Response to Referee

We thank the referee for taking the time to read our manuscript so carefully and to make so many thoughtful suggestions for improvement and clarification.

All changes compared to the original submission are marked in red in the revised version of the paper.

0. ...they are also written in something akin to a private language, which makes it difficult to follow the exposition

   – A reason for this could be that the last named author is new in the field of computational integral equations, and brings with him a style which is common in harmonic analysis and operator theory.

1. pg 1, l -6: The usual model for a real superconductor is not the same as that of a PEC: the constitutive relation $J = \sigma E$, is replaced with the London Equation: $\nabla \times J = -\lambda_L^2 B$, where λL, the London length, is a very small, but non-zero parameter. These authors only utilize the PEC limit, which is an important, but non-physical idealization, so why bring up superconductors?

   – PEC boundary condition is treacherous in the eddy current regime. It is relevant for superconducting objects in the entire regime, but not relevant for metals, and other conducting materials, if k_-L is below a certain limit. As seen from Figure 1 the limit is 10^{-5} when $L = 1$ m. Below the limit the PEC boundary condition can only be used if the object is a superconductor. Even so, there are published papers that present numerical evaluations for metals using PEC boundary conditions below the limit. To clarify our purpose we have rewritten the second paragraph in the Introduction and in the fourth paragraph we have replaced the sentence

   It is seen that for $L = 1$ m, the PEC approximation becomes invalid for ordinary conductors when $k_-L < 10^{-5}$.

   with the sentence

   It is seen that for $L = 1$ m and $k_-L < 10^{-5}$ the PEC approximation is invalid for ordinary conductors, but it holds for superconductors. This is so since the surface resistance of a superconductor is low enough to be considered to be zero in the entire eddy current regime, see [16], and zero surface resistance implies the PEC boundary condition on Γ.

   There are different theories, such as an extended London theory and BCS theory, for the frequency dependence of the surface resistance of superconductors. They all imply that at low frequencies the surface resistance is very small. These theories are out of the scope of our paper, but now we at least give a reference to the paper by Halbritter where such theories are discussed.

2. pg. 2, l 10: It would be clearer to just say $L \ll 1/k_-$, and $|k_+| \gg k_-$.

   – We agree that this could be clearer and have changed.

3. pg. 2, l -10: I would emphasize that the PEC boundary condition entails $\sigma \to \infty$, or at least mention that in the figure $\arg k_+ = \pi/4$.

   – We follow the first advice and have added a sentence.

4. pg. 3 l 10: I'm unclear what the authors mean by the statement that the MTP is "ill-posed" if the genus of Γ is greater than 0. This is certainly not generally true, see for example reference [14]. I imagine that the authors are thinking about some specific limit where $\omega \to 0$, and they should make a precise statement as to when the MTP is ill-posed. They should also see page 14 et seq. of Epstein, Greengard, and O'Neil, Debye Sources and the Numerical Solution of the Time Harmonic Maxwell Equations, II, Comm. Pure and App. Math., doi: 10.1002/cpa.21420, 2012, 37pp. This is mentioned again on pg. 4, l 13, and here it should also be explained exactly when and why the problem is ill-conditioned.

   – As the referee rightly points out, the MTP with genus 1 surface is well-posed for fixed wave numbers. We have added an explanation that we mean in the limit as $k_- \to 0$ and $|k_+|/k_- \to \infty$ (the eddy current condition). Before this was implicit in the "as discussed below" where this is made clear. We also added (here and on p. 4) a short explanation of what precisely this ill-conditioning means, at this first place where ill-conditioning is mentioned in our paper.

   Note that the Debye reference and reference [14] do not consider the eddy current regime and assumes that $|k_+|/k_-$ is fixed, or at least bounded, in the low-frequency limits considered. In this simpler case the MTP is well-conditioned.

5. pg 4, l 15: While I assume the authors are well of this: for a domain with a genus $g > 0$ boundary there is a $2g$-dimensional space of k-Neumann fields (E, H) with vanishing normal components for any k with $\Im k \geq 0$.

   – We thank the referee for pointing out this, and we have added references and comments in the second last paragraph in the Introduction.

6. pg 4, l -22: A global ON frame like ν, τ only exists if the boundary surface has genus 1.

   – Yes, we are well aware of this. We have added a comment that this does not present a problem.

7. pg 8, l 2: Explain why being "nilpotent modulo compact operators" is relevant to the solvability of the equations under consideration.

   – We have added the explanation that this is important for GMRES, since the system only will have one accumulation point. See Section 5.1 in reference [14].

8. pg 8, l 18: This is not the standard definition of a Fredholm operator (finite dimensional null-space, and closed range of finite codimension), and I'm not sure what it means.

   – The referee is quite right. It is much better and correct to follow the standard definition, and we have changed.

9. pg 8, l 24: What does it mean for G to be "close" to being nilpotent modulo compact operators?

   – What we have in mind is norm-closeness, and we have added this detail.

10. pgs 9-10: This is a nice discussion of the usage of adding rank-1 operators to remove problems caused by small eigenvalues. In their applications of this method within the body of the paper, it is often very difficult to tell whether the problem being corrected is inherent to the problem being solved (that is, the physical problem actually displays non-uniqueness, or has necessary conditions for solvability), or an artifact of their choice of representation. These issues are mostly clarified in the Appendix, but it would make the paper a lot easier to read if the authors would explain exactly what the problem is that they need to resolve before they describe the augmentation that resolves it. There should also be a reference to: Randomized methods for rank-deficient linear systems by Josef Sifuentes, Zydrunas Gimbutas, and Leslie Greengard, Electronic Transactions on Numerical Analysis. Volume 44, pp. 177–188, 2015.

    – We have added some guiding sentences above Section 6.1 and 7.1 respectively, that we hope help the reader understand the augmentation problems.

    Concerning random augmentations, we know from experience that for most our augmentation that a random choice does not give augmentation vectors b transversal enough to ranges and c close enough to the null space to give full accuracy in the computations. Moreover we do not see how to make a random (R) augmentation, since this would involve a random choice of fields. We have added comments and references in the third last paragraph in the Introduction.

11. pg 11: I don't understand what the "eigenfields appearing in the eddy current regime as $k_- \to 0$, which generalize..." are. Are there actual

eigenfields and if so what equations and boundary conditions do they satisfy? What are the material parameters. Or are the 3 authors referring to some sort of resonance just beyond the range of parameters of interest that leads to poor conditioning of the problem they are try to solve. Or is this an artifact of their choice of representation? From the remainder of the paper, it seems that the last is the correct interpretation of this section, but I find this section very hard to understand.

– We added a sentence which immediate explains what is discussed in the following paragraphs: the Dirichlet eigenfield is non-physical but the Neumann eigenfield is physical and not an artifact of the representation. (45) explains that we are talking about eigenfields just beyond our range of parameters (at $k_- = 0$), causing poor conditioning already in the eddy current regime. We also added a forward reference to (47) (now (46)), which is the exact equations and boundary conditions for the Neumann eigenfield for ordinary conductors, so that the reader immediately sees this. After (47) (now (46)) we added a sentence pointing out that all finite conductivities yield the same Neumann eigenfield in the low-frequency limit. We also added a sentence in the second last paragraph in the Introduction to stress this fact.

12. pg 11, l -11: What is the eigenfield appearing in ordinary conductors? I find this whole paragraph incomprehensible. Is the H-field referred to in equation (47) the Biot-Savart operator applied to $\eta_0 J$? Is E^+ a harmonic field in Ω_+, with vanishing normal component at the boundary? Can the authors explain in simple terms why these sorts of fields are problematic for the cases of the MTP that they are interested in solving?

    – The referee's understanding is quite right, and we have added some of these remarks to clarify (47) (now (46)). We have also added the details of the derivation of this Neumann eigenfield from (7). As to the reason the eigenfield is problematic: it is vital to understand that there indeed exists an eigenfield in the low-frequency limit in the eddy current regime. Indeed, it shows the impossibility of constructing a completely well-conditioned algorithm which solves the MTP in the whole eddy current regime in the genus 1 case. We have added an explanation of this between (46) and (47).

13. In sections 6 and 7 it would be very helpful if the authors explain the exact nature of the problems they are seeking to overcome by augmenting their systems of equations. It would also be nice if equations (48) and (53) could be combined, so that the choices of matrices can be easily compared.

– We have included such explanations in the added sentences that address the referee's comment 11.

We appreciate the idea and benefit of placing (48) and (53) next to each other. However, we fear that this could have unforseen effects elsewhere in the paper and prefer to leave it as it is.

14. pg 20, l -10: Emphasize that the error plots are absolute errors, which is why the "size" of errors is apparently incompatible with the stated number of digits of accuracy $\{13, 14, 13, 13\}$.

    – We have added a sentence on line 7 in Section 8.2 that clarifies this.

15. pg. 22-23: I like the discussion of the two aspects of conditioning for a numerical method to solve a Maxwell-type BVP.

    – We thank the referee for the positive criticism. This is also useful to know!

16. pg 25, l 4: I am completely mystified by this matrix. Are entries like $1/\hat{k} - 1/\hat{k}$, $1 - 1$ actually 0, or does this notation mean something else?

    – Yes, it is meant that these element are zero. We did not write zero in order to show what cancellations that occur. We have added a comment to avoid the confusion that the referee experienced.

17. pg. 26, l 8: It is stated that the analysis is done assuming that $k_+ \to 0$, which is not the case in the examples presented in this paper. The authors should say something about why the results derived here are nonetheless relevant in their calculations.

    – This is a good comment and we have added a few sentences that should clarify.

18. pg 27, l -16: Is a "cohomology vector field" a "closed" vector field or is it a harmonic vector field?

    – We mean harmonic field, and have changed (2 places) to this standard terminology.