



Discussion

On inequity aversion: A reply to Binmore and Shaked[☆]Ernst Fehr^{a,*}, Klaus M. Schmidt^b^a Institute for Empirical Research in Economics, University of Zurich, Bluemlisalpstrasse 10, CH-8006 Zurich, Switzerland^b Department of Economics, University of Munich, Ludwigstrasse 28, D-80539 Munich, Germany

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ABSTRACT

In this paper we reply to Binmore and Shaked's criticism of the Fehr–Schmidt model of inequity aversion. We put the theory and their arguments into perspective and show that their criticism is not substantiated. Finally, we briefly comment on the main challenges for future research on social preferences.

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1. Introduction

The theory of inequity aversion (Fehr and Schmidt, 1999) was developed to solve a puzzle: why do most experimental subjects behave very selfishly in some games, e.g. exploiting their bargaining power in competitive market games and free-riding in the final periods of voluntary cooperation games, while demonstrating rather “fair” behavior in other games, e.g. in bilateral bargaining games, in trust games, and in public good games with punishments? In Fehr and Schmidt (1999, FS 1999 in the following), we searched for a common principle that can explain this contradictory evidence. We wanted a simple theory, one that can be used as a tractable tool in more complicated models and that yields quantitative, testable predictions.

The theory of inequity aversion proposed in FS (1999) has triggered a lively debate. The theory has been applied to many different experimental games, it has been generalized and put on an axiomatic foundation, and it has been tested against other notions of fairness or reciprocity. This debate was often critical, but always fair and to the point.

In a current paper Binmore and Shaked (2009) criticize FS (1999) and some of our subsequent work on inequity aversion.¹ They purport to use our work as a “case study” to illustrate more general methodological issues. The fact is, however, that they are hitting a straw man. In this paper we will show that their criticism is unfounded.²

We are going to show this by proceeding as follows. In Section 2 we sketch the background of our 1999 paper to put it into perspective. In Section 3, we discuss in which sense we “calibrated” our model in FS (1999). In Section 4 we reconsider the four games to which we applied the theory of inequity aversion in FS (1999) and show that Binmore and Shaked's critique of

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* Corresponding author.

E-mail addresses: efehr@iew.uzh.ch (E. Fehr), klaus.schmidt@LMU.de (K.M. Schmidt).

¹ As some readers may know, Shaked's criticism of our work dates back to what he called a “pamphlet” entitled “The Rhetoric of Inequity Aversion”

our analysis is unfounded. In Section 5, we discuss the difference between the distribution of preferences used in FS (1999) and that in Fehr and Schmidt (2004), Fehr et al. (2007) and Fehr et al. (2008) (the three “contract papers”), and show that Binmore and Shaked’s criticism is not substantiated. Finally, in Section 6 we deal with Binmore and Shaked’s interpretation of the data in the contract papers.

We conclude in Section 7 with a few general remarks on the future tasks in the domain of social preference research. We anticipate that a complete characterization of the distribution of different social preference types in the population may introduce so much complexity at the individual level that models that attempt to capture this complexity may become analytically intractable. For this reason, a simple model such as the theory of inequity aversion may still be useful, even though there is evidence that it does not provide a full description of other-regarding preferences.

2. Putting the theory of inequity aversion into perspective

A substantial amount of experimental evidence has been accumulated over the last 30 years that indicates that many subjects do not behave in a purely selfish, money-maximizing way (Camerer, 2003). Even Binmore and Shaked (2009, p. 120–121), who remained sceptical about the existence of social preferences for a long time, regard this evidence now “as an informal proof that such preferences exist”. When we wrote our QJE paper in 1997–1998 the facts already clearly indicated that other-regarding preferences play an important role in many experimental games. However, at the time it was common practice in many experimental papers to offer ex post, situation dependent, explanations of the observed behavior that referred to altruism, spite, status seeking, or some particular social norm, but each explanation told us very little beyond the particular experiment for which it was invented. Furthermore, the existence of other-regarding preferences was difficult to reconcile with the experimental evidence on some other experimental games. For example, in voluntary contribution public good games and in many competitive market games the majority of subjects converged towards outcomes that were predicted by the standard self-interest approach. The question thus was whether there is a general model that gives a unifying explanation of both the evidence that suggested the existence of other-regarding preferences and the evidence that seemed to suggest the absence of other-regarding preferences. FS (1999) was one of the first attempts to develop such a model.³

In FS (1999) we assume that people are heterogeneous: some people suffer from linear inequity aversion (to different degrees) while others are purely self-interested. The bulk of our paper is concerned with stating, proving and interpreting five theoretical propositions that provide qualitative insights into the outcomes generated by the interaction between self-interested and inequity averse players in several prominent experimental games.

In particular, the propositions offer a solution to the puzzle that despite the known existence of a large share of other-regarding players certain market games generate very unfair distributions of the gains from trade that closely resemble the predictions of the standard self-interest model that assumes that *all* players are completely selfish. Thus, our paper helps us to understand why the self-interest model predicts so well in these market games even though the assumptions of the model are wrong.⁴ We also show that a small minority of selfish players in a one-shot public good game may suffice to generate a

(Shaked, 2005). Shaked distributed this pamphlet via email to more than 300 members of the game theory community and posted it on his website and at SSRN—explicitly without intending to publish it in a refereed journal. Fehr and Schmidt (2005) wrote a reply rapidly, followed by a revised version of the pamphlet (Shaked, 2005). Shaked’s pamphlet has raised some confusion in the profession about the contribution of the theory of inequity aversion. Putting the offensive and polemical rhetoric aside, Shaked (2005) made three main accusations against FS (1999) and against our invited lecture at the World Congress of the Econometric Society (Fehr and Schmidt, 2003):

1. Shaked claimed that the conclusions drawn from our Proposition 4 are “false”, and that this “*seemingly minor mistake is crucial for the analysis of the data*” (p. 11).
2. He claimed that our analysis of the market game with proposer competition and the market game with responder competition does not show that the equilibrium in these two games is close to the competitive outcome even if the population is highly inequity averse. He argued “*that the first game is logically unsuitable to demonstrate (this) point and that the second game does not show it*” (p. 15).
3. He claimed that we fail to calibrate our model by using data from Ultimatum Games and that we do not explain the experimental observations in four other games using this calibration.

Binmore and Shaked have since withdrawn the first two points. In our reply (Fehr and Schmidt, 2005) we showed that the first accusation is false and results from an error in Shaked’s analysis. Shaked may have been confused by a typo (“>” instead of “≥”) in the statement (but not the proof) of Proposition 4. Shaked dropped this charge in the revised version of the pamphlet (Shaked, 2005).

The second point has now been withdrawn by Binmore and Shaked (2009). They conclude in their discussion of the two “*auctioning games*” (the market game with proposer and with responder competition that we considered in FS 1999): “*Fehr and Schmidt’s inequity aversion model is no worse as a predictor of the two auctioning games than the money-maximizing model.*” The money-maximizing model predicts the competitive equilibrium outcome, and so does the inequity aversion model. This point can thus be put aside as well.

However, Binmore and Shaked did not give up on the last point but extended their original criticism to three other papers (the “*contract papers*”) that we had written in the meantime. We will show in this comment that the third point and the new criticisms are also ill-founded. We hope that this settles the remaining point of the controversy, enabling us – and the profession – to devote our time to more productive enterprises.

² We do not comment on the more general methodological points raised in the first sections of Binmore and Shaked (2009). For a more general discussion of these issues, we refer the reader to Eckel and Gintis (in press). See also Schmidt (2009) for a methodological discussion of the role of experiments for economic theory without reference to the current controversy.

³ Other attempts include, Levine (1998), Bolton and Ockenfels (2000), and Falk and Fischbacher (2006). See Fehr and Schmidt (2006) for a survey.

⁴ See also Dufwenberg et al. (2008) who established this result more generally in a general equilibrium framework.

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