

Paper: Interactive obstruction-free lensing for volumetric data visualization

We have carefully read the four reviews of our submission, and we are thankful for the high level-of-detail that the reviewers took time to provide. We have covered all the indicated points, and the current letter describes our answers to them (and, where needed, modifications done to the paper). We believe that by these changes we have managed to considerably improve the focus, quality, and positioning of our work. To help reading, the original comments are in plain text. Our answers are in *italic blue*. For brevity, we do not mention below the correction of small-scale errors such as typos and word order, nor the comments which did not require a response.

Sincerely,
The Authors

1 Summary

1) Requirements R1 through R4 are not properly described. How does the method proposed in this paper meet these four requirements? Please consider each reviewer’s comments / requests for clarification related to R1 through R4.

TBD.

2) The authors need to compare their method to other F+C methods. Do these previous methods meet the four requirements? (one reviewer suggests including this information in a table).

Good suggestion. We added in Sec. 2.3 a table summarizing how other F+C methods discussed in the related work address (or not) R1..R4.

3) The authors claim the method is rapid and easy to use although there are not sufficient user studies to substantiate that claim. The authors should point out the limitations of the evaluation and refrain from making unsubstantiated claims.

Agreed. We address this point in the revision by (a) revising the claims made and refraining from making strong ones where there is no sufficient evidence; and (b) adding an user study (executed in the revision period) for one of the five use-cases, namely the baggage inspection (Sec. 5.1).

4) The reviewers point out missing references. These must be added.

TBD.

5) Several reviewers point out typo / grammar errors. These must be fixed.

Fixed, thank you for this type of detailed feedback (!)

2 Review 1

The authors’ contribution as a ‘technique’ is not novel. Focus+Context and lens- based distortion are well-studied techniques.

Furthermore, the paper discusses the application of their ‘technique’ in different scenarios with different types of datasets, however, there lacks a comparison (pictorial or quantitative) to justify why their method is more effective than the already existing techniques for each scenario.

*Indeed, we do not present pictorial or quantitative comparisons with existing techniques for **all** the five use-cases. As also R4 noted, this would be unfeasible (both with respect to the revision time-frame but, equally or even more importantly, given the fact that implementations of many related F+C techniques that would fit the respective use-cases and datasets are not readily available). However, we do provide various types of comparisons, as follows.*

- *For the baggage inspection (Sec. 5.1), we executed an user evaluation in the revision time-frame and added its qualitative and quantitative results to the respective section.*
- *For the 3D fluid simulation (Sec. 5.2), we mention that the circular vortex that we found with our lens was not found by five techniques that we are aware of which used the same dataset (see references in the text). One of them (added in the revision) is actually also a F+C technique. Of*

course, this is only indirect evidence; however, we believe it supports at least partially the claim that our technique was useful in discovering something unknown in a well-known standard dataset widely used in 3D fluid flow visualization.

- For the CT chest tumor use-case (Sec. 5.3), we have had two medical specialists (radiologist and pulmonologist) use two techniques for the same type of problem (slices and classical DVR) and compare them with our lens technique. They commented on the added-value of the lens technique. We fully agree that this is indirect evidence (since they did not use another lens technique, but non-lens techniques only).*
- For the air trails use-case (Sec. 5.4), we used FromDaDy, which also includes F+C and filtering techniques, to isolate the eight-shaped trail outlier, and explained in the text that the new lens is more effective than such techniques. We fully agree that this is only qualitative evidence only. To clarify this point, we added extra explanations as to the limitations of the comparison in the text, especially in the discussion section.*

The initial requirement of the target to be 'partially visible' for the technique to work weakens the uniqueness of the paper.

*Actually, the target does not need to be partially visible; it **helps** if this is the case, since one can then de-occlude and bring-in-focus such a target very easily but, as the DTI use-case shows (Sec. 5.5), the target does not need to actually be visible. One can point the lens at some location in the 2D rendered image and 'dig' to some arbitrary/desired depth with it. We made this point clear(er) in that section and the discussion, this was indeed not clear in the original paper.*

The authors' contribution as a 'technique' (Focus+Context and lens-based distortion) are well-studied techniques and not novel. However, the application of these existing techniques for the visualization of occluded targets in volumetric datasets is an interesting approach. Furthermore, the idea of interactively modifying exploration parameters, such as transfer function, lighting, zoom, and rotation, limited within the target lens is (to the best of my knowledge) new work.

See above. Maybe we can fix this by calling the whole thing a refinement of a lens technique, or something like that; I don't quite get where the problem is, since R2 already says there are new technical elements, so.. where's the problem?

Secondly. the paper is lacking formal justification of their work by comparing their results to existing methods. [...]

We addressed this in the revision by adding a summary table listing requirements vs selected relevant F+C techniques (see Sec. 2.3 and related comment of R1). Also, we outlined here the amount of similar comparisons done by existing F+C papers. This way, we hope that our relative position (both in contributions and amount of comparison) with respect to existing work is more clear.

Since the paper claims that the technique could be applied to general datasets (and shown in section 5), the authors need to justify how their technique is better than the specific scientific visualizations resolving for occlusions.

We are not sure what the 'specific scientific visualizations' here refer to. If these are alternative visualization techniques that aim to look at the same datasets we consider (to treat the same use-cases), then we believe that we do this, see our comment with respect to the comparisons for R1.

A minor point: even though the authors have had discussions with experts in the field for each application scenario (section 5), a formal user study between different methods and their technique would strengthen the paper's claims.

In this direction, we added a (more) formal user evaluation for the baggage inspection use-case (Sec. 5.1) to the revised paper.

One major change that could really strengthen the paper would be to eliminate the need for the target to be 'partially visible'. If the user is able to 'poke a hole' into the volume and scroll through the volume to fix a target, it would greatly add to the paper's contribution.

Actually, this was already possible from the very beginning (!), see the comments above related to the DTI use-case (Sec. 5.5), which are precisely this situation. It seems we didn't explain this clearly, but opening a hole to see the corpus callosum is done just this way. We rewrote that part of the text and the

discussion to explain this.

- Revise the subsection "Opacity" in Sec 3.3. Why did you choose 150 points?

Explanation added in the text. We tried, indeed, with various values; higher...

For voxels having similar densities how will the voxels outside B become 'transparent' for TF_{global} ? Needs more explanation. If you change the TF_{global} set by the user, then you violate the consistency of the global context.

*The explanation was not clear. What happens is that we **never** change the user's TF_{global} outside the lens (as the original text actually said). We only make the effective transfer function used **inside** the ball B to be more opaque as one approaches the center of the ball B. This way, even if the user set a TF_{global} which makes things outside of B quite transparent, within and close to B's center we make structures increasingly more opaque. This way, we are sure that items in the lens will never get so transparent as to be hard to see (which would defy the purpose of the lens). We adapted the explanation accordingly.*

Link each subsection of Section 3 to Fig 2 for a diagrammatic explanation.

Good idea, done. We also added the final step of local lighting and viewing direction adjustments to the pipeline.

3 Review 2

Their implementation appears to work for the demonstrated use cases. The authors claim that the design of the lens makes ray parameters easy to manipulate, but the user feedback is too sparse to support their claim. In addition, there is no comparison with prior focus+context methods in the described use cases.

To address this, we added a user study to being more evidence to the ease of use and general user feedback for one of the five discussed use cases (Sec. 5.1).

First, the authors claim the proposed technique meets 4 requirements, but the requirements are not stated in enough detail. As such, it becomes confusing when prior work is discussed in the framework of these requirements.

Agree. TBD: refine reqs

Second, the use case evaluation relies too much on insufficient user feedback. If a full user study is not possible, then there should be other forms of evaluation.

Agree, we can do better. See the added user study referred to above.

1. Please clearly explain the 4 requirements R1 through R4 before evaluating prior work under these criteria. Specific points as follows: 1a. It is not clear if R1 emphasizes "unobstructed" or "rapidly". Also, is it rapid due to a good user interface (in which case a user study might be needed to substantiate the claim), or is it due to computational performance?

1b. R2 needs to be justified: Why is it desirable to be able to change *all* parameters?

1c. R3 is confusing because "global" could include both focus and context, therefore the meaning of "global context" is not clear.

1d. R4 can be better re-stated as handling datasets where targets and occluders can not be separated by transfer functions, which may depend on more than densities.

TBD!

2. Regarding R3 and the preservation of the context: In the proposed method, the user is sometimes required to peel away the context by adjusting transfer functions until there is a line of sight to the target, as is shown in the supplemental video. This does not preserve the context. There should be a justification on how this adjustment does not violate R3, or there should be a discussion in the limitations subsection.

As explained above (see the comments for R1), we do not necessarily need to see a part of the target to activate/use the lens. This use-case is however the most interesting, we believe, so this is why we focused mainly on it (Secs. 5.1-5.4). However, as explained, one can activate/use the lens to 'dig' in a volume even at some location where there is originally nothing visible like a fragment of some target. This is the use-case in Sec. 5.5. We added these explanations to Sec. 5.5 and the discussion and also adapted the introduction and abstract to reflect this clearer.

3. The user feedback described in section 5 is not enough to substantiate the claim that the proposed technique works rapidly and is easy to use. Either the claims need to be adjusted, or formal user studies need to be conducted. 3a. Indeed only 3 out of 5 use cases have user feedback, and the other 2 lack evaluation.

Correct. As explained also in response to R1, we (a) softened the claims where we cannot make them hard, and (b) added a user study for the baggage inspection scenario. While not exhaustive (no user study will probably be when evaluating and end-to-end application...), this brings more support to our claims.

4. If space permits, the prior work and the requirements they meet should be organized as a table. *Good idea (!) We added this table to Sec. 2.3.*

5. The scattered rays intersect un-distorted rays outside of the lens. This potentially causes duplicate sampling. Please explain if this is an issue, and how it is suppressed.

This is indeed potentially the case. We did not notice any visible artifacts in our usage of the lens (nor did the 8 airport security experts, plus 2 medical specialists, plus the air-traffic controller expert, who used our tool) which can be ascribed to such ‘crossing’ rays. The explanation can be in the fact that (a) the number of crossings (voxels samples by 2 or more crossing rays) is small and (b) the compositing TF being used essentially low-pass filters the potential artifacts. We added a comment to this end.

6. Section 5 is too verbose for its content. Long paragraphs should be reorganized into shorter, more focused ones.

Agree, shortened/compacted.

7. Missing relevant prior work to which the proposed approach should be contrasted: "A curved ray camera for handling occlusions through continuous multiperspective visualization. Cui et al., 2010" "Multiperspective Focus+Context Visualization, Wu et al., 2016"

TBD

4 Review 3

However, while the coverage of the related work generally is fair and equitable, I do find that some of the existing occlusion management techniques have not been properly covered [...]

Indeed, the reviewer is right, there are more papers in this area. We had to somehow choose a balance between size (of the related work section) and level of detail. We have added the citations suggested by the reviewer to the discussion of related work.

One question which is not fully discussed in this paper is if it ever would make sense to decouple the viewpoint from the lens? At least, it seems an academically interesting exercise, perhaps only for demonstrating the work in the paper, but I didn't see it mentioned.

Yes (!) Actually we toyed with this idea during the various design/implementation stages of the lens. It is not that this cannot be easily implemented. The problem is that we found it confusing (in terms of usage), and one of our ‘silent’ requirements was to have a simple interaction design. Yes, this can be done, but it does not seem (at least, if we understand correctly what the reviewer had in mind) to really add significant flexibility atop of the already existing mechanisms, see especially the local viewpoint change. We thought of adding this to the discussion, but given the space limits, and the other more pressing concerns we saw in the reviews, we think that we'd better dedicate the available space to the other concerns.

Unfortunately, the companion video with the paper only shows two of these examples—all five would have been better.

We feel there is a soft limit to what a watchable video (for this type of paper) would be, and there are also size constraints to the additional material submission (at least, if we want a good quality). We surely see the point, but explaining in sufficient detail what all five use-cases really are and want to achieve in a self-contained video seems to be just a bit too much, so this is why we chose to stay with only two use-cases. We hope the reviewer understands our choice and will not see this adversely (!)

5 Review 4

The authors do admit that a comparative study is outside the scope of this paper (due to the complexity of having an implementation of multiple methods to work with). However, the authors do not perform enough basis analysis (from principals presented in the papers) to show the advantages of this proposed method over previous work.

*This is a hard but fair point, we of course have thought about it. The key problem was/is not so much the time or effort, but being **sure** we could reimplement existing F+C techniques (at least, state of the art ones which are the most relevant for comparing against) is not just time consuming, but it is often just not doable because there are so many ‘implementation-level’ details missing in their description. That is: we could choose indeed technique X or Y from the literature and aim to replicate it, but especially for interactive-lens techniques, this is extremely hard, since there are usually many details being glossed over. As such, how would we know we are ‘really’ replicating technique X or Y in our implementation? And, if we did some mistakes in our implementation (as opposed to the original X or Y), how can we then justify that our evaluation of X or Y leads to unsatisfactory results? Would we then evaluate the inherent limitations of X or Y, or our interpretation of how they are to be implemented? This, we believe, is a great(er) problem of evaluation in interactive infovis, for which we do not have a ready answer. But this is a very fair point. We mention it explicitly in the discussion section.*

In section 4 the authors indicate that they used a compositing ray function but that any other ray function could be used instead. I am not sure that is true - this wouldn’t necessarily (or at least not obviously) work with something like minimum / maximum intensity projection.

*Indeed, we cannot claim **any** other ray function would be directly usable (though, we do not see why not, since all we do is modify the paths of the rays, and not how encountered voxels are treated along such a ray). But, since we indeed have no formal proof this is the case, we removed the respective claim from Sec. 4.*

The authors should significantly expand their definitions of the requirements R1 through R4. Currently these are covered only in passing in the introduction with very vague definitions (what is rapid? what is easy? what are the parameters?)

TBD

The authors need to revisit these four requirements at some point in the paper to show how this newly proposed method meets all the requirements yet prior methods fail for one or more of these requirements.

This is done in two parts. First, we added a summary table of relevant related methods and how they behave vs the listed requirements (see earlier comments to R1..R2). Secondly, the way that our method relates to R1..R4 is discussed in Sec. 6 (discussion).

Overall, the organization of the paper is a bit odd. Important concepts (such as the requirements) are covered only in the Introduction, and the advantages to the proposed method are covered in the section on Related Work (before the method is formally presented). The paper organization should be revised to help clearly convey (and identify!) the important information in the expected sections (no one expects that an idea covered in just one sentence in the introduction is key to the paper).

We did not see how else we can introduce requirements else than before discussing the related work, since the related work is to be reflected vs the requirements. As such, requirements have to be listed before the related work, either in the beginning of Sec. 2 or in Sec. 1. We believe it is better to do this in Sec. 1 so as to make the scope of the paper clearer (otherwise, we cannot state in Sec. 1 what we aim to achieve). The advantages of our method are indeed listed in Sec. 2 so as to contrast what we aim to do vs related work (otherwise Sec. 2 would look like a passive enumeration of papers, and it would not be clear what we are aiming to improve upon that). However, we also re-discuss our work vs the requirements in the discussion section (Sec. 6), when the reader already knows the proposal. However, we fully agree that the requirements listing in Sec. 1 was too brief, so we refined this, in line with comments from other reviewers too.

Much of the paper is devoted to covering five case studies in detail. These are great - but do not clearly convey how this particular method is better than any other F+C method. I realize that a full comparison would require an implementation of several other methods and a user study (rather than just something closer to a demo with feedback) - I am not suggesting this (it is infeasible in the revision

time frame). However, in each of these case studies the authors should point out how some novel aspect of this method was critical to the successful visualization of the hidden object.

We think we are doing this (and we have tried to improve upon this), as follows. Briefly put, we can do two types of evaluation: (1) an ‘absolute’ one where we show that our method covers well the goals/constraints of some task; or a ‘relative’ one where we show that our method does better than similar methods for a given task. As mentioned earlier (also see comments for other reviewers, and our added text in the discussion section), doing (2) simply requires we have access somehow to a correct and complete implementation of competing methods. As explained earlier, this is unfortunately not the case (and replicating such methods is not just complex, but risky, since details are missing, so we may just produce a suboptimal or even wrong implementation which would lead us to the wrong evaluation conclusions). As such, we can only do (1), which is what we do in the five use-cases in Sec. 5. We refined such insights by adding one detailed user evaluation for the baggage inspection use-case (Sec. 5.1). As stated earlier in this letter, we are fully aware this is not a bulletproof evaluation, but we believe it adds more evidence to the added-value of our proposal in the context of a (1)-type evaluation.