Reviewer's report

Title: The prevalence and types of discordance between physician perception and objective data from standardized measures of RA disease activity in real-world clinical practice in the US

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Reviewer: Ricardo Ferreira

Reviewer's report:

This is an interesting manuscript, contributing to a better comprehension of the subjectivity that might be involved in the definition of remission in rheumatoid arthritis, specially related with Physician assessment. The author's determined the percentage of rheumatologists that used only their own judgment to determine remission, i.e. without using standardized measures. They compared the physician's judgment of remission against DAS28-ESR(3v) remission, and explored the factors associated with physician's overstatement of remission. This may contribute to the optimization of remission assessments in clinical practice.

Overall, the research question seems relevant and the methodology adequate (some questions bellow). The word count can be significantly reduced, especially in the methods (some examples provided bellow) and in the results, were authors repeat in the text the results presented in tables. The discussion, especially the first two pages could be significantly improved/reduced. The manuscript could be written more concisely and clearer.

I choose to present my comments/suggestions per section and not by relevance.

1) Title

Despite being clear and concise, it does not refer to the analyses of factors associated to the overstatement of remission by physicians. I would suggest a title such as: "Factors associated with rheumatologist overstatement of remission compared to DAS28 definition in rheumatoid arthritis: a clinical practice, cross-sectional study". To study the factors requires to assess the differences first and this might be more informative.
2) Abstract

2.1. the sample (101 rheumatologists and 843 patients) is presented in methods. Please consider to move this info to results.

2.2. Please clarify why do you considered 101 rheumatologists and 843 patients if in the results section (page 11, line 240) it is stated: "A total of 531 RA patient records from 78 rheumatologists had all the information needed to calculate the DAS28(3)-ESR score for the analysis on concordance between remission according to rheumatologists' evaluation and according to a standardized measure"

From what I understand the n=101 was used only when rheumatologists were asked about how they use to assess remission (Fig.1), not how they did for this study. For the remaining analysis, which is mainly based in the the comparison against DAS28-ESR, only data from 78 rheumatologists and 531 patients can be used. Also, the authors present the characteristics only for 531 patients in table 1.

In the methods (page 7, line 138) is also clear that: "Therefore, only patients for whom a DAS28(3)-ESR score could be calculated were included in the primary analysis."

2.3. may the conclusion be more informative regarding the results?

3) Background

Well written and supporting the study.

Ref 1 is not the most updated version of T2T recommendations, which is the reference 11. Was this done to be like this?

4) Methods

4.1. the n is presented in methods and results. I would prefer to see it only in results.
4.2. Page 6 of the manuscript presents some examples of repetition of information that can be improved.

For instance: line 115 "All data were de-identified in adherence with...", line 120 "Prior to receipt for analysis, all data were fully deidentified", and even line 121-122 "no personal identifiable information was collected". Also, the term "de-identified" is not written in the same way.

Another clear example of over-repetition of information is presented in line 118-119: "The analyses carried out in this study were conducted on an existing database. All analyses were conducted on the existing database;"

4.3 It is not clear how rheumatologists classified remission: was a yes or no question? (page 7, line 149-150) Or did they answered in a VAS (0-10cm) scale (EGA), which allows also to assess CDAI? (page 8, line 172)

4.4. The cut-off of 2.6 was used to determine remission based on the DAS28ESR(3v) definition. However, more strict cut-offs were recently proposed by Fleishman et al for DAS28 using CRP and ESR. Why did the authors not used this new cut-off for primary or for sensitivity analyses?

Furthermore, in the discussion (PAge 15, line325-328) the "stringency" of DAS28 is also compared, as a limitation, with other definitions: "It is also less stringent than CDAI, SDAI, or Boolean remission."

This should be stated as a limitation or included in the analyses.


4.5. The description of "CDAI (sensitivity analysis)" is another example of overwriting, with too many details and words to describe something simple. Examples:
a. "CDAI was evaluated as an alternative measure of disease activity to DAS28(3)-ESR [28]. In the current study, CDAI was included in the sensitivity analysis as it is an alternative disease activity assessment that does not require measurement of an acute phase reactant." - The first sentence can be deleted.

b. "and its definition of "remission" is more stringent than that of DAS28(3)-ESR". This was also referred in background and is in the discussion

c. "CDAI was calculated for patients with RA in the sample", in is not need, at all, to say that was in this RA sample...

d. defined as .... defined as .... (lines 174-175)

I think that the 9 lines of this section can be easily reduced to less than half.

4.6. Might the section "remission concordance" be reduced as well?

4.7. The McNemar's paired chi-squared test referred at page 13 line 238 is not mentioned in methods. The comparison being made and reported in this section was also not explained in methods. I do not see the need for this comparison.

4.8. "Variables with a P value < 0.05 were considered to have a significant association with either rheumatologist overstatement of remission or concordance" (line 216) - is this information really needed?

4.9. "Analysis was performed both for the primary concordance variable (using DAS28(3)-ESR) and the sensitivity concordance variable (using CDAI)." (line 218) - Is this information really needed? A separate section exists only for the "sensitivity analysis".

5) Results

5.1. The table 1 was initially very difficult to read because it has to many information and also because it should have 3 main sections: overall, concordant and discordant. The overall seems to be together with the concordant due to the top horizontal line. For different reasons I thin that
this table can be splitted in two or arranged in a different way. For instance, the statistical comparison is being made only between the 3rd and 4th column but this turns clear only when reading all results section, even being stated the note "a" in the bottom. It is not clear why some much information (variables and columns/sub-groups) are presented. I would prefer a table with the overall characteristics and a table with the 3rd and 4th columns.

5.2 Also in the table 1, what do the authors consider by "TJC/SJC/ESR most recent"? Was it possible to include clinical data - with upmost relevance for the study aims - from previous consultations? This was not reported in the methods.

5.3. Also in the table 1, how were the number of flares in the last 12 months assessed? is this information relevant for the study?

5.4. Still in Table 1, The inclusion of "marginal bone erosion, Synovium inflammation, osteoporosis, and non RA-related bone/joint inflammation" variables are not clear. The first time I saw them was in the table and I did not know how they were assessed and their direct relevance to the study. Only latter (page 14, line 296) I was informed about that: "They were more likely to have experienced a higher level of pain and had more joint inflammation and damage (e.g., destruction of cartilage, thinning of bone, and/or synovium inflammation) (P < 0.005)."

5.5. Is Fig 1 a really needed? It presents only 2 complementary percentages. It is also not the proper graphic representation. Reference to values in text only seems enough. Also, remember the (possible) discrepancy in the n.

5.6. In the end of the first section of results "Rheumatologist self-reported behaviors in use of standardized measures" the authors present a statistical test comparing hospital and office-based practice. This should be presented in the proper section.

5.7. The sentence in line 240-242 is one more example of not needed words: "A total of 531 RA patient records from 78 rheumatologists had all the information needed to calculate the DAS28(3)-ESR score for the analysis on concordance between remission according to rheumatologists' evaluation and according to a standardized measure." Info after DAS28(3)-ESR score can be deleted.
5.8. Info in line 246-247 is also not needed "from the analysis of DAS28(3)-ESR concordance because of missing at least one component required to calculate DAS28(3)-ESR scores."

5.9. Please consider to present the comparison of the characteristics of patients included from the exclude in supplementary files and refer only if they were similar or not. Or be more concise in the description.

5.10. The second paragraph of page 12 is a complete description of Table 2. It should be more concise. The info in the table should not be all reported in the text.

5.11. the 3rd par of page 12 (lines 261 - 2669 includes discussion in my perspective. Should be reduced. It report to fig 2. I'm not sure if all the 4 sub-groups of patients should be reported in the graph or only the discordant. But it can be like it is. The description in the text could be shorter. It is logical that if both assessment are concordant in remission, the patients will be bellow 2.6 cut-off etc.

5.12 line 267-268 - % can be presented and "as an alternative standardized measure to DAS28(3)-ESR." can be deleted.

5.13 - the n in lines 268-270 may not be needed

5.14. Lines 270-279 - over-description. Should be more concise

5.15 - lines 280 - "The percentage of rheumatologists overstating remission when using DAS28(3)-ESR (25.8%) was lower than that obtained when using CDAI (35.6%)." seems discussion to me.

5.16 - lines 281-284. As referred above, the McNemar’s test and the comparison between rheumatologist overstatement compared with DAS28 and with CDAI was not explained in methods. the percentage in all patients was 25.8 (DAs28) and 35.6% (CDAI) and only for patients with data for both was 24.1 and 31.7%, respectively. So what seem relevant is that the %
do not change too much. What authors compare is the difference from DAS to CDAI and this seems redundant as it is known - and stated by the authors - that CDAI is more strict.

5.17. paragraph from line 288-292 is not easy to read/understand and seems that come info are for methods

5.18 - Please clarify the reader in Fig 1c that more than 1 rheumatologist could have used more than 1 and thus more than 100% is possible when summing the figures.

6) Discussion

6.1. line 313-314 - Is this first sentence needed? Why not begin with the highlights of the study?

6.2. - line 321-322 "Although the literature suggests an increase in rheumatologists' use of standardized RA assessment measures, such as HAQ or DAS28" please include references.

6.3. the sequence of arguments/topics in the discussion does not seem logical. The 2nd par. of discussion is about limitations, but limitation are addressed in the end (page 18). So lines 325-336 could be deleted or moved to other place (more concisely). the decision to select DAS28(3)-ESR is explained in the methods, does not need to be repeated in line 328-329. Also authors refer that using DAS283v may be a limitation because it does not include PGA, but PGA accounts only 0.014 for the score... so this was not a limitation. They used the CDAI with takes into account much more the PGA...

Still about this paragraph, the topic being introduced in lines 332-334 (PGA being affected by comorbidities etc) does not relate with the discussion being done here, bit with the factors that might influence discrepancy.

6.4. paragraph 337 to 341 could be better summarized.
6.5. The size limitation of understated remission cohort could be discussed in the limitations section.

6.6. The following sentence is confusing to me (lines 347-349): "Regardless, the factors associated with rheumatologists' overstatement of remission were expected to inform future identification of patients or rheumatologists where overstatement of remission was more likely, and thus ensure that patients are not undertreated."

6.7. lines 350 to 357 - please see Fleishmann cut-off mentioned above. Also, after reading this paragraph, the question is: "So what?".

6.8. It is not clear what is being discussed with lines 358 to 361. It misses a clear connection with the results of the present study: "A meta-analysis of six studies performed between 1995 and 2009 showed that close monitoring of RA based on disease activity outcomes, combined with protocol-specific treatment adjustments, is associated with better outcomes than usual care [38]. The frequent assessment of RA disease activity with a standardized measure is an integral component of this approach."

6.9 - The info in lines 362 to 367 is also not new and stated in the background. The link with the present results is also lacking

6.10. line 405-406: "Thus the generalizability of our results to all rheumatologist practice may be influenced by willingness to participate." I would suppose that rheumatologists more willing to participate use more the disease activity indexes, thus the argument would be other way around. Could you please discuss this?

6.11 lines 409-410 - "...only severity at diagnosis was shown to differ significantly between the groups, with included patients being more severe at diagnosis" - This information seems new to me. Was it presented in the results?
In summary, the two major issues of this paper that might need a reformulation of analyses is:

A) the n being considered for analysis: 101 or 78 rheumatologists

B) the cut-offs of the DAS28-ESR(3v)

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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