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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. 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 following URL:

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

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 May, 1998. Then the contents were edited and formatted, resulting in the present, formatted version which was published on July 29, 1998, 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 200 copies, legally archived, and made available for distribution.

Vol. 2: *179-179*, 31.5.1998
http://www.ida.liu.se/ext/etai/actions/nj/9805/

The Month of May

Summary of News Journal Contents

The ETAI submitted articles by Michael Thielscher and by Antonis Kakas and Rob Miller were refereed during May. In both cases, the referees had a few additional questions to the authors, besides the questions that had already surfaced during the open reviewing period. These referee questions have been posted in the Newsletter together with the authors' replies. - Both articles were accepted to the ETAI following the recommendations of the referees.

An additional article was submitted by Iliano Cervesato, Massimo Franceschet, and Angelo Montanari. At this point we introduced the policy of inviting one or a few reviewers to make comments and questions about the article openly and at the beginning of the reviewing period. The first set of these invited review questions is included in the present News Journal.

Also, the discussion about the ontology or ontologies of time continued also during the month of May.

Electronic News Journal on Reasoning about Actions and Change 180
Vol. 2: *180-181*, 31.5.1998
http://www.ida.liu.se/ext/etai/received/actions/002/

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

Michael Thielscher

A Theory of Dynamic Diagnosis

C2-1. Anonymous Reviewer 2 (3.5)

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.

C2-2. Michael Thielscher (7.5)

I hope that the following remark constitutes a satisfactory reply to your question. This is the basic idea in the paper [1] [5], 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).

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

Q4. **Anonymous Reviewer 2** (3.5)

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

A4. **Michael Thielscher** (7.5)

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.

Vol. 2: *182-187*, 31.5.1998
http://www.ida.liu.se/ext/etai/received/actions/003/

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

Antonis Kakas and Rob Miller:

Reasoning about Actions, Narratives and Ramification

Q6. **Anonymous Reviewer 2** (23.4)

The paper by Baral, Gelfond and Provetti published recently in JLP describes an $\mathcal{A}$-like language, $\mathcal{L}$, which, like $\mathcal{E}$, attempts to combine ontologies of situation and event calculus. It is done in a manner substantially different from that in $\mathcal{E}$ and so a reference to this paper may be appropriate.

A6. **The Authors** (3.5)

Yes, this paper is clearly related to the themes of both the present article and our previous paper on the Language $\mathcal{E}$ (in the same special issue of the JLP as Baral et al.). We've referenced it in the revised version of our paper now available via the ETAI web pages, and had discussed the relationship between these two approaches in some detail in our JLP paper. (See also Question 4 from Michael Gelfond on our ETAI interactions page, and our reply.)

Q7. **Anonymous Reviewer 3** (23.4)

Your paper makes the following contributions:

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

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

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

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

You do the last (comparing your approach to other approaches) briefly, in the beginning of section 5, but this treatment is too cursory and raises almost more questions than it answers. For example, in comparing your approach to those of Thielscher, McCain and Turner, and Lin, you aruge that their approach is essentially a causal-based approach, because the effect of action occurrences cannot be propagated backward through r(amification)-propositions. To this reviewer, this fact hardly seems to be the characteristic fact of causal theories. A deeper analysis of what makes a causal theory, whether sets of axioms in E can be considered causal theories, and how causal approaches can be used to solve the ramification problem, would be helpful here.

Also very desirable would be a discussion of how solutions to the ramification problem interact with solutions to the frame problem. In particular, there is often a duality between the two problems, in that the frame problem is often seen as a mainly representational problem, whose solutions may worsen things from the computational point of view, and the ramification problem is often seen as mainly a computational problem, whose solutions may worsen things from the representational point of view. How do your two solutions interact? A discussion would be useful.

A7. **The Authors** (3.5)

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

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

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

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

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

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

Q8. **Anonymous Reviewer 3** (23.4)

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

- On p. 6, you state:

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

  It would be useful to see that model explicitly.

- On p. 8, you state:

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

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

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

A8. **The Authors** (3.5)

The domain description on page 6 was:

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

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

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

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

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

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

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

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

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

The reviewer also wrote:

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

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

Q9. **Anonymous Reviewer 3** (23.4)

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

A9. **The Authors** (3.5)

You summarised the contributions very well at the top of your report:

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

Q10. **Anonymous Reviewer 3** (23.4)

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

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

A10. **The Authors** (3.5)

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

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

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

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

Q11. **Anonymous Reviewer 3** (23.4)

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

A11. **The Authors** (3.5)

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

Electronic News Journal on Reasoning about Actions and Change 188
Vol. 2: *188-189*, 31.5.1998
http://www.ida.liu.se/ext/etai/received/actions/007/

Iliano Cervesato, Massimo Franceschet, and Angelo Montanari:

The Complexity of Model Checking in Modal Event Calculi with Quantifiers

Summary of the article

The original version of the full article was accepted for the 1998 International Conference on Knowledge Representation and Reasoning (KR-98). It is available in the conference proceedings, published by Morgan-Kaufmann Publishers, Inc., and at the authors' website at
`http://www.dimi.uniud.it/montana/kr98.ps`

The Event Calculus

The *Event Calculus*, abbreviated *EC*, is a simple temporal formalism designed to model and reason about scenarios characterized by a set of *events*, whose occurrences have the effect of starting or terminating the validity of determined properties. Given a *possibly incomplete* description of when these events take place and of the properties they affect, *EC* is able to determine the *maximal validity intervals*, or *MVIs*, over which a property holds uninterruptedly.

State-of-the-art

A systematic analysis of *EC* has recently been undertaken in order to gain a better understanding of this calculus and determine ways of augmenting its expressive power. The keystone of this endeavor has been the definition of an extendible formal specification of the functionalities of this formalism. This has had the effects of establishing a semantic reference against which to verify the correctness of implementations, of casting *EC* as a model checking problem, and of setting the ground for studying the complexity of this problem, which was proved polynomial. Extensions of this model have been designed to accommodate constructs intended to enhance the expressiveness of *EC*. In particular, modal versions of *EC*, the interaction between modalities and connectives, and preconditions have all been investigated in this context.

Contributions

The main contributions of the present paper are the following:

- *The extension of a family of modal event calculi with quantifiers.*

  We enhance the expressive power of *EC* by considering the possibility of quantifying over events in queries, in conjunction with boolean connectives and modal operators. We also admit requests to check the relative order of two events. We thoroughly analyze the representational and computational features of the resulting formalism, that we call *QCMEC*. We also consider two proper sublanguages of it, *EQCMEC*, in which modalities are applied to atomic formulas only, and *CMEC*, which is quantifier-free.

- *The use of the proposed formalisms to encode diagnosis problems.*

  We consider a case study taken from the domain of hardware fault diagnosis. We model it using *EC* and use the various extensions of *EC* to draw conclusions about it.

- *The use of the higher-order features of modern logic programming languages in temporal reasoning.*

  We provide an elegant implementation in the higher-order logic programming language λ*Prolog* and prove its soundness and completeness.

- *Analyzing the complexity of model checking in the proposed extensions of* EC.

  We prove that model checking in *CMEC*, *EQCMEC*, and *QCMEC* is **PSPACE**-complete. However, while solving an *EQCMEC* problem is exponential in the size of the query, it has only polynomial cost in the number of events, thus making *EQCMEC* a viable formalism for *MVI* verification or computation. Since in most realistic applications the size of databases dominates by several orders of magnitude the size of the query, the former is asymptotically the parameter of interest.

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

Iliano Cervesato, Massimo Franceschet, and Angelo Montanari

The Complexity of Model Checking in Modal Event Calculi with Quantifiers

Q1. **Paolo Liberatore** (17.5)

Questions 1 and 2 are comments on the presentation; questions 3 and 4 are requests for clarification.

Page 2, col 2, second-last para: "We represent an MVI for p as $p(e_i, e_j)$ ...". I found this sentence a little misleading. Indeed, the notation $p(e_i, e_j)$ refers to *any* 3-tuple $\langle$*property*, *event*, *event*$\rangle$. The aim of queries is to decide whether such a 3-tuple is a MVI. From the sentence written in the paper, it seems that $p(e_i, e_j)$ is used to *state* that e_i, e_j is an MVI for p. Perhaps this sentence should be rewritten in some way.

A1. **Angelo Montanari** (24.5)

In the following, we quote a bigger paper excerpt, including the sentence: "We represent an MVI for p as $p(e_i, e_j)$ ...". We slightly changed the notation to make it easier to read.

> Given an EC-structure H = (E, P, [.>, <.],].,.[) and a knowledge state w, the Event Calculus (EC for short) permits inferring the maximal validity intervals (MVIs) over which a property p holds uninterruptedly. We represent an MVI for p as $p(e_i, e_t)$, where e_i and e_t are the events that respectively initiate and terminate the interval over which p holds maximally. Consequently, we adopt as the query language of EC the set
>
> $$\{p(e_1, e_2) \mid p \text{ belongs to } P \wedge e_1, e_2 \text{ belong to } E\}$$
>
> of all such property-labeled intervals over H. We interpret the elements of the language as propositional letters and the task performed by EC reduces to deciding which of these formulas are MVIs in the current knowledge state w and which are not.

We believe that reading the sentence in its context should allow one to conclude that every MVI is 3-tuple $\langle property, event, event\rangle$, but not every 3-tuple $\langle property, event, event\rangle$ is an MVI.

Q2. **Paolo Liberatore** (17.5)

The definition of MVI states that between e_1 and e_2 there is no event that initiates p. I would like you to spend a few words for explaining why.

A2. **Angelo Montanari** (24.5)

We discussed this point in some detail in previous publications (cf. reference [4] as well as in [j-ci-12-359]. Your comment indicates that it is probably useful to add a brief explanation immediately after Definition 2.2.

In the following, we report part of the discussion provided in [4].

Definition 2.2 states that an event e interrupts the validity of a property

if it initiates or terminates p itself or a property q which is incompatible with p. This rule adopts the so-called strong interpretation of the initiate and terminate relations: a given property p ceases to hold over a time interval if an event e which initiates or terminates p, or a property incompatible with p, occurs during this interval. The strong interpretation is needed when dealing with incomplete sequences of events or, as in our case, incomplete information about their ordering. For example, consider a switch that can take two different positions: on and off. Its behavior can be described by means of two types of event: one that changes the position from off to on (turn-on), the other from on to off (turn-off). While two turn-on events cannot occur consecutively in the real world, it may happen that an incomplete sequence consisting of two consecutive turn-on events, followed by a turn-off event, is recorded in the database. The strong interpretation of the initiate relation allows EC to recognize that a missing turn-off event must have occurred between the two turn-on events. However, since it is not possible to temporally locate such an event, EC only concludes that the switch is on between the second turn-on event and the turn-off event, and it considers the first turn-on event as a pending initiating event. It is worth mentioning that an alternative interpretation of the initiate and terminate relations, called weak interpretation, is also possible. According to such an interpretation, a property p is initiated by an initiating event unless it has been already initiated and not yet terminated (and dually for terminating events). The weak interpretation is needed to aggregate homogeneous states.

Further details about the strong/weak distinction, including formalization and other examples, can be found in [4].

References:

[j-ci-12-359] L. Chittaro and A. Montanari. *Efficient Temporal Reasoning in the Cached Event Calculus.* Computational Intelligence, vol. 12, pp. 359-382.

Q3. **Paolo Liberatore** (17.5)

Page 9, col 1, para 3: the bound $\mathcal{O}(n^2)$ for reachability is due to the fact that you consider the number of events as the size of the input. Indeed, reachability in a graph is linear in the total size of the graph, that is, is $\mathcal{O}(n+m)$, where n is the number of vertices and m is number of edges. This implies for example that the cost of EC is $\mathcal{O}(s^2)$ if s is the total size of the input and not only the number of events. Is there any reason for choosing the number of events to represent the size of H?

A3. **Angelo Montanari** (24.5)

An instance of the model checking problem we analyzed in the paper is a triple (H, w, φ), where H is an EC-structure, w is a (strict) partial order on the events in E, and φ is a well-formed formula.

Given an EC-structure H, E is the set of events (we can assume that they are instances of a finite set of domain-specific event types), which can be arbitrarily large, while P is the set of relevant properties, which are fixed once and for all and characterize the considered domain. For instance, in the light example, we may have two events e_1 and e_2, both of type *turn-on*, which initiate the property *on*. Since the cardinality of P does not change from one problem instance to another one (unless we change the application domain), while the cardinality of E may grow arbitrarily, we chose the cardinality of E, that is, the number n of events, as the size of H (it is usually called data complexity).

The cardinality of w, here denoted by m, is bounded by n (more precisely, it is less than or equal to $n(n-1)/2$). We did not choose m as a complexity parameter, because we are interested in explicitly characterizing the amount of available ordering information with respect to the given number of events. As an example, we are interested in distinguishing between situations in which $m = O(n)$ (sparse graphs) from situations in which $m = O(n^2)$ (dense graphs).

Finally, the size k of φ in not bounded by n, and it can arbitrarily grow, independently from n (clearly, not in the case of basic EC). Hence, we took k as another relevant complexity parameter (which is usually called query complexity).

Q4. **Paolo Liberatore** (17.5)

Page 10, col 2, comment after Theorem 5.5: if k is just a bit high (for instance, 4), the cost of solving the problem becomes something in the order of $\mathcal{O}(n^7)$, which is usually considered not very useful. In the conclusions, you mention that the cost of the implemented version of QCMEC is very close to the theoretical value. Do you have empirical evidence that the efficiency of EQCMEC is also similar to the upper bound you proved? If so, the problem should be considered not actually feasible.

A4. **Angelo Montanari** (24.5)

This last question is concerned with the following sentence, at the end of the complexity section:

> The directness of the implementation allows checking easily that the complexity of the implemented algorithms coincide with the bounds proved in this section for the problems they implement."

Well, you are right, this sentence is probably a little misleading. Its intended meaning is that, given the directness of the proposed implementation, we expect that its worst-case complexity coincides with the proved bounds. We can possibly reformulate the above sentence as follows:

"The directness of the implementation allows checking easily that the complexity of the implemented algorithms coincide, in the worst case, with the bounds proved in this section for the problems they implement."

However, we are convinced that the actual costs of the implemented versions of EC, ECMEC and EQCMEC are NOT close to the theoretical upper bounds, even though we have not empirical evidence of this fact.

Take, for example, the case of EQCMEC. Here, the critical factor is the level of nesting of quantifiers. The worst case holds when we have an EQCMEC-formula involving q nested quantifiers at its top-level. Let $Q.F$ be such a formula, where Q is the quantification part, and let d be the dimension of F, q the number of quantifiers in Q, and n the number of events. Exploiting the unfolding strategy, we are led to check a formula of size $d \cdot n^{\{q\}}$. Since both d and q are less than or equal to the size k of $Q.F$, we get the upper bound $k \cdot n^{\{k\}}$ for the size of the unfolded formula (without loss of generality, we can interpret the size k of $Q.F$ as the number of propositional connectives, quantifiers and modal operators occurring in $Q.F$ plus one).

Furthermore, as mentioned in the paper, practical applications using modal event calculi with quantifiers are expected to model situations involving a large number of events, while the size of the queries, and in particular the nesting of quantifiers, will in general be limited. The hardware fault diagnosis example falls into this category (the nesting level of quantifiers is 2).

Finally, it is possible to show that the upper bound of $O(n^3)$ for EC can actually be lowered of one degree by exploiting the notions of transitive reduction and closure of an ordering relations. Preliminary results in this direction can be found in [2].

From the above elements, it follows that the hardware fault diagnosis problem can actually be solved in $O(n^4)$.