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Today

Yesterday's issue contained Graham White's concluding comment in his debate with the referees. Today's issue contains a write-in comment by Murray Shanahan in defense of the article, and the editorial decision concerning acceptance. Please read on to find out how the story ended.

ETAI Publications

Discussion about received articles

Simulation, Ramification, and Linear Logic

by **Graham White**

Comment from: Murray Shanahan

Here's a small contribution to the Graham White paper debate.

First, let me point out that I haven't (I'm ashamed to say) got around to reading Graham's paper, yet. So I'm not in a position to have any overall opinion on its merits. But I have followed the debate, and this has led me to the following thought.

Graham lists the following advantages of his approach.

- *Correctness on a wider range of examples than any other approach, apart from Thielscher's [6];*
- *Extensionality: that is, the formalism admits a robust notion of change of vocabulary (what is technically known as interpretation), and this notion of interpretation is supported by a semantics;*
- *Computational efficiency.*

Certainly (i) on its own, if true, suggests that the significance of the paper's contribution is sufficient for a journal. (I can't comment with respect to the other usual criteria for acceptance, not having read the paper.)

But I think Graham is selling himself short. Merely to match the capabilities of existing formalisms using a radically different logic, namely linear logic, is itself a worthwhile contribution, assuming it's a non-trivial exercise. Redoing existing work in a very new way adds to our knowledge of the space of possible action formalisms, and could lead in all sorts of new directions. (Of course, the new way of doing things has to be radically different to be justified. Otherwise we end up with a proliferation of uninteresting variations on the same old theme.)

Murray

Comment from: **Area Editor**

The outcome of this refereeing process is inconclusive.

By the conventional wisdom, journal reviewing is supposed to identify which papers are to receive the quality stamp of approval, and which are not. This works fine if the quality of an article can in fact be identified by the review reports. But what do we do if there is a considerable remaining *uncertainty* - not that the paper is "medium good", but at the end of refereeing we still *do not know* what is the case – we do not have the basis for a reliable verdict?

The present article is apparently such a case. Both the two anonymous referees were strongly critical of the article. The author has answered to their criticism, and defended his case well, but some questions do remain. A first and obvious question is - if the proposed approach (as is claimed) is superior to all previously published approaches *except one* which it does not surpass, should it then be accepted to the journal? Many may be tempted to say no, but then quite a number of articles in the literature of this field ought not to have been accepted, so precedent is not consistent with such an abstract principle. Murray Shanahan, in his statement today, argues that it is in fact worthwhile to publish more than one approach in such cases. This is in itself an important methodological question in our area, where some continued exchange of opinions may be useful.

From the point of view of the editorial decision, the debate has answered some questions but raised some new ones. In particular, consider the author's argument in his concluding remark that his approach has also two other advantages, besides epxressiveness, namely its extensionality and its computational efficiency. The problem is that neither of these advantages is very much substantiated in the article. With respect to extensionality, it would be necessary to show the advantage in concrete comparison with the most natural alternatives. The article makes an argument about problems in alternative approaches due to intensionality, but the problems he describes can possibly be explained as due to the use of domain constraints in the examples he refers to. One would have liked to see an argument why Thielscher's approach (which is otherwise mentioned as the contender) does not have the proposed extensionality property.

Similarly, the author makes a general argument that "there is a well-developed technology for implementing proof search in these systems", but again a more concrete comparison with implementations of the major alternative approach(es) would be needed in order to defend this argument.

These are objections to the effect that the article's arguments are incomplete, and not necessarily wrong. Some of these questions might be resolved by continued debate, but we also need to get to a decision with respect to the journal acceptance. The discussion can continue anyway, regardless of whether the article is accepted or not. And finally we must observe that the use of linear logic in itself often arouses strong feelings in the community, so it may not be possible to arrive at a consensus.

With this we return to the original question: what is the most appropriate decision when the quality of an article is undetermined, with a high degree of uncertainty in the estimate? If a journal is seen as a means of active communication between researchers, which presumably is how modern journals started, then controversial contributions ought to be very highly valued. On the other hand, if journals are assigned prestige levels based on the perceived quality of their articles, and other articles in the same journal are assigned a quality level based on the journal's prestige, then it is in the interest of other authors that only uncontested articles be accepted.

We may regret the contemporary practice of evaluating articles based on the ranking of journals, both because that practice institutionalizes lack of insight, and because of its side-effects on the editorial policies of the journals, but it is a fact of life and can not be ignored.

Our solution to this dilemma uses the fact that we also publish the *Electronic News Journal on Reasoning About Actions and Change*, besides the ETAI. The primary purpose of the News Journal is to publish, on a monthly basis, the debate contributions and debate notes that appeared in the Newsletters, so that they also obtain formal, published status and can be cited.

In any case, it seems appropriate that Graham White's article ought to be accompanied by the discussion that we have had here. In that way, it becomes transparent that the article is taken seriously by the community, but that there are strong differences of opinion about the proposed approach, and it is left to each reader to consider the arguments pro and con, and to make her or his own assessment.

The conclusion, therefore, is to decline the submitted article for the ETAI, but to offer the author that the article be published in the ENRAC (News Journal) for April, 1999 together with the discussion and the present concluding comment. The continued discussion will then appear in later issues of the News Journal, like for all other articles.

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