University of Warsaw

The academic Council of Physical Sciences

Faculty of Engineering

Review of PhD thesis submitted by Krzysztof Czajkowski

Dear Professor Wojciech Satula

17 August 2022

Below, please find my review of the PhD thesis submitted by Krzysztof Czajkowski.

namo@mci.sdu.dk
M +4593507139

The thesis is entitled *"Impact of multiscale electromagnetic coupling on the properties of antenna-reactor complexes"* and the thesis work was supervised by Dr. hab. Thomasz J. Antosiewicz. For your information, I have professionally known the supervisor for several years, while we have had neither formal collaborations nor joint publications. As for the PhD candidate, I am aware of some of his published works, while we had no prior direct interactions.

Overall, this is a comprehensive thesis describing convincing PhD work, both in terms of quality and quantity of the research outcome. The thesis includes five first-author publication published in recoqgnized journals in the international peer-review literature, while in addition, the PhD candidate authored an additional nine journal publications. In this context, the PhD candidate has maturely selected key publications that document the core of the PhD work, while the longer list serves to demonstrate an also quite diverse research background with contributions to several topics within the area of optical sciences and photonics.

I am convinced that the PhD thesis documents Krzysztof Czajkowski's generally high theoretical knowledge and research compentensies within optical sciences and condensed-matter physics with clear signs of his ability to independently conduct and dissiminate his scientific work. As an example of his general knowledge of his research area, the thesis includes a both elaborate and mature review of the literature in the area of the thesis (Chapters 2-5), containing also many insightful personal remarks, comments, and considerations.

University of Southern Denmark
Campusvej 55
DK-5230 Odense M
Denmark
T +45 6550 1000
www.sdu.dk

Furthermore, I find the subject of the PhD thesis is an original contribution to a scientific problem – the need for theoretically exploring and understanding antenna-reactor complexes has been well-articulated in the community). The original aspect of the thesis results and Krzysztof Czajkowski's ability for creative and independent thinking is further endorsed by his already peer-reviewed contributions that document the core of his thesis work.

In summary, my conclusion is positive, i.e., the PhD thesis meets the expectations for sound and successful PhD work described in the invitation letter to review the thesis.

Below, I offer some futher details and criticism on the PhD thesis. They are all minor and do not change any of the conclusions of the already published works and main conclusion of the thesis. As such, they are not critical for my overall positive recommendations of this PhD work, and there is no need for me to further review any possible revisions.

My comments could conveniently form the basis for a discussion at the thesis defence, and if the candidate is anyway making revisions to his thesis, I hope my comments can be used to further strengthen an already strong presentation of the PhD work.

Yours sincerely

Niels Asger Mortensen

N. Asger Mortensen

Professor & VILLUM Investigator
Centre for Nano Optics

Chair of Technical Science
Danish Institute for Advanced Study

Detailed comments on the PhD thesis submitted by Krzysztof Czajkowski

Chapter 2:

Section 2.1 is well-written and to-the-point, while I perhaps feel that the sections ends a little abruptly. In particular, the boundary condtions in Eqs. (2.13) and (2.14) would be a nice opportunity to explain how contributions from surface currents and surface charge are normally neglected in the classical treatments, while the same equations also form a starting point for invoking quantum-corrected boundary condtions. I understand that the candidate did not work on this himself, while this would be an important direction to mention in the current context, where the candidate aims to invoke quantum mechanical effects. For a recent review that introduces this, I would like to direct the attention to the following paper: Nanophotonics **10**, 2563 (2021).

It is optional for the author to address the above comment.

Section 2.2.1 correctly introduces the linear-response function, Eq. (2.24), emphasizing the aspect of temporal dispersion (frequency dependence), while it would be natural to at the same time also mention spatial dispersion (wave vector dependence). Furthermore, the integration limits are not really discussed which would tie up nicely with also a brief discussion of causality and Kramers-Kronig relations. A good text-book to consult on this matter would be Y.P. Svirko & N.I. Zheludev, Polarization of light in nonlinear optics (Wiley,1998).

Finally, it is mentioned that nonlocality (spatial dispersion) is not considered in this thesis. This is however stated without any further comments or any justification. Such effects do indeed seem relevant in the regime explored by the author. Of course, the classical electrodynamic results in this thesis neglect these nonlocal effects, but eventually, it is these nonlocal effects that the TD-DFT approach aims to capture in a more accurate manor.

I considered it important for the author to briefly mention this.

Chapter 3:

Section 3.1.1 introduces the quasi-static regime, focusing the discussion on the spherical geometry, while only very briefly mentioning subwavelength particles beyond spherical symmetry. Here, it would be natural to briefly discuss the more universal plasmonic properties in this regime, citing also seminal work: Physical Review Letters **97**, 206808 (2006).

It is optional for the author to address the above comment.

Section 3.1.4 briefly discusses various quantum aspects of plasmonics. In particular, the blueshifting (shift of the resonance toward higher energies) with respect to the classical quasi-static result is mentioned (with a citation to Ref. 67 from the group of Dionne). However, the reference to spill-out with electron density extending beyond the metal surface is wrong, since this alone would manifest in a frequency redshift. This topic has been widely discussed in the literature, and a good starting point for the discussion is to consult the following paper and references therein: [Physical Review Letters **118**, 157402 (2017)].

I considered it important for the author to correct this problem.

Section 3.1.4 also briefly discusses the case for quantum tunnelling. This is a topic of controversy, and I encourage the author to offer a slightly more balanced picture and perhaps also mention the competing dissipation mechanism of surface-enhanced Landau damping in addition to tunnelling currents. A more critical discussion was recently summarized in the following review paper: [Nanophotonics **10**, 2563 (2021)].

It is optional for the author to address the above comment.

Section 3.1.5 introduces the concept of hot-electron generation, while not really mentioning its relation to surface-enhanced Landau damping. In many contexts, surface-enhanced Landau damping is the perhaps most important quantum correction to the classical electrodynamics (which is neglecting quantum corrections at the abruptly terminated surface of the metals) which is indeed the cause for the hot-electron generation at the surface. For a recent discussion, please consult the following review paper and references therein: [Nanophotonics **10**, 2563 (2021)].

It is optional for the author to address the above comment.

Chapter 7:

Section 7.1 discusses briefly how chemical species on the surface may modify the electronic properties of the metal, by changing the carrier density and thus in turn the plasma frequency. While the author is definitely not the first to use this minimal model, it nevertheless should call for some reflection on which part of the volume that additional charge will occupy. For the metals with an already high electron density, it is well established that added charge will effectively occupy only the surface, while the electron density in the bulk region remains unaffected. In other words, the plasma frequency is unaffects contrary to what Eq. (7.1) suggests. I am aware that this part only serves as a movitation for the LSPR shifts that one might have and with no consequence for the conclusion drawn later on, but the model's physical meaningfulness is questionable and somewhat misleading, since only the surface of the metal will be charged.

It is optional for the author to address the above comment.