ELECTRONIC NEWSLETTER ON REASONING ABOUT ACTIONS AND CHANGE

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Today

Eugenia Ternovskaia has contributed comments and questions on the ETAI submitted article by Marc Denecker et al. Also in this Newsletter, Erik Sandewall answers Pat Hayes and Jixin Ma in the resumed discussion about the ontology of time.

ETAI Publications

Discussion about received articles

An Inductive Definition Approach to Ramifications

by Marc Denecker, Daniele Theseider Dupré, and Kristof Van Belleghem

Comment from: Eugenia Ternovskaia

Dear Marc, Daniele and Kristof,

It was interesting to read your paper, especially because I am interested in inductive definability myself. I wrote a paper "Inductive Definability and the Situation Calculus" presented at Dynamics'97 last October. It was published in a volume of LNCS later. You can get it from `http://www.cs.utoronto.ca/~eugenia/papers/indef.ps`

Among other things, I describe an inductive solution to the frame and ramification problems. I treat causal rules specifying direct and indirect effects of actions as rules of an inductive definition. Causal (inductive) rules specifying direct effects of actions may contain both cycles and negations. Causal rules describing indirect effects may not contain cycles. This condition can be relaxed, but in this case the correct form of successor state axioms will not be obtained. I would be grateful to you if you could read this paper and provide some comments.

Here are some comments on your paper. First of all, it's a good paper.

The main contribution, I think, is that you further elaborated the generalized inductive definition principle and applied it to the ramification problem. I consider Marc's result about the equivalence between this principle and the well-founded semantics significant, and it was good to see how it works in the context of the ramification problem. It was nice to see examples for the syntactic subclasses of the generalized inductive definition.

I think that introducing the third truth value helps you to explain the subtle details of the iterative process which was quite difficult for me since I was working in classical first order logic with fixpoints.

From my experience, it is a long way from understanding how causal (inductive) rules work and capturing it in a classical logic-based formalism such as the situation calculus. In your paper you say that your approach can be embedded in the situation calculus. Perhaps the main question to you is how the third truth value would be represented in the two-valued setting?

The following will help you to make a few little improvements, I hope.

p. 28, after Th.1.

"Unless nondeterminism is explicitly introduced, our theory leaves no room for ambiguity."

I do not think this is an advantage of your theory. I think the truth value "u" should be obtained in two cases.

First, it may be a result of a "bad axiomatization". Second, it may appear due to lack of complete information about the current state of the world. (It seems that you apply the Closed World Assumption in your definition of a state as a set of (positive) fluent literals, if I understood correctly.)

I believe your definitions might be easily changed to represent the second case as well. For example, a state could be associated with a set of "known" positive and negative literals.

It also seems strange that you cannot derive anything about "holds", although I understand your motivation. Would it be difficult to introduce this feature?

p. 0 (abstract)

bad definitions should be "bad definitions", I guess.

p. 4 Please do not start sentences with a formula, e.g. $caus(l)$, $init(l)$!!! This is hard to read.

This is especially confusing on p. 16 : I am reading: "action start1.cause(turn1) has two ...."

p. 10 "In sections 4, 3....." Should be "In sections 3, 4....."

p. 10 "..., where *top* denotes the inconsistent state."

This is a confusing notation. Usually *bot* is used to denote false or contradiction (as well as the bottom of a lattice) and *top* is used to denote true (or the top of a lattice), as you perhaps know. I noticed that you already use *bot* later in the text, but maybe another solution is possible. For example, in automata theory *emptyset* is used to denote a "junk" state without outgoing transitions.

p. 10 ...rule $caus(l) \leftarrow \ldots caus(l')$ Perhaps you should say what the dots are for.

p. 10 Footnote 7: "The possibility of bad definitions exists only in the most general case and will be discussed there."

Where? Perhaps you should say in what section.

p. 10 Footnote 8: I did not understand the second sentence at first reading. "Initial state", "presence and absence" are confusing.

p. 16 Third and fourth line after Def. 4. Typo in math environment or too many symbols in one place?

I think all footnotes should be sentences.

p. 23, footnote 17: A more or less meaningful "real-world" example could be something like this: an object cannot be at location p and q at the same time (but must be somewhere), and actions $move - to - p$ and $move - to - q$ are always applicable.

p. 28, top. "is" is used twice.

"completely orthogonal to successor state calculation" is too strong, see comments for p.38-39.

p.38-39 "This approach contains several contributions: - A complete uncoupling of ramifications from state constraints..."

I do not think they can be uncoupled **completely**. Even in your theory, ramifications impose some restriction on your function Trans, I believe. Trans maps states and actions to states. In this sense reachable states are restricted and state constraints are implied. Can you clarify?

The other direction, from state constraints to ramification, as described by Thielscher, is a nice idea. As far as I know, no one claims that this is *the only way* to obtain causal rules specifying indirect effects. It is a useful method to obtain some of these rules, even though it does not always work.

Also, in your summary you say "the standard view on causal laws as rules serving to restore the integrity of state constraints is too restrictive." From talking to different people, I never had the impression that this is the only and standard view on the role of causal rules in the community.

Overall, even if you think your statement is absolutely right in its current form, it's strange to see it as a contribution #1. I think you have done more interesting things. Again, it's a good paper.

I hope my comments will be helpful.

Regards,

Eugenia

Debates

Ontologies for time

From: Erik Sandewall

Pat Hayes wrote:

> *Erik wants to know why we should bother to consider punctuated times... But more concretely, I disagree with Erik's way of thinking. I start with axioms and ask what models they have. Erik starts with models (the real line, for example) and assumes that we somehow have access to the 'standard' ones. But of course we don't: there is no complete computationally enumerable theory of the standard real line. All we have are axiomatic theories.*
>
> ...
>
> *Erik asks for examples of what can be done with punctuated-time models. I don't know what he means. It is theories that do things, not their models.*

I have no problems with theories that allow both punctuated and non-punctuated real time models. However, since punctuated time is proposed as a solution to the "Dividing Instant Problem", and since accordingly some or all non-punctuated models suffer from such a DIP, I'd like to understand what concrete difficulties are obtained in a computational system that admits non-punctuated time, and even relies on it.

This question of mine arises in a cognitive robotics perspective, which may in fact be distinct from the perspectives of commonsense reasoning or of natural language understanding. In cognitive robotics, it's of paramount importance to be able to deal with hybrid scenario descriptions, combining qualitative and quantitative estimates of duration, distances, consumption of resources such as fuel and battery power, and so on.

Consequently, we are led to use state-of-the-art algorithms for dealing with these kinds of constraints, including, in particular, temporal constraint solving in the tradition of Dechter-Meiri-Pearl and the more recent developments that are used on linear programming. All of these methods make use of standard arithmetic operations such as addition and multiplication, and don't make any exception for them not being everywhere defined. Therefore, it would seem adventurous to use them in a context where the time line is assumed to be punctuated, so that for some numbers in the (e.g. real) domain there does not exist any corresponding point in time.

Now, back to Pat's comments.

> *Erik asks for examples of what can be done with punctuated-time models. I don't know what he means. It is theories that do things, not their models.*

isn't it more exact to say that software does things? From the point of view of the algorithms mentioned above, it just doesn't matter whether and how the (e.g. real) numbers are axiomatized. However, it does matter that addition is always defined, which is why it is convenient to assume the use of "standard" models of time.

If software is identified only with theorem provers, then of course theories are of primary interest. But would you seriously propose using a theorem prover plus an axiomatization of the time domain as a computational instrument?

Some axioms and some deduction is needed, for sure, but suppose I develop and use a hybrid system that combines algorithms with Pat's core theory. Is there something that can then go wrong? Can I obtain some unwarranted conclusions? Will I miss some warranted conclusions? Or, will I get into inordinate difficulties when writing my remaining scenario description axioms? (At least *one concrete example* of such problems would be useful).

On the other hand, if no such problems have been reported and none can be found, they why is all this fuss about the so called DIP? Why can't we just take Pat's core theory, observe that it allows both the integers and the reals as timepoint domains, and that in addition it allows a whole lot of other domains that noone needs to care about from a practical point of view?

Or, to reverse the question - *concretely why* do people care about those other models? On this topic, Pat has referred to others:

> *Erik wants to know why we should bother to consider punctuated times. Well, I guess my first answer would be: ask the people who use the Allen theory. I think it has something to do with natural language understanding.*

and in another context to the temporal database community. Fine, but what is it that they can do using non-"standard" time that can't be done using "standard" time?

Jixin's answer to that question was:

> *The Dividing Instant Problem is a typical problem with the approach of simply constructing time intervals from points (such as reals, rationals or integers), e.g., by means of defining an intervals as a set of points...*
>
> *The fundamental reason is that in a system where time intervals are all taken as semi-open, it will be difficult to represent time points in an appropriate structure so that they can stand between intervals conveniently.*

I had actually asked for something more concrete: a scenario that can't be expressed, or a scenario query that requires inordinately long completion time, for example. Is it possible to be more specific about in what sense it "will be more difficult" to represent time points that stand between intervals?

As an aside: Jixin also wrote:

> *However, it seems that by some careful and proper treatments, we may also reach the same results by defining the concepts of intervals based on points. The key point here is, in addition to the concept of lower and upper bounds for point-based intervals, the concept of left type and right type for intervals needs to be addressed as well. What follows is the skeleton of the structure:*
>
> 1. *P is a partially-ordered set of points;*
> 2. *Type is a two-member set {open, closed};*
> 3. *An interval i is defined as a quaternion seq$(p1, p2, l, r)$ such that:*
>   - $p1 \leq p2$
>   - *l and r belong to Type*
>   - *if $p1 = p2$ then $l = r =$ closed*

Yes, but how does this differ from the standard notions of open and closed intervals? It would appear that

```
(a,b,open,open) = (a,b)
(a,b,open,close) = (a,b]
```

and so on. Are there some models where your fourtuple intervals can not be reduced to standard definitions, such as

```
(a,b) = {x | a < x < b}
```

and so on? Do those "nonstandard" models have some interesting properties?

Returning to the use of algorithms that are incompatible with punctuated time lines: I realize, of course, that computation on quantified estimates of durations and resources is rarely used in commonsense reasoning, at least as studied in A.I. Therefore, researchers in CSR may not find the algorithms mentioned above very useful. However, it would certainly be an advantage, from a general scientific point of view, if a common framework could be found for reasoning about actions in both CSR, natural language understanding, and cognitive robotics. Doing this would also facilitate the design of combined systems covering all of those aspects. However, one constraint in such a search for a common ground is that cognitive robotics needs the full set of reals (or some other dense domain) for the time axis.

My question is therefore: **is there something in those other areas that** *can not* **be rendered using full real time?** In other words, is it the case that these different areas require intrinsically different and mutually inconsistent extensions of a core theory such as the one proposed by Pat in the previous Newsletter?

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