Tzvetomir TZVETANOV
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To: The Editors of eLife 6. November, 2018

Dear Editors,

About a month and a half ago I read an article published in eLife at end of January 2018 (DOI: https://doi.org/10.7554/eLife.30334).

The above mentioned article made claims about how motion perception is shaped through suppressive and facilitative effects in humans: (1) claiming that it can be understood without the recourse to excitatory/inhibitory interactions but through the use of a known modelling results – "divisive normalization"; (2) claiming that in that particular design, to explain spatial suppression, one does not need inhibition.

I am a psychophysicist and modeller, specialized in visual perception in Humans. When I read the detailed report I ended up not happy. There were multiple reasons. Some related to the behavioural methods. Other related to the computational modelling approach used by the authors of that paper. In sum, the final conclusions seemed far from convincing to me.

It is now approximately more than a month that I write counter arguments, perform modelling, and reanalyse published data with the model. I found it difficult to make a *Scientific Correspondence*. There were too many points to comment on, especially since the model needed a very lengthy analysis, correct rewriting, and check of its applicability. Instead, after discovering the *Research Advance* format of your journal, I decided to write it in this format. In fact, my work is presenting new results that are based on analysis of the published article.

Therefore, please find attached my submission of a *Research Advance* manuscript that builds over the work presented in the published article. My work presents the correct computational model for predicting the psychophysics results in the experimental design used in the published article. Then, by using the model, it demonstrates that the claims made by the authors of that article are not well founded, and that instead human perception of motion is shaped by the exact excitatory and inhibitory interactions. Furthermore, the application of the model allows to demonstrate that one particular claim in the published article is wrong – in fact, the psychophysics observation of suppression in motion perception is due to inhibition – and that another result they report, anti-correlation between non-invasive measures of GABA+ inhibition and perception, cannot be analysed directly as it is because the results are dependent on inhibition and non-inhibition related factors.

It is regrettable that your reviewing system has let pass the work against which I argue with simple facts and mathematics. It is regrettable because your journal claims to be of high standards in the reviewing system. I am interested to read your opinions about my submitted work, as well as the opinions of reviewers and the original authors.

I tried as much as possible to write in correct and measured tone. I am not a native English speaker. If you decide my submission has some interest but some parts are not appropriately presented, we could discuss the contentious elements for reformulating them.

I will be glad to hear from you.

Sincerely,

Tzvetomir Tzvetanov, PhD

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To: Dr. Peter Rodgers & The Editors of eLife 3. December, 2018

Subject: re-submission of manuscript # 05-11-2018-ADV-eLife-43398

Dear Dr. Rodgers and Editors of eLife,

One month ago I submitted a manuscript for consideration in eLife as a Research Advance. It was rejected because it was considered to be a Scientific Correspondence. I was asked to contact the corresponding author of the original published article (Dr. MP Schallmo) to discuss the differences in opinions. I did so. Drs. Schallmo and S. Murray (the last senior author) provided an open reply.

I am aware that my manuscript is not fully compliant to the Research Advance format, but I also would like to emphasize that, despite that it is based on a critique of the original work, this manuscript goes further and proposes correct description of the model to be used in Dr. Duje Tadin experimental design. This model was missing until now. Dr. Tadin's design is now a standardly used method for assessing or testing effects of inhibition or excitation changes in groups of persons with established changes in visual perception (ageing, clinical population...). Thus, it is important that future users of the design are aware of the difficulties in the modelling approach, as well as its importance, and to be warned about pitfalls in its application.

I note that the reply of Drs. Schallmo and Murray does not point any wrong description of the modelling part in my manuscript, neither a reply to my critique of using "Size Index" as a variable of "Spatial suppression/facilitation". Although there could surely be further possible discussion between the authors and myself about our respective commentaries, I do think that my manuscript, as submitted originally at beginning of November, provides important insights and issues that are of relevance to the general community.

Therefore, I maintain my submission as a Research Advance in its exact original form. Following your request, I had communication with the original authors. I provide the email exchanges we had in this new submission (attached at the end of this Cover Letter).

I maintain my original letter for the submission as the main Cover Letter (on next page), to which I attach this part.

I will be glad to hear from you.

Sincerely,

Tzvetomir Tzvetanov, PhD

PS: two attachments: (1) the main cover letter; (2) email correspondence with Dr. Schallmo

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First Cover Letter

To: The Editors of eLife 6. November, 2018

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Email correspondence with Dr. Michael-Paul Schallmo

Subject: **Re: article "Suppression and facilitation of human neural responses" and my issues with it**

Date: 2018-11-30 9:21:52

From: Michael-Paul Schallmo <schal110@umn.edu>

To: tzvetan@hfut.edu.cn

CC: Scott Murray <somurray@uw.edu>

Attachment: Schallmo Reply to Tzvetanov.pdf (203.58K) Download

Hi Dr. Tzvetanov,

Thank you for your patience as we prepared a reply to your article, and for your interest in our work that has spurred this discussion. We have submitted the attached manuscript to bioRxiv.

Regards,
Michael-Paul

> On Fri, Nov 9, 2018 at 12:40 PM Michael-Paul Schallmo <schal110@umn.edu> wrote:
>
> Hi Dr. Tzvetanov,
>
> Thank you for sending the manuscript, and for your interest and feedback on our work. We will consider your manuscript and provide a full reply as quickly as possible, ideally within 2 weeks. We welcome the opportunity for scientific discussion, and to learn from your work.
>
> Best wishes,
> Michael-Paul
>
> > On Fri, Nov 9, 2018 at 1:00 AM 赐为 <tzvetan@hfut.edu.cn> wrote:
> >
> > Dear Dr. M.P. Schallmo,
> >
> > About one and a half month ago i read your publication "Suppression and facilitation of human neural responses" that you published in eLife (2018).
> > The topic is of direct interest for me since i did work on visual psychophysics of motion perception as well its modelling.
> > In reading your detailed report, I found multiple issues that I was not convinced about and I thought should be improved.Therefore, I wrote a manuscript to discuss the issues I think should be made known to persons interested and working on the topic of motion perception and its suppression and

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facilitation, as well as its modelling.

In this manuscript I discuss two sides of your work: (1) the psychophysics methods, analysis and data interpretation, where I think you have analysed the psychometric function such that it may contain unnecessary biases, and defined "suppression" and "facilitation" with respect to a variable (Size Index) that is not appropriate; (2) with respect to the model, where first I demonstrate that "divisive inhibition" and "subtractive inhibition" cannot be dissociated in the experimental design you use, and second I derive the correct equation predicting the "Duration threshold" from psychophysics modelling. From these analyses, I made deductions that go against some of your major claims in the paper.

I want to submit my manuscript to eLife. They consider it as a Scientific Correspondence and, thus, following their rules, it is first appropriate to discuss my scientific analyses and critique with you and your co-authors.

I will be glad to hear your opinions and possible rebuttal of any part in my work.
Please find attached to this email my manuscript.

Regards,
Tzvetomir Tzvetanov, PhD

--
Michael-Paul Schallmo, Ph.D.
Assistant Professor
Department of Psychiatry and Behavioral Sciences
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--
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To: 23. January, 2019
Dr. Randy Scheckman, Editor-in-Chief of eLife
Dr. Anna Akhmanova, Deputy Editors of eLife
Dr. Eve Marder, Deputy Editor of eLife
Dr. Detlef Weigel, Deputy Editor of eLife
Dr. Michael Frank, Senior Editor handling my 2nd submission
All other Senior Editors

Subject: Appeal to eLife decision: 02-12-2018-ISRA-eLife-44097

Dear Editors,

I received a rejection to my submission for a Research Advance article, a proposal that substantially improves over a published article in eLife (Schallmo et al., 2018).

My submission demonstrates that the published article in eLife provided a wrong conclusion from the data and modelling that was published. My manuscript describes the correct model for the psychophysics experimental paradigm used by the authors. It goes further and also describes how a user of the model should carefully extract, from the experimental results, conclusions about the underlying neurophysiological processes.

This experimental paradigm about visual motion processing was original proposed about 15 years ago by Dr. Duje Tadin and, now, it is starting to be used in more application oriented environments (Ageing, psychiatric oriented studies) to infer changes in underlying inhibitory/excitatory processes. My manuscript also clarifies that the published article in eLife uses a model whose foundations were from its first, and latter, usage not well described.

From the rejection letter I received, I understand that my submission was rejected on the following rgounds:

1. though my modelling is acknowledged "*more appropriate*", the journal considers that "*we did not find the claims or the key modeling elements to be sufficiently novel given the larger literature, to meet the high bar we have for publishing in eLife*";

2. two example of feedbacks from other Senior Editors of eLife claim about my submission that: "*it is trying to explain low dimensional psychophysics data with a very complex model, and although this might be a new way to model this particular task, it is not novel in terms of modelling the suppression effect in general*" and "*Another editor added that the modeling was an apples-to-oranges comparison, which relates to the original authors' rebuttal*".

To summarize the rejection criteria, your journal considers that my modelling is "*more appropriate*", I claim correct, but that it is too complex for "*low-dimensional psychophysical data*" and that "*the modeling was an apples-to-oranges comparison*".

Dear Editors, the ground of my modelling is not different from the one of the original article of Schallmo et al. (2018) over which I built my Research Advance proposal: to provide a model description of psychophysical measures in Dr. D.Tadin's design and what we can infer from it based on the available data. In fact, my modelling targets the same aim as Drs. Schallmo and collaborators. The fact is that in the psychophysical design of D.Tadin the model naturally leads to high-dimensional model parametrization of the data. I will not comment on "*apples-to-oranges comparison*" but I note that Drs Schallmo & Murray do not consider it as such (bioRxiv 495291; doi: https://doi.org/10.1101/495291). Thus, your comments

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disregard not only my work but also Dr. Schallmo et al.'s article that you have published, as well as any other publications that provide psychophysics modelling approaches to visual perception investigation.

Last, but not least, your journal considers that my work is better suited in a specialist journal ("*we feel this manuscript is better suited for a specialist journal*") and then proposes that I could eliminate the major modelling part in order to match ("*focus on*") the rebuttal of the Drs Schallmo & S. Murray and thus write a scientific correspondence. Normally this letter is not the place for arguing about Drs Schallmo & Murray's rebuttal. But it seems that the Reviewing Editors are very happy with this rebuttal. Thus, your journal puts me in the position to argue here against their rebuttal. I make it simple. The rebuttal from Drs Schallmo & S. Murray does not comment about major modelling criticism of my submission, that (1) their model is not soundly based on known computational modelling of psychometric function (but this was the case of this model already in previous publications, which I have clearly stated) and common knowledge about decision Criterion, (2) that *Size Index* variable used by the authors is not appropriate for inferring something about suppression and facilitation of motion processing and as consequence that the model prediction should be weighted with respect to the raw duration thresholds, which, when done, shows that when one increases inhibition strength duration thresholds increase and it matches the experimental results of their Lorazepam experiment. A conclusion in opposition to theirs, which they maintain in their rebuttal.

If I understand correctly the demand from the Editor, the Editors agree with the conclusion of the rebuttal. In my opinion the Editors are also wrong, as well as Drs Schallmo & Murray.

I do maintain my claim and request a review of my Research Advance submission by independent reviewers. The point is simple. I have made clear argumentation about the inferences that can be made from the correct model. I did point exactly, and showed data and model fits, demonstrating what I think should be the correct conclusions from the article of Schallmo et al. If I am wrong in any claims I have made, I would like to have a clear answer pointing to the exact problems, not a rebuttal (that was extended from their first feedback) on which I have to write a further rebuttal (because it is done outside the reviewing system).

But my submission is not only about their work. First, it is general for all other works that want to use this paradigm to infer something about visual motion processing in some particular subset of the population. My presentation clarifies how one should use this experimental paradigm and its model. Second, the general approach that is laid down is easy to use for any other simple paradigm that researchers are used to apply.

Therefore my questions are simple: do you want to correct a wrong, in my opinion, scientific conclusion that your journal has published, which makes the scientific community go on a wrong path, instead of submitting it to "another specialist journal"? Do you want to proceed with my submission as a Research Advance, that shows and demonstrates how to perform this type of modelling and what can be inferred from it?

I will not trim, delete, cut, suppress, substantially decrease any part of my submission, unless it is clearly pointed wrong.

Sincerely,

Tzvetomir Tzvetanov, PhD

PS: attachments: eLife decision: 02-12-2018-ISRA-eLife-44097

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---------- Original ----------

From: "editorial";<editorial@elifesciences.org>;
Date: Jan 22, 2019
To: "tzvetomir.tzvetanov"<tzvetomir.tzvetanov@gmail.com>;
Subject: eLife decision: 02-12-2018-ISRA-eLife-44097

Dear Dr Tzvetanov,

Thank you for choosing to send your work entitled "Suppression and facilitation of motion perception in humans" for consideration at eLife. Your initial submission has been assessed by a Senior Editor in consultation with two other Senior Editors and the editorial office. Although the work is of interest, we are not convinced that the findings presented have the potential significance that we require for publication as a Research Advance or regular Research Article in eLife.

We all appreciated that your modeling likely makes a scientific contribution to the understanding of the phenomena covered in the original article based on rigorous modeling principles. But even if some of these are indeed more appropriate than what the original authors did, unfortunately, we did not find the claims or the key modeling elements to be sufficiently novel given the larger literature, to meet the high bar we have for publishing in eLife. We also felt that, given the argument and rebuttal from the original authors, some of your points may indeed be legitimate, but others that depend on one's inclination for different levels of explanation. For example one of the other editors noted that "it is trying to explain low dimensional psychophysics data with a very complex model, and although this might be a new way to model this particular task, it is not novel in terms of modelling the suppression effect in general." Another editor added that the modeling was an apples-to-oranges comparison, which relates to the original authors' rebuttal. So, while we do not question the utility of the debate, we feel this manuscript is better suited for a specialist journal.

You are still welcome to very substantially trim the manuscript and outline the main criticisms as a correspondence, to which the authors could respond, But this would require eliminating a large portion of the text on neural network modeling and focus on the two criticisms that are laid out extremely clearly in the original authors' rebuttal and to treat the resulting dialogue as a correspondence.

We return a high proportion of articles to authors without passing them on for in-depth peer review so that they can be promptly submitted elsewhere. This is not meant as a criticism of the quality of the data or the rigor of the science, but merely reflects our desire to publish only the most influential research. We wish you good luck with your work and we hope you will consider eLife for future submissions.

Best wishes,

Michael Frank
Senior Editor
