Reviewer’s report

Title: Delay in diagnosis of muscle disorders depends on the subspecialty of the initially consulted physician

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Reviewer: Anne Marie C. Plass

Reviewer’s report:

The paper was on diagnostic delay in muscle disorder patients. I read it with great interest, it is a topic of great importance. The central question was whether the delay depended on the physician that was first consulted with symptoms related to the muscle disorder. This is especially of interest to those living in countries where the medical system allows patients to choose who to consult, but it is of interest of those living in countries with a so-called gate-keeper system as well, because the GP (who is guarding the medical system) was one of the specialties involved in this study. This makes the paper worthwhile for a larger audience. Further, it adds something new to the existing literature. I would therefore recommend publication, however there are some major concerns that must be met before publication can be considered.

Major Comments

1. My first major concern is about the definition of the aim of the study and its research question. I formulated it above, but I could only indirectly retrieve this from the text. The introduction is very short, and the aim of the study is not formulated. Moreover, the authors state that "the medical and economic consequences of different [medical] systems are poorly studied". This is very interesting, but the authors do not reveal how they are going to do so, and as a matter of fact, the don't. This study is carried out solely in Germany, so only the effectiveness of this very system related to diagnostic delay in muscle disorders can be questioned. So please formulate a clear aim of the study consistent with what is written above, and do not suggest to study things you're unable to study.

2. My second major concern is about the factors studied that may contribute to a longer or shorter diagnostic delay. No differences were made between late and early onset disorders, or between disorders. To me it seems logical that the diagnostic delay depends on the disorder itself as well. Further the number of complaints first presented to the first physician consulted and the seriousness of the complaints was not measured. In my opinion serious complaints, as well as the number of complaints presented, may influence diagnostic delay. These factors were not mentioned in the paper. If they were measured, I would recommend the authors to include the number and nature of the complaints into the paper, and also to specify the disorder involved.

3. In the methods section of the paper some demographic variables are included. This is very confusing. Further it is unclear why the medical specialties that were
named as non-neurological specialists were chosen, and others, eg obs and gyns, klin genetecists etc were not. The methods section of the paper needs to be revised and clearly structured. I would advise the authors to look at other papers and write the methods section consistent with what is common.

4. The results in the results section of the paper are presented in a way I do not understand, e.g. on page 5, last sentence of the first paragraph, it is stated that the mean time between the first consultation of a physician and the final diagnosis was 4.3 +/- 5.9 years. I do not understand that at all. Does this mean that the mean diagnosis was set at 4.3 years with a standard deviation of 5.9 years? Does that mean that in some cases the diagnosis was made even long before the first symptoms occurred (e.g., by a genetic test???) or does 4.3 +/- 5.9 indicate a mean interval period? This really puzzles me, I couldn't make any of it. In the second paragraph, third line (p5) the interval is even 3.3 +/- 7.2, this seems huge and therefore meaningless to me. I would therefore recommend to revise the way results are presented or to first explain to the readers how to interpret your results.

5. A hierarchical linear regression has been carried out stepwise, but it is not explained why some variables were entered in the first step and others in the second or third. There is no theoretical framework presented, nor is the number of steps used being justified. Table 2 is a rather uncommon way of presenting a hierarchical linear regression, r-values and r square change are missing.

6. The discussion starts with an interesting finding that the diagnostic delay was even longer for men than for women. This is an interesting finding that gets the attention of the authors only at the bottom of the discussion. In trying to explain this gender difference the authors name possible differences in complaints and ways of presenting these. These should have been possible explanatory factors investigated in this study, as mentioned in my comments concerning the introduction part of the paper (see 1). Now we seem to know less at the end of the study as we did in the beginning. That is a real missed opportunity, especially since findings of others on gender differences were already presented in the introduction part of the paper, and thus the authors could have anticipated on this at the start of the study already.

7. The English used needs to be improved. The authors should ask a native speaker to check their paper.

8. In the midst of the introduction part of the paper, the authors state on p3: "An elevated creatine kinase level, when determined at all, can be misinterpreted as heart disease". No reference was mentioned after this statement. Therefore I would like the authors to put a reference here, for in my opinion, an increased creatine kinase level is always associated with muscle disorders.

9. In the first line of the methods section, I'd say that patients were invited to participate by their physicians, instead of being informed (p3).

10. In the first paragraph of the methods section (bottom p3) the authors mention a private clinic and the Charité University Hospital in Berlin. In being not familiar
with the german medical system I do not know whether the latter is a private clinic or a municipal clinic or whatsoever. So please do not name the name, but describe the kind.

11. At the top of p4, line 4, the authors speak of IRB approval?! I do not know what that is.

Other remarks:

12 In order to be precise the authors write Thirty six, and Fourty four in starting a sentence with this number. Although correct, I would prefer 36, and 44. If you dislike putting a number at the start of a sentence, you could avoid it by describing your finding, eg: Less than halve of the respondents were women (44%), instead of: Fourty-four % (I would advice to write percent in that case) of subjects were women.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

'I declare that I have no competing interests'