Response to reviewers

We first wish to thank reviewer #2 for reading our manuscript so carefully and for raising so many relevant and interesting questions. Below we have numbered the reviewer questions from 1 to 37 and detailed our response and actions taken in regard to each question.

The author compares high-order integral formulations for the numerical solution of 2D Helmholtz transmission problem (arising eg in z-invariant time-harmonic Maxwell) with arbitrary complex wavenumber ratios, and sometimes corners. The main issues studied are accuracy (focusing on high accuracies close to machine precision), and robustness (existence of spurious solutions or eigenwavenumbers). The accuracy addressed includes that of evaluation of the potential and its gradient close to the surface, an important issue common to all integral equation methods. The author’s RCIP scheme for refined panels at corners is used. Errors are assessed by convergence, apart from the disc case.

Response: Thanks for an excellent summary of the paper.

The paper is of high quality, addresses a tricky problem using powerful tools, and is useful for the numerical PDE and optics/EM community. The techniques demonstrated require careful analytic work (Sec. 4) but are essentially unbeatable in terms of accuracy (close to machine precision), using small numbers of unknowns. The novelties include a new split for the hypersingular operator, and using difference kernels and the null-field method for accurate evaluation of the grad near the surface. There is not room to present all numerical details; hence I hope the author will include a URL to codes that others can try.

Response: Thanks for appreciating what we have done. And, in particular, thanks for understanding that there is not room to present all numerical details (most of which have already been explained in the various references). Actually, the program which produces Figure 4 is already available via an URL. See:

http://www.maths.lth.se/na/staff/helsing/Tutor/demo19.txt

The other programs are similar, but of course we can make more programs available should there be a demand.

1) The writing and math is generally very clear. The paper has useful reviews on surface plasmons and their absorption at corners (Sec 6-7). Numerical results focus on eigenwavenumbers ($k_1$ values, for a fixed eps; maybe the introduction could emphasize this EVP as much as it does the scattering BVP?).

Response: Thanks for the suggestion to emphasize the eigenvalue problem aspect of our work more.
When we started this project, the intention was to write a short note where we summarized the most common integral equations for this scattering problem (the 2D Helmholtz transmission problem) in a unified notation and investigated them numerically for an inclusion with a corner, using the RCIP-accelerated Nyström discretization, under **plasmonic conditions**. With this we mean that $\kappa_1$ is real and positive and $\varepsilon$ is real and negative (possibly with a vanishingly small imaginary part). The first point – to present the most common integral equations in a unified notation – felt important for a particular reason: when talking to colleagues at conferences etc., we have found that there often is a big confusion of what concepts like “the Müllner-Rokhlin equation” exactly refer to. As the project progressed we found “forgotten” integral equations with good spectral properties and we decided to add some sections where the physics behind our problem was briefly discussed. There was also a need to analyze the kernels of various integral operators and the paper grew longer.

Eigenvalues, and in particular, false resonances and false near-resonances are important when analyzing the differences between various integral equation formulations for the transmission problem. Eigenvalue computations are also important when verifying programs for correctness (that the system matrix and its discretization is correct), since eigenvalues (for inclusions in the shapes of disks) can be obtained using semi-analytic methods.

But our numerical scheme is a scheme intended primarily for scattering BVPs. The main topic of the paper is scattering under plasmonic conditions. It is true that, in our first few numerical examples, we do compute eigenvalues/resonances very accurately for reasons mentioned in the previous paragraph, but we do not have any new fast schemes for eigenvalue search. We simply use a combination of brute force and Broyden’s method as we have done before. See Section VI B of


In order maintain the focus of the paper we prefer not to further promote eigenvalue aspects of our algorithm.

although they also study absorption cross-sections. There are other minor confusions and mathematical issues to fix, and some vagueness about the take-home message for the various schemes. These are listed below along with various optional suggestions. If most of these are addressed I am very happy to recommend publication.

Response: OK. We understand this and will do our best to fix these issues.

2) In the intro, the author might want to cite Vico, Greengard and Ferrando, arxiv 1704.06741, who also address a large range of complex mu and eps parameters.

Response: Yes, we are well aware of this paper on an integral equation for time harmonic Maxwell in 3D that is free of false resonances and we
have read it many times (the DFIE). The scope of the paper is broader than
that of our paper in that the authors vary both $\varepsilon$ and $\mu$. Our plasmonic
conditions correspond to $\mu = 1$ and $\varepsilon$ real and negative.

A problem with this paper, from our point of view, is that in its abstract
it says that the DFIE is false-resonance-free for negative permittivities, while
in equation (6) and in the text afterwards it says that negative permittivities
are not included.

This is confusing, and we have asked one of the authors of the paper
about it. The response we got was that the issue was unclear. So that is
why we did not cite the paper. We simply did not know what to say about
the properties of the DFIE under (our) plasmonic conditions.

3) p.2: "An incorrect choice of integral equations or an inadmissible choice
of eps inevitably leads to spurious resonances, or to no solution at all." Could
this be split into two clear statements? Does it mean: for some eps, there
is no solution to the BVP? Moreover, for the eps where there is a solution,
a bad IE rep choice leads to spurious resonances? (numerical failure for a
discrete set of eps?)
Response: Yes indeed. The reviewer has understood this unclear sentence
in a perfectly correct way.
Action: We split the sentence into two, changed their order, and tried to
be clearer.

4) p.3: please state here if $k_1$ is real or complex.
Response: We agree that the constraints on $k_1$ should be stated.
Action: We have added that $0 \leq \arg\{k_1\} \leq \pi/2$.

5) When $k_1$ is not real, such as for an eigenwavenumber with $\text{Im}(k_1) \neq 0$,
(8) no longer applies since $U^{sc}$ grows exponentially with $r$! This should be
explained and if possible the correct mathematical radiation conditions given.
Response: In the PDE formulation the radiation condition is sufficient
since we only consider an incident fields with $0 \leq \arg\{k_1\} \leq \pi/2$. As we
in the paper also discuss eigenfields, we rewrite the radiation condition to
include also such fields.
Action: We now write the radiation condition as

$$U^{sc}(r) = \frac{e^{ik|r|}}{\sqrt{|r|}} \left( F(r/|r|) + \mathcal{O}\left(\frac{1}{|r|}\right) \right), \quad |r| \to \infty$$

and give a reference to Colton–Kress for this condition.

6) (9)-(10) look peculiar to physicists/engineers, who expect $\exp(i.\omega.t)$
Maybe write $\phi$, a general phase, instead of $t$?
Response: Yes, it might look strange. It is however convenient, so we keep it but give a comment.

Action: We have added: where the angular frequency is scaled to one.

7) (14) is twice the usual definition (in eg Colton and Kress works); this should be commented on.

Response: Thanks for pointing this out. We like the present definition because of its simpler appearance. In particular, this definition makes it clearer what the kernel of the operators are in (16)-(19). But we agree that it could also be confusing.

Action: We have now added a remark in Section 3.2 where we comment on the original appearance of these expressions in the Colton and Kress book.

8) Although (17), (18) are standard, it would be good to clarify that $\partial/\partial\nu'$ means with respect to variation of $r'$ in the $\nu'$ direction.

Response: We agree.

Action: We have added that $\partial/\partial\nu = \nu(r) \cdot \nabla$, and that $\partial/\partial\nu' = \nu(r') \cdot \nabla'$.

9) p.5: unlike the KM1 system which arose from substituting the ansatz into the transmission conditions, the derivation of (32) is not obvious; the reader has to discover that doing the direct substitution gives a 1st-kind hypersingular system. I suggest adding a sentence to explain that particular null-fields must be added to give a 2nd kind system. Otherwise also the final sentence of Sec 5.3 is mysterious.

Response: We agree.

Action: Below eq. (31) we now explain how the system of integral equations is obtained.

10) p.6: "by adding the null fields" & referring to equations the reader has to look up in KM might be more simply explained as: using the Greens representation formulae [eg cite Colton-Kress] in the complementary domain cases where they generate the zero field.

Response: We agree.

Action: See Action under 11) below.

11) Maybe stating the two null field equations would allow the paper to clarify both the above issues?

Response: We agree. In order to simplify for the reader we should add the null-field equations.

Action: The null-field representations corresponding to (28) and (29) have been added. Unfortunately, we could not figure out how to highlight these two new equations, but they have new numbers (35) and (36).
12) Sec. 3.6: Since (36)-(37) are equivalent to (5)-(6) with a rescaling of $U$ in $\Omega$, it might be clearer to not mention (36)-(37) and instead simply rewrite the KR and MR representations for the case (5)-(6). The point seems to be that KR won't be studied, and MR is equiv to KM1. So, I'm not sure why sec 3.6 is needed at all; maybe just cut?

Response: Well, the MR system is the same as the KM1 system with $c = 1$, but the boundary conditions and the field representation are different in the MR equations and in the KM1 equations. That is, a different (but related) problem is modeled by the MR equations. Furthermore, when $\varepsilon k_2$ is not real and positive the MR system and the KM1 system are different – so one cannot say that they are generally equivalent to each other. They are only equivalent to each other when $\varepsilon k_2$ is real and positive. It is precisely these kinds of fine details that, in our experience, cause great confusion in the community. So we rather not rewrite or cut the MR equations out. See, further, the response to 1).

13) (65) I'm not sure what $|\varepsilon| \neq \infty$ means - I think it's unnecessary. Isn't eps already given as a number, and therefore finite?

Response: The uniqueness theorem in Subsection 5.1 is the same as in Section 2 of the paper [15] (now [20]) by Kleinman and Martin. One of their conditions for uniqueness is that $\rho \neq 0$, where $\rho$ in [15] (now [20]) corresponds to $(\varepsilon_2)^{-1}$ in our paper. This means that $|\varepsilon_2| \neq \infty$ in order for the uniqueness theorem to hold. A consequence of the condition is that the uniqueness theorem does not hold when $\Omega_2$ is perfectly conducting.

14) Sec 5.3: refers to "conditions of Sec 5.2", but I find this unclear. Does this mean (65), in which referring to that equation is clearer?

Response: Yes, the entire Section 5.3 is a little unclear. It means (65) (now (67)) plus a few tings more. For KM0 and KM2 it also means that the condition that $k_1$ and $k_2$ are real and positive is violated.

Action: We have rewritten Section 5.3. Hopefully it is clearer now.

15) p.11 Sec 5.3: "null fields" means identically zero? State this more clearly (since "null field" is previously used to mean a Green's representation formula that vanishes).

Response: The reviewer is correct. The phrase "null field" is not appropriate to use here.

Action: The sub-clause "those that correspond to $U(r)$ that are null fields, or violate the boundary condition" have been changed to "those that correspond to $U(r) = 0$, or violate the boundary condition"

16) p.12 after (671): Sobolev-Besov - does this mean a usual Sobolev space? (I am not used to the Besov naming).
Response: Thanks for pointing this out. The Sobolev–Besov space is a generalization of the regular Sobolev space.

Action: We have now changed from “Sobolev–Besov space” to “fractional Sobolev space”, which should be easier to understand, and added reference.

17) Sec 6 is a useful review, but for (72) a reference (Jackson’s book?) would be useful.

Response: We agree.

Action: A reference to Jackson has been added.

18) p.13 middle: it should be clarified that the condition (eps ¡ -1) and transverse wavenumbers discussed only hold for planar surface Gamma. Somehow the opening sentence doesn’t state that the

Response: We agree.

Action: We have rewritten parts of Section 6. We first make it likely that results from the propagation of surface plasmon waves along planar surfaces are applicable also for non-planar surfaces. We then give the main results for surface plasmon waves along planar surfaces.

19) For Sec 7.1-7.2 it might also be worth citing Greiser’s mathematical review on plasmonic eigenvalues (although it may not deal with corners): https://doi.org/10.1142/S0129055X14500056

Response: Thanks for pointing this reference out. The reviewer has an important point in that “plasmons” and “plasmonic eigenvalues” are concepts whose definition in the literature is not always clear. In our opinion, the unclarities are worst for the combination of non-smooth objects and the Helmholtz and time harmonic Maxwell equations (that is, for arbitrary wavenumbers). We would love to see a clear and universally accepted definition of plasmons in this context. For smooth objects and in the quasi-static limit (Laplace’s equation) things are clearer and also somewhat less relevant for the present work. To us, it seems as if Grieser’s review is chiefly about smooth objects and Laplace’s equation, and we are not sure how much citing it would contribute to enhancing clarity in the present work.

20) (80) is for the case of Ω2 an infinite wedge, both inside and out? (ie same set of r as (77)?)

Response: The first paragraph of Section 7 is misleading as it may give the impression that Section 7.1 is about the wedge problem. That is not true and the paragraph has been modified accordingly.

Action: In the first paragraph of Section 7 we removed the reference to the wedge. It is then clear that Section 7.1 is for a bounded object. We have also added that r ∈ Ω1 in (80) (now (82)).
21) After (84) it might be clearer to add that power is flowing as surface plasmons towards the corner, where it is absorbed. (Is the physical reason that the linear BVP physically breaks down at the corner? Otherwise corner rounding at the atomic scale would prevent power absorption for eps negative real?)

Response: The condition (84) (now (86)) comes from the quasi-static analysis and does not involve surface plasmon waves. In the numerical examples we see surface plasmon waves that couple to the quasi-static eigenfields close to the tip of the wedge. It then looks like the surface plasmon waves transport power to the tip where the power then is absorbed. However, (84) (now (86)) is valid even if there are no surface plasmon waves on $\Gamma$. For this reason we like to keep (84) (now (86)) as a property of the quasi-static fields of a wedge and not involve the surface plasmon waves in this paragraph.

22a) After Sec 6-8 I am left a bit confused if $\sigma_{abs}$ can be positive in the case of eps negative and real, if the limit $\epsilon + i.\delta$ is taken.

Response: The last paragraph of Section 7.2 shows, in the quasi-static case, that power is absorbed by a wedge, even when the imaginary part of $\epsilon$ goes to zero, provided the real part of $\epsilon$ is negative and not equal to -1. This property is inherited by the absorption cross section in the case of scattering of a wave from an object with corners. See, further, the response to 22b).

Action: We have rewritten the last sentence of Section 7.2 and also added a sentence after (87) (now (89)) in order to clarify that absorption is possible even in the limit of the imaginary part of $\epsilon$ going to zero.

22b) Is it correct to say that the order of limits between making a corner sharper vs taking $\delta \to 0^+$ matters?

Response: The reviewer is absolutely correct. The order of limits matters enormously. We only consider infinitely sharp corners and let $\delta \to 0^+$. Should one take limits in the opposite order, that is, start with $\delta = 0$ and a rounded corner, and then increase the sharpness of the rounded corner, there would be no (pointwise) convergence of quantities like $\sigma_{abs}$ (when $\epsilon$ is such that (71) (now (73)) holds). This phenomenon is discussed and illustrated (in the Laplace setting) in Section 10 of Ref. [12] in the original submission: https://doi.org/10.1016/j.acha.2012.07.006

23) In Sec.8 all makes sense apart from (89), although it appears to be the "optical theorem" which is standard in physics books but I cannot find in Colton-Kress’s book. A more standard math reference than [18] would be useful.

Response: Yes, (89) (now (81)) is the optical theorem. We now state this and give a reference also to Jackson, which is a more standard reference than [18] (now [23]).
**Action:** Below (88) (now (90)) we now state that (89) (now (91)) is the optical theorem and add a reference to Jackson.

24) Sec 9.1: it’s curious why the 2nd Calderon relation demands quite a large number of nodes for such a low $k$.

**Response:** We totally agree and wish we understood this phenomenon better. It does not seem to be related to the magnitude of $k$ since the differences remain in also in the limit $k \to 0$. We have done extensive testing and the pattern is always the same on the “star geometry”: The 1st Calderon identity converges quickly with at least 16th order convergence while the 2nd identity has delayed and slightly more irregular convergence of order (perhaps) 14. The achievable accuracy for the two identities, however, is about the same and very high. Both identities are also very stable under increased overresolution. Maybe the slower convergence of the 2nd identity has something to do with $S_k$ being a smoothing operator while $T_k$ is a differentiating operator? Since we start with a smooth function $f(r)$, it is better for the final result to first “differentiate” and then “smooth”, rather than to do it in the opposite order? The most important thing, however, in the present paper is that both Calderon identities hold to high precision as the mesh is refined. Since we do not fully understand the difference in convergence between the two identities we refrain from commenting upon them in the text.

25) p.17: "As it turns out, the eigenwavenumbers of the KM1/GL system and those of the KM2 system are the same." Please clarify if this is a heuristic or analytic statement. Also to me this seems inconsistent with the statement that KM1/GL cannot have false eigenwavenumbers (Sec 5.3), yet Table 1 has false ones. I am confused.

**Response:** Yes, we understand the confusion. This is a heuristic statement. Furthermore, it is only the GL (now renamed KM0) and the KM2 systems that have the same eigenwavenumbers (true and false). KM1 does not have false eigenwavenumbers and it was a mistake to mention KM1 in this context (and also in Section 9.3 and 9.4). We apologize for that. See, further, the response to 28) below.

As for the question about inconsistencies in Section 5.3, we are not sure what reviewer is referring to. We cannot find any statements about “KM1/GL” in Section 5.3.

**Action:** in addition to the action taken under the related question 28), below, we also add some text in the second paragraph of Section 9.2 where we state that the similar spectral properties of KM0 and KM2 is an experimental finding.

26) Sec 9: how are eigenwavenumbers found in the complex $k_1$ plane? Since
they play such a role in the examples, the method seems important to mention.

Response: OK. The eigenwavenumbers are found using a combination of brute-force random search and Broyden’s method: we choose a “search box” in the complex $k_1$-plane and measure the smallest eigenvalue of the discretized system matrix at an extremely large number of randomly placed $k_1$-points in that box. If we detect a particularly small eigenvalue we activate Broyden’s method and see if there is a zero of the smallest eigenvalue nearby. Matlabs built-in function eigs is used for finding eigenvalues. There may be more time-efficient methods, but this was the best we could think of.

Action: We add a reference in Section 9.2 to one of our papers where we have used this “method” before and where a few more details of the method are explained.

27) Sec 9.2 - it might be worth mentioning that eps=2.25 corresponds to refractive index of 1.5, typical for glass at optical frequencies.

Response: We agree that $\varepsilon = 2.25$ is a value that is in the range of typical permittivities for glasses at optical wavelengths, but we prefer not to write this in the paper. One reason is that in our paper, $\varepsilon$ is the ratio of the permittivities of the interior and exterior domains. Another is that the permittivity of glass is quite wavelength dependent and we do not want to state a wavelength in the paper. A third is that section (9.2) is also of interest at microwave wavelengths.

28) p.18 bottom and Figure 2: refer back to (67) for choice of c. If GL is a special case of KM1, how can GL have false eigenwavenumbers? It also needs to be stated what KM2 does here - is it the same as either GL or KM1?

Response: Yes, we totally agree that things could be clarified in Section 9.2 (and also in 9.3 and 9.4).

GL (now renamed KM0) is a special case of KM1. More precisely, KM0 corresponds to KM1 with the fixed choice $c = 1$, as stated in Section 3.5. Furthermore, the choice $c = 1$ guarantees that KM1 has no false eigenwavenumbers on the positive real $k_1$-axis when $\varepsilon$ is positive (according to (67) (now (69))). So KM0 has no false eigenwavenumbers on the positive real $k_1$-axis when $\varepsilon$ is positive.

Now, in Figure 2, $\varepsilon$ is negative. Then $c = -i$ in KM1, according to (67) (now (69)), and KM0 and KM1 are no longer the same (KM0 always has $c = 1$). KM1 is still guaranteed to be free of false eigenwavenumbers on the positive real $k_1$-axis (from the theory of Kleinman–Martin and as confirmed by the numerical experiment) while KM0 now exhibits (at least) nine false eigenwavenumbers on the positive real $k_1$-axis for $k_1 \in [0, 10]$
(and only numerically demonstrated – there seems to be no theory for false eigenwavenumbers in KM0 under plasmonic conditions).

Neither for KM2 there is any theory for false eigenwavenumbers under plasmonic conditions. The behavior of KM2, in this example, is very similar to that of KM0 (that is why we forgot to mention it). The condition numbers of the system matrices in KM2 are similar to, but slightly lower than those of the KM0 system. The false eigenwavenumbers are the same.

**Action:** New text is added in Section 9.2 that tells what KM2 does here and reminds the reader that KM0 and KM1 are not the same unless $\varepsilon k_2$ is real and positive. Four references back to (67) (now (69)) are added throughout Section 9. Furthermore, an error is corrected in the first two paragraphs of Section 9.2: Figure 1 shows results for KM0 and KM2 only. KM1 is not involved in Figure 1. Neither is KM1 involved in Section 9.3 and 9.4 and we have corrected that too. The mentioning of KM1 in too many places was a mistake which caused confusion and we apologize for that.

29) Sec 9: 16 digits seem to be quoted in all eigenwavenumbers. Are all such digits believed correct? If so, this should be stated.

**Response:** This is a good point. No, all 16 digits are not always believed to be correct. In the numerical experiments presented, we typically get convergence to plus/minus one in the last digit. The results reported are our best guess (with regard to the last digit). Furthermore, even when we get very stable convergence also in the last digit, that is not a rigorous proof that the last digit is correct. Comparison with semi-analytic solutions on the unit circle confirms our numerical results (for the unit circle) with a relative precision of $4 \cdot 10^{-16}$. But these semianalytic solutions also involve non-trivial computations and it is unclear if they are more accurate than the estimates produced by our integral equation based codes. In conclusion, we only feel sure about the first 15 digits in the eigenwavenumbers. Still, it could be useful to present our best guess for the last digit if someone is to repeat our experiments.

**Action:** “eigenwavenumber” is replaced by “estimated eigenwavenumber” on 5 occasions. We also add a sentence in Section 9.2 where we state that the relative error estimate $4 \cdot 10^{-16}$ is indicative of the error in all presented eigenwavenumbers.

30) In the corner examples, quoting the number of panel refinement levels before RCIP might be useful.

**Response:** It is true that in the original version of RCIP, there is a one-to-one correspondence between the number of refined panels on the fine mesh close to a corner vertex and the number of steps in the recursion for the compressed weighted inverse, called $R$ (not mentioned explicitly in the present paper). When solving integral equations modeling common boundary value problems for elliptic PDEs with “traditional” material parameters
and boundary conditions it is often enough with, say, 25-100 panel refinements = 25-100 recursion steps.

However, the combination of plasmonic conditions and corners give rise to a truly extreme demand for panel refinement in order to satisfactorily resolve the unknown layer densities close to corner vertices. The underlying Gauss–Legendre quadrature is not ideal for resolving the layer densities which do not belong to any $L^p$ spaces, but fractional Sobolev spaces are needed for their characterization and several hundreds of thousands of panels could be needed, or more.

An important feature of the RCIP method is that it refines and compresses in tandem. (It is not “refinement first” and “compression later” so the notion of “before RCIP” is not exactly applicable.) Another important feature is that for long recursion sequences, the recursion for $R$ assumes the form of a fixed-point iteration. The fixed-point iteration property, in turn, opens up for using Newton’s method to find $R$. (See, Section 12 of Ref. [9] in the original submission). Newton’s method often works fine (quadratic convergence for $R$), but makes the precise correspondence between the number of recursion steps and the number of refined panels less clear. Each Newton step probably corresponds to a doubling of the number of refined panels.

As if this was not enough, under plasmonic conditions there is also a need to mix Newton’s method with a homotopy method in order to get convergence. (See, Section 24 of Ref. [9] in the original submission). That makes it even harder to say how many panels are involved. The “number of panels”-analogy between RCIP and traditional mesh refinement is simply gone.

Action: In the second paragraph of Section 4 we have added a sentence mentioning these issues and a reference to a 2011 JCP paper where RCIP, augmented with Newton’s method and homotopy, is discussed in the context of plasmonic conditions for Laplace’s equation.

31) Figure subplots are not labeled with (a), (b), etc... but are referred to in captions and text - please label. Also font sizes, especially for the log10 colorbar, should be bigger, since they communicate error level.

Response: OK. But the larger one makes the font sizes, the smaller relative area of the figure will be occupied by the actual field plots (which also communicate error level). So there is a delicate balance.

Action: We followed the reviewer recommendation an re-did all the figures with “set(0,'DefaultAxesFontSize',12)” and added labels (a), (b), etc... in “FontSize 14”. Admittedly, this looks better.

32) p.20: dynamics → dynamic range.

Response: Thanks for pointing this out.

Action: “full color dynamics” has been replaced with “full dynamic color range”.

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33) p.20 last line: accurate → relative (?)

Response: Yes, “ten accurate digits” is maybe not correct language. But “ten relative digits” also sounds strange.

Action: We changed to “ten digit accuracy”.

34) Fig.8(b): the color of points are hard to distinguish - make larger or use different symbols?

Response: Yes, the colors might be hard to distinguish. We tried many different colors but ended up with green, pink, dark brown, and blue. To use dark brown for error is consistent with the other figures, as is green for the main quantity (here $\sigma_{\text{abs}}$). Since there are several thousands of data points in Figure 8(b,d), making the symbols (the dots?) larger will lead to clutter.

Action: We implemented the same changes as in the other figures: “set(0,'DefaultAxesFontSize',12)” and added labels (a), (b), etc... in “FontSize 14”.

35) Could it be commented how RCIP (which uses geometric panel refinement) handles the plasmon waves which are also geometrically shrinking towards the corner? In the limit of approaching eps negative real, surely the waves do not die towards the corner (since they are carrying power towards the corner)? Does this prevent RCIP from converging?

Response: The reviewer is absolutely right in that the plasmon waves are geometrically shrinking (in wavelength) towards the corner vertex while their amplitudes are not (in the limit of approaching "eps" negative real). This does not prevent RCIP from converging, for a finite but very small imaginary part of "eps", but is the very reason that so extremely many panels are needed for overall convergence to high precision and that Newton’s method and homotopy are needed (as discussed under 30)). Furthermore, the need for resolution on an individual refined panel ($n_{pt}$) to a lesser degree also depends on the real part of “eps”.

It is important to observe that these issues are the same for Laplace as for Helmholtz and that they have been discussed before in the RCIP-Laplace context. So we prefer not to comment on them too much in the present paper in which the focus is on comparing the properties of KM0, KM1, and KM2.

Action: A sentence and a reference to a recent RCIP-paper is added at the end of the second paragraph in Section 4.

36) In Sec 9, a couple of datapoints on solution and field evaluation times should be given. The reader doesn’t know if it is seconds or days.

Response: OK. It is seconds.

Action: An example of timings is now given at the end of Section 9.6. The timings relate to the construction of Figure 7(a).
37) The Conclusion needs a summary statement on which formulation, and which choice of \( c \), should be used. The reader is confused, since KM1 seems best for robustness of the eigenproblem, but KM2 is best for accuracy of field evaluation close to the boundary. Should one be used and then the other if found to be robust? What is the big picture for users of the methods?

**Response:** Yes. KM1 is definitely best for robustness (unless \( k_1 \) and \( \varepsilon \) both are real and positive in which case KM1 and KM2 are equally robust) and KM2 is best for accuracy of field evaluation close to the boundary. We believe that robustness is more important than a few extra digits very close to the boundary and therefore recommend KM1 in all cases except when \( k_1 \) and \( \varepsilon \) both are real and positive. Then we recommend KM2.

**Action:** We added two paragraphs to the Conclusions in Section 10 where we summarize the merits of the KM0, KM1, and KM2 equations and formulate a take-home message on which equations and choice of \( c \) should be used.