evidence to a property of parameters in a theory is known as calibration."* Isn't this exactly what we do? Yes, but Shaked charges that it is impossible to get a *"fine calibration"* (PAM, p. 6, title of section 3.1, emphasis added). Pointing to the degrees of freedom in Table III, he says: *"It is therefore impossible to fully calibrate the inequity aversion model with the UG data."* (PAM, p. 6, emphasis in the original). We have to admit that the terms *"fine calibration"* and *"full calibration"* are new to us. But we never claimed that we "finely" or "fully" calibrated our model. We just said that we calibrated it, which involves the same problems that many other calibrations in the literature face as well.

As a final remark, the sentence quoted by Shaked should also be seen in its context. INV is a survey paper on recent developments in this field. Out of the 60 pages of this survey, exactly one page is devoted to a summary of our 52 page QJE paper (QJE, p. 221-222). We had to summarize the section on "Predictions Across Games" in the two sentences quoted by Shaked. With more space, we would have explained what we did in more detail, but the term "calibration" seems to us to be the best description of our procedure in such a short summary. In INV (p. 242-250) we devote much more space to the discussion of several new experiments that we conducted in the three years between QJE and INV. The results of most of these experiments are not only consistent with our model of inequity aversion, but also with the calibration that we used in QJE. This gives us the confidence to claim that our model "yields quantitatively accurate predictions across many bargaining, market, and co-operation games." [INV, p. 222][3]

[3] The latest versions of the experiments discussed in INV are Fehr, Klein and Schmidt (2004), Fehr and Schmidt (2004b), and Fehr, Kremhelmer and Schmidt (2004). Further experiments that we conducted and that yield additional support for the theory of inequity aversion include Fehr and Schmidt (2004a) and Fischbacher, Fong and Fehr (2003). We also did some experiments demonstrating the limits of the model of inequity aversion, in particular Falk, Fehr and Fischbacher (2003), but to some extend also Fehr and Gächter (2002) and Fehr and Schmidt (2004a). All of these papers are available on our websites. Shaked cites several of them in his pamphlet, but he fails to mention that they provide the additional support for the theory of inequity aversion that he pretends to be looking for.

4. Fairness and Competition

Shaked promised to demonstrate that we failed to show the result that "even in populations with high degree of inequity aversion the equilibrium is the competitive one (or close to it)". (PAM, p. 2) However, the arguments offered by Shaked do not substantiate this claim in any way.

We formally analyze two "market games" in QJE, one with proposer competition and one with responder competition Shaked questions why we picked these two games. The answer is given in footnote 9 (QJE, p 829) where we say that these two games are well suited to make our point and that *"they allow for an explicit game-theoretic analysis"*, while other *"experimental market games such as the continuous double auction as developed by Smith (1962) have such complicated strategy spaces that no complete game-theoretic analysis is yet available."* In fact, at the time when we wrote the QJE paper we were not aware of any other simple market experiments that we could have used nor does Shaked suggest any such game either.

Shaked argues that the game with proposer competition (Roth et.al. 1991) is not suitable for supporting our claim that competition drives out fair behaviour. His argument is that the responder is forced to accept the highest offer in this game. This is true, and we explain the role of this assumption made by Roth et.al. in much detail in QJE. For example, we say with explicit reference to this game: "The crucial observation in this game is that no single player can enforce an equitable outcome". [This is not the "general principle" that Shaked misquotes on p. 15, but refers explicitly to Proposition 2 and the game with proposer competition.] Does this mean that this game is therefore irrelevant as Shaked suggests? No. Many real world market games have exactly this structure where the responder *must* accept the *highest* offer. For example, in almost every auction the bidder with the *highest* bid must get the object.

Furthermore, in a subsequent paper (Fischbacher, Fang and Fehr, 2003) one of us conducted another experiment with proposer competition in which the responder was free to accept whichever offer he wanted. As Shaked points out correctly (PAM, p. 15), if the responder satisfies "$\beta > (n-1)/n$, then the only equilibrium is the equitable partition in which all the proposers offer ½ and the responder accepts it." It is easy to see that an increase of n increases this threshold for β. Thus the larger n (the more competition), the smaller is the probability that this is going to happen. This is exactly what we observe in the Fischbacher

et.al. (2003) experiment. The introduction of one competing proposer (going from *n=2* to *n=3*) yields a large increase (away from the equal split) in the proposed offer. Shaked cites this paper but fails to mention that one of us did exactly the experiment that he would like to have seen.

Shaked argues that the game with responder competition is also not suitable, even though in this game a single proposer can enforce a more equitable outcome by simply offering it. Again, if the proposer is so inequity averse that $\beta > (n-1)/n$, he will offer to split the pie equally with one of the responders. The crucial point remains that as *n* increases, so does the threshold for β and an equitable allocation becomes less likely. Thus, this confirms our claim that inequity aversion is consistent with the experimental evidence that an increase in competition drives out fairness.

Shaked acknowledges (PAM, p. 16) that *"as the number of responders (n-1) increases, the proposer is less likely to be sufficiently inequity averse"*, but then he complains that *"(t)his property is a direct consequence of the normalization of the utility function (dividing by n-1)."* Yes, it is a direct consequence of our model! This is why we say that our model "explains" or "is consistent with" the experimental evidence from market games. Didn't Shaked start out to prove us wrong on this claim?

But Shaked does not give up so easily. He now ceases to be a literary critic, metamorphoses to an economist and questions our assumption. *"Is it really reasonable that an individual is less likely to contribute to a charity, helping the victims of a natural disaster, just because the number of victims increased? Indeed, tsunamis do not often occur in the laboratories but this does not make this consequence more palatable, nor are the populations in the laboratory particularly large to guarantee that any* β *will be smaller than* $(n-1)/n$*."* We do not want to speculate about the motives of people giving to tsunami victims here, but it is worthwhile to point out the logical flaw in Shaked's argument. Even if one applies our theory to charitable giving, it does not say that a person is less likely to give if the number of victims increases, but rather if the reference group increases. If the size of the reference group remains constant and some members of the reference group incur a larger loss, than our model predicts that in expectation those who did not incur the loss give more than if the loss was small.

5. Conclusion

Shaked has introduced us to a subculture hitherto unknown to us, a world of literary criticism in which different rules of logic and different rules of proof apply. In this sphere it seems permitted to misquote other people, to ignore crucial information that is provided in footnotes, appendices and even in the body of the text, to treat quotations in a casual and nonchalant way, and it is allowed to make grave charges without substantiating them.

All these transgressions can be found in the pamphlet of Shaked. It is not for us to say why he resorted to such techniques, but whatever the reason may be, the eventual effect of these rhetorical devices is to discredit his own reputation. We very much hope that he becomes an economist again and contributes to the exciting scientific discussion of the insights of experimental economics.

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