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## Commentary on Benjafield, James & Saroka

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**In Response To:** John/Tammy/Lewis Benjafield/James/Soroka's [Are economists rational or just different?](#)

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1. James, et al., are attempting to replicate and extend earlier studies which suggest that students majoring in economics are more likely to act self-interestedly - and are less likely to act cooperatively - than students majoring in subjects other than economics. Motivating many of these investigations is a concern that such behavioral differences may reflect ideological divergence and that the promotion of values and behaviors valued by mainstream economics may prove increasingly unhelpful in the world that is emerging. James et al. write that they hope their work "could be a productive source of discussion about the educational values that are appropriate in the 'new world order' (or disorder, as some would have it)."

Because I share their broader social concerns, I am delighted to have the opportunity to offer comments on a work that is still in progress and whose results are still tentative. What I say will consist in a few cautionary notes, offered in a spirit of cooperation and encouragement. My comments fall under three headings: the first is a conceptual point; the second concerns a hermeneutical problem that arises when we try to make sense of the data obtained from playing the ultimatum bargaining game; the third are some loose and nontechnical points about the quality of evidence that motivate the intriguing suggestions at the end of the paper.

2. *A conceptual point or two.* James et al. work with a contrast between two extremes. They write, "Social value orientation is a dimension anchored by those who have an extremely competitive, winner-take-all, individualistic orientation and those who have an extremely cooperative, share-and-share-alike, prosocial orientation." This contrast is introduced with reference to a study by De Dreu & Boles 1998. In that study, De Dreu and Boles (1998, p. 254) actually recognize three motivational orientations:

- a prosocial value orientation (those who value their own and others' outcomes equally)
- an individualistic orientation (those who value their own outcomes only)
- a competitive orientation (those who value doing well at the other's expense).

James et al. seem to lump individualistic orientation (which is assumed or promoted by mainstream economic theory's notion of rationality) together with a competitive "winner-take-all" orientation. And that, I think, is a mistake.<sup>1</sup> Cooperation can, after all, appeal to a person with an "*individualistic* orientation" when cooperation offers the best chance of maximizing his or her payoff.<sup>2</sup> But cooperation cannot, I think, appeal to someone with a *competitive*

orientation as defined by De Dreu & Boles. The difference has been important to philosophers – especially to philosophers like David Gauthier and Jan Narveson<sup>3</sup> who want to argue that the basis of morality consists in mutually beneficial principles arrived at by essentially self-interested individuals.

One must also be careful about suggesting that "competitive values promoted by at least some economists may have led to a cultural preoccupation with a 'winner take all' strategy" – and especially about claiming that Frank and Cook's *The Winner-Take-All Society* shows how "the growing inequality between rich and poor may be partially the result of the widespread acceptance of the view that competition is rational and that sharing is for fools." As I read *The Winner-Take-All Society*, it is about the emergence of winner-take-all *markets*. In describing the causes of the emergence of such markets in Chapter 2 and in outlining proposals in the closing chapter for minimizing the negative effects of such markets, Frank and Cook don't seem to me to stress the effects of an *ideology* that discourages cooperation.

3. *A hermeneutical point.* James et al. partially<sup>4</sup> replicate Carter and Irons' earlier study of how economics majors and non-economics majors played the ultimatum bargaining game. While their data doesn't permit them to make meaningful comparisons among disciplines within their sample, they do compare their overall results using Canadian subjects with the overall results obtained by Carter and Irons using American subjects. On average, Canadian subjects kept (i.e. proposed for themselves) less than their American counterparts, while the American subjects were willing to accept less than their Canadian counterparts. The conclusion James et al. want to draw is this: "Canadians appear to conform less to the 'winner take all' stereotype. Canadians not only want to share the proceeds more equitably, but also to be unwilling to accept as little as Americans when they are on the receiving end."<sup>5</sup>

The mean "amount kept" by the Americans as a whole is \$5.77; for the Canadian sample it appears from the graph to be about \$5.20. Assuming that the difference is statistically significant, what are we to make of it? Does it really mean that "Canadians ... want to share the proceeds more equitably." That is only one possible way of interpreting their behavior. Game theory notwithstanding, proposing a 5/5 split may well be the best thing to do *from the point of view of self-interest*. To be sure, Carter and Irons suggest that if "both players act in accordance with the rational/self-interest model" then Responder prefers any positive offer to \$0. "Knowing this," they say, Proposer proposes a division of \$9.50 to herself and \$0.50 to Responder. But of course Proposer *doesn't* know that Responder will in fact prefer any positive offer to \$0, and in fact real-life Responders don't act that way.<sup>6</sup> If Proposer is savvy, she will try to estimate the likelihood that various proposals will be accepted by real-life Responders. A five-five split might well be considered a sure bet – in which case its expected utility is  $\$5.00 \times 1 = \$5.00$ . If a six-four split were estimated to have an 80% chance of being accepted, then its expected utility is  $\$6.00 \times .8 = \$4.80$ . If a nine-one split has only a 20% chance of acceptance, its expected utility is  $\$9.00 \times .2 = \$1.80$ . With such probability estimates, those determined

to maximize expected utility for themselves would propose a five/five split.

In other words, the difference between the Americans and Canadians may not be due primarily to Canadians' desire for equitable distribution, but may simply signal that Canadians are more cautious and conservative when estimating probabilities, or prefer safer bets to riskier ones.<sup>7</sup>

More puzzling to me is the question why Canadians are unwilling to accept as little as Americans (on average, those in the Canadian sample would not accept less than \$3.50, while Americans on average would accept \$2.09). We might take this as evidence that, as Responders, Canadians are insistent on a more even distribution. But that is not evidence of a *willingness to cooperate* on the part of Canadian responders, but rather of an *unwillingness to cooperate* with aggressive Proposers.

4. *Evidence for the more interesting inferences entertained.* James et al. don't draw any firm inferences from their prisoner's game data; indeed, they warn us that their results are only tentative. They do, however, "entertain" or hold out for consideration three fascinating inferences on the basis of their data. Despite the fact that statistical analysis indicates most of the differences reported in mean defection scores are statistically significant at the .05 level, I find myself *very* hesitant even to lean in the direction of these inferences on the basis of the data gathered so far. Let me offer three comments, that I hope are constructive and helpful.

(a) *The inference that "psychology students defect less (are more cooperative) than others, including economics majors"* – i.e., that it may well be that it isn't economics students, but rather psychology students, who are "different." At this point, I'm not at all inclined toward such an inference. There are only six subjects in their "Other" category, too few in my opinion to begin to be representative of the broad range of other disciplines. Moreover, the subjects are all male, a factor that has a clear effect on the dependent variable being investigated, namely defection rates. My hesitation is reinforced by the fact that Frank, Gilovich and Regan, working with much larger subsamples, appear to have found significant differences between economics students and the general category of others.

(b) *The inference that "it is male psychologists who are different"* – an inference suggested by the graph summarizing gender/major interaction. Here again my concern is with the size of the subsamples. The total number of psych students in the sample was only 11, which means that either the number of male psych majors was less than six or else the number of female psych majors was less than six. Moreover, we're given no indication of how many of the 17 economics majors were female – though if the sample is representative of economics majors in this regard, the number of female economics majors in this study is presumably small<sup>8</sup>. Statistical analyses notwithstanding, I'm leery about floating any generalizations from subsamples this small, especially when subjects are not randomly selected.<sup>9</sup>

(c) *The inference that individuals have a "competitive part" and a "cooperative part" and that the order in which those parts manifest themselves varies with gender.* This is a fascinating idea, and it shouldn't be dismissed out of hand. However, two factors make me skeptical of this reading without further investigation that lends it considerably more support. (1) It is quite possible that playing prisoner's dilemma games is a new experience for most of the subjects, and the variation in performance may be a sign that subjects are simply exploring the possibilities of the game.<sup>10</sup> It would be interesting to see what happens when subject are asked to play a large number of such games. Do subjects eventually fall into a consistent pattern, or do their responses continue to alternate without any "objective" reason for variation? (2) There appears to be much more variation in response in this study than there was in the Frank, Gilovich and Regan study, which had 8 times as many subjects. Frank, Gilovich and Regan (p. 165) report that 207 of their subjects either cooperated with or defected from each of their two partners and only 60 subjects cooperated with one partner and defected on the other. In other words, only about 20% there subjects were "switch hitters" in a sequence of two games.

For me, the moral of the observations I've labeled (a)-(c) is simply that the research should proceed apace. The results are potentially suggestive, but hungry as I am I'd like to see more data before I bite.

## Endnotes

<sup>1</sup>De Dreu & Boles also exhibit some tendency to lump these two together. See for instance the passage on p. 257 where the contrast is drawn between those who adopt cooperative heuristics and those who adopt "competitive heuristics (such as: 'your gain is my loss,;' 'the winner takes all,' and 'never trust your opponent')...." See also p. 258.

<sup>2</sup>It is, I think, interesting that in Frank, Gilovich and Regan, in the "unlimited" version of the experiment subjects were permitted to get to know each other before playing the prisoner's dilemma game and could make promises not to defect during the game (though there was no way to enforce such promises). The authors report, "For subjects in the unlimited subsample, we found that the difference between economics majors and nonmajors virtually disappears once subjects are permitted to make promises to cooperate" (in Frank, Gilovich and Regan 1993, p. 166).

<sup>3</sup>Or for that matter, John Rawls, though with a decidedly different cast from that of Gauthier and Narveson.

<sup>4</sup>In addition to the differences noted in James' paper, a further difference consisted in the fact that Carter and Irons worked with 92 subjects divided into four cells: freshmen economics majors, freshmen non-economics majors, senior economics majors, and senior non-economics majors.

5 James et al. say these results should be interpreted cautiously, because in their study the "imaginary games" were preceded by "real games," while in the Carter and Irons study that was not the case. It is possible, therefore, that the difference in means between the Canadians and Americans could result from that particular difference in the two studies.

6 Commenting on the Carter Irons study, Frank, Gilovich and Regan report (p. 161) that other researchers have shown that "most highly one-sided offers are rejected in the name of fairness."

7 Several years ago, the American humor magazine *Spy* had a guide to spotting Canadians traipsing around American cities. One tell-tale sign was supposed to be that when applying for a job, a Canadian asks about pensions benefits before asking about salary.

8 Or so Frank, Gilovich and Regan think. See p. 164 of their study.

9 It is true that James et al. report that subjects were randomly assigned to the 11 triples that played the game. The effect of doing so, however, is not the same as the effect of randomly assigning subjects to different treatment groups, since the behavior of the subjects was not affected by which group they were in - the subjects were not reacting to the behavior of other members of the group, and every group played the same game according to the same rules and with the same payoff matrix.

10 I think such an explanation can't be ruled out, even though the subjects knew they would receive no feedback on the consequences of their choices.

## References

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