PUBLICATION METHODS

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.

ENJ's are primarily published as WWW pages in HTML encoding, since they are intended for on-line use. In particular, they contain 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. However, they also contain parts that can be read without clicking the hot links, for example, the debate contributions.

The present version of the News Journal is a derivative, formatted representation and is intended to be printed out on paper and read off-line. Due to the limitations of the paper medium, only some of the WWW links have been retained as footnotes. There are also some other differences of minor importance between the HTML version and the present one. − In order to make practical use of the WWW links, as well as to see and use other links in the structure, please retrieve the on-line ENJ from the following URL:

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

which contains a table of back issues of ENJ's and Newsletters on Reasoning about Actions and Change.

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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, HTML version during the month of September, 1997. Then the contents were edited and formatted, resulting in the present, formatted version which was published on February 12, 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.

September Issue Selected News

Erik Sandewall

Linköping University, Sweden

New initiatives

ETAI Newsletter complements News Journal

[30.9.1997] Starting with this issue, the monthly News Journal is by and large a compilation of the contents of the Newsletters which have been sent out successively during the past month. The News Journal is published for the record, and for the benefit of those readers who prefer to get all the month's news at the same time. For those who have already followed the Newsletter during the past month, there are no more news here.

Details of the editing policy for Newsletters are at the end of this journal issue.

NRAC panel discussions continued

[21.9.1997] The NRAC workshop (Non-monotonic Reasoning, Actions and Change) at the recent IJCAI featured three panel discussions: on causality, methodology, and ontologies. (The methodology panel dealt with theory evaluation, or "what makes a theory good?"). We are presently trying to arrange for these panel discussions to continue on-line, just like the discussion with Wolfgang Bibel. More about this will appear in forthcoming Newsletter issues.

Recent developments

Complexity comes to action and change

[21.9.1997] Computational complexity has not been much studied in the research on Reasoning about Actions and Change, although of course there has been considerable work on complexity in the neighboring topics of planning and in temporal reasoning (in the sense of

only reasoning about points on the time line, but not allowing for change of state and the frame problem(s)). Recently, however, there have been two contributions on complexity in our field, namely:

- Thomas Drakengren and Marcus Bjäreland
  Reasoning about Action in Polynomial Time
  Proc. IJCAI 1997, pp 1447-1452, also available at
  ftp://ftp.ida.liu.se/pub/labs/tosca/people/thodr/ijcai97rac.ps.gz

- Paolo Liberatore
  The Complexity of the Language A
  Linköping University Electronic Press, 4.7.1997,
  available at http://www.ep.liu.se/ea/cis/1997/006/

The latter article has been received by the ETAI, and is presently in its discussion period.

Debate

Discussion with Wolfgang Bibel about his invited lecture

In the first example of an on-line discussion, Wolfgang Bibel has answered questions about his IJCAI invited paper during the past month. This discussion is reported as a separate article later on in this issue of the News Journal.

Are applications moving faster than theory?

[26.9.1997] The discussion about Austin Tate's article, which has been received by ETAI, has raised a question about how our field relates to (or ignores?) contributions that are made in the framework of broad application areas. The two debate contributions follow here, the latter one slightly edited:

From: **Austin Tate** on 26.9.1997.

The ETAI Colloqium on Actions and Change (see: general debate) is raising issues from a formal representation of action perspective which could usefully be linked with the more practically derived representation that <I-N-OVA> represents. Murray Shanahan's message raises a number of requirements for an action formalism that could usefully be checked against any proposed action, plan or process representation. He also suggests the use of practical scenarios as a way to validate any proposal.

In this context it may be worth noting that <I-N-OVA> is based on 20 year's experience of the use of plan representations for a wide range of domains in AI planners. It also seeks to bring in work

from a very wide range of process and activity modelling communities beyond AI.

Analysis of about 20 candidate activity representations against an extensive set of requirements and against a set of engineering, manufacturing and workflow scenarios is being undertaken in recent work in the National Institute of Standard's and Technology (NIST) on the Process Specification Language which is seeking to create a meta-model for activities that has a formal semantics (see http://www.nist.gov/psl/). The OMWG Core Plan Representation work (now at RFC version 2) is also being validated against a range of military planning problems.

<I-N-OVA> has being used as a conceptual framework to input to both these programmes.

From: **Erik Sandewall** on 26.9.1997.

Austin, I think you are bringing up a very important point when you mention "process and activity modelling communities beyond AI" in the discussion (your comment C1). Besides the work in engineering and manufacturing, there is active work in the healthcare area, where they have an interest in characterizing the medical history of a patient as a process, involving both health events ("raise in temperature", "severe back pain" and medication and other treatment events. The work has progressed so far that there is reportedly a European prestandard, ENV 12831, called "Medical Informatics - Time Standards for Healthcare Problems".

In addition, there is of course the work in the research communities for databases and information systems, where they want to model processes within an enterprise. The recent conference on "Active, Real-Time, and Temporal Database Systems" is one example of research in that area. (See the Actions & Change conference calendar for a link to that conference).

It seems to me that the AI field is not sufficiently aware of these developments. The world doesn't stand still while we try to figure out the best way of dealing with the ramification problem. The present newsletter will be a good forum for exchanging pointers and points of view with regard to contributions from applied areas.

Other publications

New Books

[21.9.1997] "Spatial and Temporal Reasoning" is a collection volume edited by Oliviero Stock which has just been published by Kluwer. The volume grew out of two Advanced Schools held in Bolzano, Italy, which were directed by Oliviero. The book contains contributions by the following authors: Laure Vieu, Alfonso Gerevini, Roberto

Casati and Achille C. Varzi, Anthony G. Cohn et al., Andrew U.
Frank, Annette Herskovits, James F. Allen and George Ferguson,
Drew McDermott, Erik Sandewall, and Anthony Galton.

Research Articles

From: **Judea Pearl** on 22.9.1997.

The following recent papers and technical reports on actions and
causality are available on the UCLA web site,

http://bayes.cs.ucla.edu/jp_home.html

- David Galles and Judea Pearl, An Axiomatic Characterization
  of Causal Counterfactuals. To appear in Foundations of Sci-
  ence, Kluwer Academic Publishers. (1997)

  Key idea: We study the causal interpretation of counterfac-
  tual sentences using modifiable structural equation models and
  compare this interpretation to Lewis' closest-world semantics.

- Galles, D. & Pearl, J., "Axioms of Causal Relevance," To ap-
  pear in Artificial Intelligence (1997).

  Abstract: This paper develops axioms and formal semantics
  for statements of the form "X is causally irrelevant to Y in
  context Z," which we interpret to mean "Changing X will not
  affect Y if we hold Z constant." The axiomization of causal ir-
  relevance is contrasted with the axiomization of informational
  irrelevance, as in "Learning X will not alter our belief in Y,
  once we know Z." Two versions of causal irrelevance are an-
  alyzed, probabilistic and deterministic. We show that, unless
  stability is assumed, the probabilistic definition yields a very
  loose structure, that is governed by just two trivial axioms.
  Under the stability assumption, probabilistic causal irrelevance
  is isomorphic to path interception in cyclic graphs. Under the
  deterministic definition, causal irrelevance complies with all of
  the axioms of path interception in cyclic graphs, with the ex-
  ception of transitivity. We compare our formalism to that of
  [lewis:74b], and offer a graphical method of proving theorems
  about causal relevance.

- Pearl, J., "Graphs, Structural Models and Causality". To ap-
  pear in C. Glymour (Ed.) "Causation, Computation and Dis-
  covery", AAAI/MIT Press (1997).

  Key idea: How graphical models can be used as a mathematical
  language for integrating statistical data and causal knowledge.

- Pearl, J., "Structural and Probabilistic Causality." In D.R.
  Shanks, K.J. Holyoak, and D.L. Medin (Eds.), The Psychol-
  ogy of Learning and Motivation, Vol. 34 Academic Press, San
  Diego, CA, 393–435, 1996.

Key ideas: How difficulties in probabilistic causality are resolved using graphical models.

- Pearl, J., "On The Foundation Of Structural Equation Models, or, When Can We Give Causal Interpretation To Structural Coefficients?" (Part of a commentary prepared for Multivariate Behavioral Research).

- Pearl, J., "Bayesian Networks." To appear in MIT Encyclopedia of the Cognitive Sciences (1997)

- Pearl, J., "Causation, Action, and Counterfactuals." In Yoav Shoham (Ed.), Theoretical Aspects of Rationality and Knowledge, Proceedings of the Sixth Conference (TARK 1996), The Netherlands, 51–73, March 17-20, 1996.

  Key words: Action as a Local Surgery, Laws vs. facts, Causal ordering, Imaging vs. conditioning, Causal theories and actions, causal effects and identifiability, A calculus of acting and seeing, Processing Counterfactuals.

Calendar

Forthcoming conferences and workshops

[21.9.1997] Following the installation of new software and a database for conference announcements, the Colloquium for Actions and Change now has its own web page for forthcoming and recent conferences and workshops. Previously, we only had a link to a similar page elsewhere. The new arrangements offers more complete coverage and better integration with our bibliography. [The current contents of the conference menue were included in the original, on-line newsletter].

The conference menue is a part of the colloquium at
http://www.ida.liu.se/ext/etai/actions/colloq/frame.html

ECP-97: European Conference on Planning.
Toulouse, France, 24.9-26.9, 1997.
INFO: http://cswww.essex.ac.uk/conferences/ecp-97.htm

FCR-98: Formalization of Commonsense Reasoning.
London, U.K., 7.1-9.1, 1998. Papers due: 10.10 1997.
CFP: http://www.dcs.qmw.ac.uk/~rsm/CS98/CS98cfp.html
INFO: http://www.dcs.qmw.ac.uk/~rsm/CS98/index.html

TIME-98: International Workshop on Temporal Representation and Reasoning.
Sanibel Island, Florida, USA, 15.5-16.5, 1998. Papers due: 8.12 1997.

CFP: http://www.cs.fit.edu/~lina/time/cfp.txt
INFO: http://www.cs.fit.edu/~lina/time/time98.html
MSG: http://www.ida.liu.se/ext/brs/confi/cfp/TIME-98.txt

KR-98: International Conference on Knowledge Representation and Reasoning.

Trento, Italy, 2.6-5.6, 1998. Papers due: 1.12 1997.
CFP: http://www.kr.org/kr/kr98/cfp.html
INFO: http://www.kr.org/kr/kr98/
MSG: http://www.ida.liu.se/ext/brs/confi/cfp/KR-98.txt

FOIS-98: Formal Ontology in Information Systems.

Trento, Italy, 6.6-8.6, 1998. Papers due: 15.12 1997.
INFO: http://mnemosyne.itc.it:1024/fois98/
MSG: http://www.ida.liu.se/ext/brs/confi/cfp/FOIS-98.txt

AIPS-98: Artificial Intelligence Planning Systems.

Carnegie-Mellon University, Pittsburgh, PA, USA, 7.6-10.6, 1998. Papers due: 9.12 1997.
INFO: http://www.cs.cmu.edu/ aips98
MSG: http://www.ida.liu.se/ext/brs/confi/cfp/AIPS-98.txt

ECAI-98: European Conference on Artificial Intelligence.

Brighton, UK, 23.8-28.8, 1998. Papers due: 23.1 1998.
CFP: http://www.cogs.susx.ac.uk/ecai98/call.html
INFO: http://www.cogs.susx.ac.uk/ecai98
MSG: http://www.ida.liu.se/ext/brs/confi/cfp/ECAI-98.txt

General

Newsletter policy

[21.9.1997] During the past month, the **Newsletter** for reasoning about actions and change started as a direct-mail complement of the ETAI News Journal, and in response to numerous requests this kind of service within ETAI. (The News Journal is what you are reading just now). Many readers feel that it is easy to forget about places that are available for browsing, and that it is more convenient to receive news as E-mail messages and with as little delay as possible.

ETAI Newsletters will meet this demand in the following fashion. They are issued as often as news items come in, but not more frequently than once a day, and are distributed to subscribers by direct E-mail. At the same time, each Newsletter issue will be posted on

a [1][back issue page] for newsletters. Therefore, the Newsletter mails are disposable: you can safely discard them after you have glanced through them, since if you need to look back at a particular issue, can retrieve them from the back issue page.

Contributions to the Newsletter are welcomed for all news of specific relevance to research on Reasoning about Actions and Change. The following are examples of relevant contributions:

- **Research articles** (including departmental technical reports) which have appeared recently, provided that the article or report is available over the net.

- **Announcements of conferences and workshops**

- **Contributions to discussions**

A number of discussions are being organized within the area, and all messages within those discussions will be included in the Newsletter. In this way, the Newsletter also serves as a moderated newsgroup.

The Newsletters will offer a more rapid alternative to the [2][News Journal], but for those subscribers who only want larger chunks of news in longer intervals, all news that appear in Newsletters will also be included in the next following issue of the News Journal. Similarly, those news that fit into existing categories of the [3][Colloquium for Action and Change] will be accumulated to there.

Initially, all current subscribers in the area will receive Newsletter mailings, as well as notification about new issues of the News Journal. All requests to be added to, or deleted from the mailing lists should be sent to the present editor.

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[1]Ref. http://www.ida.liu.se/ext/etai/actions/njl/
[2]Ref. http://www.ida.liu.se/ext/etai/actions/nj/noframelist.html
[3]Ref. http://www.ida.liu.se/ext/etai/actions/colloq.html

Summary of on-line discussion during September, 1997 about the following research article:

Wolfgang Bibel:

Let's Plan It Deductively!

The recent IJCAI conference featured an invited talk by Wolfgang Bibel, titled "Let's plan it deductively!" The talked addressed issues within Reasoning about Actions and Change as well as within planning, and is of considerable relevance for our area. Since the format of a conference lecture with a large audience does not allow much discussion, we have invited Wolfgang to participate in an on-line discussion about his article in the present medium, and he has agreed heartily. Questions to Wolfgang can be sent to the Newsletter editor, and will be forwarded to him for answering. Questions and answers will be distributed through Newsletter issues, and will also be accumulated in discussion sessions in the Colloquium. It is intended that this discussion will have the flavor of a conference question period, so it is perfectly fine if the contributions develop into a broader discussion. The following formatting conventions are used. Roman style is used for the statement, question, or answer by the person indicated in the headline just above. *Italic style* is used to indicate sections of text which are customarily indented and marked by > at the beginning of each line in E-mail discussions, that is, quotations from a preceding statement and which the present author is responding to. The **date** at the top of each contribution indicates the date of the Newsletter where that contribution was first presented. This is not necessarily the date when the contribution reached the Newsletter editor; in some cases there has been a delay of one or a few days.

[30.9.1997] Wolfgang Bibel's article is now reported to be available on-line on his web page,

http://aida.intellektik.informatik.th-darmstadt.de/b̃ibel/#publications

From: **Marc Friedman** on 22.9.1997.

Dr. Bibel,

I am intrigued by your passion for posing planning as a deduction problem. Since your lecture at IJCAI 97 in Japan, I have been trying to learn all I can about it. But I have some concerns which I think will

have to be addressed for your work to be fully appreciated by those of us who are familiar with planning but not really with deduction.

In the process of questioning your work, I will probably do violence to it. I apologize in advance. Please correct me.

0. Linear transition proofs solve the classical planning problem. This is true. Linear backward chainer (LBC), and LIF and LIF+ (Fronhoefer's more recent solutions) are correct. LBC is very clean, too.

1. I think you suggest that deduction has a priveleged place as a basis for classical planning. But planning has other theoretical foundations: the modal truth criterion, and the theory of refinement search. What makes deduction superior to these other bases?

2. You present LBC as an encoding of transition logic (TL) into logic, in particular the languuage of the SETHEO theorem prover. If this were true, AND the implementation compared well with other classical planners, this would be a major step – giving at once a formal AND operational reduction of the problem to deduction!

However, if we look closely at LBC, there is a work-around to make SETHEO into a transition logic engine. TL is not in fact translated to first order logic. Instead, the available propositions are tracked, to prevent two connections from sharing a single proposition. This approach is not truly a reduction. It is an encoding, much like a program that implements a formally sound algorithm, like UCPOP or graphplan, in a formally sound substrate, like PROLOG, or a functional programming language, or as a satisfiable formula. TL loses its priveleged position. Thus it must compete with other approaches on their terms: is it faster or easier to understand, does it do less search, etc.

3. LBC beats UCPOP. But many algorithms have. How does it compare with these?

4. Transition logic solves the frame problem. So does TWEAK. Transiion logic solves the ramification problem. So does UCPOP, via a theorem-proving subroutine. Perhaps TL's ramification solution is a more uniform mechanism, but it is not truly uniform – the linearity restriction is removed. Why prefer one solution to the other?

I think some of the answers are beginning to come to light, and I eagerly await hearing your answers.

From: **Wolfgang Bibel** on 26.9.1997.

Dear Mark Friedman,

Thank you very much for taking your time and listening to my lecture at IJCAI-97, and even giving it further thoughts afterwards.

> *0. Linear transition proofs solve the classical planning problem. This is true. Linear backward chainer (LBC), and LIF and LIF+ (Fronhoefer's more recent solutions) are correct. LBC is very clean, too.*

Thanks for these kind remarks.

> *1. I think you suggest that deduction has a priveleged place as a basis for classical planning. But planning has other theoretical foundataions: the modal truth criterion, and the theory of refinement search. What makes deduction superior to these other bases?*

Let me take this question to start with reminding you of the general gist of my lecture. My lecture ended by generalizing the title to "Let's plan AI deductively!". As we all know the AI endeavor is a very complex one. This complexity led many of us to specialize in small niches such as planning, nonmonotonic reasoning, scheduling, theorem proving, vision, speach, NL, ... you name one of the hundreds of others. In each of them smart (functional - in a broad sense of the word) solutions are developed in a great variety of different, mostly incompatible languages. I do not see how all this could ever converge towards something coherent which deserves the label "artificial intelligence", our common goal. I am not the first to recognize a deplorable splintering of our field. An artificially intelligent agent will have to feature those (and more) smart solutions all at the same time. I do not see how the functional approach could ever achieve this if it does not even get us to the point of hooking a new machine easily to a local network (a problem I faced recently coming here to UBC with my Voyager).

I am therefore one of those who strongly believe that only through a rather uniform approach to any of these different facets can we ever hope to accomplish systems that are able to do more than "just" playing chess on a worldmaster level (but nothing else) or prove open problems like Robbins' one as done recently (but again nothing else) etc. It is an illusion to think we could just combine all these niche systems to get out something like a general intelligence. Rather the entire approach must be a more universal one from the very beginning. And if so only through a uniform approach could the enormous complexity be overcome.

If you buy these arguments the next question will be "what uniform approach". There are not that many available. In fact I believe that the logical approach triggered by John McCarthy has no real competitor satisfying all the requirements coming up for such a universal task. Of course this is a rather vague statement since it leaves open what we in detail mean by "logical approach". For the time being many believe that core first-order logic would be part of it, but that there might be variations of it not yet found (like the transitions in TL, second-order predicates etc). Another concern is the lack of efficiency of existing deductive methods a point I come back to shortly.

After these, as I think, important remarks in view of what we are up to I come now back to the details of your question. The modal truth criterion or the theory of refinement search could actually be seen as logical theories. But they are very specialized theories customized to planning and nothing else than planning. While I could well imagine a logical planning environment where they ALSO might play some role (as meta-knowledge) by itself they are by far not sufficient to allow even the simplest extension of the planning task to include for instance a bit of classical reasoning of the planning agent. By the way deduction provides a generic problem solving mechanism in a uniform and rather universal logical language and as such plays a different role in comparison with the examples you mention. For instance the theory of refinement search applies to general deductive search as it does to specific search in the space of partial plans.

> *2. You present LBC as an encoding of transition logic (TL) into logic, in particular the languuage of the SETHEO theorem prover. If this were true, AND the implementation compared well with other classical planners, this would be a major step – giving at once a formal AND operational reduction of the problem to deduction!*

> *However, if we look closely at LBC, there is a work-around to make SETHEO into a transition logic engine. TL is not in fact translated to first order logic. Instead, the available propositions are tracked, to prevent two connections from sharing a single proposition. This approach is not truly a reduction. It is an encoding, much like a program that implements a formally sound algorithm, like UCPOP or graphplan, in a formally sound substrate, like PROLOG, or a functional programming language, or as a satisfiable formula. TL loses its priveleged position. Thus it must compete with other approaches on their terms: is it faster or easier to understand, does it do less search, etc.*

I have to start again with a general remark. We (ie my group in Darmstadt, my former group in Munich now represented by people like Fronh"ofer, Letz, Schumann and others, and the entire deduction community for that matter) see our task to provide the best possible generic problem solving mechanism for this universal language (mostly fol). And this challenge keeps all of us busy enough. As an aside I might mention that we have been quite successful in it. For instance in 1996 SETHEO won the world competition among all existing theorem provers. Why then should we above all these efforts do more than offering other specialists (such as those working in planning - but there are many more potential applications) our tools for

use in their special field of application? So the little experiment that Fronh"ofer did with SETHEO was indeed only a side-effort done in a few days. It is true that beyond SETHEO we need a TL-SETHEO, ie a theorem prover customized to the logic TL. We did such extensions already for other logics, especially for intuitionistic logic which is relevant for program synthesis (another of our interests) and will, if circumstances permit, eventually do the same for TL. But given that there is so much to be done anyway no promises are given at this point.

As to the privileged position of TL I just point to what I said before: it is the universal logic (ie language and calculus) which gives it the privileged position in comparison with UCPOP (a special algorithm with a narrow range of applications) or graphplan (a coding in propositional logic which does not provide rich enough a logical language to serve the more general purposes - by the way graphplan is a deductive solution to planning as well! but as recent experiments by a Swedish/German group seem to demonstrate rule-based encoding seems to be a more suitable encoding).

> *3. LBC beats UCPOP. But many algorithms have. How does it compare with these?*

Sure. But if I take a general tool from the shelf (such as SETHEO), spend a few hours or days to customize it to a general task like planning and beat with the result a system that was specially developed for the task of planning and only a few years ago was deemed the best of its kind then this is a very strong experimental hint that the deduction technology subsumes that needed for planning and that the efficiency problem already mentioned above is less severe than many might think. Therefore, to my strongest conviction, Dan Weld and others would have contributed more to the advancement of AI had they built those special systems ON TOP of the mature technology reached in deduction at the time of implementation rather than as an independent sideline (which does not at all diminish their remarkable achievements seen in themselves).

> *4. Transition logic solves the frame problem. So does TWEAK.*

TWEAK is based on STRIPS (sort of) and - as I mentioned explicitly in my lecture - STRIPS is very closely related to TL as far as transitions are concerned. But STRIPS (and TWEAK) is not a logic so lacks the generality needed for the purposes outline above.

> *Transition logic solves the ramification problem. So does UCPOP, via a theorem-proving subroutine. Perhaps TL's ramification solution is a more uniform mechanism, but it*

> *is not truly uniform – the linearity restriction is removed.*
> *Why prefer one solution to the other?*

To the best of my knowledge Michael Thielscher (to whose work I referred in my lecture in this context) was the first who gave a solution to the ramification problem which overcomes deficiencies of any previous solution (including UCPOP's one). The lecture as well as the paper (and further references therein) point out these examples where no previous solution would model reality in a correct way. A better solution in this sense must be preferred to a deficient one. In addition there is the uniformity and universality provided by the logic as pointed out now already several times. I do not understand what you mean by the phrase "the linearity restriction is removed".
Wolfgang Bibel

From: **Marc Friedman** on 26.9.1997.

> *...by the logic as pointed out now already several times.*
> *I do not understand what you mean by the phrase "the*
> *linearity restriction is removed".*

Oh. Maybe I said it wrong. I meant that if there are synchronic rules, and transition rules, represented in your talk by two different kinds of implication arrows, then there are two different mechanisms – one which limits each proposition to use in a single connection, and one which does not.
Thanks, Marc

Answer from: **Wolfgang Bibel** on 29.9.1997.

> *Oh. Maybe I said it wrong. I meant that if there are syn-*
> *chronic rules, and transition rules, represented in your*
> *talk by two different kinds of implication arrows, then*
> *there are two different mechanisms – one which limits*
> *each proposition to use in a single connection, and one*
> *which does not.*

But then - what should be the problem with this technical change in the syntactic characterization of valid formulas which reflects our semantic use of (two different) implications?

From: **Patrick Hayes** on 29.9.1997.

I'm surprised to read that any theorem-prover has solved the frame problem, since the FP is a problem in representation, not in theorem-proving, and has nothing particularly to do with how deductions are processed. It is also rather surprising to read that some specialised logic has solved the FP, since to do so its semantics would have to embody all known and future causal laws. Could someone

briefly explain how a better deductive search engine, or an exotic logic, can solve a problem in representation?

On what might be a related matter, Bibel claims that ' deduction provides a generic problem solving mechanism ' (response to Friedman, ETAI Newsletter on Actions and Change, 26.9.1997). Taken literally, this is clearly false, since deduction itself provides no mechanism whatever: one only gets a mechanism when one chooses a strategy for performing deductions. For example, unification is not imposed by deduction; other strategies for instantiating universal variables are possible, computationally ridiculous but deductively perfectly valid. So Bibel must be understood as referring not to 'deduction' per se, but to a particular deductive strategy, or class of deductive strategies. Perhaps in his original lecture (which I havn't yet got access to) he tells us which ones they are, but a brief summary would be helpful.

Answer from: **Wolfgang Bibel** on 30.9.1997.

> *I'm surprised to read that any theorem-prover has solved the frame problem,*

Where did you read this phrase? If I said so (which I hope I did not) then I apologize for it. You are clearly right with:

> *since the FP is a problem in representation, not in theorem-proving, and has nothing particularly to do with how deductions are processed.*

> *It is also rather surprising to read that some specialised logic has solved the FP, since to do so its semantics would have to embody all known and future causal laws.*

How do you mean this sentence? Does, in analogy, the semantics of first-order logic "have to embody all known and future" declarative knowledge?

> *Could someone briefly explain how a better deductive search engine, or an exotic logic, can solve a problem in representation?*

Why don't you read the respective parts of the paper. Again, at issue is a logic not a search engine (although my paper does also connect the logic with fol for which there is a great variety of search engines available). The logic is equivalent with linear logic which you may have heard of before.

> *On what might be a related matter, Bibel claims that ' deduction provides a generic problem solving mechanism ' (response to Friedman, ETAI 26.9.1997). Taken literally, this is clearly false, since deduction ...*

The context makes it clear that "deduction" refers to an area of research. It is indeed the goal of the deduction community to work out such generic problem solving mechanisms.

> *... itself provides no mechanism whatever: one only gets a mechanism when one chooses a strategy for performing deductions. For example, unification is not imposed by deduction; other strategies for instantiating universal variables are possible, computationally ridiculous but deductively perfectly valid.*

of course, of course

> *So Bibel must be understood as referring not to 'deduction' per se, but to a particular deductive strategy, or class of deductive strategies. Perhaps in his original lecture (which I havnt yet got access to) he tells us which ones they are, but a brief summary would be helpful.*

As I just said "deduction" refers to a field, a community.