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CORRESPONDENCE

Editors,

A recent paper of Binmore appears to contain a fundamental logical error.

At the beginning of his contribution to the recent symposium on information and knowledge, Binmore (Why the Distinction Between Knowledge and Belief Might Matter *EJW* April 2005) adduces the following motivating example:

> Alice is a perfectly rational decision-maker who values her own safety. She therefore won't step in front of a car when crossing the road. I am so sure of my facts that I attribute probability one to this assertion. But what was my reasoning process in coming to this conclusion? I have to contemplate Alice comparing the consequences of stepping in front of a car with staying on the kerb. But how can Alice or I evaluate the implications of the former event, which we know is impossible? In mathematical logic, anything whatever can be deduced from a contradiction.

The entire seven-page article ensues in this spirit.

With all our respect and admiration for Ken Binmore, we are dumbfounded by his analysis. The assertion in question is, "Alice won't step in front of a car when crossing the road." Let's call this p. Binmore became convinced of p by a reasoning process involving several elements, including Alice's concern for her safety. He then asks, "What was my reasoning in coming to this conclusion?" That is, he wishes to review the reasoning leading to p. To do so he—and Alice—contemplate the consequences of $\neg p$ (the negation of p); namely, that she does step in front of a car. Considering the consequences of $\neg p$ in seeking to establish p is a universally accepted procedure both in everyday thought and in formal logic; it is called reasoning "per absurdum," or "indirectly." So far, so good.

Now Binmore asks, "How can Alice or I evaluate the implications of the former event [i.e. $\neg p$], which we know is impossible? In mathematical logic, anything whatever can be deduced from a contradiction." But Ken, that you know $\neg p$ to be impossible is only because you previously convinced yourself of p. You are now reviewing the reasoning leading to that previous conclusion, i.e., to p. In this review, surely you cannot assume p itself—you cannot assume what you wish to prove!

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KEN BINMORE RESPONDS:

Dear Bob,

I don't think you will find many takers for the claim that it is a logical error to say that the statements:

> *Alice never acts irrationally*
> *Alice has acted irrationally*

contradict each other. I guess that you mean that these statements should not be allowed to arise simultaneously when working out what is rational for Alice. However, *after* you have worked out what is rational for Alice in some situation—perhaps using your own favored definition of rationality—then doubtless you do agree that it becomes meaningful to pose the counterfactual:

> *What if a perfectly rational Alice were to act irrationally?*

Such counterfactuals seem to me of the essence in discussions of backward induction, because what keeps rational players on the backward-induction path is their prediction on what would happen if someone were to stray. You defend a theory in which such counterfactuals don't need to be interpreted in considering what common knowledge of rationality implies—

and which I think therefore can't possibly be right (Binmore 1997).

My own position is that it is not possible to say what is rational in a game without specifying how it makes sense to interpret the relevant counterfactuals. In the example given in my note, I differ from Reinhard Selten (1982) on how best to interpret counterfactuals only in allowing my "trembles" to be correlated with each other. In such cases, it is easy to see that rational play need not always follow the backward-induction path. If one makes the trembles independent of each other, then rational play will always follow the backward-induction path.

I do not like the conclusion that rational play depends on the context in which a game is being played any more than you, but at least it makes game theory a more interesting subject.

Ken Binmore
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References:

K. Binmore. 1997. Rationality and Backward Induction. *Journal of Economic Methodology* 4: 23-41.

R. Selten and U. Leopold. 1982. Subjunctive Conditionals in Decision Theory and Game Theory. *Studies in Economics: Volume 2 of Philosophy of Economic*s, ed. Stegmuller, Balzer, and Spohn. Berlin: Springer-Verlag.

Editors,

Regarding Warren Gibson's thought-provoking piece (The Mathematical Romance: An Engineer's View of Mathematical Economics, *EJW* April 2005):

At the end of his significant contribution, Gibson asks: "What if real answers to urgent problems could be delivered in plain English?"

But if mathematical economics (particularly, the model-building side) is a science like any other, isn't its mathematical nature part of its scientific character? It follows that model-building economics is to be assessed in this respect with the other sciences. So the following counter-questions arise:

1. Which genuine science uses plain English? All the sciences we know use mathematics at their core. Why should economics alone be different?
2. All the other sciences are significantly mathematical *and* highly successful. Gibson, however, feels that economics—*because* it is mathematical—does *not* discuss serious problems! There's something wrong here.
3. Furthermore: The overwhelming majority of economists are there because they wish to pursue a science. Again, how can a science be conducted in plain English? To be scientific is to be mathematical.

In sum: those who complain about mathematical modeling, etc. in economics should explain how a science can be *non*-mathematical.

Of course, one could argue, that economics is not a science. But then it could not be neoclassical economics—it would have to be something else.

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Editors,

Three remarks about Daniel Klein's The PhD Circle in Academic Economics (EJW April 2005): First, the data on Chicago contains at least one mistake—Szentes is from Boston University, not Boston College. Second, it would probably be best to exclude emeritus, and other very elderly profs, because I believe that PhD granting was more concentrated 50 years ago, in large part because the market scale was so much smaller. On the other hand, and third, I have done this kind of analysis for just Asst profs (my concern was quantifying Chicago's placement), and found that Harvard, MIT, and

Chicago PhDs (all 3 in about equal numbers) were sitting in most of the Asst Prof spots at top 15 depts, so maybe your results would be similar if you use only Asst profs.

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