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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.

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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 August, 1998. 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.

Vol. 2: *269-269*, 31.8.1998
http://www.etaij.org/rac/nj/9808/

The Month of August

Summary of News Journals Contents

This is conference season - AAAI has just passed, ECAI took place at the end of this month - as well as vacation month for many. The ENRAC web-page structure provides lists of the articles presented at those conferences, and in particular the articles that are of direct relevance for reasoning about actions and change.

The ENRAC matters were dominated by discussions about submitted articles during August, like during the preceding months. The articles by Marc Denecker et al and by Murray Shanahan have both attracted long commentary. The topic discussion on ontologies for actions and change instead proceeds at a more leasurily pace compared to what it was like in the spring.

Electronic News Journal on Reasoning about Actions and Change 270
Vol. 2: *270-270*, 31.8.1998
http://www.etaij.org/ra/rac/006/

Protocol of on-line discussion during August, 1998
about the following research article:

Erik Sandewall

Logic-Based Modelling of Goal-Directed Behavior

C4-1. **Anonymous referee 3** (19.8)

Thank you for your reply to my comments. One point is still not clear to me. You claim that, in Theorem 1, there is no need to assume that the set of intervals is finite. The following seems to be a counterexample:

$$s_i = 1/(2 * i + 1), t_i = 1/(2 * i)$$

where i=1,2,... Please explain.

C4-2. **Erik Sandewall** (19.8)

Theorem 1 states that under the axioms and for *some ordering* of the intervals, $s_i < s_{i+1}$ and $t_i \leq s_{i+1}$ for all i. In your example, there is an infinite sequence of intervals that has a Zeno property and proceeds to the limit from above. This infinite sequence of intervals therefore has to be renumbered so that

$$s_{-i} = 1/(2 * i + 1), t_{-i} = 1/(2 * i)$$

In other words, the numbering is reversed and you have to consider $i = -1, -2, ...$ With this ordering the consequent of the theorem holds.

Your example also shows that if there is an infinite sequence of intervals, the theorem does not hold in the limit as i tends to infinity. However, this is also not claimed.

Your example does however remind me that if one is going to be very technical, it might be better to phrase the theorem so that it states that if interval i precedes interval j under the ordering, then $s_i < s_j$ and $t_i \leq s_j$. In this way one does not give the impression that a numbering of the intervals always exists using natural numbers. Such a numbering does exist in your example, but if one combines several Zeno sequences then it doesn't. Of course all of this is very remote from the situations that arise in the context we're addressing.

Electronic News Journal on Reasoning about Actions and Change 271
Vol. 2: *271-274*, 31.8.1998
http://www.etaij.org/ra/rac/007/

Protocol of on-line discussion during August, 1998
about the following research article:

Iliano Cervesato, Massimo Franceschet, and Angelo Montanari

The Complexity of Model Checking in Modal Event Calculi with Quantifiers

A3. **The Authors** (5.8)

Dear Rob,

Thank you very much for your questions. In the following, we provide you with some remarks about our modal event calculi that hopefully will contribute to clarify the questions you raised.

> *Generally, I would like to understand the relationship between the modal logic event calculi you describe and refer to in your paper and the classical logic versions of the event calculus that people such as myself and Murray Shanahan use. So any remarks you have in this respect would be helpful. Here are some more specific questions related to this:*

Let us briefly run again along the path that led us to develop our set of modal event calculi.

Roughly speaking, the original Event Calculus (EC) is a simple temporal formalism designed to model situations characterized by a set of events, whose occurrences have the effect of starting or terminating the validity of determined properties. Given a description of when these events take place and of the properties they affect, EC is able to determine the maximal validity intervals (MVIs) over which a property holds uninterruptedly.

We focused our attention on situations where only partial information about the ordering of event occurrences is at our disposal. More precisely, we limited our investigation to problems consisting of a fixed set of events that are known to have happened, but with incomplete information about the relative order of their occurrences. In these situations, the MVIs derived by EC bear little relevance since the acquisition of additional knowledge about the actual event ordering might both dismiss current MVIs and validate new MVIs. It is instead critical to compute precise variability bounds for the MVIs of the (currently underspecified) actual ordering of events. In [7,8], optimal bounds have been identified in the set of necessary MVIs and in the set of possible MVIs. They are the subset of the currents MVIs that are not invalidated by the acquisition of new ordering information and the set of intervals that are MVIs in at least one extension of the current ordering of events, respectively. In [1], we proposed a Modal Event Calculus (MEC), which, given a possibly incomplete specification of the ordering of event occurrences, extends EC with the possibility of inquiring about MVIs (MVIs computed by the original calculus), necessary MVIs (MVIs which hold in all extensions of the given ordering), and possible MVIs (MVIs which hold in at least one extension). In [3,4,5,6], we generalized such a modal framework to model effectively significant situations (we considered case studies taken from the domain of hardware fault diagnosis). We defined a number of modal event calculi that allow a free mix of modal operators, boolean connectives and preconditions, focusing on the interactions between modal operators and boolean connectives in [3,4], and between modal operators and preconditions in [6]. In [5], the expressiveness and complexity of the resulting hierarchy of modal event calculi are systematically investigated. In the ETAI paper, we further extend our modal event calculi by admitting arbitrary event quantifications.

Other differences between our modal event calculi and the classical logical versions of EC, that are worth mentioning, are the following:

- we provided EC with a model-theoretic formalization by means of the notions of EC-structure (which specifies the set of event occurrences, the set of relevant properties, the initiating and terminating maps that respectively associate each property with the events that initiate and terminate it, the exclusivity relation that identifies incompatible properties), knowledge state (a strict partial order over the set of occurred events), EC query language (the set of all property-labeled intervals over the EC-structure), and EC intended model (the set of MVIs computed by EC);
- we generalized such a formalization to all modal event calculi we proposed; the generalization actually consists in the extension of the EC-structure to deal with preconditions, of the (modal) query language to encompass modal operators and boolean connectives, and of the intended model to cope with the additional structural and/or linguistic features;
- we gave sound and complete implementations of the various modal event calculi in higher-order logic programming languages (hereditary Harrop formulas, lambda-Prolog).

  - *Do you see your formalisms as what we describe as "narrative" formalisms - i.e. formalisms in which the flow of time is described independently of any actions that occur?*
  - *Is the underlying concept of time linear? By "partial order" of events, do you mean "incomplete information about a total order", or are your techniques applicable to genuinely partially ordered time structures (such as the forward branching time structure employed by many versions of the Situation Calculus)?*

As described above, at the beginning of our research work (cf. [7,8]), we restricted our attention to complete actual courses of events, whose temporal ordering, specified by means of a set of ordered pairs of events, is only partially known. The considered scenarios are thus characterized by a set of occurred events, which is fixed once and for all, and does not include concurrent or divisible events, and by incomplete (but consistent) information about an actual total order, given in terms of the relative order of pairs of event occurrences. Modal EC (cf. [1]) allows one to determine the sets of MVIs, necessary MVIs, and possible MVIs, and to constrain their evolution as further (consistent) information about the ordering (that is, further pairs of ordered events) is added.

However, most of the restrictions of the original Modal EC (which, by inertia, we do not explicitly remove from its successive generalizations [3,4,5,6]) can actually be relaxed:

- completeness of the set of event occurrences (we can have incomplete knowledge about both the event occurrences and their temporal ordering);
- occurred events only (the events can be either actual events or hypothetical ones).

All the proved results, indeed, do not rely on such restrictions.

- *I'm confused about the syntax of your language(s). Are there any meta-predicates in your language other than "br"? Perhaps some examples*

According to the proposed approach, the meta-predicate "br" does not belong to any modal query language (syntax); it comes into play in the definition of the intended models of our modal languages (semantics) as well as in their implementations.

Obviously, nothing prevents us to extend query languages to allow one to retrieve information about, for instance, the initiating and terminating maps. Our original (and current) goal, however, was to support queries about (any combination of) MVIs only.

would help. How roughly might you express information such as:

"Pushing causes the door to open if it's not locked"

"The door was pushed at 9 o'clock"

(In the classical logic variants of the event calculus, you'd write formulas such as

$$\forall t[\mathit{Initiates}(\mathit{Push}, \mathit{Open}, t) \leftarrow \neg \mathit{Holds}(\mathit{Locked}, t)]$$
$$\mathit{Happens}(\mathit{Push}, 9)$$

)

Such as example can be easily expressed in the Event Calculus with preconditions (PEC) described in [6]. A structure for PEC (PEC-structure) differs from an EC-structure for the addition of a context argument to the initiating and terminating maps (that specifies the set of preconditions that must hold in order that an event actually initiates or terminates a given property) and the removal of the exclusive relation (the presence of preconditions in PEC permits an easy emulation of the exclusivity relation).

We express the condition that

"pushing causes the door to open if it's not locked"

by using the initiating and terminating maps of the PEC-structure as follows.

Let OPEN and UNLOCK be two properties, and PUSH, SHUT, KEY_L, and KEY_R be four events. The event PUSH initiates the property OPEN with precondition UNLOCK, that is, a push event opens the door if it is unlocked. Moreover, the event SHUT terminates the property OPEN with no preconditions. Finally, the event KEY_L initiates the property UNLOCK, while the event KEY_R terminates it.

It is worth noting that, for simplicity (as in the original Kowalski and Sergot's EC paper), we usually define the initiating and terminating maps in terms of event occurrences and property types. In such a case, the initiating map only includes information stating that the occurred PUSH event initiates the property OPEN if the property UNLOCK holds at its occurrence time, while the terminating map provides no information.

However, it is not difficult to redefine the initiating and terminating maps in terms of event and property types, and to properly instantiate them when an event of the given type occurs (we detailed such an approach in some of our early papers on EC). In such a case, the initiating and terminating maps can be used to model all the above described event and property relationships; moreover, the relationship between the event PUSH, the property OPEN, and the precondition UNLOCK is instantiated for the given occurrence of an event of type PUSH.

Furthermore, we express the fact that

"the door was pushed at 9 o'clock"

by stating that the set of occurred events of the PEC-structure only includes an occurrence of a PUSH event.

We do not associate explicit times with event occurrences (we model temporal information by a strict partial order over the set of occurred events), but we exploit temporal knowledge provided by explicit timestamps to obtain information about the relative ordering of event occurrences.

Aside. We only consider positive preconditions. For this reason, we introduce the condition UNLOCK. However, as long as we restricted ourselves to acyclic dependencies among properties (as we did in [6]), it is not difficult to generalize our notion of PEC-structure in order to support both positive and negative preconditions.

Electronic News Journal on Reasoning about Actions and Change 275
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http://www.etaij.org/ra/rac/008/

Protocol of on-line discussion during August, 1998
about the following research article:

David Poole

Decision Theory, the Situation Calculus and Conditional Plans

Q1. **Uffe Kjærulff** (30.8)

The focus of the paper seems to be on representational issues, which is fine. But, being a Bayesian-network person, I'm would find it interesting to see how computations of expected utilities of plans are performed in the ICL framework. This issue is only touched upon very briefly in the paper, mentioning that it involves enumerating all possible cases. In my understanding that amounts to constructing the corresponding decision tree (which may be huge), and then computing the probability and utility for each leaf of the tree.

So, my question boils down to: Is there an efficient computational scheme for ICL?

Uffe Kjærulff
Dept. of Computer Science, Aalborg University, Denmark

Electronic News Journal on Reasoning about Actions and Change 276
Vol. 2: *276-282*, 31.8.1998
http://www.etaij.org/ra/rac/009/

Protocol of on-line discussion during August, 1998
about the following research article:

Marc Denecker, Daniele Theseider Dupré, and Kristof Van Belleghem

An Inductive Definition Approach to Ramifications

C2-2. **The Authors** (5.8)

Dear Eugenia,

> *Thank you for your answers to my questions. These answers clarify an important difference between our works.*
>
> *Using generalized inductive definitions to detect ill-defined literals is the advantage of your work.*
>
> *However, after 2-valuedness is shown, all reasoning involving multiple models have to be performed on the metamathematical level. In contrast, first order logic augmented with inductive definitions allows us to work with all models at once therefore allowing for incomplete information. Perhaps your approach could be developed in this direction, but you do not address this problem in the paper. Notice I do not talk about non-determinism here. I completely agree with your point of view about non-deterministic actions.*
>
> *Therefore it seems that we mostly developed different parts of the problem.*

To an important extent your work is indeed complementary to ours. Indeed, we must stress the following: our formalism is not a stand-alone temporal reasoning logic. It does not deal with descriptions of initial states, action preconditions, narratives, state constraints. It does not even make an ontological choice on the nature of time. To use our effect language for temporal reasoning, it must be extended with language constructs for describing these other aspects of temporal knowledge. Or it must be embedded in an existing approach which provides this expressivity. Your work provides such an embedding. We don't.

Instead, we focus entirely on ramification and (instantaneous) effect propagation. You will admit that there is merit in our approach: it shows that the ramification problem can be investigated independently of all these other issues - at least if one views causal rules as we do, namely as descriptions of effect propagations independent of say, state constraints-. In other words, our work can be used embedded in a number of other approaches ($\mathcal{A}$, situation calculus, event calculus, temporal logics..).

On the other hand, we pay a price for singling out the ramification problem; namely we do not show in our paper the interplay between causal rules and the other types of information. For instance, uncertainty on a certain state of affairs may be due to uncertainty on the initial state or on the narrative (the sequence of events that leads to the state). Obviously, in our paper, we cannot show how to model these sorts of uncertainty; however, our language and semantics is compatible with and is embeddable in many temporal logics in which such uncertainty can be described. Such a logic should satisfy only two conditions: in the logic there must be a notion of momentaneous state of the world which can be represented as a set of true fluents, and uncertainty on the state of the world at a particular moment or situation must be modeled by having different models of the theory in which the state at the time or situation is different (but always two-valued). This is precisely how uncertainty is modeled in your integrated approach.

We may add that in the past we have also investigated this combination of inductive definitions and first order logic with uncertainty in a global temporal logic and in logic in general. We have deliberately kept the issue of representing uncertainty on the world (except in our study of nondeterministic effects) out of the current paper to keep the different issues separated. However, uncertainty is addressed in the technical report referred to as [38] in the paper, since there an embedding in a general theory of time is discussed. (The report, like several of the papers mentioned below, can be obtained from

http://www.cs.kuleuven.ac.be/~kristof/publications.html

As a matter of fact, the idea of integrating inductive definitions with first order logic is the main idea behind Open Logic Programming, used by Marc and Kristof in most of their previous related work (e.g. the papers by Van Belleghem, Denecker and De Schreye in ICLP'94, ICTL'94 and ICLP'97, by Denecker in LPNMR93 and LPNMR95) and is related to ideas about Abductive Logic Programming (in the paper by Console, Theseider Dupre' and Torasso in JLC'91).

The idea in Open Logic Programming is that some predicates are considered "defined", and therefore a "closure" semantics, i.e. a Logic Programming (or Inductive Definition)-like semantics is applied to them, while the interpretation of other predicates is left open, possibly being constrained by partial knowledge given in the form of integrity constraints. So, OLP is to be seen as a integration of classical logic with inductive definitions and is suitable for representing uncertainty. Representing and reasoning on uncertainty in this logic is not on the metamathemathical level but on the object level. The embedding of our ramification approach in a linear time structure [38] is actually firmly based on Open Logic Programming.

> In your approach, you impose a syntactical restriction (no cycles) which guarantees that the effects are unique and well-defined.

The syntactic requirement of acyclicity is by no means inherent to my approach. It simplifies the presentation in the LNCS paper. Otherwise I would have to consider the cases where the correct syntactic form of successor state axioms is not obtained, thus making the paper even longer.

Sorry, that remark was by no means intended as a critique of your approach. We were only trying to explain the use of the third truth value. It helps us to avoid imposing a syntactic restriction, by detecting "bad" definitions in the semantics. Bad definitions need to be dealt with somehow, either by restricting the syntax, which we chose not to do, or by detecting them in the semantics, as we have done. We did not mean to say "your approach does not work with cycles", but only that in any approach some restriction or a way around bad definitions needs to be provided (and that introducing the third truth value is the way we chose to do it).

> We can't see in what way we would be applying the Closed World Assumption.

> *I use this term in a sloppy way here to say that a state is represented by a set of positive fluent literal, and the literals not in the set are false in this state.*

Classical logic has a 2-valued model semantics. Yet it seems to us that even with a sloppy use of the term "Closed World Assumption", one could not say that classical logic "applies the Closed World Assumption". Neither can this be said about our two-valued state semantics.

We do not allow any state to contain undefined fluents just because by "state" we mean a real state, not knowledge of an agent on a state. We do not at all intend that there should be complete knowledge on states. If there is partial knowledge on a state, there will be more than one state matching this partial knowledge. It's again an issue of reasoning about different models, and for each of them our transition function gives a resulting state (it gives one as long as there are no bad definitions, and not more than one as long as there is no explicit nondeterminism).

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

> This is not really true. The result of the transition function defines the holds predicate in the next state. One can verify that the next state is derived from the previous state and the causal literals as described by the normal situation calculus persistence axiom.

> *Perhaps I should have chosen a different wording. I used "derive" which I understand as "derive in a formal proof system". I did not mean "makes true in a particular model".*

Is your remark that we have not formalised a proof system but only the model theory ? Or are you referring to the fact that our language can be used only to describe state transitions and nothing else? With respect to the second interpretation, we have motivated this choice (see above).

With respect to the first interpretation, of course we have not defined any formal proof system in the paper, just a declarative and constructive model semantics. To derive anything about *holds* in a certain state, given a previous state, one can calculate all the *caus* literals in the state transition using the fixpoint operator. Calculating the new *holds* literals from the previous *holds* and the *caus* literals is then straightforward: $holds(l, s') = (holds(l, s) \wedge \neg caus(\text{-}l)) \vee caus(l)$. Is your remark that we have not formalised a proof system but only the model theory ?

> Observe moreover that ramifications need not imply state constraints even if they restrict Trans. Of course a restriction of Trans rules out certain transitions, but that does not need to mean that any states are ruled out: a state is ruled out only if all transitions leading to it are invalid. The "counter" example in our paper gives an example where this is not the case: whenever an output changes to true, the counter must be augmented. But a resulting state in which the counter were not updated (but all other effects applied) need not be invalid in itself. This state might well have existed at an earlier time in the history of the network. It can only not be reached from the currently given state. So ramifications need not imply any state constraints.

I do not understand neither this comment nor the "Counter" example. The rules imply a state constraint relating any two consecutive states. This constraint is perfectly valid.

It seems there is just a misunderstanding about terminology here. We use the term "state constraint" to mean a relation between fluents within any one state, not between different states (see also the definition in the paper). As such the term corresponds to for example Reiter's and Shanahan's notions of state constraint and Thielscher's and Lifschitz's notions of domain constraint (to name just a few). We were not aware of a different use of these terms in the literature.

It is evident that any derived effect rule necessarily implies some relation between fluents in two consecutive states, e.g. for any state s and any successor state s' of s after any action, the rule "initiating F causes G if H" implies that

$$holds(H, s) \wedge \neg holds(F, s) \wedge holds(F, s') \rightarrow holds(G, s')$$

for any F, G and H. But that is not a state constraint.

Does this clarify the examples and discussions given before ?

Finally, a question about Definition 1. You say "and body B a nonempty set of positive and negative literals of D". Do you mean "and body B, a nonempty set of symbols from D, including $\{t\}$ and $\{f\}$"?

No, D already includes $\{t\}$ and $\{f\}$ (see just above definition 1). With positive and negative literals from D we mean symbols of D or their negations. As such the definition allows for t or f to occur alongside other literals in a rule body. Observe that $\neg t$ or $\neg f$ can not occur since t and f can never belong to *Defined*(D) (see just below definition 1).

Thanks for your comments,

Kristof, Marc and Daniele

C2-3. **Eugenia Ternovskaia** (20.8)

Dear Marc, Daniele and Kristof,

Thank you for answering my questions and clarifying the motivation for your work.

A few words regarding our discussion about proof system versus model-theoretical approach. I was asking, without criticizing, about performing reasoning in your system on the object level, i.e., embedding your approach in a proof system. From your answer I understood that you consider both OLP and some logical representations as candidates for formalizing and performing reasoning. This answer is satisfactory, and it would be interesting to learn more about OLP from your papers.

> I do not understand neither this comment nor the "Counter" example. The rules imply a state constraint relating any two consecutive states. This constraint is perfectly valid.

> *It seems there is just a misunderstanding about terminology here. We use the term "state constraint" to mean a relation between fluents within any one state, not between different states (see also the definition in the paper). As such the term corresponds to for example Reiter's and Shanahan's notions of state constraint and Thielscher's and Lifschitz's notions of domain constraint (to name just a few). We were not aware of a different use of these terms in the literature.*

This is true, I have not seen this wider notion of a state constraint in the literature either. However, Chitta Baral and Ray Reiter used this term in a conversation recently. I do not see a reason why this wider notion could not be used as well.

Even if we choose not to extend the notion of a state constraint, it's hard for me to see in what way your observation would imply any interesting theoretical consequences. It seems obvious that a causal rule imply universal statements about situations (states) involving as many states as there are those mentioned in the causal rule. Your "counter" example only shows that there are indirect effects of actions depending on two consecutive states, not just one. It's hard to disagree. All causal rules of this king will imply universal statements about two consecutive states, not about just one, of course.

Regarding Definition 1, I do not remember whether you define what a literal is. Maybe I just missed it. When I was reading the definition, I did not assume that $\{t\}$ and $\{f\}$ can be literals. In the definition, you say "and body B a nonempty set of positive and negative literals of D". I understood that you take positive and negative literals of D, leaving $\{t\}$ and $\{f\}$ out because they are *not* literals. This contradicted to the discussion below. Of course I figured out what you mean, and then wrote to you thinking that a little rewording would help the reader.

Regards,
Eugenia

C2-4. **The Authors** (30.8)

Dear Eugenia,

Here are some thought about your comments.

> > The rules imply a state constraint relating any two consecutive states. This constraint is perfectly valid.
>
> It seems there is just a misunderstanding about terminology here. We use the term "state constraint" to mean a relation between fluents within any one state, not between different states (see also the definition in the paper). As such the term corresponds to for example Reiter's and Shanahan's notions of state constraint and Thielscher's and Lifschitz's notions of domain constraint (to name just a few). We were not aware of a different use of these terms in the literature.

This is true, I have not seen this wider notion of a state constraint in the literature either. However, Chitta Baral and Ray Reiter used this term in a conversation recently. I do not see a reason why this wider notion could not be used as well.

Even if we choose not to extend the notion of a state constraint, it's hard for me to see in what way your observation would imply any interesting theoretical consequences. It seems obvious that a causal rule imply universal statements about situations (states) involving as many states as there are those mentioned in the causal rule. Your "counter" example only shows that there are indirect effects of actions depending on two consecutive states, not just one. It's hard to disagree. All causal rules of this king will imply universal statements about two consecutive states, not about just one, of course.

Recall that our opinion of the current state of the art was that effect rules were much perceived as tightly coupled to state constraints (in the narrow sense; i.e. statements relating properties at the same time point). In any case, if one looks at current work on ramification, then usually effect rules imply state constraints (like in Lin's approach), or their semantics is determined by state constraints (like in Thielschers approach).

The role of our counterexample was simply to show that there may be no such relation.

What are interesting theoretical consequences? We do not point to some interesting new and fundamental relationship; actually we cut one: in general an effect rule may be unrelated to state constraints (but this was never your viewpoint anyway, right?). But there is a gain: the effect propagation can be described mathematically independently of state constraints and also of time topology, action preconditions, initial states, etc..

But yes, also to us

"it seems obvious that a causal rule imply universal statements about situations (states) involving as many states as there are those mentioned in the causal rule."

An interesting question generated by your remark is whether Thielscher's approach to derive causal rules from state constraints (about one state) and dependency information can be generalised to "constraints between states" (plus dependency information as far as needed).

Regarding Definition 1, I do not remember whether you define what a literal is. Maybe I just missed it. When I was reading the definition, I did not assume that $\{t\}$ and $\{f\}$ can be literals. In the definition, you *say "and body B a nonempty set of positive and negative literals of D". I understood that you take positive and negative literals of D, leaving $\{t\}$ and $\{f\}$ out because they are are* not *literals. This contradicted to the discussion below. Of course I figured out what you mean, and then wrote to you thinking that a little rewording would help the reader.*

We will take care to clarify this in the text. Thanks.

Thanks for pursuing this discussion, Eugenia.

Marc, Daniele, Kristof

Electronic News Journal on Reasoning about Actions and Change 283
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Protocol of on-line discussion during August, 1998
about the following research article:

Murray Shanahan

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

Q1. **Paulo Eduoardo Santos** (1.8)

1- In section 1, when defining the assimilation of sensor data, it is proposed a background theory Σ_B comprising axioms for change, actions, space and shape. However along the paper I did not see any other mention of such a theory, my question is whether Σ_B is represented by the last formula of section 4?

2- If so, why include in the theory Σ_E (section 5) again the axioms for actions and change, space and shape already included in Σ_B, since it is considered the conjunction of Σ_B and Σ_E for the abduction process?

3- All along the paper, Circumscription is done in parts of theories (rather than in the whole theory). I understand it is done 'by construction' since the chosen version of Event Calculus considered, is the one defined using forced separation (as presented in Shanahan's book "Solving the Frame Problem" [b-Shanahan-97]. My question is why do we have to circumscribe parts of the theories in the way presented, and not in any other way? Is there any formal justification for using forced separation ?

4- At the beginning of section 9 we read:

> *An alternative approach is to tailor make algorithms for specific tasks, such as sensor data assimilation, whose correctness with respect to the logical account can be demonstrated. This is the methodology I will adopt here, and the logic programming approach is left for further research.*

In another paper by Shanahan **"What Sort of Computation Mediates Best between Perception and Action"** [Shanahan-96] we read:

> *It is important to note that the logicist prescription does not demand a one to one correspondence between the data structures in the machine and the sentences of the chosen formal language. (...). In other words, the machine does not have to implement a theorem prover directly. Between the abstract description of a logic-based AI program and the actual implementation can come many steps of transformation, compilation, and optimisation.*

I have two questions about these statements: first, by assuming a logic programming approach, aren't we contradicting the last statement above? and, if it is not to have a theorem prover implementing the logic-based description of the system, what is exactly the role of logic in this framework?

My last question is about computational complexity:

In the framework presented in the paper under discussion two important points are 'explanation by adbuction' and 'circumscribing theories'. As presented in [c-stacs-93-Eiter] the complexity of logic-based abduction is NP-hard (for the problem of finding an abductive explanation with the additional constraint that it has to contain a predefined letter p); the results about complexity of Circumscription are not much more impressive (as can be seen in [j-jlp-17-127].

My question is, taking into account these complexity results, can we still apply this framework in robotics ?

Yours,

Paulo Eduardo Santos

Unpublished reference

[Shanahan-96] Murray Shanahan, *What Sort of Computation Mediates Between Perception and Action?*, Working notes of the AAAI Fall Symposium on Embodied Cognition, 1996.

References:

[c-stacs-93-au/Eiter] [Author unknown]. *NIL.* Proc. Symposium of Theoretical Aspects of Computer Science, 1993.

[j-jlp-17-127] Marco Cadoli and Marco Schaerf. *A Survey of Complexity Results for Non-monotonic Logics.* Journal of Logic Programming, vol. 17 (1993), pp. 127-160.

A1. **Murray Shanahan** (25.8)

Many thanks for the questions.

> *1- In section 1, when defining the assimilation of sensor data, it is proposed a background theory Σ_B comprising axioms for change, actions, space and shape. However along the paper I did not see anyother mention of such a theory, my question is whether is Σ_B represented by the last formula of section 4?*

The description in Section 1 is supposed to give just an impression of the way the abduction works. It's just a caricature of the real formalisation, intended to help the reader. The actual formalisation we see later is very complex, and I thought it would be hard to understand without a gentle introduction. The axioms for change, actions, space and shape in Σ_B are actually the event calculus axioms CEC plus the spatial axioms (Sp1) to (Sp8).

> *2- If so, why include in the theory Σ_E (section 5) again the axioms for actions and change, space and shape already included in Σ_B, since it is considered the conjunction of Σ_B and Σ_E for the abduction process?*

I guess this question is irrelevant in the light of the last answer.

> *3- All along the paper, Circumscription is done in parts of theories (rather than in the whole theory). I understand it is done 'by construction' since the chosen version of Event Calculus considered, is the one defined using forced separation (as presented in Shanahan's book "Solving the Frame Problem"). My question is why do we have to circumscribe parts of the theories in the way presented, and not in any other way? Is there any formal justification for using forced separation ?*

By splitting the circumscription into parts, we get a more managable theory, one whose consequences are easier to work out. In fact, the role of circumscription here is mainly to complete certain predicates, specifically Initiates, Terminates and Happens. If we circumscribe everything together, we have to do a lot more work to avoid difficulties like the Hanks-McDermott problem.

The technique is similar to what Erik Sandewall calls filtered preferential entailment. I think I've seen Erik provide a carefully argued justification of this technique in one of his papers. Perhaps he will remind us where to find it.

> *4- At the beginning of section 9 we read:*
>
>> An alternative approach is to tailor made algorithms for specific tasks, such as sensor data assimilation, whose correctness with respect to the logical account can be demonstrated. This is the methodology I will adopt here, and the logic programming approach is left for further research.
>
> *In another paper by Shanahan ("What Sort of Computation Mediates Best between Perception and Action") we read:*
>
>> It is important to note that the logiscist prescription does not demand a one to one correspondence between the data structures in the machine and the sentences of the chosen formal language. (...). In other words, the machine does not have to implement a theorem prover directly. Between the abstract description of a logic-based AI program and the actual implementation can come many steps of transformation, compilation, and optimisation.
>
> *I have two questions about these statements: first, by assuming a logic programming approach, aren't we contradicting the last statement above? and, if it is not to have a theorem prover implementing the logic-based description of the system, what is exactly the role of logic in this framework?*

I don't think there's a contradiction here. As both these quotes emphasise, the role of logic in the implementation can be more or less direct. The implementation described in the paper under discussion doesn't use a theorem proving approach, so the role of the logic with respect to that implementation is solely as a specification. (I regard the logic, not the implementation, as the main contribution of the paper, by the way.)

On the whole, as I argue in the other paper you quote from, I think it's preferable to take a more theorem proving approach, in which case the logic is more intimately related to the implementation. But this approach is hard when we're dealing with difficult mathematical objects like the plane, as in this paper. In my current work, however, I have a simpler description of the robot's environment, and a logic programming implementation of sensor data assimilation is more feasible.

> *In the framework presented in the paper under discussion two important points are 'explanation by adbuction' and 'circumscribing theories'. As presented in ... the complexity of logic-based abduction is NP-hard (for the problem of finding an abductive explanation with the additional constraint that it has to contain a predefined letter p); the results about complexity of Circumscription are not much more impressive (as can be seen in ...).*
>
> *My question is, taking into account these complexity results, can we still apply this framework in robotics ?*

This is a difficult issue.

My feeling is that complexity results should be used only as a guideline, and a disappointing complexity result rarely justifies the wholesale rejection of an approach. This is because the complexity results are worst-case, while in actual usage, a technique is often confined to a narrow, tractable sub-class of problems that no-one has pinned down yet.

In fact the use of circumscription to overcome the frame problem here is a case in point. The form of the theories we're interested in guarantees that the circumscriptions always reduce to predicate completions, which can be handled efficiently by Prolog.

Moreover, in the context of robotics, I think there's another argument that worst-case complexity results are misleading. No robot should be allowed unlimited computation time for any reasoning task before that task is suspended to sense the world and respond reactively to it. And ultimately, the world always moves on and renders old, unfinished reasoning tasks irrelevant. (I used to spend a lot of time thinking about the mind-body problem in philosophy, no doubt a "computationally intractable problem". Now I have children, and don't have time for such luxuries.) A robot's designer needs to organise things so that most reasoning tasks the robot sets out to perform can be completed in a short time. And if, once in a while, the robot is unfortunate enough to hit on one that would take the lifetime of the universe to solve, who cares? It'll soon give up on it when it has to dodge a falling rock or grab a passing robot of the opposite sex :-)

I hope this answers your questions, and thanks for taking the time to read my paper.

Murray Shanahan

Q2. **Chitta Baral** (19.8)

Dear Murray:

It was nice (but a little exhaustive :-)) reading your paper. I hope the following feedback will be useful.

In this paper you have initially (Section 2-5) expressed using logic: actions and their effects, occurrences of robots actions, initial value of fluents, event calculus axioms, domain constraints, formulation of continuous change using the release predicate, representation of space and shape, and axioms about triggered events.

Given information on occupies (at the initial situation) the formulation can predict occurrence of triggered events. You rightly argue that an abductive method can make conclusions about occupies when told about (or when it senses) triggered events. This is the basic essence of the first part of the paper I like the detailed logical formulation.

Some of the questions and suggestions that I have about this part are as follows:

(i) I think that in general a robot does not really sense events, rather it notices changes in certain fluent values (in say resistors linked to the sensors) from which it abduces the occurrences of triggered events. Nevertheless, it is ok with me to skip this part and directly talk about robots observing triggered events. But a clarification would help.

(ii) Since you mention several times (abstract, Section 2, etc.) that you use a novel solution to the frame problem using 'Releases' it will be nice if you say about it a little more than what is said in Section 2 (just after EC 5). (You do use it in axioms in later sections, but you don't discuss them.)

Although from the illustrations I can appreciate the use of 'Releases' in formulating continuous change, its utility in formulating constraints is not clear to me. For example, why not eliminate B4 and instead of 'Releases' have 'Terminates' in the head of rule E2.

To me the advantage of having constraints such as B4 is that by having it we avoid explicit compilation of it to effect axioms. I.e. we can use constraints for automatically deducing effects indirectly. But if we have both B4 and E2 then we are not really avoiding the compilation, as E2 is like an effect axiom.

(iii) I am not sure if 'Bearing' is a standard geometrical notion. Perhaps an intuitive meaning of it will help. Or may you can use the more familiar notion of 'slope'.

(iv) You say (just after Sp3): "the term $Line(p1, p2)$ denotes the straight line whose ...". Perhaps it is more appropriate to say "straight line segment" instead of "straight line".

(v) I think in axiom (B3) you are assuming velocity to be one. If you do please mention it.

(vi) The sentence after (B6). In it you explain $Blocked(w1, w2, r)$ by 'if object $w1$ cannot move any distance at all in direction r without overlapping with another object'. Perhaps you should say: "if object $w1$ cannot move any distance at all because of $w2$ in direction r without overlapping with another object'.

(vii) When defining $HoldsAt(Touching(w1, w2, p), t)$ are you making some assumptions about the shape of $w1$ and $w2$. Imagine two triangles which touch at a point, you can align them such that they touch but yet do not satisfy the conditions you have in B6.

(viii) I am not clear about the intuition behind the notion of partial completion in Section 5; especially in the text after (5.7).

For example in proposition 5.8, I would encode the intuition "bump switches are not tripped at any other time" by

$$Happens(Switch1, t) \rightarrow t = T_{bump}$$

and

$$Happens(Switch2, t) \rightarrow t = T_{bump}$$

I am not clear about the intuition behind your formulation.

Also, why not use the standard Clark's completion and explain the Clark's completion of Ψ, where Ψ is as mentioned in Definition 5.9 and the paragraph before. I.e, have the Clark's completion of Ψ in the right hand side of the turnstyle in Proposition 5.8

(ix) In section 6 you logically express a region as a list of straight lines. (You say it just before Bo3.) Is there any particular reason you use lists instead of sets. If not, since you already use set notation in the rest of your formulation, by using sets here also, you might avoid additional axioms such as Bo3 and Bo4

(x) Also (Bo3) is a fixpoint expression and you probably need (as needed when defining transitive closure) to minimize 'Members' to get the right models.

(Recall that the logic program

$anc(X, Y) \leftarrow par(X, Y)$

$anc(X, Y) \leftarrow par(X, Z), anc(Z, Y)$

is not equivalent to the formula

$anc(X, Y) \leftrightarrow par(X, Y) \vee (par(X, Z) \wedge anc(Z, Y))$

You are not minimizing 'Members' in Proposition 6.2. Or perhaps I am missing something.

(xi) In Section 7 you first take into account noise and formulate it and later define preferred explanations.

From Section 2-7 your formulation is in logic. It seems to me in Section 8 you give an independent formulation and relate it to the logical formulation with necessary and partially sufficient conditions. Your algorithms in section 9 are justified based on the formulation and results in Section 8.

(xi) Although I can appreciate the usefulness of the logical formulation in Section 2-7, some may pose the following question: Why not just formulate as in Section 8 (with some extensions perhaps) and then have the correctness of the algorithm in Section 9 proven with respect to the formulation in Section 8. Why go through the formulation in Section 2-7? I think it will be a good idea to address this or say a few lines about this to preempt such questions/attacks on logical formulation.

These are some of the questions and/or suggestions I have so far. I am reading the proofs now. If I have additional questions I will take the opportunity provided by this wonderful forum.

Best regards Chitta