Electronic News Journal on Reasoning about Actions and Change

Editor: Erik Sandewall

Volume 3, Number 1-3

Contributions during
January - March, 1999

ISSN, print: 1403 - 2031
on-line: 1403 - 204X

Linköping University Electronic Press
http://www.ep.liu.se/ej/enrac/1999/01/

Publication Trail and Update Rules

The Electronic News Journals (ENJ) are a medium for exchange of scientific information and debate. In particular, they serve as the forum where articles received by the ETAI (Electronic Transactions on Artificial Intelligence) are discussed publicly for review, in separate News Journals for each ETAI area.

The Electronic News Journal on Reasoning about Actions and Change (ENRAC), in particular, uses a publication trail where contributed information is first distributed on a daily basis as *Newsletters* using electronic mail and web pages in HTML. Then, contributions during the same month are compiled into an HTML version of the monthly *News Journal*. Finally, the same information is converted via Latex to a postscript version that is suitable for printing on paper and reading off-line. It is formally published and archived by the Linköping University Electronic Press in both its electronic and its paper form, as described on page (iv).

Offprints of individual segments. Each News Journal issue consists of a sequence of *segments* relating to specific topics. "Offprints" of these can be obtained from the URL:s indicated at the head of the first page of each segment, or from the ENRAC service page specified below. It is intended to maintain these URL:s and their contents for the foreseeable future. Misprints of minor significance are corrected in these offprints (but not in the E-Press version since it is considered archival).

Corresponding HTML edition with dynamic links. The HTML edition contains considerable numbers of links to other pages and structures on the net: articles that are available on-line, home pages of conferences and of individual researchers, links to other part of the ETAI structure, and so on. Due to the natural limitations of the paper medium, only some of the WWW links have been retained here as footnotes. In order to retrieve articles and other information that are referenced in the present issue, it is recommended to look up the corresponding issue in HTML and to use its link. The HTML issues of the News Journals on Reasoning about Actions and Change can be found at the ENRAC *service page,*

`http://www.etaij.org/rac/nj/`

It is intended that the HTML issues will by updated continously to the largest extent possible, for example by replacing URL links to the home pages of authors that have changed to another site.

Table of Contents

DATES OF PUBLICATION

Since the date of publication may be understood either as the date of first public appearance, or as the day of reproduction on paper in many copies, and since both of these definitions may be difficult to apply in the case of electronic publication, we make the following clarifying statement.

The contents of the present issue were put on-line in their original, versions as Newsletters in plaintext, HTML, and latex/postscript during the month of January - March, 1999. Then the contents were edited and formatted, resulting in the present, formatted version which was published on 16.7.1999, in two concurrent editions: an on-line edition and a paper edition. The on-line edition was timestamped electronically and put on-line by Linköping University Electronic Press at the URL specified on page (i). The paper edition was obtained by printing the on-line edition on a standard computer printer. It was reproduced in 25 copies, legally archived, and made available for distribution.

Electronic News Journal on Reasoning about Actions and Change
Vol. 3: *1-1*, 31.3.1999
http://www.etaij.org/rac/nj/9901/

The Months of January - March

Summary of News Journals Contents

The three months of January through March, 1999, have been combined into a single News Journal issue since there was relatively little discussion during this period. Almost all the discussion concerned the ETAI submitted articles.

In the discussion about the ontology of time, Johan van Benthem answered to Erik Sandewall's questions from a previous month. Pat Hayes made an additional contribution to the same topic.

Ernest Davis contributed a "Guide to Axiomatizing Domains in First-Order Logic", which is included in the present News Journal issue.

The ENRAC initiative to invite reference papers for established approaches to actions and change had its first external effects as Enrico Giunchiglia and Vladimir Lifschitz presented a paper that builds on the reference articles in relating several of the proposed approaches to each other. This is very much in the spirit of the intentions with our reference article initiative. The Giunchiglia and Lifschitz paper is entitled "Action Languages, Temporal Action Logics and the Situation Calculus".

Vol. 3: *2-2*, 31.3.1999
http://www.etaij.org/rac/nj/9901/

Publication practices

Pat Hayes

John McCarthy's 'grumble' in ENRAC 30.3 (99007) raises a general issue of nomenclature.

The following scenario is quite a common one. A formal term of art becomes generally used to refer to something in the literature which was invented or introduced by someone. After some time has elapsed, however, another author or group re-adopts or re-uses the term in a somewhat different sense, and this second sense is then widely used by a related community, or among a different generation of researchers. This has happened for example to the word "ontology", which is now widely used in the, er, ontology community to refer to what philosophical logicians (who originally had possession of the word) called "axiomatic theories". In our community it has happened to "situation", which is now widely taken to mean what Reiter (et. al.) defines it to be, ie a finite sequence of actions, and its cognates such as "situation calculus", which Reiter (et al) uses to refer to a particular formalization developed at Toronto in the last decade. Neither of these meanings are what these terms meant before Reiter changed the language.

The result of such alterations in meaning is pernicious only if people fail to realize that it has happened, or are unclear about which sense they intend when they use the term; but when this does happen, a great deal of confusion, not to mention emotional heat, can be generated. Once introduced into a community, terminology has a life of its own, and nobody can expect to exert a kind of eternal authority over it. But any useage of such a term should clarify which sense is meant; and any writing which purports to be a definitive *survey* of a topic must be especially careful about such ambiguities, and to make clear to a reader that the word has changed in meaning. Evidently what Reiter and his co-authors mean by "situation calculus" isn't what John McCarthy invented. Regardless of the relative merits of the calculi being referred to, a survey with that title should at least clarify and respect the way that the terminology was used for about a quarter of a century before Reiter re-defined it.

Pat Hayes

Vol. 3: *3-4*, 31.3.1999
http://www.etaij.org/rac/nj/9901/

Report on ETAI Visibility

Erik Sandewall

ETAI now addressable under .org

The ETAI and its divisions now have a URL under `.org`, called **etaij**, so the entry webpage for reasoning about actions and change is found at `http://www.etaij.org/rac/`, and the entry webpage for ETAI as a whole is at is found at `http://www.etaij.org/`. Please update your links accordingly. Please change any links to the ETAI that you may have posted. (The old URL will remain valid for the immediate future, however).

The 'J' can be interpreted as 'Journal' or 'Junction', making a virtue out of necessity since "etai.org" had already been taken.

ETAI getting attention for new approach to electronic publishing

The ETAI represents a new approach to communicating research results as well as to communicating *about* research results, as we all know. There has been a lot of interest in this initiative recently, as witnessed by the article in the January, 21 issue of *Nature*. Under the headline "The writing is on the web for science journals in print", Declan Butler writes about new trends in research publishing; he mentions the ETAI and also refers to the possible adoption of a similar approach by the British Journal of Medicine.

The present area editor has also been invited to a number of workshops about publishing, including the *AAAS/UNESCO/ICSU Workshop on Developing Practices and Standards for Electronic Publishing in Science* that took place in Paris on 12-14 October 1998. I have as well received a number of inquiries from collegues in other fields who are interested in adopting the same approach as we are using.

Several of these articles and workshops are available on the net. In order to keep such information organized, we have introduced an additional part in the ETAI webpage structure, with the heading "Methodology and Publication in Research". This area will contain the same kinds of information services as we have for the ENRAC, except of course that it does not receive research articles for reviewing. If there is enough interest we may start a newsletter for that area, but at present it merely contains links to articles about electronic publishing in the ETAI and elsewhere.

To access the area for Methodology and Publication in Research (MPR), please proceed as follows: In the main webpage for the ETAI Junction, `http://www.etaij.org/`, select "ETAIJ information areas" in the lower part of the left-side menue. This will replace the blue menue with a similar one containing the link to MPR.

Electronic News Journal on Reasoning about Actions and Change
Vol. 3: *5-8*, 31.3.1999
http://www.etaij.org/ra/rac/010/

Protocol of on-line discussion during January - March, 1999
about the following research article:

Murray Shanahan

A Logical Account of the Common Sense Informatic Situation for a Mobile Robot

C2-1. **Murray Shanahan** (31.1)

In the newsletter of 11.9, in answer to one of Chitta Baral's questions of 19.8, I wrote: "There is an axiom missing, which says that nothing is a member of Nil. I'll put this in the final draft". In fact, this axiom is unnecessary, as it follows from Axioms (Bo3) and (Bo4) of my formalisation.

Murray

Q5. **Anonymous referee 1** (27.3)

Much of the work in the paper has already been published. That's not a criticism, since it is signaled by the author himself, but the paper seems to initiate a third kind of article in the ETAI, between reference papers and crisp new results. I took a real interest reading it, because it collects a bunch of results into a single framework. Showing how those fragments, considered as specifying pieces of a common task, can consistently work together and can help mastering the architecture of a reasoning robot is, according to me, a real contribution worthy of publication in the ETAI (parenthetically, this contribution could be more neatly stated in the abstract).

The discussion with Paolo Eduardo Santos and Chitta Baral has been extensive and several points have been improved. Here under are some more suggestions, which are not conditions to the publication, but that Murray Shanahan can follow if he feels they are improvements.

- In part 4, I suggest: "a region is *an open (path)-connected* subset of $R \times R$ ... Objects occupy regions". That's a more standard definition of region, which is not a synonymous of subset in standard math. Moreover, with the present definition, according to (Bo1) p16, a closed set has an empty boundary.

- while the reader can understand what is meant by a direction, the definitions could be more precise. In (SP3), since *Bearing* is real-valued, it is not a function (it is only defined modulo 360) and is not defined if $P1 = P2$. It is then unclear in (SP4) if $P1$ belongs to $line(P1, P2)$, while $P2$ does. On the other hand, in axioms (H1)...(H3) p12, the modulo arithmetic is implicit, and e.g. the bearing value -10 can not be replaced by +350 in the inequalities.

I suggest to point that a bearing is in fact an equivalence class which may be represented by any of its members, without further developments. (SP3) could then be simplified to :

$$Bearing(P1, P) - Bearing(P2, P) = 180[mod\ 360]$$

(and including the end points if prefered). Also, the definition of intervals in (H1)...(H3) could be left to the reader's background knowledge.

- The question raised by Chita Baral about touching triangles in (B6) can be solved if $PbelongsLine(P1, P2)$ is omitted in the third line of this axiom.

- (Bo5) could be simplified:

Connect(c) is the region g such that $\forall P2, l[Boundary...]$*, if such a region exist, then empty set else...*

I suppose the definition of c is meant to exclude crossed polygons (two lines of c having a common point appart from common end points mentined in the list)

Also in paragraph 3, line 3 of 6., change $Boundary(g, l)$ and in the last line of (Bo5) $\exists P3, z \forall p2...$

- p18, just above (B8): I doubt that the condition for the robot leaping over an obstacle is this one, which does not depend on the size of the robot (if I do not misunderstand the sentence). Could it be clarified which location/circle of uncertainty is relevant ?

- p22 def 7.11 I suggest clarifying that MIN is a parameter of the definition.

Q6. **Anonymous referee 2** (27.3)

The paper describes interesting results, which have been originally presented at ECAI-96, that researchers working in the area of reasoning on actions and change ought to know about. It is well-written and clear. At some points, where rather natural solutions to specific problems are proposed, the presentation is too turgid.

The following are my answers to the standard referee questions:

1./2. There is a short (10 lines) abstract, but there is not a summary where the main results are specified. I would ask the author to provide it.

3. No.

4. Even though the paper is basically well organized, it can be shortened. Some general discussions as well as some straightforward proofs, e.g., the proof of Proposition 2.8, can be omitted and/or summarized.

5. The major limitation of the paper is that it is not up-to-date (as reported in its cover page, its last revision dates back to April 1996).

I wish to make the following additional comments regarding policy. One of the major advantages of an electronic journal as ETAI is that it guarantees a rapid publication of new results. In particular, it reduces the delay between the presentation of the results of a research work at a conference and their publication in a journal paper.

I believe that one cannot publish old results without putting them in the current state-of-the-art. In the specific case, I do not think that the proposed contribution has been invalidated by later developments in the field, but I would ask the author to discuss such developments in the paper. My suggestion is to provide an additional section relating the work to what has happened elsewhere since then as well as to its subsequent developments (e.g., the simpler description of the robot's environment, mentioned in an answer to Paulo Eduoardo Santos).

More precisely, I would like to ask the author whether nothing has been published on the subject since then that deserves to be taken into account (if this is his opinion, it must be explicitly formulated). I just mention the subsequents developments of the logical approach proposed by Lespérance et al. (cf. the special issue of the Journal of Logic Programming on Reasoning about Action and Change, as well as the Proceedings of KR'98) and Galton's work on representation and reasoning about spatial knowledge. Furthermore, the paper reports the results of preliminary experimentation with the robot. Has experimentation been systematically executed? What are its outcomes?

Another relevant point: complexity issues are completely neglected in the paper and dealt with rather quickly (and superficially) in one of the interactions with Paulo Eduoardo Santos. I ask the author to briefly discuss them in the paper. In particular, he should at least summarize the known existing results about the (worst-case) complexity of logic-based abduction and circumscription, and explain his opinion about the impact of these complexity results on research in logic-based/cognitive robotics.

Minor point: I would remove from the abstract the sentence about the novel solution to the frame problem (inspired by the work of Kartha and Lifschitz). It seems to me a rather specific technical point (as the author explicitly recognizes in his answer to Chitta Baral), mainly concerned with the adopted formalisms rather than with the considered problem. Moreover, if the form of the considered theories actually guarantees that the circumscriptions (almost) always reduce to predicate completions, then I believe that this fact should be explicitly stated.

References: please check/integrate [Charniak & McDermott, 1985], [Miller, 1996], and [Shanahan, 1996].

C6-1. **Area Editor** (30.3)

The second part of the comments by Anonymous Referee 2 raises an interesting policy question for the ETAI, namely to what extent shall commentary material be placed in the article itself, and to what extent is it better placed in the article's discussion session. With 'commentary' material I include comparison to related work that occurred concurrently with or after then work being reported in the article; discussions of alternative ways of realizing subsystems of a largeer system being presented in the article; discussion of intrinsic theoretical limitations of the proposed approach; and so forth.

The A I literature has traditionally required fairly extensive consideration of commentary material. This is clear in comparison with other fields, such as physics, where articles usually seem to be much more focussed on a single mission: define the problem, discuss the state of the art relating to this problem, present the new solution, done.

The emergence through ETAI of a systematically maintained dialogue connected to each article offers an alternative approach: instead of the author trying to answer all questions about the topic that some readers might ask, he or she could focus on the main problem that's being addressed and on its solution, and leave the rest to the question period. A change in this direction would have its pros and cons. The advantages are in the increased flexiblity and the possibility of adding material over time, even after the date the article appeared in the journal. The disadvantage might lie in a possible loss of systematicity, and in the resistance among readers against changing old habits.

As the second half of the statement by Anonymous Referee 2 is a suggestion for including several types of commentary into the article, it would be possible syntactically to rephrase most of these suggestions into questions that the author could answer in the discussion page. Whether it is better to answer the questions this way or by amendments in the article itself may be a matter of debate, and it would be useful to have a decision about a recommended policy in this respect.

Since the same question is likely to come up at other times, I will ask the ETAI policy committee and our area editorial committee to study it and to make a recommendation. Comments by subscribers are invited.

For the time being and for the present article, I will ask the author, Murray Shanahan, to consider the second half of the referee statement as a set of questions that can be answered either by amendments to the article, or through the review dialogue. Based on the referee reports, the article is accepted for the ETAI while giving the author an option of minor modifications in response to the suggestions by both the anonymous referees.

Vol. 3: *9-11*, 31.3.1999
http://www.etaij.org/ra/rac/012/

Protocol of on-line discussion during January - March, 1999 about the following research article:

Graham White

Simulation, Ramification, and Linear Logic

Q3. **Anonymous Referee 1** (27.3)

The following are my answers to the refereeing questions.

> *1. Are the results of the article, as specified in the summary, of significant interest for researchers in our area?*

No. The main objective of the paper is to propose a solution to the Frame Problem which satisfies certain properties, which are convincingly argued for. My only but serious concern is that existing solutions seem to satisfy all of the requirements and that the author fails to argue for merits of his approach in comparison. In particular Reiter's theory [1] of generating successor state axioms from simple effect axioms fits perfectly the author's desiderata. And yet this work, which is among today's most cited papers in the area of Reasoning About Actions And Change, the author does not even mention, let alone compare it to his own method.

[1] Ray Reiter, "The frame problem in the situation calculus: A simple solution (sometimes) and a completeness result for goal regression," in: Artificial Intelligence and Mathematical Theory of Computation, Academic Press, 1991.

> *2. does the full text substantiate the claims made in the summary?*

Yes.

> *3. is the text intelligible and concise?*

Yes.

> *4. do you know of any previous publication of the same result?*

No. (But see my answer to Question 1.)

5. are there any other considerations that may affect the decision?

No.

Q4. **Anonymous Referee 2** (27.3)

My recommendation as regards the acceptance of this paper for ETAI is no. Accordingly, I have perhaps to explain that.

Roughly speaking, my answer is no, because there is a very little difference between the submitted and revised versions of the paper. On the other hand, the remarks, especially those made by Andreas Herzig, were quite important, and so they should be seriously taken into account in the final version. I am not going to be very detailed, but one example should illustrate this.

Andreas writes:

> *I guess this relates to my main criticism: in order to identify actions in proofs, you must group together sequences of rules related to the same action.*

Answer of Graham:

> *No, not necessarily; if you look at the examples in 'Actions, Ramification and Linear Modalities', you can see that you don't need to group rule applications together like this.*

The problem is that I would like to know certain important things not necessarily studying various tech reports, even if they are achievable by Internet. In preparing conference papers, we are usually short of space. But this is a journal paper and there is no reason to make it shorter than necessary. What is worse, many points mentioned in the open discussion have been completely ignored in the final version.

I do not claim that the author cannot do certain things mentioned in the open disscussion phase. But his task is to convince me that he can. Since I am not convinced, my answer as regards the acceptance of the paper is no.

Q5. **Area Editor** (29.3)

Before I draw the conclusions from these two referee's reports, first some observations on procedure, and in particular on the room for debate in the context of referee's reports.

Referee reports may fall into three categories: *content*, *criteria*, and *policy*, where criteria commentary consists of questions to our standard questions to referees; content commentary concern the material in the article as such, and policy comments concern or touch on the meta level as to what requirements we ought to have for ETAI accepted articles.

In the simplest case, the referees only return comment according to the criteria, and in this case the area editor has no reason to add anything. Comments on content are treated as part of the open review discussion, and may be responded to by the author(s) and by others in the usual way. Comments on procedure, finally, will be interpreted in the context of what has been stated by the ETAI policy committee and the area editorial committee for our area. As area editor I may relate referee's policy comments to our established policy. If it is not clear about the issue under consideration, then the question will be referred to either of the mentioned committees, for their decision. However the readership is welcome to contribute to the meta level discussion as well: the ETAI is still formative, and comments by readers will be very useful for the policy and editorial committees in their consideration.

In the present case, Anonymous Referee 1 returned criteria comments, but his/her answer to question 1 also goes into questions of content. I will therefore give the author and others an opportunity to respond before the decision is made.

Anonymous Referee 2 refers to the lack of consideration of issues that have been brought up by Andreas Herzig in the review discussion; please note by the way that referee 2 is *not* identical to Andeas and does not work with him. The referee refers to the fact that the article relies on a technical report, and suggests that it ought to be self-contained instead.

In fact, the ease of access to articles over the Internet has raised the question whether we ought to relax the habit that articles be self-contained, and instead encourage inclusion of material by explicit linking. This was up for debate a year ago, and the answer was inconclusive: different readers have different preferences. The invitation for reference articles in our ETAI area represents one distinct step in the direction of reducing self-containedness. That case is a bit different however: reference articles are supposed to be logically included in their entirety, whereas the present author refers to an unspecified but local position in a long technical report.

In the present case we agree with Referee 2 that Herzig's questions would have motivated a more extensive coverage in the article itself, in particular because of the length of the referenced technical report. The broader policy issue of self-containedness in articles will be referred to the policy committee and our area editorial committee.

We will be back with the decision about the article after the author has had an opportunity to respond.

Electronic News Journal on Reasoning about Actions and Change 12
Vol. 3: *12-20*, 31.3.1999
http://www.etaij.org/ra/rac/013/

Protocol of on-line discussion during January - March, 1999
about the following reference article:

Michael Thielscher

Introduction to the Fluent Calculus

Q1. **The Editor** (15.3.1999)

The present introductory remarks refer to the statements by Anonymous Referee number 1, which are listed below.

The standard procedure in the ETAI is that we have the discussion during the open review period, and referees only make recommendations for a yes or no decision on acceptance. However, we also wish to accomodate the case where a referee adds substantially to the discussion about an article, as is the case here. In keeping with the ETAI's idea, we wish to make these comments open to the author (to give him a chance to answer) as well as for the readers of the Newsletter and web page who may also contribute to the discussion.

Both the author's contribution and the referee's comments must of course be interpreted in the context for which the article was submitted, that is, as a reference article. The explanation of the reference article concept, and the refereeing criteria for acceptance, can be found from the general ETAIJ web page by clicking "For Authors" and "Referee". The first criterium was specified as follows:

- Does the article represent a tradition or "approach" where there is already a sufficient volume of work in the field?

Our assumption was, of course, that any approach that satisfies this requirement should already have established its "case" which everyone does not necessarily agree with, but in case of controversy the approach should have its defense and its defendants. Refereeing with respect to the pros and cons of the approach would not then be required. However the first part of the comments by Anonymous Referee 1 do question the approach as such.

Since reference articles were not supposed to include a defense of the respective approach, it is appropriate to open the discussion at this point and to obtain the author's point of view. Other contributions to the discussion are also welcome.

The second part of the Referee's comments refer to refereeing criteria number 2 and 5:

- Does the article concisely specify the assumptions, motivations, and notations used in that approach? Does it correctly capture the assumptions, etc. that have been used and are being used?
- Is the article pedagogical and sufficiently easy to read, but at the same time precise and correct?

The remaining criteria include the question whether the article can effectively be used as a standard reference for the notation and the basic concepts of the approach being presented, for use in other articles using that approach.

Q2. **Anonymous Referee 1** (16.3.1999)

Objections to the approach

The idea in this approach is a fairly simple one. 'States' (it would be better to say 'state descriptions') are lists of atomic assertions encoded as terms, and one uses a unification trick to ensure that any things in the list that aren't explicitly mentioned in the 'update axiom' get automatically transferred to the list which describes the post-action state. However, the 'deletion' here depends on an assumption about unification which isn't logically valid: that if two terms don't unify, they have disjoint referents.

Apart from the logical difficulties, this is computationally trivial (sub-STRIPS) and unoriginal. It is indequate for realistically complex domains. For example, take the toy blocks world as given, and add 'Above' to the vocabulary, defined recursively in terms of 'On'. The update axiom now needs recursion. Examples like this have been around for a quarter of a century. In general, the FP is only nontrivial when one has a vocabulary such that some aspects of the state description depend on others in ways that vary with the particular situation: in STRIPS terms, not all the vocabulary is part of the core. If not, then one can just have explicit add and delete lists for each action description, which is essentially what Thielscher is doing here, but in Prolog.

It doesn't have a clear semantics. The account given uses logical terminology, but it doesn't really use first-order logic. The extended unique names assumption is so strong that it renders equality trivial, as the authors take pains to show.

The limitations of the techniques being used ought to be carefully stated. In particular, the authors should refer to the STRIPS planner developed at SRI in the 1970s and explain how these techniques are inferior to those used by it.

A2. **The Author** (27.3)

These are responses to the objections raised by the anonymous reviewer against the approach:

> *However, the 'deletion' here depends on an assumption about unification which isn't logically valid: that if two terms don't unify, they have disjoint referents.*

True. This was the reason for adding the additional equational axioms EUNA, under which the conclusion becomes logically valid.

> *Apart from the logical difficulties, this is computationally trivial (sub-STRIPS) and unoriginal.*

Not true. Even the basic Fluent Calculus goes beyond STRIPS in that it does not assume complete information about world states.

> *It is indequate for realistically complex domains. For example, take the toy blocks world as given, and add 'Above' to the vocabulary, defined recursively in terms of 'On'. The update axiom now needs recursion. Examples like this have been around for a quarter of a century.*

True. This is the origin of the Ramification Problem. The Fluent Calculus uses one of the most expressive of the existing solutions to this problem (reference [13]). As has been illustrated in Section 4.3., the state update axioms rely on some sort of recursion indeed.

> *In general, the FP is only nontrivial when one has a vocabulary such that some aspects of the state description depend on others in ways that vary with the particular situation: in STRIPS terms, not all the vocabulary is part of the core.*

State constraints of this kind are straightforwardly formulated in the Fluent Calculus, and the abovementioned solution to the Ramification Problem handles with those cases.

> *It doesn't have a clear semantics. The account given uses logical terminology, but it doesn't really use first-order logic.*

Not true. The Fluent Calculus has the standard semantics of classical logic. Full first-order logic is being used almost everywhere: in the conditions of state update axioms, when formalizing partial knowledge of situations, in state constraints.

> *The extended unique names assumption is so strong that it renders equality trivial, as the authors take pains to show.*

I don't know what you mean by "trivial." In fact an efficient treatment of equality (AC1-unification) is among the crucial aspects towards an efficient automated reasoning system based on the Fluent Calculus.

Q3. **Anonymous Referee 1** (16.3.1999)

Objections to the presentation as a reference article.

I confess it was very hard to finish the first page without getting very puzzled. The text assumes the reader shares nonstandard definitions of technical terms from obscure conference papers, and the illustrative example is very hard to understand.

Example 1 uses a (binary!) function symbol $\circ$ which applies to (three!) arguments referred to by the expressions $On(A, x)$, $On(x, Table)$ and z, where both x and z are quantified. What does this mean? Presumably On here is a function from blocks and tables, but what are its values? (Apparently, whatever they are, one gets a "state of the world" by applying the function $\circ$ to them, but that isn't much help. Thielscher casually distinguishes between 'situations' and 'states of the world' without explanation.)

Apparently, z here is supposed to be a *fact* (or, still more curious, "facts"), yet we have somehow managed to quantify over it (them?). How can one consistently quantify over facts, written using a logic, in that logic itself? (A rhetorical question, of course: one can't. Ref. Gödel).

Thielscher says that asserting $z \neq On(y, A)$ means that z does not have the "form" $On(y, A)$. But suppose we assert that (say) $On(B, A) = C$; now C does not have that form, yet it is equal to it. The author seems to confuse 'form' with 'reference' throughout the paper, in fact.

On page 2 we are told that $Holds(f, s)$ is a "macro". What does this mean?

The text ought to be rewritten and reorganised. First, the English is turgid and sometimes hard to follow. Second, the terminology being used should be carefully explained, since the uses here of "fluent" and "situation" (for example) are not those used by many other authors. Third, the logical semantics of the methods being used should be carefully explained; one cannot casually refer to a domain of quantification as consisting of "facts".

A3. **The Author** (27.3)

These are responses to the objections raised by the anonymous reviewer against the presentation as a reference article:

> *I confess it was very hard to finish the first page without getting very puzzled. The text assumes the reader shares nonstandard definitions of technical terms from obscure conference papers, and the illustrative example is very hard to understand.*

The reference you refer to is meant just as a pointer to the publication in which was introduced the concept of state update axioms. In fact the present paper shall serve as a reference article and should be self-contained.

> *Example 1 uses a (binary!) function symbol $\circ$ which applies to (three!) arguments*

I have omitted parentheses because associativity of $\circ$ is among the foundational axioms, as has been indicated in the paragraph preceding that formula.

> *referred to by the expressions $On(A,x)$, $On(x, Table)$ and z, where both x and z are quantified. What does this mean? Presumably On here is a function from blocks and tables, but what are its values?*

Meaning is given by the standard semantics. Just read the formula as a first-order formula with quantifications, predicate symbols (here: the equation sign), function symbols (here: $\circ$, *On*, A, and *Table*), and variables (here: x and z).

> *Apparently, z here is supposed to be a* fact *(or, still more curious, "facts"), yet we have somehow managed to quantify over it (them?). How can one consistently quantify over facts, written using a logic, in that logic itself? (A rhetorical question, of course: one can't. Ref. Gdel).*

My fault. I shouldn't say "fact" at this point and be more precise about the method used here known as "reification."

> *Thielscher says that asserting $z \neq On(y, A)$ means that z does not have the "form" $On(y, A)$. But suppose we assert that (say) $On(B, A) = C$; now C does not have that form, yet it is equal to it.*

The equation would contradict foundational axiom set EUNA as it stands. If one wishes to express such relations then a modification of EUNA is required.

> *On page 2 we are told that $Holds(f, s)$ is a "macro". What does this mean?*

Holds is not part of the signature; it is used just to ease readability of formulas. Whenever it occurs, it stands for a particular equational formula.

> *Second, the terminology being used should be carefully explained, since the uses here of "fluent" and "situation" (for example) are not those used by many other authors.*

By "situation" I mean exactly what for example Reiter means when he talks about situations in the SitCalc: finite sequences of actions. What I mean by "fluent" has been said formally in Section 2.

C3-1. **Anonymous Referee 2** (27.3)

Referees should never be refereed (but judged by editors). In this case, since the editor explicitly welcomed contributions to the discussion of the report of Referee 1 in ENRAC of 16 March, I feel that the following remarks are appropriate, or rather necessary. Thereby I will not take up technical issues, since these are the matter of the author, except for generally pointing out that in my understanding the referee mostly failed to do a technically careful job. Eg. his first complaint

> *Example 1 uses a (binary!) function symbol $\circ$ which applies to (three!) arguments ...*

is absolutely void. The binary function symbol $\circ$ *is* used as a binary one and applies to two - not three - arguments. The paper clearly states that $\circ$ is associative so that terms like $f \circ g \circ h$ should not at all confuse a careful referee nor the reader. Similarly with most subsequent points of a technical nature.

Well, referees are not to blamed for making technical mistakes. We all are prone to this human weakness. And authors are able to defend themselves against such mistakes. However, there are issues involved in Referee 1's report which are awkward to be addressed by any author, especially a young one, and which are not in compliance with the behavior expected from fair and responsible referees of international journals. Here I point out three such issues.

1. The referee disqualifies the paper's notation as being taken "from obscure conference papers". The author cites five papers as already mentioned above. Two of them appeared in the AIJ, one is conditionally accepted for publication in the AIJ, the remaining two appeared in the journals New Generation Computing and Annals of Mathematics and Artificial Intelligence. To flash five journal papers of this calibre as "obscure conference papers" amounts to an extremely unfair behavior which should be banned by the community.

2. The referee maintains that "the English is turgid and sometimes hard to follow" without providing even a single example or hint. I challenge Referee 1 to write a paper in a language of his/her choice, and foreign to him/her, as competent as Thielscher did in the present case. The way the referee's complaint is brought forth in this case demonstrates an example of uttermost narrow-mindedness and arrogance inappropriate for a referee of an international journal.

3. The referee disqualifies the entire approach as "computationally trivial (sub-STRIPS) and unoriginal". This remark alone would already have disqualified the competence of Referee 1. There have been many attempts to embed STRIPS in a logical framework and it was not until the discovery in the mid-eighties (prior to Lifschitz' STRIPS semantics paper) of the predecessor of the fluent calculus (the linear connection method, now also referred to as transition logic) that this fundamental issue was solved.

Anonymity of referees is an important and sacred principle. As is the avoidance of public discussions of referees' reports, for which reason I feel very uneasy with this type of discussions (yet encouraged by the editor). The rules of ETAI might be reconsidered in this respect.

C3-2. **Murray Shanahan** (30.3)

Referee 2 writes,

> *Referees should never be refereed (but judged by editors).*

I strongly disagree with this. I very much support the idea of open refereeing precisely because it means that referees are accountable for their reports. This is the only way to eradicate referees' reports that combine ignorance with arrogance. Many of us have, no doubt, been victims of them.

Now, I can imagine a researcher responding to this view in the following way: "If I'm open to public criticism myself, why should I referee papers? It's hard work, it's a nuisance, and it's voluntary. So I won't bother any more." The answer is that reviewing is not a favour, but a duty. All of us who contribute papers ourselves and expect them to be well refereed have a duty to referee other papers, and to do so with the utmost professionalism.

Q4. **Anonymous Referee 2** (27.3)

The paper is submitted to ETAI as an ETAI Reference Article. As such it does not claim to offer original results (which it does not). Rather I am asked to referee the paper under the published refereeing criteria for this particular ETAI category.

Under these considerations

I recommend acceptance of the paper provided ...

the author follows a few recommendations given below, and adds pointers (with comments) to some further articles which give a full account of the history of the approach and its relation to variants and alternative approaches (and which do exist).

This referee is in a somewhat awkward position since this report was written after having seen the report by Referee 1. Not that that report would have influenced this referee's judgment. But of course it does influence the justification of the judgment which now follows, beginning with responding to the set criteria.

> *1. Does the article represent a tradition or "approach" where there is already a sufficient volume of work in the field?*

Although this is more of an editorial issue, I can confirm that there is a substantial body of work in the literature on the fluent calculus. The five references given in the paper are just a tiny fraction of the volume of work which is even more substantial if closely related approaches are taken into consideration. It is somewhat unfortunate that the paper denies those close relatives of the fluent calculus (including STRIPS but also several others). For this reason the above-mentioned proviso attached to my recommendation in my view is of great importance.

2. Does the article concisely specify the assumptions, motivations, and notations used in that approach? Does it correctly capture the assumptions, etc. that have been used and are being used?

Basically yes, if reification is taken for granted. Since reification is a standard technique in AI it may be taken for granted as done in the paper. However as the experience with Referee 1 demonstrates, this point might as well deserve a few extra sentences and more explicit references to discussions in earlier papers. The examples given are well-selected and tied to the respective formulas with good explanations. The notations are in full compliance with standard practice. This also applies to the by now standard use and distinction of the notions "situation" and "state". As supposed to do, the paper presents the basic definitions for the approach in a concise way.

3. Would reading the present article enable one to skip the introductory definitions section of many previously published articles that used the approach?

Yes.

4. Is the article also concise in the sense that it does not contain a lot of material that is unnecessary for the above criteria?

Absolutely.

5. Is the article pedagogical and sufficiently easy to read, but at the same time precise and correct?

Yes. The author, as a non-native speaker, commands an unusually qualified use of English. Other than a missing "as" in the sentence "In order ..." in the middle of page 3 I could not find the slightest problems with the text. It is easy to read which of course is also a matter of taste and of familiarity. If more redundancy is permissible, more explanations might of course be added.

Q5. **Area Editor** (27.3)

At this point (27.3) two referees disagree quite strongly, and the author has given substantial answers to the objections raised by Referee number 1. Naturally now we invite Referee number 1 to express his/her view before the accept/decline decision is taken.

Anonymous Referee 2 also raises some important policy questions in his/her comment to question 3. In particular, he subscribes to the view that "avoidance of public discussions of referee's reports ... is an important and sacred principle", and "referees should never be refereed (but judged by editors)". This has indeed been the traditional practice, but ETAI is not traditional. One of our fundamental innovations is to open up the review process to the largest extent possible, without compromising the quality control that is obtained through anonymous referees that vote to accept or decline an article after the review period.

If all relevant questions and critique comes out during the open review, then it is sufficient for the referees to vote 'pass' or 'fail', and there is not much to debate. (It is always possible to continue the open review debate, of course). However we have had a few cases, including the present one, where an anonymous referee added his/her own contributions to the review debate, and in order to be consistent we must then make it possible to discuss such opinions by referees as well. In a similar vein there is no reason why the policies and decisions of the area editor should not be open to discussion. (However the final decisions are taken by the area editor and not by popular vote).

The system of open debate has been made practically possible through the advent of Internet, e-mail, and web pages for managing on-line debates. It would have been virtually impossible to apply it in the earlier age of conventional print-and-mail communication. In thinking about whether and how to use the new possibility, one must consider both the advantages and the possible disadvantages of the open reviewing approach.

Its advantages have been described extensively before, when the ETAI was started and in its present web pages. The following are some possible objections to the openness:

- "It may hurt the author if the critique by referees is made public". However, this is only so if authors are not allowed the right of effective rebuttal, and if referees are generally assumed to be infallible, so that a negative referee report is taken to mean that the work was below standards. In an open discussion situation, the informed audience can weigh the evidence presented by the referees and the authors.

  On the other hand, think about all those cases where articles have been rejected because the referees disagreed with the author's approach. Open reviewing and refereeing is much better at protecting the authors in such situations.

  In an environment that is characterized by open discussion, an author will obtain much more credit if he defends himself well against attack, than if he is never tried.

- "But there has always been a good way for new approaches or research paradigms to react if they are blocked out for reasons of methodology: they can start another journal". Yes, but (1) a lot of work may have been lost because it never got that far, (2) spinning off new periodicals does not contribute to an effective exchange of views; it just means that everyone does his/her own thing.

- "It may hurt the field to show these disagreements". I think it hurts the field much more to not deal with them effectively.

These are important questions, both for the ETAI and for the field, and the comments by the readership are cordially invited.

Vol. 3: *21-21*, 31.3.1999
http://www.etaij.org/ra/rac/017/

Protocol of on-line discussion during January - March, 1999
about the following reference article:

Hector Levesque, Fiora Pirri, and Ray Reiter

Foundations for the Situation Calculus

Q1. **John McCarthy** (30.3)

I have a grumble about the title of the article by Reiter, et. al. about situation calculus. It presents a particular system of the situation calculus, and therefore can't quite serve as a reference article.