Discuss Paper

Paper# 3819: Controller Synthesis of Discrete Event Systems via Planning

Abstract We show how AI automated planning techniques can be leveraged effectively to solve control problems of Discrete Event Systems. To do so, we first propose a careful (but simple) encoding of the DES controller synthesis problem into a planning problem that provably captures the compositional and reactive nature of DES specifications. We then report on experimental results comparing planning techniques under our encoding with existing synthesis tools for DES. The results show that the planning approach outperforms the controller synthesis tools, but also suggest that compositional analyses are more effective in some settings.

Paper Type Full Paper

Keywords [Knowledge Representation, Reasoning, and Logic] Non-classical logics for Knowledge Representation, [Multidisciplinary Topics and Applications] Knowledge-based Software Engineering, [Uncertainty in AI] Uncertainty in AI

Average Rating 5.10

Submission File

Assigned Area Chairs Sheila McIlraith (#30968) (University of Toronto) assigned by Carles Sierra

Assigned Senior PC M... Joerg Hoffmann (#27250) (Saarland University) assigned by Carles Sierra

Assigned PC Members Brian Logan (#20955) (University of Nottingham) assigned by Carles Sierra
Sasha Rubin (#21286) (University of Naples, Federico II) assigned by Carles Sierra
Guillaume Aucher (#22924) (University of Rennes 1) assigned by Carles Sierra

Assigned Review Assi... Daniel Gnad (#32311) (Saarland University) assigned by Joerg Hoffmann

Reviews

★★★★★★☆☆☆☆

Review from PC Member Brian Logan (#20955) (Created: 2017-04-05 14:31:35, Last modified: 2 days, 21 hours ago)

Originality

★★★★★★☆☆☆☆

Technical Quality

★★★★★★☆☆☆☆

Significance

★★★★★★☆☆☆☆

Relevance

★★★★★★☆☆☆☆

Quality of writing

★★★★★★☆☆☆☆

Overall Score

★★★★★★☆☆☆☆

Confidence on your assessment

★★★★★☆☆☆☆☆

Comments to Authors.

The paper presents an approach to synthesizing controllers for discrete event systems using AI planning techniques. The author(s) give a translation from control problems to FOND planning, and compare the number of problems solved and execution time for a number of benchmark controller synthesis problems using synthesis tools and AI planners. The results show that AI planners can solve a larger number of problems, at least in some benchmarks.

As far as I am aware, the encoding and results presented are novel. There has been work on solving control problems using planning, but as the author(s) note, this has not utilized a compositional encoding or compared tools. Synthesis of controllers and planning are clearly relevant to IJCAI. The work seems technically sound -- there is no proof of the main (only) theorem, but the encoding seems straightforward. The work is significant in linking controller synthesis with AI planning. I am not an expert in controller synthesis, so can't comment on the synthesis tools compared in the experiments, or the appropriateness of the benchmark problems. The paper is generally well written, though there are a few minor issues noted below.

Minor points:

The rephrasing of [Muise et al 2012] is unhelpful in confounding the solution to a FOND problem (a policy that maps a state to an action) with its encoding in classical planning (at least I think that's what this is saying).

The description of DES, and in particular the role of the controller could be more clearly explained. Partly this is inconsistent terminology, e.g., alphabet/events, but the definition of a controller on p.3 is not very clearly explained, and the example does not illustrate a control or how it is achieved.

In Section 3, the description of the fifth element of the encoding is unclear. It becomes clearer later, but it would be helpful to have a high-level overview here.

On p.4, the specification of G is obscure (it becomes clear in the last bullet on p.4 explaining the effects of the event operators).

In the discussion of the setR operator, what are 'events that are locally available'?

The definition of the effects of the setE and pickU operators are incorrectly specified/contain typos.

It would be useful to have some more detail on the benchmark problems, e.g., the size of the state spaces. Can you speculate on why the CM and BW benchmarks are more difficult?

The claim that PRP solves instances 'usually in less time' is not clear from the results presented. For CM, TA, BW and AT PRP takes more time than MTSA. In the case of TA it solves nearly twice as many instances, but in the other cases it solves fewer (or only one more in the case of AT) but takes significantly longer to do so. Perhaps it would be clearer to present the average time required to solve an instance rather than the total time?

The discussion of the most closely related work [Patrizi et al 2013], [Camacho et al 2016] is not very clear. Can you explain how these approaches differ from yours in more detail?

There is some repetition and text that could be condensed to allow the points above to be expanded.

There are also some minor grammatical mistakes/typos or awkward English, e.g, 'the transitions cannot surpass the connection', 'inspired in the control literature'

Comments after rebuttal

Thanks for the clarifications, which were helpful.

Confidential Comments (Not visible to the authors)

In the light of the other reviews, I have reduced my scores.

Rebuttal #4896 (Created: 1 week, 1 day ago, Last modified: 1 week, 1 day ago)

Rebuttal

- DES [..] could be more clearly explained.
After Def.6 we tried to explain this, we understand that it might not been enough and we will elaborate.

- In Sec.3 the description of the fifth element of the encoding is unclear.
We wanted to give a preview that we do not restrict the choice of uncontrollable events. We will clarify.

- [..] what are events that are locally available?

Locally available events are those available from the current state of each component. We will make the definition explicit.

- The definition of [..] setE and pickU operators are incorrect [..]
We believe that the source of confusion may be the nesting of sets, we will use symbols of different sizes to clearly indicate their scope.

- It would be useful to have some more detail on the benchmark problems [..]
The state spaces vary in size up to 500000 states. Our hypothesis is that the CM and BW benchmarks are more challenging due to their topology. We will elaborate.

- The claim that PRP solves instances usually in less time is not clear [..]
We wanted to highlight that in some cases PRP was faster than the others despite solving more instances. This is hardly generalizable and we agree that we should change the statement to reflect this.

- The discussion of the most closely related work [..] is not clear.
The main difference is that while they use an LTL to describe the environment we use a compositional of automata which we then exploit in the reduction to planning.

Assessment from Sasha Rubin (#7291) (Created: 1 day, 14 hours ago, Last modified: 1 day, 14 hours ago)

Review assessment. Only visible to Area Chairs.

★ ★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆

[Optional] Assessment comments. Only visible to Area Chairs.

★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆ ☆

Review from PC Member Sasha Rubin (#21286) (Created: 2017-04-06 21:09:53, Last modified: 1 day, 14 hours ago)

Originality

★ ★ ★ ★ ★ ★ ☆ ☆ ☆ ☆

Technical Quality

★ ★ ★ ★ ★ ★ ★ ☆ ☆ ☆

Significance

★ ★ ☆ ☆ ☆ ☆ ☆ ☆ ☆ ☆

Relevance

★★★★★★☆☆☆☆

Quality of writing

★★★★★★★★★☆

Overall Score

★★★★☆☆☆☆☆☆

Confidence on your assessment

★★★★★★☆☆☆☆

Comments to Authors.

Summary:
In discrete event sysytems (DES) the system to be controlled is typically modeled as a set of interacting finite-state machines. A controller should be devised that disables controllable actions so that the resulting traces satisfy some property (such as safety conditions).

This paper supplies a polynomial (in fact, cubic) translation of a controller synthesis of DES to the problem of finding strong plans of fully observable non-deterministic planning problems with conditional effects.

The paper evaluates the translation by comparing the runtimes and number of problems solved for six DES problems of sizes up to about 46,000 states each.

Justification for scores.

Originality:
The proposed translation is new and, as the authors state, also simple.

Technical Quality:
I could not verify that the translation is correct. The reason is that the semantics of FONDP and DES are not given precisely enough. My particular worry is that when the variable "wild" is set by action pickU, then some uncontrollable event l is enabled, and thus the precondition for the "default" action does not hold, and thus the uncontrollable event l is executed. This means that no controllable event is executable if some uncontrollable event is executable.

In contrast, the semantics of DES suggest (it is not clear from the definitions) that the next event to be executed is chosen non-deterministically from amongst the set of enabled events, whether or not this set contains uncontrollable events or not.

The claim that "The results show that the planning approach

outperforms the controller synthesis tools, but also suggest that compositional analyses are more effective in some settings." seems justified by the experimental results. From what I can tell, the examples are toy examples rather than benchmarks from the DES literature. As such, the experimental results supply a proof-of-concept rather than a robust analysis.

Significance:
This work supplies a connection between two very similar areas of AI that have their own communities and that are known to be similar. It is limited by the fact that it does not discuss the relevance of the particular DES model studied.

The model of DES in this paper (Section 2.3) is said to be "based on [Ramadge and Wonham, 1989]". However, there are a number of differences, some superficial and some important, that are not addressed, and so I am left unconvinced about the significance of the model of DES in this paper.

I point out the important differences:

1. Controllers in [Ramadge and Wonham, 1989] are functions that depend on the trace seen so far, while in the present paper the controllers only depend on the current state.

2. The specification languages K in [Ramadge and Wonham, 1989] are sublanguages of the set of finite traces generated by the DES, given by finite-state machines, while in the present paper the specifications are given as safety and co-safety goals on the product system.

3. [Ramadge and Wonham, 1989] prove that there is a unique maximal subspecification of the given specification K that can be controlled (this is called the "optimal" or "minimally restrictive" or "supremal controllable" sublanguage). One usually looks for a controller of this optimal language (if one does not exist for K).

A related notion is that of a controller that must disable events only when strictly necessary, and that under certain conditions there exists a unique such controller, see, e.g., Theorem 1 in "Bridging the Gap between Supervisory Control and Reactive Synthesis: Case of Full Observation and Centralized Control" by Ehlers et. al., 12th IFAC/IEEE Workshop on Discrete Event Systems Cachan, France. May 14-16, 2014. Again, one usually aims to compute this unique controller.

Relevance:
The paper will be of interest to the planning and verification/synthesis communities at IJCAI.

Quality of writing:
The paper is mostly well written. That said, a number of objects are vaguely defined, i.e.,
- the semantics of FOND with conditional effects are only vaguely described and do appear in the cited work. Indeed [Rintanen, 2003] supplies a richer syntax but no semantics for nondeterministic effects, and [Geffner and Bonet, 2013] provide a different syntax.
- "policy", "closed policy" and "strong plan" should be properly defined, e.g., what are "appropriate actions", how does a policy induce a set of executions, does a closed policy return a single action or at least one action for every non-goal state, etc.?
- what are the "set of labels" in the definitions of safety and co-safety?

The citation in the proof of Theorem 1 to [Piterman et al., 2006] seems out of place since it neither discusses DES nor planning.

Comments after the response

I am reasonably satisfied with the response regarding technical quality (especially in light of discussions with another reviewer).

However, the paper and the response falls short in arguing for the significance of the model, especially in light of the DES literature.

Confidential Comments (Not visible to the authors)

I could be persuaded to accept if the following could be established:

a) the model in this paper is rich enough to capture the essential properties that distinguish DES from planning and synthesis on graphs,

b) the examples and empirical evaluation is interesting enough to either the planning or DES communities,

c) the translation is not "too simple" to be published at a premier AI conference.

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My feeling is that

- we have evidence against a).

- we have doubts about b).

- we are not concerned about c).

After the response and the discussions, I have raised my score on "technical quality", and lowered my score on relevance.

Rebuttal #4907 (Created: 1 week, 1 day ago, Last modified: 1 week, 1 day ago)

Rebuttal

- [..] no controllable event is executable if some uncontrollable event is
Indeed, controllable events in mixed states are pruned. Mixed states represent a race that can be won by the environment, thus, a solution cannot depend on winning the race. This is valid under the standard progress assumption (the environment will eventually act).

- [..] experimental results supply a proof-of-concept rather than a robust analysis
By no means we aim to imply the superiority of an approach, we just highlight that planning is competitive. We will clarify this. Furthermore, there is no benchmark for control except for the one from WODES08 (which we used).

- Controllers in RW are functions that depend on the trace seen so far [..]
RW accounts for history dependent controllers. It is known that for safety and co-safety, controllers can be memoryless.

- The specification languages K in RW are [..]
Recently, formulations using logic or observers have been used. Equivalences between these formalisms allows presenting the same problem in various ways.

- RW prove that there is a unique maximal [..]
Classical supervisor control looks for maximal controllers for safety properties, since not doing so allows controllers that do nothing. When considering co-safety, there may not be a unique maximal controller [Ehler WODES14]. We focus on a standard problem in reactive synthesis that is open. For instance MTSA and Supremica have scale limitations even when not guaranteeing maximality.

★★★★★☆☆☆☆☆

Review from PC Member Guillaume Aucher (#22924) (Created: 2017-03-27 15:44:26, Last modified: 1 week, 4 days ago)

Originality

★★★★★★☆☆☆☆

Technical Quality

★★★★★★☆☆☆☆

Significance

★★★★★★★☆☆☆

Relevance

★★★★★☆☆☆☆☆

Quality of writing

★★★★★★★☆☆☆

Overall Score

★★★★★☆☆☆☆☆

Confidence on your assessment

★★★★★★★★☆☆

Comments to Authors.

Originality: 6

The paper follows a line of research aiming at connecting control theory with other frameworks. So, in that sense, their idea is not so original. However, they do not cite these related works which propose a similar embedding (see relevance).

Technical quality: 6

I could not check the proofs but I have a number of questions concerning their embedding of the control theory framework into the planning framework (see my comments).

Significance: 7

It is significant in the sense that it provides new bridges between different communities and a mutual cross-fertilization could ensue of these interactions.

Relevance: 5

The authors seem to be unaware of prior work. Moreover, the control theory problem that they address is a very specific one. In fact, they do not consider the control theory framework in its full generality with partial observation and non-blocking control (see my comments).

Quality of writing: 7

The paper is clearly written, well organized and makes good use of examples and

figures. It is rather a nice read.

Overall score: 5

Decent paper, but may be below the IJCAI threshold. I tend to vote for rejecting it, although would not be upset if it were accepted.

Confidence: 8

Comments to the authors:

The control synthesis framework that the authors present does not really correspond to the usual control theory framework that you can find in the control theory literature [Ramadge & Wonham 1989, Cassandra and Lafortune 2008] and it is not presented and defined in the same way either. In fact, I could not identify to which control problem the authors refer to. Is it control with modular specification [Section 3.6, Cassandra and Lafortune 2008] ? It does not seem so. Moreover, the goal behavior is not defined as it is usually defined in control theory by means of an automaton or a formal language. The definition of the goal behavior is closer to the definition that you can find in game theory [Doyen and Raskin 2011]. I really doubt that researchers from the control community will manage to recognize themselves and their work in the description of the control theory framework as it is done in this paper. Moreover, crucial notions such as non-blocking control are not even addressed or defined.

There are also very few references to the control theory literature (only 3 of them). Moreover, a number of attempts have already been made to connect control theory to other logical frameworks or theories and none of them is referred to in this paper:

- Game theory [Arnold & Al 2003, Muschol & Al 2009]
- Epistemic temporal logic [Aucher 2014, van der Meyden and Vardi 1998]
- Mu-calculus [Riedweg and Pinchinat 2003]
- Situation calculus [Giacomo & Al 2012]

Overall, I find the authors' knowledge of control theory rather shallow and I doubt that they will be able to convince researchers in this area given their description of the control theory framework. I would rather recommend the authors to submit their work to a conference like WODES to have more feedback about it.

Also, I have some technical comments regarding the definitions of the operators on page 4:

- inprogress(l) is sometimes enabled. But when is it disabled ?

- even if I understand what the authors mean, the formalization is sometimes not very well-defined. For example, in Eff(setE), the term {wild | l \in A_u} is not appropriate and should not be defined like that. The same remark applies to {goal | l \in G_\Diamond} and {error | l\in G_\square} in Eff(l).

- the definition of Pre(l) should be case dependent and depend on whether l \in A_C or l \in A_U.

On the other hand, a significant number of experiments have been made and compared to the results obtained with the tools of control theory. This is clearly a good thing. Yet, I find that the authors' approach is not entrenched enough in the work of control theory and does not cover it enough to be able to claim that their approach « outperforms the control synthesis tools ».

Typos:

- boolean => Boolean

- Definition 1: where each e_i is a conjunction of conditional *deterministic* effects.

- p. 3: Example: « two LTSs that capture*s* the behavior ».

- p. 4: « A_U = {l_0, …, *l*_k} and « the fluents consist*s* ».

_____

Aucher, G. (2014, May). Supervisory control theory in epistemic temporal logic. In *Proceedings of the 2014 international conference on Autonomous agents and multi-agent systems* (pp. 333-340). International Foundation for Autonomous Agents and Multiagent Systems.

Arnold, A., Vincent, A., & Walukiewicz, I. (2003). Games for synthesis of controllers with partial observation. *Theoretical computer science*, *303*(1), 7-34.

Muscholl, A., Walukiewicz, I., & Zeitoun, M. (2009). A look at the control of asynchronous automata. *Perspectives in Concurrency Theory*, 356-371.

Van Der Meyden, R., & Vardi, M. Y. (1998, September). Synthesis from knowledge-based specifications. In *International Conference on Concurrency Theory* (pp. 34-49). Springer Berlin Heidelberg.

De Giacomo, G., Lespérance, Y., & Muise, C. (2012, June). On supervising agents in situation-determined ConGolog. In *Proceedings of the 11th International Conference on Autonomous Agents and Multiagent Systems-Volume 2* (pp. 1031-1038). International Foundation for Autonomous Agents and Multiagent Systems.

Riedweg, S., & Pinchinat, S. (2003, August). Quantified mu-calculus for control synthesis. In *International Symposium on Mathematical Foundations of Computer Science* (pp. 642-651). Springer Berlin Heidelberg.

Doyen, L., & Raskin, J. F. (2011). Games with imperfect information: Theory and algorithms. *Lectures in Game Theory for Computer Scientists*, 185-212.

Rebuttal #4927 (Created: 1 week, 1 day ago, Last modified: 1 week, 1 day ago)

Rebuttal

- do not consider [..] PO and non-blocking control
PO is beyond this paper's scope, however PO can be cast as full observability building on results like [DOI: 10.1007/978-3-540-85361-9_27]. Regarding non-blocking control, please refer to page 3, where we require a solution to be nonblocking.

- The synthesis framework [..] does not correspond to the usual
Our specific choice is used as far back as [DOI: 10.1007/BF02551233] where parallel composition is defined to simplify the construction of the examples. Also see DOI: 10.1109/TAC.2013.2283109. In the literature it is common to use interchangeably automata product, languages intersection, and temporal logic conjunction.

- a number of attempts have been made to connect control theory to other frameworks [..]
Certainly these are relevant papers too, and we will include a citation to some of them. Still, none of these reduce control to planning.

- inprogress(l) is sometimes enabled. But when is it disabled?
This is a presentation error, there should be a clause in the reset action that disables inprogress. Implementation included this clause.

- the formalization is sometimes not very well-defined [..]
We tried to keep the notation simple, we will redefine some terms in cases.

- [..] claim that their approach outperforms the control synthesis tools
By no means we tried to establish the superiority of an approach, we just highlighted that the planning approach is competitive. We will clarify.

Assessment from Sasha Rubin (#7291) (Created: 1 day, 14 hours ago, Last modified: 1 day, 14 hours ago)

Review assessment. Only visible to Area Chairs.

★ ★ ★ ★ ★ ★ ★ ★ ★ ☆

[Optional] Assessment comments. Only visible to Area Chairs.

★ ★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆

Review from Senior PC Member Joerg Hoffmann (#27250)

(Created: 2017-04-06 19:12:51, Last modified: 1 day, 16 hours ago)

Originality

★ ★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆

Technical Quality

★ ★ ★ ★ ★ ★ ★ ☆ ☆ ☆

Significance

★ ★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆

Relevance

★ ★ ★ ★ ★ ★ ★ ★ ☆ ☆

Quality of writing

★ ★ ★ ★ ★ ★ ★ ☆ ☆ ☆

Overall Score

★ ★ ★ ★ ★ ☆ ☆ ☆ ☆ ☆

Confidence on your assessment

★ ★ ★ ★ ★ ★ ★ ★ ☆ ☆

Comments to Authors.

The paper looks correct, but is not particularly surprising or deep. The main weakness is that the authors appear to have a limited scholarship in DES.

First, they don't mention the key problem of DES, i.e., computing not any random controller but a "maximally permissive one", i.e., one that keeps the plant to be maximally free in its choices while preserving some desired properties. I would

suggest the authors to read and revise the tone of the paper in light of the following survey paper:
Rüdiger Ehlers, Stéphane Lafortune, Stavros Tripakis, Moshe Y. Vardi:Bridging the Gap between Supervisory Control and Reactive Synthesis: Case of Full Observation and Centralized Control. WODES 2014: 222-227
The fact that the authors do not aim at maximally permissive controllers make control synthesis a (relatively simple) standard problem in reactive synthesis and in generalized planning. So the theoretical contribution (Section 3) is interesting but limited.

Also in the intro they mention interest in DES in AI but they cite papers where DES is not mentioned: page 1 col 1 (e.g., [Patrizi et al., 2013; Sardina and D'Ippolito, 2015b; Camacho et al., 2016]).

However, there are some papers that indeed place a bridge between AI, Planning and DES. For example (at least the first two or three should be cited):Giuseppe De Giacomo, Yves Lespérance, Christian J. Muise: On supervising agents in situation-determined ConGolog. AAMAS 2012: 1031-1038
Paolo Felli, Nitin Yadav, Sebastian Sardiña:Supervisory Control for Behavior Composition. IEEE Trans. Automat. Contr. 62(2): 986-991 (2017)

Giuseppe De Giacomo, Paolo Felli, Fabio Patrizi, Sebastian Sardiña: Two-Player Game Structures for Generalized Planning and Agent Composition. AAAI 2010

Nitin Yadav, Paolo Felli, Giuseppe De Giacomo, Sebastian Sardiña: Supremal Realizability of Behaviors with Uncontrollable Exogenous Events. IJCAI 2013: 1176-1182

Bita Banihashemi, Giuseppe De Giacomo, Yves Lespérance: Online Situation-Determined Agents and their Supervision. KR 2016: 517-520

Bita Banihashemi, Giuseppe De Giacomo, Yves Lespérance: Online Agent Supervision in the Situation Calculus. IJCAI 2016: 922-928

Please comment on these points in the author feedback.

Confidential Comments (Not visible to the authors)

These are comments that Guiseppe de Giacomo sent me, after kindly doing a quick assessment of the paper on my request, as he is the leading expert on the intersection of planning and synthesis.

My own review has been entered by Daniel Gnad, who assisted me here; we coordinated our views before he entered the review.

Metareview

As discussed in the reviews, the reviewers have concerns regarding (a) the fragment of DES considered, (b) the clarity/correctness of encoding write-up, (c) the literature discussion, and (d) the significance of the current experiments.

Nevertheless, the paper makes some interesting observations, and it provides a direct empirical comparison nof DES vs. planning tools which is a first in the literature.

This review panel's assessment is that the paper is on the borderline and can be accepted depending on the strength of the competition.

If accepted, the authors are requested to thoroughly address (b) and (c), and to better motivate (a) the relevance of the DES problem addressed relative to the DES literature.

Rebuttal #4932 (Created: 1 week, 1 day ago, Last modified: 1 week, 1 day ago)

Rebuttal

- The fact that the authors do not aim at maximally permissive controllers make control synthesis a (relatively simple) standard problem [..]
Classical supervisor control looks for maximal controllers for safety properties, since not doing so allows controllers that do nothing. When considering co-safety, there may not be a unique maximal controller [Ehler WODES14]. We focus on a standard problem in the reactive synthesis and generalized planning fields that is still open as existing techniques do not scale. Two examples from the reactive synthesis field area (MTSA and Supremica) have scale limitations even when not guaranteeing maximality. Supremica offers to do so optionally but at a prohibitive computational cost.

- There are some papers that indeed place a bridge between AI, Planning and DES.
Certainly these are relevant papers too, and will include a citation to some of them. Still, none of these reduce control to planning, and thus, do not provide evidence that planning can be a competitive computational approach for the synthesis of controllers for DES as we do.
In the related work section we cite [Sardina, 2015], the main difference is that our work has a strong focus on compositional descriptions. Despite the differences it is clear that there is interest in relating the fields of planning and control, and we tried to be clear in that we are trying to contribute to this effort by highlighting the importance of compositional representations.

Assessment from Sasha Rubin (#7291) (Created: 1 day, 14 hours ago, Last modified: 1 day, 14 hours ago)

Review assessment. Only visible to Area Chairs.

★ ★ ★ ★ ★ ★ ★ ★ ☆ ☆

[Optional] Assessment comments. Only visible to Area Chairs.

★★★★★★☆☆☆☆

Review from Review Assistant Daniel Gnad (#32311) (Created: 2017-04-05 15:54:01, Last modified: 2 days, 19 hours ago)

Originality

★★★★★☆☆☆☆☆

Technical Quality

★★★★★★★☆☆☆

Significance

★★★★★☆☆☆☆☆

Relevance

★★★★★☆☆☆☆☆

Quality of writing

★★★★★★★★☆☆

Overall Score

★★★★★★☆☆☆☆

Confidence on your assessment

★★★★☆☆☆☆☆☆

Comments to Authors.

The paper introduces a compilation of controller synthesis of discrete event systems to FOND planning. While previous work is able to handle (arbitrary) LTL goal properties, the presented compilation is restricted to simpler reachability objectives. The authors claim that, in contrast to previous work, the proposed approach does not explicitly construct the parallel composition of the component transition systems, but models synchronous events by a control entity in the planning model. The compilation is evaluated using a set of synthesis benchmarks, running two FOND planners (namely PRP and MBP). As a comparison, the authors show coverage and runtime data of two synthesis tools.

In general, the paper is well written and easy to follow. However, it lacks important details of the proof stating the correctness of the compilation. The full proof is only provided by an external link. Still, the compilation is rather straightforward and looks sound.

A major weakness of the paper is the coverage of related work. In particular, the whole work branch by Gioseppe de Giacomo etal is missing, but seems highly relevant.

Some technical remarks/questions:

I didn't entirely get the point of the "default" loop in the busy state. Couldn't you simply default to "not error" in case the chosen uncontrollable event is not enabled? Why do apply *all* uncontrollable events, instead?

Also, why does the reset operator remove the goal? Doesn't this imply that any goal event needs to be applied in the last step, instead of at an arbitrary point in a trace?

In the theorem, you claim that "there exists a controller solution iff there exists a strong plan for the FOND compilation". What if there exists a strong *cyclic* plan? Can't this be translated into a controller solution? How is a controller solution obtained from a strong plan, in general? I suggest to replace the lengthy coin flipping example by a more detailed proof sketch.

After Def3, you say that "the states [..] can grow exponentially". You probably mean the *number of* states.

In the paragraph that describes the status fluents, it would be helpful if you would explicitly refer to the name of the phase you are talking about.

Reg. Evaluation:
- Do all tools result in the same/similar policies/controller solutions? If not, can you say something about the quality of the solutions?
- What exactly does the runtime column show? The time on commonly solved instances or simply the sum over all instances solved by the respective tool?

Minor:

- There are many citations that should be \shortcite, e.g., Muise etal in the FOND background, and throughout the related work section.

- Introduction: "for engineering*'s* reasons"
- Introduction: "has *a* special focus on"

- Synthesis background: "Label*ed* Transition System"

- Section 3, point 5: "we default *then* the choice"

- In the definition of setE and reset you use a set of labels "A", which is never defined. I guess you mean "A_E".

- The "default" operator is missing in O.

- Evaluation: "travel agency *agency*"
- Evaluation: "relying on a*n* explicit"

- The success column of MTSA in BW should probably be bold-faced.
Anyway, you don't say what the bold face highlights.

- You never introduce terms like BDD and SMV.

- The Sardina and D'Ippolito 2015 paper appears twice in the
references.

Post rebuttal comments:

- I'm still not convinced why the restriction of accepting traces that end with a goal label should be necessary.

- Summing-up the runtimes of all instances solved by a tool sounds like a weak measure to compare different approaches. I suggest to restrict the sum to commonly solved instances.

Rebuttal #4935 (Created: 1 week, 1 day ago, Last modified: 1 week, 1 day ago)

Rebuttal

- A major weakness of the paper is the coverage of related work
Certainly these are relevant papers too, and will include a citation to some of them. Still, none of these reduce control to planning.

- I didnt entirely get the point of the default loop in the busy state
We understand that this is not intuitive. We do not restrict the non-deterministic choice, and hence a non-enabled uncontrollable event could be selected in the choice phase. In such a case, by setting all the inprogress fluents, we allow the planner to choose of the next uncontrollable event (the planner can only apply the effects of one event before repeating the phase cycle).

- Also, why does the reset operator remove the goal?
This simplifies the characterization of desirable traces since it restricts them to the set of finite traces that end in a goal.

- What if there exists a strong cyclic plan?
In [Sardina 2015] a characterization of fairness assumptions wrt control is presented. Based on that we cannot translate a strong cyclic plan to a controller without weaker requirements or stronger (fairness) assumptions.

- Do all tools result in the same/similar policies/controller solutions?
We have manually compared the results for the simpler cases and found the results were similar. However, it is not easy to automatically compare the

quality of the solutions for the more complex cases.

- What exactly does the runtime column show?
The sum over all instances solved. We will clarify this.

Assessment from Sasha Rubin (#7291) (Created: 1 day, 14 hours ago, Last modified: 1 day, 14 hours ago)

Review assessment. Only visible to Area Chairs.

★ ★ ★ ★ ★ ★ ★ ★ ☆ ☆

[Optional] Assessment comments. Only visible to Area Chairs.

Comments

Joerg Hoffmann (#27250) wrote 1 day, 16 hours ago:

Hi,

after all, I had the option to leave the decision open (bit unusual). Given the situation, this seems the right thing to do.

You should be able to see my meta-review at the end of my review. Let me know if you want me to add/change something.

Thanks for your work and the discussion.

best,

Joerg

Joerg Hoffmann (#27250) wrote 2 days ago:

Hi all,

hm. This is the most difficult case in my batch of 8.

I'm not sure what we're converging to. There are many shortcomings reg the fragment/kind of synthesis addressed here. At the same time, there is a potentially interesting connection and experiments.

Like I said before, I'm reluctant to give this work too hard a time from a synthesis perspective. If, say, synthesis guys publish a paper on planning in a synthesis conference, I daresay that I, as a planning person, would have much to criticize about their perception/use of "planning". In other words, there is an inherent tendency for work

at an area intersection to be hard pressed. I'd like to counter that tendency a bit.

I suggest to recommend accept on this, but clearly mark it as a borderline paper that may go either way depending on the competition. Does that make sense? If you strongly disagree, please speak up.

best,

Joerg

 Guillaume Aucher (#22924) wrote 2 days, 16 hours ago:

What I find of value nevertheless are the experiments. I think they should be published somewhere somehow.

 Guillaume Aucher (#22924) wrote 2 days, 16 hours ago:

I also wanted to add that after the rebuttal, I have sent the paper to a local colleague in Rennes who is more knowledgeable than me on DES (Thierry Jéron, INRIA) and he came to similar conclusions as mine (he was in fact tougher than me).

Best,

Guillaume

 Guillaume Aucher (#22924) wrote 4 days, 18 hours ago:

Dear All,

The authors' response does not really convince me. The fact that they use labels on states (like in infinite games) rather than prefix-closed languages like in DES makes the comparison with the DES framework rather difficult for me.

Besides, modular control is only a specific problem of DES. Also, the reference they quote for dealing with imperfect information uses some techniques from infinite games, not control theory, and translating them into this framework is not straightforward.

Daniel, concerning the "inprogress" fluent, this is a comment I made to them and they replied that it is a "presentation error".

Best,

Guillaume

Daniel Gnad (#32311) wrote 5 days, 2 hours ago:

Hi all,

I cannot say a lot regarding the significance of this work, since I'm not an expert in DES, nor in FOND planning. From my perspective, it looks like a (probably limited) translation from one into the other that is still interesting to know about. If the authors improve on the related work coverage, certainly a big weakness can be addressed in the final version.

Regarding the model:

I still don't see why the authors limit accepting traces to those that end with a goal label. To me, this looks like a grave restriction of their own co-safety definition. I don't get why this restriction is necessary and the authors failed to give a good intuition (in both the paper and the response).

Sasha: I'm more or less confident now that the default operator works as intended. It's applicable if none of the environmental actions is *inprogress*, i.e. has actually been chosen *and* is appliable. It then does some kind of all-outcome determinization of the effects of pickU. There is one (minor) bug, because the "inprogress" fluents are never reset. So any uncontrollable event will remain inprogress if this has been set once.

Best
Daniel

Sasha Rubin (#21286) wrote 5 days, 20 hours ago:

Joerg (apologies for having written Jeorg before): Yes it makes sense at a high-level, no it is not a deal-breaker, but it is yet another restriction on their model that is neither properly discussed nor properly motivated, i.e., it further stresses the low significance of this work.

Daniel: I see you also had a concern about the "default" action. Did the response make sense?

Joerg Hoffmann (#27250) wrote 5 days, 21 hours ago:

Hi,

reg: "The authors pointed out that they are *assuming* (in their modeling of DES) that no controllable event is executable if some uncontrollable event is executable. Is this a reasonable assumption?"

I can't answer this question with any confidence. The author rebuttal reg this -- "Mixed states represent a race that can be won by the environment, thus, a solution cannot depend on winning the race. This is valid under the standard progress assumption (the environment will eventually act)" -- makes sense to me at a high level intuitively. Doesn't it make sense to you? If no, why not? In any case, do you consider this decisive, ie., is it a deal maker or breaker for you reg wether to accept the paper?

best,

Joerg

 Sasha Rubin (#21286) wrote 5 days, 22 hours ago:

Dear Jeorg,

You asked:

Regarding (a), imho the question is whether we give the authors the benefit of the doubt. Sasha, Guillaume, did the author feedback do something towards alleviating your concerns?

The authors pointed out that they are *assuming* (in their modeling of DES) that no controllable event is executable if some uncontrollable event is executable. Is this a reasonable assumption?

Regarding the translation, I don't yet understand the purpose of the "default" action (I did not see this issue at the time I wrote my review). It seems to me that the preconditions for the "default" action are never satisfied. Apart from that, the translation looks fine.

Sasha

 Joerg Hoffmann (#27250) wrote 6 days ago:

Hi Sasha,

your comment seems to have crossed with the time I was taking to read the reviews and assemble my previous comment.

I would say that the paper is not trying to argue significant improvements from the point of view of DES -- it would probably not be an acceptable paper in a DES conference. But it links (a simple form of) DES to planning and shows an actual empirical comparson between the two. This is preliminary, but of value, and I think constitutes a contribution marginally worth publishing in IJCAI.

Perhaps what I just said is in some way related to your second question -- ignoring intricacies discussed in the DES literature, what we see here is that the transition-system-composition model can be translated into FOND. I don't find this particularly surprising, but it is worth pointing out. So, again, a weak accept in my view.

All this said, please don't misunderstand me as fighting for the paper. It is a difficult case, and can go both ways. I'm just saying, right now I see this marginally on the positive side of the borderline.

best,

Joerg

 Joerg Hoffmann (#27250) wrote 6 days ago:

Hi all,

well -- this is a difficult case. Summarizing what I take from reading the reviews and rebuttal, we're all around the brderline. We'tre not happy with (a) the clarity/correctness of encoding write-up, and (b) the literature discussion. There also is (c) the question how significant these particular experiments are, though I think we can discount that and assume that the results are, perhaps not tremendously significant, but certainly of interest.

Regarding (a), imho the question is whether we give the authors the benefit of the doubt. Sasha, Guillaume, did the author feedback do something towards alleviating your concerns?

Regarding (b), I'm not very familiar with this literature myself, so I find this difficult to judge. The paper certainly misses many things that should be cited and discussed. But also, the author feedback does look like the authors know their turf. I would furthermore point out that, being placed at the intersection between two areas (synthesis and planning in this case) is often anawkward position as neither side will be happy, and so we may choose to be lenient in the presentation and discussion of deep details regarding related literature on either side.

Overall, it seems to me that this is a paper of potential interest -- to my knowledge, indeed a direct comparison of actual synthesis vs. planning tools is missing from the literature -- that can be published subject to a more thorough (though perhaps not perfect) literature discussion. This would be a weak accept with a request to improve literature discussion for the final version. Does this make sense to you? Or would you rather argue to reject? Please, each of you, let me know what you think.

best,

Joerg

 Sasha Rubin (#21286) wrote 6 days ago:

Dear all:

My main concern is that the authors claim (in the paper and in their response) that their framework is rich enough to capture what is usually considered in DES, but do not provide much evidence for these claims (e.g., citations).

E.g., I would have expected that they consider the subcase of their model with safety objectives only (for which their does exist a minimally restrictive controller).

E.g., they assume that from a state with controllable and uncontrollable actions the controlled actions are disabled.

If anyone wants to fight for this paper, I suggest asking someone from the DES community for their opinion on these matters.

That said, suppose for a moment we ignore their stated motivation (i.e., DES), and instead consider this as a paper that shows how to solve (via a reduction to planning) safety/co-safety games that are given compactly as the composition of finite state machines. Does the paper have enough merit for IJCAI? Perhaps Joerg and Sheilah can comment on this?

Sasha

 Sheila McIlraith (#30968) wrote 6 days, 14 hours ago:

Hi Everyone,

The authors have responded to your reviews. Please take the time to read their responses, and each other's reviews, have some discussion about the merits and shortcomings of the paper and try to come to consensus on a final determination. Unfortunately, we don't have much time (only until Wednesday, April 5) to complete this task. Also, regardless of the outcome, please consider updating your reviews to acknowledge that you have read and considered their comments.

Thanks for your time on this and your other papers.

Sheila