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REASONING ABOUT ACTIONS AND CHANGE

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Today

Today's issue contains Michael Thielscher's answers to the referee's comments for his accepted article, and his comments on the ETAI reviewing and publication procedure. Also, Jixin Ma on the ontology of time.

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: **Michael Thielscher**

To: **Anonymous Referee**

The Anonymous Referee wrote:

> *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.*

I hope that the following remark constitutes a satisfactory reply to your question. This is the basic idea in the paper [5]

`http://www.intellektik.informatik.tu-darmstadt.de/~mit/publications/conferences/ordered-abstracts.html#KR98`

which I mentioned in my answer to Cordier's question to which you refer: The happening of a (non-exogenous) event is considered a property of a world state and thus is formally treated as a fluent. Hence it can be (directly or indirectly) caused by actions or other events. In the aforementioned paper we have shown that minimization of event occurrences had better be conducted in the initial state only. Now, here is a (sketchy) idea for combining this approach with the present theory of dynamic diagnosis: Suppose we introduce binary fluents $Happens(e, t)$ indicating that event e is expected to happen at time t. If *Happens* is minimized initially (just like *Ab* is in the paper under discussion), then the occurrence of natural events, too, may serve as explanation for abnormalities. These explanations would 'compete' with assumptions about initial abnormalities; additional, qualitative knowledge as to the *a priori* likelihood of both events and abnormalities may then be used to tell apart the most plausible diagnoses (along the lines of Definition 10 of the present paper).

The Anonymous Referee also wrote:

> *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 $\{Loc(x, t), not Loc(x, f)\}$, in which the set E of effects is not consistent when you consider $t = f$.*

The following straightforward generalization of Definition 2 could handle your example without affecting any other part of the theory. Rather than stipulating that action laws themselves satisfy the consistency criterion, the latter shall be a requirement of *applicable instances* of these laws. In this way the statement

Move(x,f,t) transforms Loc(x,f),-Loc(x,t) into Loc(x,t),-Loc(x,f)

would satisfy the definition of an action law, and actions of the form $Move(x, f, f)$ wouldn't admit a successor state, hence wouldn't be applicable. In the paper I kept the definition of action laws as simple as I possibly could in order not to complicate matters at a point which is not an issue of the paper (namely, the specification of direct effects of actions). Since the original definition does not constitute a technical error, I hope this comment is sufficient an answer.

Meta-Debates

Article styles and refereeing

From: Michael Thielscher

First of all I can only side with Rob in saying that the experience of publishing an article in the novel way was both exciting and instructive. It took some time for the discussion on my paper to get going, but in the end it proved very useful and led to important improvements. It was, however, never as lively as, say, the discussion on Tony Kakas and Rob Miller's submission. I recently told Rob that I envied the two of them for their paper receiving so much attention. Although his impulsive remark was that there are two sides to everything, I guess that in the end the authors of articles much debated upon can be most happy about the public attention. Thus a lesson that might be learned from the experience with ETAI so far is that the more controversial a paper is, the more is gained by submitting it to the new publication scheme. And of course this holds vice versa: ETAI seems to profit most from controversial papers.

The editor of the Newsletter raised the question whether networked articles should be as self-contained as articles in the classical medium. Indeed the new medium offers new possibilities. If there is a good general introduction to the topic of one's paper, then adding a link might often be a better idea than just copying the contents in one's own words. In this way an article could be made accessible for a readership with truly different background. For classical journal papers, authors always have to struggle with the problem of how much background they should provide. Some papers even include choicepoints of the form "The reader who is familiar with topics x, y, z,... may skip sections a, b, c, ..." The new medium offers the exciting possibility of instead writing "The reader who is *not* familiar with x, y, z, ... should first follow the links l, m, n, ..." This is of course much less time-consuming and can thus be used with virtually no effort to make a paper suitable for almost everyone. Although I doubt that today too many useful electronic links exist which may serve this purpose, maybe sometime in the future there will be.

One suitable supplement to any ETAI paper is readily available already today, namely, the electronic public discussion. I support Rob's suggestion that everyone who downloads an accepted ETAI paper should be strongly encouraged to also print out and append the discussion page. My feeling is that this truly new feature is among the greatest advantages of the novel publication style.

Debates

Ontologies for time

From: Jixin Ma

In ENRAC 3.5 (980521), Sergio wrote:

> *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.*

You said here "a temporal domain consisting of time-points is good enough in many different situations". Is this ("many") a convincing argument for general treatments? Anyway, the fact that "you see no reason why **you** should pursue a different path" does not mean others don't see/have the reason (see below).

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

> *... 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.*

Have you applied the above arguments to that one proposed by youself? Sorry, I am here again using your question to ask you.

Anyway, while I (and many others) have seen the convenience of using intervals, I can also see the need of them. In fact, there have been quite a lot of examples (**many**) in the literature that demonstrated the need of time-intervals (or time-periods). Haven't you ever encountered any one of them? Or you simply cannot see anyone of them is convincing?

All right, let's just have a look at the example of throwing a ball up into the air. As I showed in ENRAC 1.4 (98033) (one may disagree with this), the motion of the ball can be modelled by a quantity space of three elements: going-up, stationary, and going-down. Firstly, or at least, we can see here the convenience of using intervals. In fact, we can conveniently associate the property that "the ball changes its position" with some time-intervals. Secondly, let's see if we indeed need time-intervals. Without the notion of time-intervals (neither primitive nor derived from time-points), can you just associate such a property with time-points? Yes, we may associate it with a pair of points. However, this doesn't mean that the property holds at these points. What it really means is that the property holds for the time periods denoted by the pair of points. Aren't these time periods in fact time intervals?

It is important to note, up to now in the above, I just talked about the need of the notion of intervals. As for how to characterise intervals (e.g., are intervals taken as primitive or derived structures from time-points?) is another important issue, and this issue, again, has been addressed in the literature for a long time.

The Point Is: while we were/are discussing/arguing about some broader issues on temporal ontology, you just jumped in and asked "why an alternative notion of continuous structure is needed at all?" First of all, the "continuity" (or more truly, density) is not the main issue we are talking about. The fundamental question is if we need to address and how to address time intervals. Based on such a discussion, in the case that intervals are taken as temporal primitive, then, we are talking about how to characterise some corresponding issues including dense/discrete structures. But your questions and arguments/replies do not seem to follow this. As stated in the former replies from both Pat and myself, first of all, the dense structure does not have to be characterised in terms of the only form of the so-called "axiom of completeness". Also, in the case where time-intervals are involved (even they are still point-based, let alone in the case they are taken as primitive), such an axiom doesn't simply apply. In fact, I have shown this twice with different notations in this discussion. I will point out more problems in detail below in my response to your reply to Pat.

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

As I already stated, your approach does not solve the DIP at all. In fact, it seems that you don't realise the DIP in the way as we are talking about (see 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?*

Still not yet?

> *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 claimed already that your domain S contains points or (exclusive-or) intervals? To fulfill the axiom of completeness, you *must* allow your intervals to be possibly some singletons (i.e., a set of single *point*). In other words, if your S contains intervals, it should also contain singletons (points). The real problem is that even if you allow your intervals to be singletons, the Dividing Instant Problem is still there, and in fact more obviously. Do you agree with this?

> *The closed intervals $[p, q]$ and $[q, r]$, with $p < q < r$, do not fulfill the relation $\langle p, q\rangle < \langle q, r\rangle$, hence they do not make a valid counterexample.*

Pat's example becomes invalid **only after** you made the "minor adjustment" that replaces the relation $\leq$ in your hypothesis $\langle s1, t1\rangle \leq \langle s2, t2\rangle$ by $<$, that is $\langle s1, t1\rangle < \langle s2, t2\rangle$. (Is this an alternative?)

It follows that you do need alternation, doesn't it? (Note that this is just for the case when you construct intervals out of points. In the case where intervals are taken as primitive, the need of such alternative is indeed more conceptually necessary). However, your adjustment is not enough, or you haven't reached the proper form for general treatments. In fact, you need to address the issue regarding different cases. To see this, you may just consider the difference between the case where at least one of $\langle s1, t1\rangle$ and $\langle s2, t2\rangle$ is "closed" at $t1$ $(= s2)$, and the case where both $\langle s1, t1\rangle$ and $\langle s2, t2\rangle$ are "open" at $t1$ $(= s2)$. In the former case, you need use $<$ in the hypothesis; otherwise, Pat's example will be a valid counterexample. In the latter case, you need use $\leq$ in the hypothesis; otherwise, your axiom cannot prevent a "gap" between $\langle s1, t1\rangle$ and $(s2, t2\rangle$, that is, there is no guarantee that the singleton $[t1, t1]$ is contained in S (Do you think this is consistent with the "classical" concept of contiunity?).

Jixin

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`http://www.ida.liu.se/ext/etai/actions/njl/`