The author is grateful to the Editor and the Reviewers for their constructive comments. Replies and modifications requested by reviewers are listed in detail below, after each relevant comment, hoping they may be satisfactory.

Reviewer A:

In this paper is not clear what are the author's contribution. What are the contributions from the literature. The author should better explain the personal contribute in this paper. As written and presented, the paper seems a simple synthesis of publications by other authors. Please the author specify the personal contribute in this paper.

The paper was revised so to emphasize that the whole discussion is based on the author’s experience. Personal contributions have been clearly stated in the text.

Reviewer C:

The main premise of Martellotta’s article is not only to question the validity of ‘Diffuse Field Theory’ but also to provide an analysis for the use of diffuse field theory. The seminal article supporting the application of ‘Diffuse Field Theory’ is the 1996 Applied Acoustics publication by the late Murray Hodgson. The article under review was presented during the special session to honor Murray Hodgson held during the 2018 CAA/ASA conference last October in Victoria, BC.

Summary and Recommendation

The paper in its current format has not adequately addressed the main premise outlined above. However, as a summary compilation of the issues connected with typical diffuse theory applications, the article is acceptable, but require substantial editing and revisions. In addition, the article fits within the theme of the Canadian Acoustics’ special issue honouring the late Prof. Hodgson.

Thanks for the comment. To be precise, the scope of the paper was to demonstrate (taking advantage of the personal research experience of the author) that even though “diffuse field theory” cannot be easily applied in many cases, the more sophisticated numerical alternatives also present many limitations, so that theory still remains a fundamental guidance. However, the whole paper was revised in order to better clarify the point in a more linear and straightforward way.

Detailed Comments on Methodology and Conclusions

The article is broken into five sections with copious citations (41 including a majority by the author and his colleagues).

Considering the broad topic, 41 citations are not such a big number, and, more importantly, among them only 8 were from the author and colleagues, which is far from being “a majority”. Anyway, an effort to remove some redundant references was made.

– Introduction, Diffuse filed and real rooms, Reverberation time related issues, Computational methods, and Conclusions. One of the main issue with the manuscript is that it mentions, in each section, many citations without delving into the salient points raised by the cited literature. An example from each section is listed below:

The paper is not meant to be a review, otherwise it would have been much longer. In addition in some cases the cited papers (or standards) are “milestones” that really do not need to be further recalled. However, an effort has been made to comply with reviewer’s requests.

1) Introduction - Line 79 – brilliantly resolved by Barron and Lee – A few descriptions of the salient points of the brilliant resolution by Barorn and Lee would assist the flow of the article;

The following sentence was added: “They assumed that total sound was made of direct sound and a linearly decaying reflected component (depending on source-receiver distance).”

2) Sections 2.1 –Sound Energy Distribution – Lines 212 to 218 mentions first model and second model and cites three references (Nos 5, 19 and 20) in passing without any explanations as to what these models are;

The text states quite clearly that Ref. 5 is Barron and Lee model and ref. 19 is the one adapted to churches proposed by the author. Ref. 20 is a paper in which the latter model is applied to a church acoustically treated to serve as an auditorium, and this was now better specified also moving the sentence to make the paragraph more readable.

3) Section 2.2 Reverberation time related issues – Lines 342 to 356 discuss the reverberation time measured in a cathedral with a rotunda including a figure (Fig. 4) and mentions the flutter echo phenomenon. It is not easy to figure out the existence of flutter echoes either from the discussion or from Figure 4. Better explanation would aid in the understanding of the issues.

Figure 4 clearly shows decay curves that have a “staircase” shape. This was now added in the text to make it clear for the reader.

4) Section 2.2 Reverberation time related issues – Lines 374 to 444 discusses ASTM Standard C423 used for measuring absorption coefficient of materials. No description was given as to the salient features of the standard.

Given the limited space available, providing general details of well known standards was considered beyond the scope of the paper. Only the aspects relevant to the discussion were mentioned and expanded by explaining how the relative variation is calculated.

5) Section 3 Computational Methods – Lines 520-535 discusses a procedure to calibrate computational models of performance spaces. However, no details of ‘this procedure’ were given.

A sentence explaining that “Calibration typically consists in changing absorption coefficient values until a better match is obtained between measured and predicted reverberation times (with the maximum error being assumed as 5%).” was added.

It is not necessary to cite so many references in a summary article. A few of them would have sufficed to discuss the issues raised by Hodgson’s paper.

As explained above, the very essential papers were cited. However, a few redundant/less relevant contributions were removed

The article uses spaces that are not truly amenable for Diffuse Theory applications. For instance using two large volumes such as the two churches in Rome is not an ideal example. A good acoustician, if he/she were to study the above mentioned churches, use of ‘diffuse theory’ will be far from their minds.

The purpose of the author was to point out cases in which the acoustic behaviour did not comply to diffuse field theory, and how. Clearly also theatres and concert halls (even with a smaller volume) might fit the purpose, and it is quite obvious that any good acoustician will never consider studying any of them by means of diffuse field theory alone. However, the selected examples were mentioned to clearly show that by using diffuse field theory applied to a system of coupled volumes things work fine. This was further emphasized in the text (also adding a reference to Figure 3 which served exactly this purpose).

The article, in Section 2.2, makes much of the shortcomings of ASTM Standard C423, by measuring the reverberation times in a space that is not described. Does the room satisfy all the limitations expressed in the standard? Even after evaluating the discrepancies in reverberation time, the calculated absorption coefficient (Figure 6) showed only a 7% variation which to this reviewer is well within an acceptable engineering accuracy.

A brief outline of the reverberant chamber has been provided. With reference to the limited variation of just 7%, the author concurs with the reviewer and this is exactly the demonstration of the thesis that limits on relative variation of decay rate may not be a sufficient measure of the reliability of a chamber.

Section 3 discusses some issues with computational methods and chooses a 20 m by 30 m by 15 m (high) room to calculate the reverberation time. The walls and the ceiling had an absorption coefficient of 0.1 and the floor had an absorption coefficient of 0.8. The above example was used to show the impact of scattering coefficient. It compares the computation solution with that of Sabine and Eyring results. It is immediately obvious that the example violates the fundamental assumption of Sabine and Eyring formulae. No acoustician, worth his/her credibility, would apply the simple formulae of Sabine and Eyring. This reviewer failed to see the value of the example used.

The idea behind that example was to point out the importance of the scattering coefficient in terms of influence on the final results, but the author agrees with the reviewer that the way in which it was presented did not make it clear and understandable. In addition, the premise was that “an inexperienced user” might make an incorrect use of the values. The text was consequently revised in order to remove the comparison with Sabine and Eyring results, while just showing the amount of variation in reverberation time and pointing out the importance of choosing the right scattering coefficients.

Detailed Comments on Readability

Unfortunately, the article lacks a narrative structure to hold it together. The article comes across as scattered, with different ideas placed one after another with no particular logic. Questions arise and are left hanging. Concepts are introduced without any background or explanation.

Jarring shifts, from section to section (sometimes sentences to sentences), are the norm.

The different parts were better connected by means of additional paragraphs and notes.

Some examples are:

a) It is customary to expand acronyms when they appear first in the text. What is GA and what is FDTD? What is JND? What are sM and <dM>?

This problem was addressed by clearly explaining meaning of symbols, expanding acronyms when they are first mentioned, and removing those that are not strictly necessary.

b) The article refers to numerical solution of the wave equation as ‘brute force’ method. Isn’t ray-tracing algorithm a numerical solution? If so, isn’t that also ‘brute force’?

Reference to “brute force” was removed. Now it only mentions ray tracing and numerical solution of the wave equation.

c) We reproduce lines 269 and 270 - other energy-based parameters like center time (Errore. L'origine riferimento non è stata trovata.). Why is an Italian phrase appearing in an English article?

Unfortunately this problem appeared after pasting the text into the MS Word template file because of a missing reference. In the revised version no such problem appears anymore.

d) There are four equations (two embedded within the text) in the article, but they are not numbered.

No numbering was included (especially for those embedded in the text) because they are not recalled later in the text.

e) There is no reference in the text to Figure 3. What does it mean?

Reference was now added

f) Figures 1 and 2 are placed in the wrong page.

This was corrected by rearranging the text

g) No reference is given for Equation 1. How was it derived?

Equation1 was derived from basic diffuse field theory and taken from Barron and Lee paper, this was now clarified.

h) Where is Equation 3 from? How is it derived?

A detailed derivation of the equation is given in ref. 17, this is now mentioned in the text.

i) The paragraph breaks make no sense as the flow is not continuous.

j) The article includes sentences that are too long with many sub-clauses. By the time the reader reaches the end of a sentence, the meaning is lost. Some examples are highlighted later.

k) Many paragraphs begin with the phrase ‘In fact.’ In fact of what?

The whole manuscript was completely revised so to improve the language and the structure of the paper.

l) Lines 290 to 310 discusses the ISO 3714 standard that is used to evaluate the sound power of sources. The article just mentions the process of the standard, but it is not clear how this part fits with the rest of the section.

This part was removed. It was supposed to introduce a part which was subsequently removed for the sake of brevity, but unfortunately I forgot to also remove the introduction.

Some glaring errors are:

A) How is relative level different from G (strength)? In that case, Equation 2 embedded in Line 183 should read 44.9 + 10log(T/V).

As stated a few lines above, they are the same. The typo was corrected

B) The article cites 41 references, but omits an important one – the textbook by T.J. Cox and P. D’Antonio, ‘Acoustic Absorbers and Diffusers: Theory, Design and Application, CRC Press – April 2004

The reference was added as suggested.

C) The volumes of two spaces (Line 197 and 198) are stated as 40’000 m3 and 50’000 m3. They should be 40,000 m3 and 50,000 m3.

All the numbers were reformatted by just removing thousands dividers, so to avoid confusion.

Long sentence examples: a) Sentence beginning on Line 25 and ending on Line 29; b) Sentence beginning on Line 61 and ending on Line 65 seems contradictory.

Sentence beginning on Line 25 ends on Line 27 where it states that “…such conditions are hardly found”. The sentence from line 61 to 65 was revised to clarify the point: “In times in which the only alternative to classical formulas were costly and not yet fast and friendly ray tracing tools, such guidance was of the greatest importance in order to understand when diffuse field theory could be applied”

The last sentence of the Conclusions section (Line 614 to Line 646) could be rewritten succinctly as, “Hence, when one considers the acoustics of a space, used for listening or evaluating the absorption coefficient of materials, the answers of Hodgson are a safe guide.”

The sentence was rewritten as proposed, with just a small variation in the end to recall the “When is diffuse field theory applicable?” question

Final Comments

The paper needs substantial revisions. It should be shortened. It needs a good English editing. Many long sentences can be split into simple sentences. Finally, this reviewer’s impression of the submitted manuscript is that the article was originally written in Italian and got translated in a hurry using Google Translate.

The author is aware that, being a non-native English speaker, the quality of the text might certainly be improved, but the last sentence is uselessly offensive. In any case, the revised version was now extensively revised by a native speaker.