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.*

How does the technique differentiate between 'target feature' and 'occluding regions', that is to say, how is spatial connectivity resolved?

TBD ???

The fact that the paper focused on the claim that the technique is novel (which is not the case) reflects my score. However, if the authors write it as an application paper, I will be inclined towards accepting the paper.

TBD: Do we want to do this really?

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.

ALEX: Yes, but this is what we can really do, cannot we?? See the DTI example.

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

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

3 Review 2

The authors proposed a technique that deforms sampling rays in a raycasting renderer to route the rays around occluders. The strengths of this technique is that it is generally applicable to various volumetric data sets, and that it incurs very low performance overhead over existing raycasting algorithms. The weaknesses would be the requirement that the target be at least partially visible, and that the context region is greatly disturbed such that much information is lost. The authors discussed both points in the text.

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.

The Review

The authors presented a novel technique: a lens for routing sampling rays around occluders to reach the target. The target must be partially visible, and the visible part is used for initializing the lens parameters. The parameters are adjustable by the user in real time. Their technique is demonstrated to work with various types of volumetric data sets.

The paper is easy to understand. The description of the technique is detailed enough to be reproducible. The figures and the supplemental video describe well the technical detail and the user interface.

However, there are some important issues that remain to be addressed. 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. 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. The technical novelty is slightly limited due to following an existing paradigm of deforming sampling rays. Therefore, it is important to evaluate the system as a whole.

Revisions required: 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. 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. 3. The user feedback described in section 5 is not enough to substantiate the claim that the poposed 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. 4. If space permits, the prior work and the requirements

they meet should be organized as a table. 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 supressed. 6. Section 5 is too verbose for its content. Long paragraphs should be reorganized into shorter, more focused ones. 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"

4 Review 3

A nice but limited contribution which is somewhat curtailed by existing work in the literature. There are some missing citations that need to be rectified, but overall, this is solid work.

The Review

This work proposes a focus+context technique for eliminating occlusion in a volumetric data visualization where the user simply defines a position, size, and depth for a cylindrical cutout area. The contents of the lens can be modified, including field of view, direction, lighting, etc, allowing for additional utility.

The idea is properly motivated, with the requirements of the new technique elicited into four specific requirements (R1-R4). This makes a lot of sense.

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. For example, the paper brushes X-ray techniques aside, but Coffin's perspective cut-away views (3DUI 2006) is quite relevant to this work, as is Elmqvist's dynamic transparency (INTERACT 2007). Similarly, while MoleView is cited, 3D explosion probes such as Sonnet's (AVI 2004) or McGuffin's (VIS 2003, cited) also deserves mention, particularly because this explosion idea is so central to this work. There is more on 3D explosion in the virtual reality literature, where such ideas have interactive appeal.

Given this literature, which is relatively ample, my feeling is that this paper is novel solely because of its "flexible and real-time interactive modification of the focal point", which is a novel and powerful idea. The idea of using screen- space raycasting to determine the hole to push through the surface, as well as the selection of a visible part of the target to determine where to place the focal point, is interesting as well. These are relatively small contributions, but to my knowledge they extend the state of the art in a quantifiable way. 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.

Validation here is performed in five application scenarios where the prototype implementation is employed for different example datasets. This is reasonable and shows the utility of the work. Unfortunately, the companion video with the paper only shows two of these examples—all five would have been better.

Overall, the paper is well written and easy to follow, with just the amount of necessary mathematical formalism to explain the idea. The illustrations are nice, as are the screenshots.

In summary, this is a nice paper with a small but reasonable contribution—small, because virtually all of the ideas except the local lens controls are already present in the literature. Despite this limitation, I lean towards a positive verdict of the work, as it fills an important and useful gap in the literature. For this reasons, the "required changes", were the paper to be conditionally accepted, must be rather limited: mainly, the authors are encouraged to add the missing literature I mentioned above, and potentially add all five examples to the video.

5 Review 4

¡b¿3 - Possible Accept¡/b¿¡br/¿ The paper is not acceptable in its current state, but might be made acceptable with significant revisions within the conference review cycle.¡br/¿If the specified revisions are addressed fully and effectively I may be able to return a score of '4 - Accept'.

Supplemental Materials

Acceptable

Justification

This paper presents a new focus+context method that allows a user to quickly select a focal point on a partially visible object, with the "lens" performing local deformation to increase visibility of that obstructed object.

While there are small contributions to the state-of-the-art in this area, the authors do not focus on emphasizing that. Instead, the authors dedicate the majority of the paper to case studies which justify the use of a focus+context technique to reveal hidden structures (but without focusing on why this particular method is better than any others). 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.

With significant revisions I think the authors could do a better job of emphasizing the novel aspects of their technique, in order to convince the reader that this version of a lens is the best option for certain visualization situations. With these revisions this paper would be suitable for publication.

The Review

This paper presents a new focus+context method for viewing hidden objects that works quickly after the user has selected an area of interest.

Overall, there are interesting aspects to this method. However they are lost in the long, detailed case studies which don't necessarily highlight the advantages of this particular method (over any other focus+context method).

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.

Revisions required:

- 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?)

- 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.

- 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).

- 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.