ELECTRONIC NEWSLETTER ON
REASONING ABOUT ACTIONS AND CHANGE

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# Today

## Thielscher Article Accepted

Several contributions have accumulated since the previous issue.

1. The article by Michael Thielscher, "A Theory of Dynamic Diagnosis", has been accepted to the ETAI following two positive referee reports. One of the referees has made a few suggestions for changes to the article; these suggestions follow below and have been posted on the article's interaction page.
2. Antonis Kakas and Rob Miller have answered to the questions and suggestions by the referees to their article, "Reasoning about Actions, Narratives and Ramification" which was recently accepted to the ETAI. Their answers follow below and have been posted as usual.
3. In the light of the refereeing of those two articles, the present Newsletter editor comments on (1) the open relationship between authors, referees, and readership in the ETAI, (2) how should the advent of electronic publication and communication change our expectations as to how scientific articles ought to be written.
4. The debate about ontologies of time continues with contributions by Sergio Brandano, Pat Hayes, and Jixin Ma.

# Meta-Debates

## Article styles and refereeing

### From: Erik Sandewall

Two more articles have now been accepted to the ETAI after an extended period of open review discussion followed by confidential refereeing. At this point, I wish to raise some possibly controversial questions that relate to the referee reports, namely: *Is it necessarily the case that articles that are published in this on-line and networked medium ought to be written in exactly the traditional style? Maybe the classical advise for How To Write A Scientific Article ought to be amended somewhat?*

Note however that I am not going to propose using hypertext in extremis, or any other revolutionary new ideas. On the contrary, I believe it's important that articles continue to have their traditional look and feel from a typograhpic point of view, and that both authors and readers feel comfortable with our way of doing things. Reasonable changes to a moderate extent is the key. However, the present topic is revolutionary in another sense: *you don't usually question the opinion of a referee.* Only the transparency policy of ETAI makes it possible. Before I proceed on that topic, first a few words of introduction and background.

The rate of articles in ETAI is not yet very high, but this is not a problem. Since we are using a novel publication paradigm, it is in fact very useful that we get enough time for analysis and discussion with the peer communities before a larger flow of articles begins to come. We also don't have any quota where a certain number of hundred pages have to be filled each year: our publication scheme is completely elastic with respect to volume.

One of the characteristic and unique features of the ETAI is the egalitarian relationship between all actors, including between reviewers and authors. The reviewer/referee in a traditional journal habitually assumes the pose of ultimate authority, and review reports tend to use phrases such as "the article is lacking in the following respects", "the author should better explain how his (her) approach is able to deal with the following problem", and so on. Behind these phrases there is one of our colleagues, and if we met face to face she (he) would not of course use this way of talking.

In the ETAI, open discussion is the basic idea. Ideally, all feedback to the authors should occur in the open debate, and referees should only vote "accept" or "don't accept". In practice, it turns out that several of the referees get to think of possible improvements to the article as well, but even in this case their suggestions are written in the same collegial style as is used in the open discussion. Please take a look at the comments by "Anonymous Referees" for the articles by Thielscher and by Kakas and Miller for some examples of how reviewers and referees so-to-say look the authors in the eyes as they ask questions and make suggestions with respect to the article that's being considered.

It has been interesting and encouraging to see how the authors have reacted when the review comments for their articles were posted on the web, first the open discussion and then the comments by referees. Until now I have only heard positive reactions and no negative comment at all, and I do believe that the civil tone that we have adopted has contributed to that result. (Comments by the authors on this topic would be welcome!)

The intrinsic transparency of the review process gives the authors a chance to answer to the referees. It also gives the rest of us a chance to listen in to the discussion between authors and referees, and this may be very useful for understanding the contribution as well as for evaluating it. An additional advantage is that this provides an entry point to a common discussion about the topic that I mentioned in the introduction: *what style ought to be used in the articles* in our field. Referees are, after all, the guardians of our quality criteria, so they must be involved in any changes of practice. Thus, the following questions are posed both to the present referees, and to the whole readership.

**When a referee feels that additional clarification is needed, should this automatically translate into a suggestion to amend the article?** The alternative would be that the referee just asks the question, the author gets to answer it, and the question and the answer are posted in ETAI's article interaction page where they are just as easily available as the article itself. Consider, for example, the suggestions of the third referee of the Kakas and Miller article: would you rather have the answers to those questions integrated with the article, or represented separately?

I don't imagine that *all* reviewers' comments for all articles can best be dealt with separately, but maybe for *many* of them this would be appropriate. After all, dialogue tends to be more lively than monologue, and seeing what questions others have asked may be more interesting than just reading passages of an article that provide answers to untold questions.

**Do networked articles need to be as self-contained as articles in the classical medium?** If an article in a conventional journal forces the reader to refer to another article for essential material (such as background, motivation, or definitions) then it may be very inconvenient for the reader to find the cited material. In the on-line context this is not so: a hot link in an ENRAC-style footnote (as used in the version of ENRAC that uses frames) allows the reader to click her way instantly to the cited material. In this light, what stand do we wish to take on the third referee's suggestions for additional background material in the Kakas and Miller paper?

Naturally, my references to the referee reports is for examplification only: the refereeing for these articles has been concluded, and it's not my intention to submit the recommendations of the referees to a referendum each time an article is up for acceptance. It is also clear that the recommendations of the present referees are perfectly consistent with traditional criteria for high-quality journals in our field. It's exactly for this reason that these referee reports may also be useful as a starting point for an important discussion: can we improve the quality of form without sacrificing (and hopefully also improving) the quality of content in research articles that are published in the electronic medium, such as the ETAI?

Comments from the readership are welcome on both of these questions, as well as on the general issue of how we can make the best use of electronic communication and publication in our area of research.

# ETAI Publications

## Discussion about received articles

Additional contributions have been received for the discussions about the following article(s). For the full context, please refer to the on-line structure which contains one article interaction page with the entire discussion for each article.

## A Theory of Dynamic Diagnosis

by Michael Thielscher

### Comment from: **Anonymous Reviewer 2**

I would like if the theory were adjusted to cover this small "defect": According to the definition of "action law" (page 11) the set $C$ and $E$ of fluent literals must be consistent for any sequence of entities in the scope of the action. This simplifies the presentation of the theory. However, it does not cover simple action laws like when $Move(x, f, t)$ transforms $\{Loc(x, f)\}$ into $\{Loc(x, t), notLoc(x, f)\}$, in which the set $E$ of effects is not consistent when you consider $t = f$.

## Comment from: **Anonymous Reviewer 2**

I have only one complaint which has been already raised by Marie-Odile Cordier during the discussion phase (point 2): the theory assumes that the system may change its status only because of some action. However, faults cannot happen during the execution of actions. Because of this, it is reasonable to "minimize abnormalities" in the initial state. However, are the assumptions reasonable? In your reply to Marie-Odile, you say that you see no fundamental obstacle in extending the theory to "natural events aside from exogenous, volitional actions". Some more words (a section with an example?) about how to do it will be of great help.

# Reasoning about Actions, Narratives and Ramification

by Antonis Kakas and Rob Miller

## Comment from: **Anonymous Reviewer 3**

*Your paper makes the following contributions:*

1. *Extending the declarative temporal language $\mathcal{E}$ to deal with ramifications*
2. *Furnishing a translation between $\mathcal{E}$ and logic programs*

*Both these contributions are welcome. The ramification problem is an important problem in temporal reasoning which is still not well understood. Studying the problem in the context of a unified temporal language has the potential to shed light on the connection between the ramification problem and other problems in temporal reasoning, though see below for further comments. The translation between $\mathcal{E}$ and logic programs is very welcome as well, as it grounds theoretical and formal results on theories of action to implementable programs.*

*Although the paper is in general well written and well organized, and I consider it acceptable for publication as it is, I also suggest that it could be improved in the following ways. [Suggestions in the present discussion item and the four following ones].*

*First, and most saliently, the paper does not explain* why *your approach solves the ramification problem. (Indeed, you don't explain why the approach solves the frame problem either, though that presumably was the job of the 1997 JLP paper.) It would be helpful to give some intuition of why this central problem in temporal reasoning arises, what other approaches have been suggested, how these approaches succeed and fail, what this approach provides, intuitively, in the way of a solution to the ramification problem, and how this approach compares to other approaches.*

*You do the last (comparing your approach to other approaches) briefly, in the beginning of section 5, but this treatment is too cursory and raises almost more questions than it answers. For example, in comparing your approach to those of Thielscher, McCain and Turner, and Lin, you aruge that their approach is essentially a causal-based approach, because the effect of action occurrences cannot be propagated backward through r(amification)-propositions. To this reviewer, this*

> *fact hardly seems to be the characteristic fact of causal theories. A deeper analysis of what makes a causal theory, whether sets of axioms in E can be considered causal theories, and how causal approaches can be used to solve the ramification problem, would be helpful here.*
>
> *Also very desirable would be a discussion of how solutions to the ramification problem interact with solutions to the frame problem. In particular, there is often a duality between the two problems, in that the frame problem is often seen as a mainly representational problem, whose solutions may worsen things from the computational point of view, and the ramification problem is often seen as mainly a computational problem, whose solutions may worsen things from the representational point of view. How do your two solutions interact? A discussion would be useful.*

## Comment from: **authors**

To:**Anonymous Reviewer 3**
We're not sure if we would go as far as to state that we have "solved the ramification problem." Like the frame problem, not everyone agrees exactly what this problem is. The analysis in our paper is that

> *the ramification problem arises in domains whose description most naturally includes permanent constraints or relationships between fluents. In formalisms which allow for such statements, the effects of actions may sometimes be propagated via groups of these constraints. The problem is to adequately describe these propagations of effects, whilst retaining a solution to the frame problem - that is, the problem of succinctly expressing that most actions leave most fluents unchanged.*

Viewed like this, the ramification problem is intimately related to (or is an aspect of) the frame problem. Our solution to the frame problem is by introducing the notion of initiation points and termination points, and ensuring that these are the only mechanisms for change along the time-structure. Our approach to ramifications is to (slightly) widen the set of initiation and termination points in a given model via a fixed point definition. This extended definition takes into account any r-propositions (i.e. ramification statements) in the domain.

The current state of A.I. doesn't unfortunately permit a definitive statement of what makes a causal theory – it seems to mean different things to different sub-communities (as witnessed in the recent AAAI Spring Symposium on Causality in Reasoning About Actions). Thielscher and others merely make a technical distinction between "causal-based" and "categorisation-based" contributions to the ramification problem. Our contribution is "causal-based" in this limited technical sense in that it doesn't categorise fluents, but does have a unidirectional ("whenever") "connective". But we accept that perhaps it's not so healthy to hijack the word "causal" for a rather specialised technical use in this way.

We reject the view that the frame problem is a mainly representational problem and the ramification problem is mainly a computational problem. We see both problems as having representational and computational aspects.

We accept that more in depth analyses are needed of the relationships between formalisms for resoning about actions in general, and approaches to ramifications in particular. Ultimately, the best way to do this is by providing translation methods and showing that these are "sound" and/or "complete" for well defined classes of domains. We haven't had time to do this yet, but it's on our agenda of future work on the Language $\mathcal{E}$.

## Comment from: **Anonymous Reviewer 3**

*The examples in the paper would be more helpful if they were expanded more. Examples:*

- *On p. 6, you state:*

  > In a domain description with no h-propositions or t-propositions at all, it would be possible to construct a model where ... WindowClosed and VentClosed were true at all time-points, but Stuffy was false.

  *It would be useful to see that model explicitly.*

- *On p. 8, you state:*

  > In particular, if we replace (sr9) with "CloseVent happens at 3" our semantics does not give rise to the type of anomalous model is problematic for some other approaches .... in which a change at 3 from not Stuff to Stuff is avoided by incorporating an unjustified change from WindowClosed to not WindowClosed.

  *Here is where it would be really good to discuss other approaches, how they run into problems, and how your approach avoids these problems.*

- *In the same vein, it's not clear why Thielscher's approach has trouble with the last variation of the switch example that you discuss in section 3. A more detailed discussion would help.*

## Comment from: **authors**

TO:**Anonymous Reviewer 3**
The domain description on page 6 was:

```
CloseWindow initiates WindowClosed
CloseVent initiates VentClosed
OpenWindow terminates WindowClosed
OpenVent terminates VentClosed
CloseWindow initiates Stuffy when {VentClosed}
CloseVent initiates Stuffy when {WindowClosed}
OpenWindow terminates Stuffy
OpenVent terminates Stuffy
```

Let H be the interpretation for this domain defined as follows:

```
H(WindowClosed,t) = true,   for all t
H(VentClosed,t) = true,     for all t
H(Stuffy,t) = false,        for all t
```

Since there are no h-propositions in this domain, by Definition 8 there are no initiation points or termination points w.r.t. H. Hence H conditions 1-4 of Definition 9, and so is a model of the domain.

Re the "stuffy room" example on page 8, this is of course the classic illustration of why naive minimisation of change doesn't work when domain constraints are included in a domain. For example, if a situation calculus theory includes the constraint

$$Holds(Stuffy, s) \leftarrow Holds(VentClosed, s) \wedge Holds(WindowClosed, s)$$

approaches to the frame problem such as Baker's will give rise the kind of anomalous model that the paper refers to. This is related to the fact that the above can be written in several equivalent ways, e.g.

$$\neg Holds(VentClosed, s) \leftarrow \neg Holds(Stuffy, s) \wedge Holds(WindowClosed, s)$$

i.e. the classical "$\leftarrow$" connective allows for contrapositive re-writings. Hence several approaches to the ramification problem, including ours, advocate the use of a "unidirectional" connective or predicate which does not facilitate the construction of such contrapositive statements.

The reviewer also wrote:

> *In the same vein, it's not clear why Thielscher's approach has trouble with the last variation of the switch example that you discuss in section 3. A more detailed discussion would help."*

The point that we wanted to make is that it's not clear to us how Thielscher's approach (and related approaches) can be extended to include explicit time. But see our answer to Francois Lévy's recent question (question 5) on our ETAI interactions page.

## Comment from: **Anonymous Reviewer 3**

> *The unique contributions of this paper over the JLP paper are not so explicitly stated, namely, the introduction for the "whenever" construct into $\mathcal{E}$, and the resulting modifications in the definitions of the language, the translation into logic programs, etc. It would be helpful to be more explicit about them.*

## Comment from: **authors**

To:**Anonymous Reviewer 3**
You summarised the contributions very well at the top of your report:

1. Extending the declarative temporal language $\mathcal{E}$ to deal with ramifications
2. Furnishing a translation between the extended $\mathcal{E}$ and logic programs

## Comment from: **Anonymous Reviewer 3**

> *The writing is in general clear, understandable, and straightforward, but there are several places which were unclear, or in which an additional English gloss would be helpful. Specifically:*

- *p. 4: It it not clear what the partial order is supposed to range over. In the 10th line from the bottom on this page, is the relation on points (1rst, 2nd, and 4th items in that line) or on sequences (3rd item in the line)?*
- *p. 7, clause 2 of Def. 14, and p. 12, clause 2 of Proposition 2: In both cases, an English gloss would be helpful. (That is, an intuitive explanation of when a ramification statement is true. This is, after all, the heart of the paper, and extra effort and space to make this well understood would be well worth it.)*

## Comment from: **authors**

To: **Anonymous Reviewer 3**

> *p. 4: It it not clear what the partial order is supposed to range over. In the 10th line from the bottom on this page, is the relation on points (1rst, 2nd, and 4th items in that line) or on sequences (3rd item in the line)?*

The partial order ranges over all items in the set of time-points $\Pi_\Delta$. This includes both finite sequences of action constants (items 1 and 3 in the expression to which you refer), and "Starts" of such sequences (items 2 and 4). They're all just (syntactic) objects in the set of time-points.

> *"p. 7, clause 2 of Def. 14, and p. 12, clause 2 of Proposition 2: In both cases, an English gloss would be helpful. (That is, an intuitive explanation of when a ramification statement is true. This is, after all, the heart of the paper, and extra effort and space to make this well understood would be well worth it.)"*

As we stated in our discussion with Tom Costello (interactions C2-6), the r-proposition "L whenever C" can be read as "C is a minimally sufficient cause for L". So, to quote from the paper, "at every time-point that C holds, L holds, and hence every action occurrence that brings about C also brings about L". So, "in order to find time-points at which the fluent literal L is established via the r-proposition 'L whenever C', we need to look for time-points at which one or more of the conditions in C become established, and at which the remaining conditions are already and continue to be satisfied (up to some time-point beyond the point in question)." Clauses 2 of both Definition 14 and of Proposition 2 are mathematical articulations of this last statement.

## Comment from: **Anonymous Reviewer 3**

> *The online ETAI discussions highlighted a number of interesting points, including the issue of using a special purpose language $\mathcal{E}$ instead of standard first-order logic, whether truth conditions can really be given for all the predicates in $\mathcal{E}$, as well as more basic philosophical (ontological) questions on how you divide changes into causations and ramifications. It would be nice to see the paper deal with these to some extent. You can't, of course, give a whole dissertation defending the use of action-type languages, but integrating short versions of your statements on these positions into the paper would be useful.*

### Comment from: **authors**

To:**Anonymous Reviewer 3**
The revised version of our paper (now available via the ETAI web pages) includes some extra remarks relating to various points raised in the ETAI interactions. We also very much hope that the paper will be read in conjunction with the online discussion.

# Debates

## Ontologies for time

### From: Pat Hayes

A comment on Sergio's reply to Jixin:

> *An interval from the real-line is an ordered set of real numbers limited by its end-points, which are not necessarily included in the set.*

It seems from this that the set of intervals is supposed to include open, half-open and closed intervals; is that right? (Or do you mean to say that there may be some doubt about whether a particular interval does or does not include its endpoints? If the latter, this is not the usual notion of 'interval' as used in real analysis, and you need to explain further.)

> *Suppose $S$ consists of intervals from the real line. Assume $\langle s1, t1\rangle \in A$ and $\langle s2, t2\rangle \in B$, intervals in $S$. We say that $\langle s1, t1\rangle \leq \langle s2, t2\rangle$ iff $t1 \leq s2$. The strict order relation $<$ is an abbreviation for $\leq$ logical-and $\neq$.*

It follows then that for intervals, $\leq$ implies $<$ except for pointlike intervals (single-point closed intervals) since if $t1 \leq s2$, the intervals $\langle s1, t1\rangle$ and $\langle s2, t2\rangle$ cannot be equal unless $s1 = t1 = s2 = t2$.

> *Suppose now that $\langle s1, t1\rangle < \langle s2, t2\rangle$. The axiom of completeness states the existence of $\xi \in S$ such that $\langle s1, t1\rangle \leq \xi \leq \langle s2, t2\rangle$.*

Consider the closed intervals $[p, q]$ and $[q, r]$ with $p < q < r$. These satisfy $\leq$ and hence satisfy $<$, but there is no interval between them. Hence, your axiom is false for intervals on the real line.
Pat Hayes

### From: Jixin Ma

To Sergio, who wrote:

> *The (temporal) domain $S$, as I meant, may consist either of time-points xor of time-intervals (exclusive "or").*
>
> *An interval from the real-line is an ordered set of real numbers limited by its end-points, which are not necessarily included in the set.*
>
> *Suppose $S$ consists of intervals from the real line. Assume $\langle s1, t1\rangle \in A$ and $\langle s2, t2\rangle \in B$, intervals in $S$. We say that $\langle s1, t1\rangle \leq \langle s2, t2\rangle$ iff $t1 \leq s2$. The strict order relation $<$ is an abbreviation for $\leq$ logical-and $\neq$.*

> *Suppose now that $\langle s1, t1\rangle < \langle s2, t2\rangle$. The axiom of completeness states the existence of $\xi \in S$ such that $\langle s1, t1\rangle \leq \xi \leq \langle s2, t2\rangle$. I reply in advance to your next question: "Why did you write $\leq$ instead of $<$ ?". The reply is that $\leq$ means "less or equal", that is $\xi$ may not be equal to $t1$ or $s2$, but it can do so. Note that since $\xi$ belongs to $S$, then $\xi$ is an interval. This is also meant as a reply to your question about the dividing instant problem.*

First of all, what you wrote in the above didn't solve the Dividing Instant Problem at all!

Anyway, you have claimed that "The (temporal) domain S may consist either of time-points or (**exclusive-or**) of time-intervals", and "an interval from the real-line is an ordered set of real numbers limited by its end-points, which are not necessarily included in the set." In this case, can your intervals be "pointlike"? That is, for an interval $\langle s, t\rangle$, is $s$ allowed to be equal to $t$? In other words, can a set representing an interval be a singleton? As I suggested in my former response, the anwser has to be yes (see below). That is, if your domain $S$ contains non-pointlike intervals, then, to satisfy the so-called completeness property, the domain $S$ must contains singletons (or namely points!) **as well**. Therefore, all my former questions for you still apply.

I have shown in my former message that if your domain $S$ contains intervals, it must contain points as well. However, you claimed that $S$ does not consist both of time-points and intervals since you specially claimed that your "or" is **exclusive-or**). I would like to use your notation to show this again.

In fact, you define the (partial) relation "$\leq$" as follows: $\langle s1, t1\rangle \leq \langle s2, t2\rangle$ iff $t1 \leq s2$. Consider the case that interval $\langle s1, t1\rangle$ in $A$ and interval $\langle s2, t2\rangle$ in $B$, satisfying $\langle s1, t1\rangle \leq \langle s2, t2\rangle$, **and** $t1 = s2$ (this is a valid case according to your definition). To fulfill the completeness property, there exists a $\xi$ in domain $S$ such that $\langle s1, t1\rangle \leq \xi \leq \langle s2, t2\rangle$. Let $\xi = \langle s, t\rangle$. Again, by the definition of "$\leq$" between intervals, we have $t1 \leq s$ and $t \leq s2$. However, remember $t1 = s2$, we infer that it is impossible for $s < t$. Therefore, we reach that $s = t$. That is $\xi$ must be a point (pointlike)!

By the way, it seems that your description of the axiom of completeness is not a first-order one.

Jixin

## From: Sergio Brandano

In ENRAC 24.4.1998 I made a typing mistake. I wrote: "$\xi$ may not be equal to $t1$ or $s2$, but it can do so", while it should obviously be: "assuming $\xi = \langle \xi_1, \xi_2\rangle$, then $\xi_1$ may not be equal to $t1$ and $\xi_2$ may not be equal to $s2$, but they can do so".

### In reply to Jixin Ma (ENRAC 23.4 and 24.4 1998) – completion:

> *So, you think intervals are* not needed*? Anyway, our arguments...*

I am actually skeptic about the need of a temporal domain which includes time-intervals. There are many convincing arguments that a temporal domain consisting of time-points is good enough in many different situations

(Newtonian mechanics and Thermodynamics, for instance, as well as Sandewall's underlying semantics for $\mathcal{K}$-**IA**), and I see no reason why I should pursue a different path.

> *... about the convenience of using intervals are based on the belief of the need of them.*

This is why I originally asked for some convincing argument(s) for the plausibility of this approach. According to the standard scientific methodology, in fact, we shall build on top of already existent solutions, and be consistent. Just to make an example, suppose one refuses a classical notion (continuity?), and encounters the problems that this notion was used to solve (the dividing instant?); it is surely not consistent to justify the need for a novel approach via the claim that the problems he encountered can not be solved by the notion he just refused. The notion of semi-continuity, for instance, has dignity, and its plausibility is far to be based on the belief that continuity is not needed... The case of time-intervals is clearly safer; one may simply give a preliminary example and show some objective advantages when using time-intervals instead of time-points.

> > Premise: It is evident that if you assume the axiom of completeness, the domain $S$ can just be continuous, while if you do not assume the axiom of completeness then $S$ is necessarily discrete.

> *Wrong! Even if you do not asssume the axiom of completeness, it is still not nessarily discrete.*

Yes, I agree. I realize I wrote that sentence having in mind the basic time structure on my paper. The question holds properly if you do not assume any axiom of density other than the one I stated.

Concerning the dividing instant problem, which seems to summarize what is left from your objections, please read below.

### In reply to Pat Hayes (ENRAC 24.4.1998):

As posted in my original message, I have not yet seen any explanation why an alternative notion of continuous structure is needed at all?

Probably, in order to prevent any misunderstanding, I should have included an additional sentence like "... is needed at all, within the search of those non-monotonic logics which purpose is to formalize common sense reasoning when reasoning about actions and change", but I thought it was evident, as the title of this Newsletter reminds.

In particular, in the same message, I asked to give at least one convincing argument on the need of a notion which is an alternative to the classical one, along the lines: "the problem P of temporal reasoning about actions and change can not be solved adopting the axiom of completeness", or "the axiom of completeness is too strong an assumption for our purposes; axiom A is better suited, because..." ()

You and Jixin Ma proposed the "dividing instant problem", apropos of the problem of switching on the light, and argued the axiom of completeness inadequate for solving that problem. The formulation I gave in ENRAC 24.4.1998, with today's minor adjustment, gives the evidence on how the axiom of completeness is, instead, safe with respect to the dividing instant problem. You and Jixin based your argument on the fact that I do not allow the domain $S$ to hold points "and" intervals, so that if $S$ admits just intervals then the dividing point $p$ can not exist. I refuted that argument by simply observing an interval from the real line may have equal end-points.

You also gave other examples, but you did not explain how they relate to the world of "Reasoning about Actions and Change". In particular, and I somehow repeat myself, it is not evident that one needs a temporal domain with non-homogeneous continuity (let me say it is even less evident the need of the imaginary number $i$ in our temporal structure). Does there exist at least one representative problem of reasoning about actions and change that can not be solved adopting the axiom of completeness, so that to justify a temporal domain with non-homogeneous continuity? (and I repeated ())

You also gave an informal argument on the plausibility of a temporal structure which formalizes the perceived smooth flux and perceived fast flux of time (ENRAC 21.4.1998). I refuted that plausibility with my contribution to ENRAC 23.4.1998.

(Is it really "free of context" to you ?)

Best Regards

Sergio

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*This newsletter is issued whenever there is new news, and is sent by automatic E-mail and without charge to a list of subscribers, in plaintext format. The present postscript issue is a bona fide rendering of the same information in alternative typography. Instruction for contributors and other additional information is found at the following URL:*

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