ELECTRONIC NEWSLETTER ON
REASONING ABOUT ACTIONS AND CHANGE

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Today

Today we have answers to Luis Pereira for questions re the ETAI article by him and several co-authors that we received in December. Also, answers by Wolfgang Bibel for the additional set of questions regarding his invited article at IJCAI 1997.

ETAI Publications

Discussion about received articles

Dynamic Logic Programming

by José Júlio Alferes, João Alexandre Leite, Luís Moniz Pereira, Halina Przymusinska, and Teodor Przymusinski

It's probably easiest and less time consuming that I reply personally, rather than producing a reply involving all five authors, at least at this stage, where I'm not saying anything controversial, I believe.

> *My first question, therefore, is to what extent is there a difference: does your approach avoid the problems observed by Winslett, and if it does, what is the key to this improvement?*

Section 5.1 in our paper, and Theorem 7 in particular, shows that our approach generalizes that of Winslett as it applies to logic programs. Winslett updates interpretations instead of theories. We update theories or programs, but obtain interpretation updates as a special case. In the introduction of the paper we motivate why we do so. Further elaboration on the motivation can be found in our own work, references [Lei97] and [LP97], which I'll gladly provide for those interested, or can be gotten from our home pages.

Note that we're NOT performing belief revision. We're performing updates. We are not dealing with contradiction. New information overrides old information and that takes care of conflicts between the two. If new information is by itself contradictory we do not revise it. If old information is seen to be contradictory itself upon introduction of new information we don't revise it. We do no revision at all. Ginsburg and Winslett on the other hand are preoccupied with revision.

Coupling updates and revision, for when contradiction arises, is future work. It seems to me we can do it based on our previous work in LP revision, as can be found say in [mb-Alferes-96] and references therein, or my home page for more recent work on revision of logic programs, which I've done a lot of. But I should refrain to comment any more here on belief revision since it is not the subject of our paper. I do think that updates and revision should be developed separately.

> *My second question is with respect to updates in the presence of observations and action laws. One of your results is that if the initial program is just a set of facts, then program updates and model updates coincide. However, in the case of reasoning about actions, one typically deals both with facts about the world at various points in time ("observations") and with rules characterizing some of the effects of actions ("action laws", "effect laws"). ... presumably one wishes to prefer changes of "facts" (that is, sign reversal of literals) over changes of the action laws, ... Only in the presence of accumulated evidence is it reasonable to revise a well established action law.*

Again, we're not doing revision at all. If a fact updates a "law" by going against its conclusion then the fact wins! However, if you want to have persistent laws, you simply make them part of every update. Although we mention the application to reasoning about actions in the paper, we relegate it to future work. We are working on it at the moment. To do so we use updates that connect a previous state in the premises to a future state in the conclusion of the update, which type of updates we did not introduce in the paper. Since this discussion is about the paper, I will refrain from discussing that here further.

> *How would you foresee representing such cases: will action laws be written out explicitly as logic-program rules, and what updates will then be obtained on the current state? On the other hand, if action laws are not represented as rules, how are they represented and how are the results in your article to be used?*

This is ongoing work. Basically, actions are modelled by rules whose conclusions are post-conditions, but these only come into effect in the next updated state, ie the conclusions have the form of an internal self-update (not defined in the paper...), and the premises contain the pre-conditions and the action name. By performing an (external as opposed to internal) update of the action name the self-updating rule gets triggered. One of the post conditions deletes the action name if the action is not a persistent one. This will be the subject of a future paper.

I'm sorry to sound rather evasive, but the issues you address regarding actions are not tackled in the paper, but only mentioned as future work.

What the paper does is to establish a foundational base for dealing with updates of logic programs, which was sorely lacking till now. The exploration of a more dynamical type of logic programming is thus made possible. It will, we hope, lead to the use of present and forthcoming efficient LP implementational technology, and recent semantical developments, to a wider scope of problems.

References:

[mb-Alferes-96] José Júlio Alferes and Luís Moniz Pereira. *Reasoning with Logic Programs.* Springer Verlag, 1996.

Debates

Discussion with Wolfgang Bibel about his IJCAI lecture

From: Wolfgang Bibel

Dear Erik,

Thank you for your continuing interest in the paper underlying my invited IJCAI-97 talk. Since the main focus of your questions concerns "the research methodology or paradigm being used" I need first to clarify the nature of this paper.

As you correctly state the paper is meant to be "a survey paper"; however you failed to add (as clearly stated in the paper in the same sentence) that it is meant to be one "with an emphasis on the contributions from research groups influenced by the author's own work" (p2). As an invited talk this is an absolutely appropriate focus. For instance, your own invited talk at the ECAI conference in Budapest was of exactly the same nature. In fact you may recall that I then for fun complained about YOUR ignorance of all of OUR work (although NOT in public as you now decided to do). Reiter's Chambery IJCAI address did again the same representing the Toronto school. And so forth.

In consequence of this purposely chosen and clearly stated nature of the paper there are of course substantial omissions wrt planning, action and causality approaches in general. In fact I would not even consider myself capable of giving a fair survey of the current state of the art in these areas (perhaps not even in deduction any more where I feel myself still better "at home"). And of course in such a paper there is no need to justify each of the contributions from scratch (even for invited papers there are at least unwritten space limits). If such a need would be, one should not have invited me to give this talk in the first place. 9 out of 11 of your questions are essentially answered by these statements, as I will briefly show you shortly, and have been disappointing in this regard.

Apart from the (in more than one respect incomplete) survey character there is one novel contribution in this paper which is the extension of reasoning about change and causality within the LCM (the action part of TL) frame to include (or to be embedded into) also classical reasoning. In other words the paper marries two complementary areas which belong together. Given this fact the focus of any questions wrt this particular paper should actually be directed to this particular contribution rather than to the contributions which have been done many years ago (as far back as 1985) and to a large extent by students and close colleagues of mine rather than to me. Now to your questions in (boring but time-consuming) detail. As to the paper's section numbering I refer to the version in the web (not to the one in the proceedings).

ad 1.

provide assessments *or* validation results *for new theories*

Since there is no new theory I do not think this particular paper is in lack of such an addition, nor is this the case for the old theories surveyed in the paper. Classical deduction is so established that I am sure you agree wrt this part. As to the action part I know from you personally that you (as well as others) appreciate Thielscher's work especially for his contributions in this respect (relation of the fluent calculus with Lifschitz' $\mathcal{A}$ etc). In my Section 6.3 I survey the equivalence results between the fluent calculus and the action part of TL. On the basis of these theorems his results apply to TL in the same way.

ad 2.

> *My question is what can be said in general about the method you propose for qualification: when is it known to work, and when not?*

In line with the paper's survey character I properly state in Section 7.2 to which you refer here the source of this approach: "The solution is again adapted to TL from [Thi96]'s FC using the example discussed there in great detail." In that paper Thielscher indeed gives theorems and proofs addressing your question. He is the right person to answer specific questions in this regard. But I can add a general comment to issues like

> *For example, suppose there are two cars, A and B, it is known that a potato is put into the tailpipe of one of them, and one asks whether car B will start properly. In such a case, the absence of positive knowledge that the tailpipe of car B has been plugged, does not allow one to draw the default conclusion.*

The advantage of staying within the framework of logic is that you have a long experience on your side (and this is my research methodology which I followed for more than 3 decades). TL can of course handle your example (and also the other one you mention) easily since it features (a resource sensitive) disjunction.

ad 3.

> *Since there is no obvious definition for optimality in this respect, I wonder which quality measure do you use, and what is the proof that the LCM method is optimal with respect to it?*

Assuming the method is semantically correct (discussed in 1) there is of course an "obvious definition of optimality" namely the ones familiar from AD. Any explicit frame axiom increases the search space for the deductive mechanism (and even the proof lengths). Since LCM, TL, FC, LL have no frame axioms at all they are optimal in this respect.

ad 4.

> *So in what sense do we "need to" categorize the actions?*

Your question refers to the paper's Section 7.1 in which I again clearly stated: "The discussion in this section closely follows [Thi97b] ...". So, given the equivalence results mentioned above, your question should again better be addressed to Thielscher directly who in his paper and his habilitation thesis (also cited in this part) provides excellent answers to your question.

ad 5.

> *Also with respect to your formalization of Thielscher's example with three switches and a relay, it is remarkable that the electric circuit in question can easily be understood in terms of dependencies and persistence, but the proposed formalization requires the axioms to represent the propagation of* changes*: "if this fluent changes in such-and-such a way, then that fluent also changes in such a way". This seems*

> *clumsy and counterintuitive. Do you claim that it is the best possible representation in the present state of the art?*

If you know a less "clumsy and counterintuitive" formalization of the example then use it. TL as any logic is a neutral formalism which does not bother about the way particular scenarios are represented in them (as Bob Kowalsky convincingly argued in the memorable Crete workshop in 1985 where, in your presence and bombarded by your criticisms, I first presented LCM - as you can still read in the proceedings transcript of the discussions then).

ad 6.

> *My questions are:*
>
> - *What are the advantages of your approach over the one I just referred to? (This is a valid question in view of your statement "A better solution in this sense must be preferred to a deficient one").*

Again this four paragraph Section 7.4 summarizes the work reported [BT97] and [Gro96] so that the answers to your question are better to be looked up in the original sources rather than in my paper and addressed to the authors rather than to me.

> - *In PMON(RCs) and other approaches that use explicit metric time, it is straightforward to make statements about durations, comparing durations of actions, and so on, basically because each interpretation represents an entire history of the world. How can this be done in a logic like TL where the => operator takes one from one state to the next, and updating the current time in the process?*

It is obvious how to state metric time durations in TL (and in fact illustrated in the lifting example of that section) by having Tt before => and $T(t+d)$, d being the duration, after it (the example specializes to $d = 1$). I am not sure at this point whether Bornscheuer and the other people at Dresden working on time did already consider this more detail. My survey is definitely not a complete one.

ad 7.

> *You write "section 6 shows how the various aspects involved in reasoning about actions and causality can be taken into account within TL". However, nothing in section 6 or elsewhere in the article presents any concrete results about how spontaneous change in the world can be represented - it is as though the world were entirely static when no actions are taken. The resulting concept of causality is quite meagre.*

Again, my survey is definitely not (meant to be) a complete one even within the LCM family of approaches let alone all the rest. Spontaneous change has been formalized again by Thielscher in the references given in Section 7. I will include a pointer to that issue to avoid the (false) impression of "meagreness". Your other point concerning differential equations does not tell me anything new and my remark in the paper remains anyway correct.

ad 8.

Yes, but how is this different from the use of the Answer predicate which was proposed and used by Cordell Green in the late 1960's?

The technique is of course the same and well established in logic. So what is the point?

ad 9.

Does the same solution also apply if some of the effects of the general case do not *arise in the special case?*

No, it does not. It only applies in "occasions of a similar nature" as stated there. Again Thielscher is the expert on these issues.

ad 10.

If the answer to the previous question is positive ...

Since it is negative I skip this point.

ad 11.

I believe that some additional coverage of modern results in the latter area would be appropriate

I disagree as explained at the outset.

Best regards, Wolfgang

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