

JATIS Manuscript 23098G: Responses to reviewer comments

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We have significantly revised the paper in response to reviewer feedback, focusing in particular on reducing the length of the paper and eliminating elements of the original text that are redundant with our earlier paper or with other locations in the manuscript. The main body of the manuscript is now 24 pages long, excluding appendix material, down from the original length of slightly more than 32 pages. Our responses to Reviewer #2's comments are included below.

While proofreading the manuscript, we discovered two slight errors in our description of the experiments performed, which we have rectified in this version:

- Line 272 (current version): the value of the Tikhonov regularization was $\alpha_{\text{probe}}^2 = 0.7$, not $\alpha_{\text{probe}} = 0.7$.
- Lines 293–294 (current version): in our experiments with EFC and AD-EFC, we used the chosen value of α_k for the first 30 iterations, and then set $\alpha_k = 10^{-1}$ after iteration 30 in all experiments. The previous version of the manuscript erroneously indicated that the value of α_k was held constant throughout all 80 iterations of each experiment.

Neither of the two corrections above affects the primary finding of this paper; namely, that AD-PSM and AD-EFC have equivalent contrast performance to SM and EFC, respectively, when operating under the same conditions.

Comment 2.1

Line 60–61: Awkward phrasing and could be condensed: i.e., “which is proportional to both the DM actuator count, and the dark-zone pixel count.”

Response: We have simplified the phrasing on this line.

Comment 2.2

Line 88–96: What is the bandwidth? Central wavelength provided indicates that it was not monochromatic.

Response: We have added information about the operating bandwidth for the Decadal Survey Testbed, HabEx, and LUVOIR.

Comment 2.3

Lines 103–113 could be condense as it is a reference to another paper. For example:

Our proof-of-concept simulations with AD-PSM indicated superior computational efficiency and comparable starlight suppression performance in comparison with SM, for DM formats as small as 50×50 actuators. While the CPU time and memory consumption of SM grew superlinearly with actuator count, the increase in both for ADPSM was negligible (i.e., for 128x128 actuators AD-PSM used 95% less memory and CPU time) suggesting that iterative methods are a promising alternative to analytical techniques.

Response: We have condensed this paragraph.

Comment 2.4

Notation is clearly explained though for myself I struggled to see the difference between boldface and non-boldface with the font used by the journal. Would be good to look at different options.

For equation 1 uses J as a variable which is later used to denote the Jacobian. Either already define it as the Jacobian or use a different variable. Do not want to redefine what a variable is within a given paper.

Response: Throughout the paper, the variable J denotes cost functions, whereas the Jacobian is represented by \mathbf{G} . This notation is consistent with our 2021 JATIS paper, and with the important 2016 review paper on the subject of focal-plan sensing and control, Groff et al, “Methods and limitations of focal plane sensing, estimation, and control in high-contrast imaging,” JATIS (2016). Our convention to use boldface for multidimensional quantities (vectors and matrices) and non-boldface for scalars is also consistent with our 2021 paper. Since this paper is a follow-on to the work described in our 2021 paper, we feel that it is important to maintain as much consistency as possible between the two.

Comment 2.5

Line 151: “DM imparts a phase-only correction” – with the plural you are suggesting your system has more than 1 DM and then you can correct for amplitude as well.

Response: That is correct. Our experimental setup includes two DMs, and the background in Section 2 assumes two-DM control. We have slightly adjusted the wording in the following sentence to make it more clear that the entire paragraph assumes two DMs.

Comment 2.6

Line 210–213: Repetitive. This is already established in the intro (lines 103-113 and potentially other places) that iterative methods like AD-SM are more attractive.

Response: We have removed this entire paragraph to reduce repetition.

Comment 2.7

Line 243: should directly reference the equation 8 is the solution to equation 6.

Response: We have reworded this line and added a reference to Equation (6).

Comment 2.8

Line 248: “finds the solution directly” → confused here how the iterative solution is considered to find the solution directly (that language suggests an explicit closed-form solution as we have for SM via equation 8). Instead, it converges to the solution.

Response: This section was heavily revised as part of the overall condensing of Sections 1 and 2, and is now clearer about the nature of the solution.

Comment 2.9

Main reference to HiCAT testbed directly at the start of the section (not just most up to date results at the end).

Response: We have moved the references as requested.

Comment 2.10

Line 322: “handwritten adjoint model” please change to something like: “including a custom analytical model/expression developed by the authors”

Response: We have revised this sentence to say “hand-derived”.

Comment 2.11

Figure 6 (a) label the pupils on the plot. Expand to fully include the pupils (unless this is set to +/-10 mm for a specific reason, then please explain).

Response: We have labeled the pupils on the plot as requested. As for the extent displayed, we chose this field of view to be large enough to make edge of the DM mask visible, but still small enough that the detail of the other masks was easy to see, since they are much smaller than the DM mask.

Comment 2.12

Entire section could be reduced as this appears to be the same method used in 2021 paper. If different please focus on explaining those differences and why they were made. Otherwise a quick overview plus reference to other paper suffices for this section.

Response: We have condensed this section as requested.

Comment 2.13

How does this compare to the typical way PPP is done on HiCAT?

Response: At the time we conducted these experiments, this was our typical approach to pairwise probing on HiCAT. In this paper, we used AD-EFC to calculate optimal probes instead of EFC so that the wavefront sensing & control loop would be entirely Jacobian-free.

Comment 2.14

Fig 9: Would be nice to see the difference between the two results in a third row as by eye they look the same.

Response: In the interest of condensing the paper, we have removed this figure. The probes are already shown in Figure 8, and as pointed out by the reviewer, the probe differences are too small to be visually discernible.

Comment 2.15

Figs 10–12: I would say by eye that convergence isn't achieved by any of them at the iteration the authors have provided. Would be useful to have the median, 10th and 90 percentiles for the reference non AD methods but maybe pick a different color than gray.

Response: We have updated the plots to be clearer, and as discussed in the comment below, we have amended our analysis to simply consider the final 50 iterations of each experiment.

Comment 2.16

Lines 431–435: you are not explicit enough with what your convergence criteria is for the data. Reading this paragraph you use the 10th and 90th percentile for visual representation but not as your criterion. I would by eye it is not yet converged. An alternative would be to say I know convergence happens somewhere between 10 and 50 iterations for all of them. That means the last 20 iterations are fully converged so use those to calculate the median and percentiles.

Response: We had used a convergence criterion based on the local rate of change of the contrast time series; when the improvement in contrast iteration-over-iteration decreased below a particular threshold, we considered the experiment to be converged. However, as you point out, this criterion appeared to be too optimistic, and considered the runs converged too early. We have therefore simplified our analysis and considered the final 50 iterations of each experiment to represent the steady-state contrast. We have accordingly updated Section 4.

Comment 2.17

Fig 13: left plot most left point matches to Fig 11, correct? Would be good to point that out in the caption for reference. Would be nice to have an example plot like Fig 11 in an appendix showing different runs like $\eta_{00} = 100$ & $\varepsilon = 10^{-2}$ as SM does better and has a tighter spread. Same for Fig 14.

Response: We have added comments to Figure 13 and 14 explicitly pointing out the correspondence of two of the test cases to the experiments shown in Figures 10 and 11. We have also added a figure to the appendix (Figure 13) showing the contrast vs. iteration curves for all experiments.

Comment 2.18

Lines 480–484: Would be nice to expand on the analysis component or discussion of stability and how it would relate to the final contrast (which is the ultimate metric here). If you are able to achieve the same contrast with both methods but one has a smaller variance, then you would pick that method, correct? Could add to discussion here as it currently focuses on compare one method to itself under different conditions. Are there specific limits/specifications that HabEx or LUVOIR would have on the variance of the spatially averaged dark-zone contrast? Reference Figures again as you talk about different results in this paragraph. Hard to keep track.

Response: In practice, the variance of the contrast time series is a secondary consideration to the deepest-achievable contrast. In a real spaceborne observatory, unlike in these experiments, the wavefront sensing & control loop would not run in steady-state to maintain deep contrast after convergence, or at least not with the algorithms described here. Rather, during science observations, the stability of the dark zone would depend on a combination of (a) observatory thermo-mechanical stability, (b) specialized wavefront sensing/control algorithms designed to maintain deep contrast during science observations (c.f. linear dark field control, extended Kalman filter-based dark zone maintenance), and (c) advanced PSF calibration/postprocessing algorithms. Our motivation for reporting 10-90th percentile bounds for steady-state contrast was purely to demonstrate that the performance of AD-based algorithms is statistically equivalent to Jacobian-based algorithms in terms of deepest-achievable contrast. In other words, if one were to turn off the wavefront sensing & control loop and begin science observations sometime after iteration 30, the dark-zone contrast would fall somewhere within the bounds for each algorithm.

With regard to choosing one algorithm over the other: the variance of the deepest-achievable contrast, and the stability of the control loop, is tunable via the choice of loop parameters (η_0 , α_k , ϵ), so there would be no practical benefit to choosing an AD-based algorithm or a Jacobian-based algorithm on this basis.

We have simplified the discussion section to make it easier to follow.

Comment 2.19

Why did you select 10th and 90th percentiles? Why not just the variance or standard deviation?

Response: We chose to use the 10th/90th percentile to capture both the mean and spread of the steady-state statistics without assuming any specific distribution or symmetry.

Comment 2.20

You are able to reach the contrast floor imposed by environmental factors which is great. Does the AD-EFC and AD-SM behave differently than EFC and SM if you were to have DM drifts (we know that currently BMC DMs don't suffer from drifts but imagine if they

did like other ground-based DM with larger stroke) or other dynamics from hardware? How robust would you be against those in this case? Do you have any further constraints on hardware that maybe SM or EFC do not have but happen to be meet by the HiCAT testbed. I don't immediately see any but think it is important to consider briefly.

Response: There is no difference between AD-EFC/EFC and AD-PSM/SM with regard to behavior in the presence of DM drifts. The robustness of the control loop using an SM-type or EFC-type algorithm with respect to drifts or other instabilities is beyond the scope of this paper.