Responses

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. Now the paper length excluding references and appendix is 39 pages. The numberings of the sections, figures and tables have changed due to changes made in response to the reviewers' comments.

In this response, when disambiguation is required, we use [Revised:X] to indicate that we refer to those figures/tables/sections using the new numbering system and [Original:X] to indicate the numbers in the original submitted 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, plateau(f), a plateau with respect both g and h, 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 "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 Dc? Is this a counter? Is there one Dc 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 the pseudocode (Algorithm 2) for 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.

Thanks for the suggestion. In order to factor away the constant low-level overhead of depth bucket management which degrades the performance of M&S on IPC domains, we compared the number of node evaluations between the depth-diversified and standard search method. We added Figure [Revised:6.2] comparing the cumulative coverage over the number of evaluations. This shows that the evaluations are almost identical for most IPC instances, meaning that search efficiency in positive cost domains was largely unaffected by depth diversification, as expected. There were differences in Openstacks (this is also as expected since Openstacks has zero-cost actions).

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.

As suggested, we shortened Section [Revised:4]. We also removed Table [Original:5.2]. (Same results can still be extracted from the other tables in the appendix. See [3.5] also.)

1.5 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 [Original:5.3] to the end of Section [Original:8,Revised:7], where it fits better (using the A*-as-sequence-of-SAT framework).

In response to item 2.3 below from Reviewer 2, we formalized some additional completeness results on infinite graphs, which was also added to this revised section [Original:5.3,Revised7.2].

1.6 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 formalized and significantly shortened the proof (Theorem 1), and Section [Original:6.1] has been folded into Section [Revised:6].

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 [Original:7.3,7.4,7.6] to the appendix: Figures [Revised:11.2,11.3,11.4]. Also, we shorten the text by removing the list improved domains.

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], compared "A*-as-series-of-satisficing-search" to IDA*, and added a paragraph connecting this introduction better as an introduction to subsections 8.1

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.

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

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

Fixed as 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. XXX - not fixed???

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 section [Original:2] and [Original:3] into section [Revised: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 (Section [Revised:9]).

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, ARA* is based on an iterated anytime framework, whereas our work is based on the standard (A*) admissible search. We point out this difference.

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 many tables and plots to the appendix, so the length of the main portion of the paper has been reduced to 37 pages (excluding references and 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.

We moved Section [Original:5.3] to the end of Section [Original:8,Revised:7] and added more formal statements regarding the completeness on infinite graphs. This material was moved because the analysis is most natural using the A*-as-sequence-of-SAT framework introduced in Section [Revised:7].

2.4 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.

We have formalized the result (Theorem 1) and made it more precise.

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.

To clarify: According to the *no-exhaustion assumption* , no depth bucket exhausts due to the expansion. This implies that there are a 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 with depth 0 before expanding any nodes in depth d >= 1. A similar situation happens at every depth. Thus, even if the entire graph is a forest model, FIFO causes a heavy bias to expanding nodes with shallow depth.

It's true that there are surely more nodes with larger depth if *all* nodes in the entire plateau are expanded, which is the case for $f < f^*$. However, in the final plateau of A*, FIFO expands only a fraction of nodes with depth $d \leq d^*$, where d^* is the *minimum solution depth*, the smallest depth of the solutions. Entire nodes above the solution depths ($d > d^*$) are not expanded due to the breadth-first behavior. During this process, the expanded nodes are biased to the shallower region.

This has been clarified in the text (Section [Revised:5.2]), and for further clarity, we also added Figures 5.2 and 5.3 which illustrate the scenarios.

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.

As suggested, we added pseudo-code for Best-First search (Algorithm 1), and depth-based tiebreaking (Algorithm 2).

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 in Section [Revised:8] (See also the response to Question 2.3).

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 8) distribution of goal depth in the final frontier

> I think it will be interesting to find out what is distribution of goal depth in the final frontier. I believe there will be a strong correlation between the goal depth and the relative performance of the strategies (which the authors mention), and it would be good to analyze this statistically. Similarly, for strategies in section 9, it would be interesting to find out the correlation between the performance of different strategies with the accuracy of the distance-to-go estimates.

We agree that goal depth distribution and distance-to-goal heuristic accuracy might be strongly correlated with tie-breaking strategy performance. This poses interesting avenues for future work, and may be very useful, for example, in an extension of this work which seeks to automatically select a tie-breaking strategy.

Thanks for these suggestions.

2.10 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.

In this paper, we focused on domain-independent planning in the classical planning framework (specifically, in the STRIPS/SAS+ framework), for which the search spaces are finite. Zerocost domains were created as variations of standard IPC benchmarks (which are all in this finite-space framework).

Empirical evaluation of tie-breaking strategies on infinite search spaces is an interesting avenue for future work, but since infinite search spaces are beyond the scope of classical planning, this will require careful design of interesting/practical benchmark domains and solvers.

We agree that completeness can be one good reason for choosing a fifo tie-breaking strategy. However, in our survey of papers mentioning tie-breaking strategies, we couldn't find any work which specifically mentioned fifo tie-breaking and also handled infinite spaces – the use of fifo which we cite in the paper is by Fast Downward, a classical planner, and as mentioned above, infinite search spaces are beyond the scope of the standard classical planning framework, so it's unlikely that the use of FIFO tie-breaking in FD was motivated by completeness concerns.

2.11 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.

As suggested we rewrote the sentences after "With this in mind. . . " to improve clarity.

> Also, "We proposes" -> "We propose".

Fixed.

> 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.

We made clear that it was observed on IPC domains.

> 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.

We have revised Section 2 to be more precise. We have also added pseudocode for best-first search (Algorithm 1), which should further clarify the meanings 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.

As suggested, we have added Figures 5.2 and 5.3 to clarify the analysis in Section [Original:6.3; Revised:5.2]

> 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.

We have rechecked and corrected spelling+grammar.

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.

As suggested, we converted Figure [Original:7.1,Revised:A.1] into histograms comparing the node evaluation ratio, because this is essentially 1-dimensional data and the domain characteristics are not important in this figure. Thank you for the suggestion.

We also considered converting Figures 1.1, 4.1, 4.2 into histograms as suggested. However, we concluded that they should remain in the present form, because the color-coded domain information in Figures [Original:4.1,Revised:3.1] and [Original:4.2,Revised:3.2] are important for highlighting the domains which are affected by different default criteria.

We acknowledge that these figures are crowded and it's hard to distinguish many of the domains/points. However, this was the best compromise of information-vs-space we could come up with, as providing full, per-domain data would make this paper even longer.

While the domain information in Figure [Original,Revised:1.1] is less important, and if it was a completely independent figure, it may be a good idea to convert it to a histogram as suggested. However, Figure [Original,Revised:1.1] is intended to be contrasted against Figure [Original:4.2,Revised:3.2] , and as explained above, we believe Figure [Original:4.2,Revised:3.2] should remain in its current format because the domain information is important. Thus, we'd like to keep Figure [Original,Revised:1.1] as is. Similarly, we believe Figure [Original:5.2,Revised:4.2] should remain its current format for the same reason.

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.

In Section 1, we added a reference to Benton et al 2010 in Section 1 when 0-cost actions are first mentioned, noting that these induce the g-value plateaus which are discussed in the Benton et al 2010 paper.

In Section [Revised:4], when zero-cost domains are motivated and described, we added the statement that the huge final plateaus are instances of g-value plateaus described by Benton et al 2010.

At the beginning of Section [Revised:4.1], we added references to works which have mentioned the difficulty of 0-cost domains (Thayer et al 2009, Cushing et al 2010, Wilt et al 2011, Thayer et al 2011, Richter et al 2011). We clarify that 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).

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 integrated the comparison with our earlier conference paper as a new paragraph at the end of Section 1, in a form similar to those of other recently published JAIR papers.

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 paragraphs in the last paragraph before Section [Original:5,Revised:4] define the set of 28 Zerocost domains used throughout the rest of the paper, and we explain why for some domains (specifically, blocks, freecell, pipesworld-notankage, miconic), we selected subsets of instances in order to avoid skewing coverage results.

Blocks, freecell, pipesworld-notankage, and miconic were NOT included in the experiment in Table [Original:5.1,Revised:4.1] because the purpose of that particular experiment was to compare coverages between Zerocost domains and their corresponding original IPC benchmark domains, and for this particular purpose, we wanted to avoid confusion (particularly for readers familiar with the IPC instances) by only including domains where the number of instances in the Zerocost domains is the same as in the IPC benchmark set.

However, the Zerocost versions of Blocks, freecell, pipesworld-notankage, and miconic are used in all of the other experiments in the paper involving Zerocost domains. (because none of the other experiments involve comparisons between coverage on Zerocost domains and coverage on standard IPC domains).

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?

We used the [f,h,fifo] strategy in this experiment in Section [Original:5.1,Revised:4.1] because we use the Fast Downward planner, which is currently one of the most widely used state-of-the-art planners and Fast Downward uses the [f,h,fifo] tiebreaking strategy by default. Thus, we believe using the default configuration for Fast Downward is a reasonable choice, since the purpose of the experiment was to demonstrate that Zerocost domains pose a challenge for state-of-the-art planners.

Although not prominently featured in Section [Original:5.1,Revised4:1], the results for [f,h,lifo] can be extracted from Table [Original:7.2, Revised:11.3] (Zerocost instances) and Table [Original:7.4, Revised:11.5] (IPC instances). Qualitatively, the results for [f,h,lifo] are similar to that of [f,h,fifo] – Zerocost instances are "harder" than their corresponding IPC instances.

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 <u>more</u> 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 Section [Original:6.2,Revised:5.1] 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 considerd 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.

Each tie-breaking strategy has advantages and disadvantages depending on the domain. These trade-offs and pathological behaviors are explained in Section [Revised:5], and Depth-based tie-breaking is designed to avoid pathological behaviors (Section [Revised:5]). While this results in strong **overall** performance, other strategies may perform better on any given domain.

We added a paragraph before the start of Section [Revised:6.1] clarifying this point.

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 [Figures in Section [Original:7,Revised:6]. 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.

The purpose of the figures in Section [Original:7.1,Revised:6.1] which show the number of nodes expanded per depth in the final plateau is to show how the behaviors of depth diversification and other strategies follow the theoretical analyses in Sections [Original:6-7,Revised:5-6]

We have significantly reduced the volume of the figures in Section [Revised:6.1] by moving the majority of the figures to the Appendix.

> (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.)

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:11.3].