

Responses

Masataro Asai

First, thanks very much to all of the reviewers for their detailed and helpful comments. To address the reviewers' comments, we have modified the paper as described below.

To reduce the length of the main portion of the paper, we moved many tables to the Appendix, eliminated some tables and consolidated some figures. We also added a few figures, Algorithms,

proofs in order to address the concern by the reviewers. As a result, the numbering of the tables/figures are changed from the original manuscript. In this response, we use [Revised:X] to indicate that we refer to those figures using the new numbering system, and [Original:X] to indicate the numbers in the original manuscript.

1 Reviewer 1

1.1 1) The definition of plateau can be improved.

The definition of plateau can be improved. The formal definition of a plateau should include a plateau with respect to some cost functions. So, you have a plateau with respect to f , $\text{plateau}(f)$, a plateau with respect both g and h , $\text{plateau}(g,h)$ etc. This should be defined at the beginning and this terminology should be later used all over. Currently, you are not fully formal and consistent on this. As you say on page 18. a plateau is related to a sorting strategy. Define this in the beginning. In fact you write the following sentence which is in fact a definition: "Having the same key values means that n and m are on the same plateau". Move this to be a definition in one of the first sections."

Plateaus are formally defined in the 2nd-to-last paragraph of Section 2, Preliminaries. However, as the reviewer noted, there were some informal usages of the term "plateau" throughout the paper. In the revision, we increased the usage of the more precise notation " $\text{plateau}(f,h)$ " throughout the paper.

1.2 2) page 18: "In order to diversify the expansion... " -> This paragraph is very hard to understand

page 18: "In order to diversify the expansion... " -> This paragraph is very hard to understand but it is a very important paragraph as it gives the pseudo code for your new technique. Please rewrite it. What is D_c ? Is this a counter? Is there one D_c or one for each depth? Please clarify. Maybe even give an example.

page 20: "round-robin sampling from the available depth buckets as described above." -> This is a key sentence that might help understanding what exactly is the diversifying method. I think you mean you that do a round robin from the deepest available depth to the shallowest available depth. You must clarify this.

We rewrote the description and added a pseudo code of depth diversification in Section [Original:6,Revised:5]. d_c is a counter assigned to each plateau.

1.3 3) It will be very interesting to see what happens if you factor away the constant time per node.

page 22: Table 7.5. It will be very interesting to see what happens if you factor away the constant time per node. Just compare the number of nodes expanded for a set of instances that can be solved by all methods (as you did in the 4.1-4.5 tables). This will tell you if indeed this is the reason for the negative behavior. This is a rather major comment. You do not always have to use the 30 minutes limit in your experiments.

In order to factor away the constant low-level overhead of depth bucket which is degrading the performance of M&S in IPC domains, we compared the number of node evaluations between the depth-diversified and standard search method. We newly

added Figure [Revised:6.2] comparing the cumulative coverage over the number of evaluations. We limited the instances to those successfully solved by both. The result shows that the evaluations are almost identical for most IPC instances. There were differences in Openstacks, as expected.

1.4 4) Section 5: Zerocost domains. I buy all your arguments on zero cost

Section 5: Zerocost domains. I buy all your arguments on zero cost domains. You spend too much effort to validate them. I suggest to shorten this entire section and the related tables. All your claims seem reasonable and you do not have to necessarily show all the numbers.

We moved one of the tables to Appendix. We also shortened the section.

1.5 [continue] 4) The claims about FIFO in infinite graphs (section 5.3) is trivial.

The claims about FIFO in infinite graphs (section 5.3) is trivial. You can shorten it or even delete it.

In the revised version, we moved the section to the end of Section [Original:8,Revised:7] and added more proofs, in order to satisfy the requests from Reviewer 2. Although the proofs are still mostly trivial, the new section improves the flow of the story and strengthen the significance of A*-as-sequence-of-SAT.

It is no longer in section [Original:5,Revised:4] because we noticed that infinite search space is irrelevant to having 0-cost edges. We had/gave false impression that 0-cost edges induces an infinite graph, which is not the case. Trivially, just changing the cost of edges does not make finite graphs infinite.

1.6 TODO 5) Section 6.1 is trivial.

Section 6.1 is trivial. It is easy to see that different "depth" values only occur in zero domains. I would shorten it or even omit it.

We shortened the section as requested.

1.7 6) Section 7.1: this section repeats what you said above and ...

Section 7.1: this section repeats what you said above and I was convinced when you said it. You can just report that you observed this in your experiments and I do not need to see all the exact results. Consider to omit these experiments and just mention that you have results that support this trend.

We moved several non-critical figures to the appendix.

1.8 7) The beginning of Section 8 is also rather trivial.

The beginning of Section 8 is also rather trivial. This is the main rational behind IDA* as you say in the end. I would significantly shorten it but it should get a subsection index if it stays. It is not an introduction to your later section 8.1 which I find quite interesting and more deep and should certainly be kept.

We shortened the beginning of Section [Original:8,Revised:7].

1.9 minor 1) – should be "current shortest known path"

page 4: "g(n) is the current shortest path cost from the initial node to the current node." – should be "current shortest known path"

Fixed as you suggested.

1.10 minor 2) – I did not like this syntax. Give the reference and...

page 5: "Holte, 2010, note that since $f = g + h \dots$) I did not like this syntax. Give the reference and then give your comment but not in the same parenthesis.

Fixed as you suggested.

1.11 minor 3) – Calling it the third is misleading...

page 21: "the third, depth-diversification criteria." Calling it the third is misleading. It is actually the second which comes before the default criterion.

Fixed as you suggested.

1.12 minor 4) – The first sections are very short. Maybe they can be one large section...

The first sections are very short. Maybe they can be one large section with different subsections.

We merged the section 3 into section 2.

2 Reviewer 2

2.1 1) Maxim Likhachev's ARA* paper...

Maxim Likhachev's ARA* paper presents an elegant solution to avoid the final plateau problem for non zero-cost domains. His algorithm notes the cost of the goal, whenever a new path to goal is discovered, and concludes the search when the minimum cost of any state in OPEN becomes greater than or equal to the current goal cost ($f = f^*$). While this approach is not applicable for 0-cost domains, I think this merits a discussion and probable inclusion of results in case of other domains used.

We added a paragraph describing the relationship to ARA* in Related Work.

ARA* could largely avoid the problem of final plateau if the previous suboptimal searches happen to have found the optimal solution already (and thus pruning most nodes on $f=f^*$). However, this applies only to the iterated, real-time search algorithms.

2.2 2) the amount of data is a bit too much...

While I appreciate the in-depth experimental investigation presented in this work, i think the amount of data is a bit too much. For example, 26 plots for number of nodes vs depth is rather confusing. I like the summarization done for most tables, which points to the key take-aways. I think the experimental results should be presented in a more compact fashion, and if needed the detailed results can be pushed to an appendix (even there, i believe some compaction will be good). This will also help to reduce the length of the paper. Currently, it seems too long for the content.

We moved some tables to appendix.

2.3 3) the theory and analysis part... Section 5.3

While the paper presents experimental results in detail, the theory and analysis part looks weak in my opinion. Most of the analytical results are presented in an informal manner. For example, 5.3 discusses the completeness of search strategies on ZeroCost domains. I would suggest that such results should be presented using formal statements with proofs.

In the revised version, we moved Section [Original:5.3] to the end of Section [Original:8,Revised:7] and added more theorems/proofs. Although the proofs are mostly trivial, the new section improves the flow of the story and strengthen the significance of A*-as-sequence-of-SAT.

It is no longer in section [Original:5,Revised:4] because we noticed that infinite search space is irrelevant to having 0-cost edges. We had/gave a false impression that 0-cost edges induces an infinite graph, which is not the case. Trivially, just changing the cost of edges does not make finite graphs infinite.

2.4 TODO 3) the theory and analysis part... Section 6.1

Similarly, the analysis in 6.1 can be more precise, results in 6.1 can be presented in terms of theorems.

2.5 4) Sec6, "more nodes will tend to have shallower depth" vs disjoint forest model

In the last paragraph of section 6, it is stated that "more nodes will tend to have shallower depth than deeper depth" whereas the analysis in 6.3 assumes a disjoint forest model which i guess increases the number of nodes with depth. These two assumptions seems to be in contrast to each other. I think a more formal treatment of the analysis can allay such confusions for a reader.

We clarify this here as well as in the paper. We also added some figures for better understanding.

The *no-exhaustion assumption* assumes that no depth bucket exhausts due to the expansion. This implies that there are sufficiently large number of nodes in depth $d = 0$ so that depth 0 does not exhaust as a result of expansion. If FIFO default tiebreaking is used, it tries to expand all those nodes in depth 0 before expanding any nodes in depth $d \geq 1$. Similar situation happens at every depth. Thus, even if the entire graph is a forest model, FIFO causes a heavy bias to the shallow depth.

Indeed, if all nodes in the entire graph are expanded, there are surely more nodes in larger depth. However, the nodes expanded during the search process are biased to the shallower region.

In practice, the nodes in depth 0 are the nodes that were generated as a result of expanding earlier plateaus, i.e. the entire set of frontier nodes whose number is sufficiently large for FIFO to cause pathological behavior.

2.6 5) I think it will be helpful if the authors include pseudocodes for...

All the strategies proposed are explained in text only. I think it will be helpful if the authors include pseudocodes for their algorithms. In fact, i think it will be helpful if the authors present a basic A* algorithm with default tie-breaking and build upon that for their strategies. It will create a nice flow in my opinion, and use of pseudocode will also remove any chance of mis-interpreting the strategies.

Added pseudo code of Best-First search, as you suggested.

2.7 6) state/prove the properties of each of these algorithms, especially important ones like completeness

Tied to point 6, i think it would be good to state/prove the properties of each of these algorithms/strategies, especially important ones like completeness. The current format leaves a lot of un-answered questions like does depth-diversification ensure completeness (for infinite spaces). The answers may be obvious in many cases, however, i would still prefer if they are explicitly stated/proved.

We proved the completeness and its conditions as requested. See Question 2.3 also.

2.8 7) I like the idea of representing A* as a series of satisficing search. Here also, i would suggest inclusion of pseudocode.

I like the idea of representing A* as a series of satisficing search. Here also, i would suggest inclusion of pseudocode. For example, A* exhausts an f-plateau before moving on to the next one. While this is expressed in text, highlighting such properties through pseudocode may improve a reader's understanding. Similar to earlier cases, here also the authors can start with a basic pseudocode (for A* as a series of satisficing searches), and present their strategies on top of that with formal discussion about the properties.

Added pseudo code of A*-as-sequence-of-SAT-search, as you suggested.

2.9 TODO 9) Finally, I think it would be nice if we have some infinite spaces in the ZeroCost domains

Finally, I think it would be nice if we have some infinite spaces in the ZeroCost domains, and understand the impact of different strategies on them. My hunch is that in many cases people use fifo/breadth-first exploration to avoid completeness problems, i believe inclusion of such graphs (or some domains that closely approximate such behavior) will enhance the analysis.

Although we added proofs of the completeness of each tiebreaking algorithms on infinite search spaces, infinite search space and 0-cost edges are irrelevant. As we already noted, just changing the cost of edges does not make finite graphs infinite. Also, infinite graphs includes some continuous search problems, which is clearly beyond the scope of this paper.

The main topic of this paper is 0-cost edges and not infinite search space. We initially had/gave a false impression that 0-cost edges induces an infinite graph, but it was not the case.

Now, given that the main topic is 0-cost edges and not infinite search space, experimental results for infinite search space is out of scope of this paper, despite being an interesting avenue of future work.

2.10 minor comments

I think the abstract needs to be re-written to precisely state the contribution. In particular i would suggest changing the sentences after "With this in mind, ..". Somehow it seems that the depth diversification is the second strategy, which is not the case. Also, "We proposes" -> "We propose".

page 27, claim 1 "A Last-In-First-Out ..". Is this a general claim, or is it tied to the domains you tested on. I think this should be made clear.

Section 2, the 4th paragraph can probably be combined with the second. Also, may be it would be better if you present exact formal definitions of the terms.

I would suggest that you include some pictorial representation of your analysis in section 6.3. There are several illustrations of A* layers in other places that are helpful, some such illustration of your model would be nice.

There are a number of typos and grammar mistakes, please correct them. For example, "did not modified" -> "did not modify", "new current parent" -> "current parent", and others.

Thank you for the detailed comments, they are all fixed according to your suggestions.

3 Reviewer 3

3.1 1) There are a large number of colourful scatterplots in the paper, most of which would probably be better presented in a different form.

There are a large number of colourful scatterplots in the paper, most of which would probably be better presented in a different form. For example, the data in Figure 1.1 is essentially 1-dimensional: what we are interested in is the distribution or frequency of ratios between the size of the final plateau and the search space; a histogram or a cumulative distribution would show this more clearly. Whether colour- coding it for domains is useful is questionable; there's only a few points that can be distinguished well enough to identify what domain they belong to (and even those do not tell the full story, since there is no way to see where other instances from the same domain fall).

The data in Figures in 4.1, 4.2, 5.2 and 7.1 would similarly benefit from a more thought-through visual presentation.

The figure [Original,Revised:4.1] and [Original,Revised:4.2] should be in the present form. The role of these figures is to identify which domain is affected by the different default criteria.

The figure [Original,Revised:1.1] is paired with [Original,Revised:4.2]. If we change the format of [Original,Revised:1.1] from the current one to the histogram, then it loses the consistency with [Original,Revised:4.2].

Separating the figure into per-domain analyses would further increase the paper length.

However, we indeed benefit from converting [Original,Revised:7.1] into a histograms comparing the node evaluation ratio, because the domain characteristics is not important in this figure. Thank you for the suggestion.

3.2 2) The description in the early part of the paper (Sections 1, 3, 4, 5) somewhat convey the false impression that there has been no previous recognition of the challenge that plateaus can create for A* search

The description in the early part of the paper (Sections 1, 3, 4, 5) somewhat convey the false impression that there has been no previous recognition of the challenge that plateaus can create for A* search, in particular in the presence of zero cost transitions, or attempts to address it. There are a number of relevant related works, for example, those by Benton et al., and Cushing et al., which are cited somewhere in the paper, but do not appear anywhere in the initial discussion nor in the related works section. (The SoCS 2011 paper "Cost-Based Heuristic Search Is Sensitive to the Ratio of Operator Costs", by Christopher Wilt and Wheeler Ruml, may also be relevant.) This should be rectified; the previous state of knowledge should be clearly established early in the paper.

Previously, the issues of zero cost transitions were not directly associated with a failure in tie-breaking. Thus, previous work focused on how to modify the main evaluation functions (use of distance-to-go functions, inflating the heuristic value) or to modify the expansion order (e.g. Thayer and Ruml, ICAPS08).

Considering the flow of the paper, which start by examining the standard tiebreaking strategies on optimal search, then proceed to identify and connect the source of the problem with 0-cost transitions, inserting additional section around the beginning that discuss the suboptimal search would be out of place and unnatural.

3.3 3) This applies also to the summary of the authors earlier conference paper.

This applies also to the summary of the authors earlier conference paper. Rather than the "note" at the end of the introduction (which I assume the authors intend to remove from the published version of the paper), the summary of that paper, and the novel contributions this article makes over it, should be integrated in the presentation.

We fixed it as you suggested.

3.4 4) The argument in the last paragraph before Section 5.1 and the second paragraph of Section 5.1 do not make sense.

The argument in the last paragraph before Section 5.1 and the second paragraph of Section 5.1 do not make sense. First, the authors say they selected subsets of instances of some domains in order to avoid skewing the results by uneven instance set sizes; but then, these domains are excluded from the following analysis.

The analyses from which these instances are excluded are Section [Original:5.1, Revised:4.1] only. They are still evaluated in the later sections.

3.5 5) Furthermore in Section 5.1, why is the comparison done using the [f,h,fifo] strategy

Furthermore in Section 5.1, why is the comparison done using the [f,h,fifo] strategy, given that the experiment in Section 4 showed tie-breaking using "lifo" to be much more efficient?

The aim of this experiment is to show that there can be some performance difference for some planner, and we consider this is sufficient. Being the planner Fast Downward, which is currently the most successful state-of-the-art planner and by default uses the FIFO default tiebreaking, we consider using FIFO as a representative would be a reasonable choice.

Also, you can extract the numbers for [f,h,lifo] experiments from Table [Original:7.2, Revised:12.3] and Table [Original:7.4, Revised:12.5]. We obtained the same results using these numbers: The coverages in the original and Zerocost domains are similarly different.

3.6 6) In Section 6.2, the authors argue that ... pruning methods ... are somehow equivalent to tie-breaking. This is not accurate.

In Section 6.2, the authors argue that pruning methods such as symmetry or partial order reduction are somehow equivalent to tie-breaking. This is not accurate. Although a bias towards some states may be created by the presence of, for example, symmetries, as the authors argue, pruning the symmetric states does more than just "remove the bias". If the states in question have f-values that are less than the cost of the optimal solution, no form of tie-breaking will prevent A* from expanding all of them, but symmetry pruning will.

In the revised version, we clarified that pruning is a stronger technique than diversification.

3.7 7) In Section 7, Table 7.1 shows that there is little consistency in the results

In Section 7, Table 7.1 shows that there is little consistency in the results, particularly on the benchmark set in which only a few domains have zero cost actions. Table 7.2 shows that this is the case even on the Zerocost problem set, when considered by domain. This is worth more emphasis in the discussion. While the experiment shows that depth-based tie-breaking **can** be advantageous, it is by no means always the case.

The inconsistency is natural considering that the aim of diversifying the depth is to choose the **safest** practice in a domain-independent manner. Depending on the domain, the **best** practice may vary – for example, fifo is the best in airport-fuel with LMcut, while lifo is the best in freecell-move with LMcut. However, although these two default strategies may work well in some domains, it does more harm than good in many other domains, encountering the worst case pathological behavior.

This is previously addressed in section 6 in the original version:

"In the former case, fifo should perform well because... However, in the latter case, exhaustively searching the shallower depths can result in ... because ..."

In the revised version, we added a paragraph in the end of section 7 emphasizing and explaining the inconsistency you suggested.

3.8 8) I'm somewhat sceptical about the value of these figures... which of the examples are showing the failure of depth-based tie-breaking strategies.

I'm somewhat sceptical about the value of these figures. They show only examples of what can happen on isolated instances. Although such deep-dives may be useful to explain what is happening in different cases (particularly given the variance in the results), the volume and unclear selection of the examples make them less informative. (For instance, it is not clear which of the examples are showing the failure of depth-based tie-breaking compared to default tie-breaking strategies.)

The purpose of these figures is not to show the performance, but how depth diversification and other strategies follow the expected depth distribution. (Sec.7.1, "To understand the behavior of depth-based policies...")

In terms of performance measured by the number of expanded nodes, freecell-move p04 in Figure [Original:7.2, Revised:7.3], mid-right, is an instance on which lifo solved the problem with much smaller expansions than depth diversification. This can also be seen as the coverage difference in Table [Original:7.2, Revised:12.2].