PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Plagiarism in research: a survey of African medical journals
AUTHORS	Rohwer, Anke; Wager, Elizabeth; Young, Taryn; Garner, Paul

VERSION 1 – REVIEW

REVIEWER	Sonia Vasconcelos Science Education Program, Institute of Medical Biochemistry Leopoldo de Meis, Federal University of Rio de Janeiro (UFRJ)
REVIEW RETURNED	16-Jul-2018

GENERAL COMMENTS	The authors conducted a cross-sectional study and systematically searched the Africa Journals Online database (AJOL), aiming to investigate policies on plagiarism for the regional journals and procedures to detect it – 100 journals were included in the study. These journals were selected after applying eligibility criteria to the 179 biomedical journals in the AJOL database. The authors randomly selected five original research articles or reviews published in 2016 for each of the 100 journals to identify the extent of plagiarism and/or redundancy in the documents (n=495), using Turnitin software. They found 313 documents with evidence of plagiarism, particularly moderate plagiarism, according to the criteria established in the plagiarism framework on page 5. This is a carefully designed work, and it fills in an important gap in the literature addressing plagiarism in publications in the biosciences, particularly in LMIC (low-or middle-income countries). The authors offer a plagiarism framework that can be a useful tool, as they say, to assess the extent of plagiarism in journals – beyond “overall similarity index”. They conclude that “plagiarism is common in biomedical research articles and reviews published in Africa.”. On the sample, the authors acknowledge limitations, and say that “Although the African Journals Online database contains a large number of biomedical journals, it is not representative of all African journals”. However, despite setting out limitations, the authors would probably have to hedge the claim that “plagiarism is common in biomedical research articles and reviews published in Africa”. I thus raise a few points that they may clarify in their response. (1) On page 4, they state that “As methods copying was common, and can happen when people are using standard methods, we adjusted the definition to take this into account. Overall redundancy was scored in an equivalent way and for each article separate scores were given for plagiarism and redundancy.”. Then, on page 7, the authors report that “plagiarism was mostly in the introduction of articles (47%) followed by the discussion (39%) and the methods section (30%).”. My questions are the following: (1.1) Why did the authors include the methods section in the analysis? Unless they are sure that the sentences identified as

	"plagiarism" are not considered common knowledge among the members of those biomedical discourse communities, the plagiarism claim might be challenged. That said, criteria for categorizing sentences as plagiarism for the introduction section are not that clear to me, as common knowledge is not discussed or even mentioned in the analysis. It may be a relevant issue when analyzing quantitative data on plagiarism in introduction sections of research and review articles. As is, it is not clear whether or not the authors took this factor into consideration. (1.2) For example, were these sentences at the beginning of the introduction section, in which authors may be claiming centrality for the work, citing similar sentences found in other works? This question is not for downplaying the importance of plagiarism in introduction sections, but this qualitative aspect of the analysis is worth noting, as it can reduce biases and generalizations. As the authors do not comment on how they handled common knowledge-like sentences, my impression is that percentages may be overestimated. I recommend that the authors consider Roig, 2009 (http://science.sciencemag.org/content/325/5942/813.3), Helgesson and Eriksson, 2014 (https://link.springer.com/article/10.1007%2Fs11019-014-9583-8) and Moskovitz, 2015 (https://academic.oup.com/bioscience/article/66/1/5/2463944) – the latter looks at plagiarism concepts in articles in biosciences, calling attention to some disciplinary traditions influencing text recycling in methods sections, for example. If these points are addressed, a broader understanding of the data may be possible for readers. The Dummy OSI report generated by Turnitin provided on page 18 is useful, but it does not solve the issues raised. (2) On page 3, the authors say that "publishing practices in some low-and middle-income countries (LMICs) are still embedded in small volunteer editorial teams in university or professional society journals and may have fallen behind policies and procedures adopted in the USA, Europe and other high-income regions. Thus, science is at risk as researchers are under pressure to publish for promotion and short cuts can include plagiarism - particularly if they know that journals do not have policies or procedures to implement them. 8"…They suggest (page 10) that these policies would include "text-matching software to detect plagiarism in submitted manuscripts" as a screening tool to be adopted. I agree, but this argument needs to be further developed. The major assumption seems to be that pressure to publish and lack of such policies alone would explain the plagiarism rates reported by the authors for these African journals. But perhaps other factors might be influencing the results, such as rejection rates for the journals – they may be low for those with higher percentages of plagiarism in comparison to the others with lower rates. Additionally, considering the broader panorama of scientific productivity in Africa and its relationship with research integrity for African institutions may shed further light on the results. I recommend that the authors consider Makoni, 2018 (https://www.natureindex.com/news-blog/network-seeks-to-lift-african-research-integrity). An interesting issue is that "The traditional focus on teaching rather than research in African higher education institutes — coupled with limited access to research resources, training and support — makes academics vulnerable to poor research and publishing practices, says Christa Van Zyl, ARIN steering committee member and education researcher at Human Sciences Research Council of South Africa. ARIN is beginning to address the problem of research integrity in Africa by sensitizing

	researchers and empowering them to be more proactive in improving the situation, says Kombe, who has contributed to international discussions on responsible conduct through such platforms as the Science Forum South Africa and the Southern Africa Research and Innovation Management Association." (3) On page 5, the authors say that "As this study aimed to generate rather than test hypotheses, we did not test statistical significance between categories", but I missed specific comments on these possible hypotheses in the conclusions. (4) On page 13, Should "Did not publish an issues in 2016: 57" read "Did not publish an issue in 2016: 57"?

REVIEWER	Ksenija Bazdaric Department of Medical Informatics, Rijeka University School of Medicine, Croatia
REVIEW RETURNED	20-Jul-2018

GENERAL COMMENTS	Dear authors, as I am a research integrity editor and I check manuscripts for plagiarism every week I was very happy to review your manuscript. I think it is nicely written, the methodology is sound, results are presented clearly and the discussion is pertinent. I think that the manuscript can be accepted for publishing with minor revisions and it is a valuable contribution that could help to raise awareness of plagiarism detection in low and middle income countries. Introduction p. 3, line 16 – Turnitin and Crossref Similarity Check are not the same products. please correct „/" into „and". p.3 , l 20 – I would leave these 2 sentences for the Discussion section p.3, l 33 – you state that „Estimates of the occurrence of plagiarism are largely based on studies conducted in high-income countries." This is true, but not completely true as there were studies about occurence (for example https://www.ncbi.nlm.nih.gov/pubmed/22994914) in Africa. You could mention the studies that detect plagiarism in submitted manuscripts (You have listed 2 in the reference list: Taylor and Zhang, there are also 2 more: one American (Higgins et al. Research Integrity and Peer Review (2016) 1:13) and one is mine, Croatian (in 2010 Croatia was a middle income country, and it is the country with the lowest income in the EU now (Baždarić K, Bilić-Zulle L, Brumini G, Petrovečki M. Prevalence of Plagiarism in Recent Submissions to the Croatian Medical Journal. Sci Eng Ethics. 2012;18(2): 223-9.)). Methods: p.4., l 51 - we selected published articles published in 2016 as – delete published once p. 4., l 54: please explain in more details the randomization, did you use Excel or a randomizer or something else p14. on the protocol I see that you have similarity of 10% as a cut off value? why did you use it? (it was used in some other studies). Turnitin has 25%. Did you record the exact OSI for each manuscript? Did you manually verify all manuscripts or only some of them? did you exclude references before manual verification? I have found several cases of plagiarism only on references similarity. If you have found similarity in the methods did you check for salami slice or redundant publication?

	Results p.6. l.7. - Only 16 journals said they – Avoid comments in the results and therefore I think it would be more neutral to state: There are 16 (xx%) journals that state the usage of text matching software and of these... Table 2 – reference to plagirism software – please delete the word plagiarism p.7. l 24. Overall similarity index (OSI) of included articles – please give a median of the OSI with minimum, maximum and CI. Table 4 – there is a % sign were it is not needed (5th row). It could be a figure instead of table. p.8. l 27 - The most important factor that appeared to influence plagiarism was whether the journal referred to textmatching software or not. – this is an explanation for the discussion section, not results. Also, this is only a hypothesis, we cannot be sure that it is an influence, it can be associated. p. 8. l 33 – see comment above p. 8. l 34 – the most striking – also, see previous comment Discussion p. 9 , l 28 – you mention Taylor and Zhang. These studies indicate the OSI, but there is a big difference and you should comment that also. They are done in submitted manuscripts and the once you have analyzed are published. That means that the actual frequency of plagiarism in submitted manuscripts in African journals is much higher than reported here. Your study could methodologically be compared to Higgins study because they use criteria of one sentence also. You could also compare the occurence of plagairism and redundancy as in the Croatian Medical Journal study, it is pretty much the same and I think the main reason is the language. This situation does not happen in the analysis where majority of the authors are native speakers. The section analysis has shown the same pattern. p.9 l 30 – Zhang found 23% of plagiarism and redundancy, not only plagiarism. a quarter of 23% (39 papers out of 662) was high level, so not 23% but 6% (read on page 9 in abstract). Also, the Zhang used CrossRef, Taylor also and you used Turnitin, it is not the same. p.9. l 49 – technical plagiarism – it was said many times before you can cite Roig M (Biochem Med 2010; 20(3): 295-300)

REVIEWER	Xavier Bosch Hospital Clinic, Barcelona, Spain
REVIEW RETURNED	01-Aug-2018

GENERAL COMMENTS	This article is excellent and I do not have any major concern. I only have three suggestions:

	1. The title “Plagiarism in research: a survey of regional medical journals” would be more accurate if it says that it is a survey of African medical journals. 2. It is great that AJOL exists and, as potential reader, I would like to know about something this database. The AJOL website explains the requirements for inclusion and I suggest adding some words about this. 3. In the Abstract conclusion, authors say that plagiarism “can rapidly be eliminated if journal editors implement screening strategies”. However, no studies have explored whether this is true (proportion of plagiarism before and after implementation of screening strategies). “May” or "could" is better than “can”.

VERSION 1 – AUTHOR RESPONSE

Reviewer comments	**Author responses**
Editors Comments to Author:	
Please revise the ‘Strengths and limitations’ section of your manuscript (after the abstract). This section should contain five short bullet points, no longer than one sentence each, that relate specifically to the methods.	We have amended the bullet points in the revised manuscript.
Reviewer: 1	
(1) On page 4, they state that “As methods copying was common, and can happen when people are using standard methods, we adjusted the definition to take this into account. Overall redundancy was scored in an equivalent way and for each article separate scores were given for plagiarism and redundancy.” Then, on page 7, the authors report that “plagiarism was mostly in the introduction of articles (47%) followed by the discussion (39%) and the methods section (30%).” My questions are the following:	Thank you for this comment. To clarify: The purpose of the study was to describe patterns of plagiarism - that is sentences that were entirely identical between papers. This is recognised as poor academic practice, even when research uses similar methods. We stratified the analysis by section of the paper to allow the reader to examine plagiarism by section (Supplementary file 5).

(1.1) Why did the authors include the methods section in the analysis? Unless they are sure that the sentences identified as "plagiarism" are not considered common knowledge among the members of those biomedical discourse communities, the plagiarism claim might be challenged. That said, criteria for categorizing sentences as plagiarism for the introduction section are not that clear to me, as common knowledge is not discussed or even mentioned in the analysis. It may be a relevant issue when analyzing quantitative data on plagiarism in introduction sections of research and review articles. As is, it is not clear whether or not the authors took this factor into consideration.	We also adjusted our definition of the extent of plagiarism for methods: copying of one to two sentences in the methods section was not regarded as plagiarism, copying of three to six sentences was regarded as 'some plagiarism' and copying of more than six sentences or at least four linked sentences was regarded as 'moderate plagiarism'. This seemed reasonable to the team and is described in the methods. We took the standard definition of plagiarism. 'Common knowledge' is something different to what we are studying: where concepts and methods are the same, and
(1.2) For example, were these sentences at the beginning of the introduction section, in which authors may be claiming centrality for the work, citing similar sentences found in other works? This question is not for downplaying the importance of plagiarism in introduction sections, but this qualitative aspect of the analysis is worth noting, as it can reduce biases and generalizations. As the authors do not comment on how they handled common knowledge-like sentences, my impression is that percentages may be	it is generally known by people within a particular community. Common knowledge or previously used methods can be written by the author or copied by someone else: if this isn't attributed, then this is considered to constitute plagiarism. One reason why copying chunks of text from published methods sections is considered poor practice is that there is a danger that authors copy
overestimated. I recommend that the authors consider Roig, 2009 (http://science.sciencemag.org/content/325/5942/813.3), Helgesson and Eriksson, 2014 (https://link.springer.com/article/10.1007%2Fs11019-014-9583-8) and Moskovitz, 2015	methods or techniques they think look good but the text does not describe what they actually did.

[redacted] (https://academic.oup.com/bioscience/article/66/1/5/2463944) – the latter looks at [redacted] plagiarism concepts in articles in biosciences, calling attention to some disciplinary traditions influencing text recycling in methods sections, for example. If these points are addressed, a broader understanding of the data may be possible for readers. The Dummy OSI report generated by Turnitin provided on page 18 is useful, but it does not solve the issues raised.	Thank you for the suggested references. We have added Roig 2009 and Helgesson and Eriksson 2014 to the manuscript.
(2) On page 3, the authors say that "publishing practices in some low-and middle-income countries (LMICs) are still embedded in small volunteer editorial teams in university or professional society journals and may have fallen behind policies and procedures adopted in the USA, Europe and other high-income regions. Thus, science is at risk as researchers are under pressure to publish for promotion and short cuts can include plagiarism - particularly if they know that journals do not have policies or procedures to implement them. 8"…They suggest (page 10) that these policies would include "text-matching software to detect plagiarism in submitted manuscripts" as a screening tool to be adopted. I agree, but this argument needs to be further developed. The major assumption seems to be that pressure to publish and lack of such policies alone would explain the plagiarism rates reported by the authors for these African journals. But perhaps other factors might be influencing the results, such as rejection	Thank you for your comment. We take the point-this is only one possible explanation, we have no evidence for it, and we have removed it. Thank you for your suggestion to read the article on the African Network of Research Integrity. We have referred to this in the discussion section.

rates for the journals – they may be low for those with higher percentages of plagiarism in comparison to the others with lower rates. Additionally, considering the broader panorama of scientific productivity in Africa and its relationship with research integrity for African institutions may shed further light on the results. I recommend that the authors consider Makoni, 2018 (https://www.natureindex.com/news-blog/network-seeks-to-lift- [redacted] african-research-integrity). An interesting issue is that "The traditional focus on teaching [redacted] rather than research in African higher education institutes — coupled with limited access to research resources, training and support — makes academics vulnerable to poor research and publishing practices, says Christa Van Zyl, ARIN steering committee member and education researcher at Human Sciences Research Council of South Africa. ARIN is beginning to address the problem of research integrity in Africa by sensitizing researchers and empowering them to be more proactive in improving the situation, says Kombe, who	

has contributed to international discussions on responsible conduct through such platforms as the Science Forum South Africa and the Southern Africa Research and Innovation Management Association."	
(3) On page 5, the authors say that "As this study aimed to generate rather than test hypotheses, we did not test statistical significance between categories", but I missed	Thanks for pointing this out. We agree that this wording is not helpful. This was a purely descriptive study. We did not want to conduct multiple significance testing

specific comments on these possible hypotheses in the conclusions.	and generate spurious associations. So we have simply removed this.
(4) On page 13, Should “Did not publish an issues in 2016: 57” read “Did not publish an issue in 2016: 57”?	Thanks for pointing out this typo. We have corrected it as suggested.
Reviewer: 2	
Introduction p. 3, line 16 – Turnitin and Crossref Similarity Check are not the same products. please correct „/“ into „and“.	Crossref has partnered with Turnitin, who provides the iThenticate system, which is referred to as the Crossref similarity check (https://www.crossref.org/services/similarity-check/). However, based on one of the other reviewer’s comments, we have now deleted this sentence in the introduction section.
p.3 , l 20 – I would leave these 2 sentences for the Discussion section p.3, l 33 – you state that „Estimates of the occurrence of plagiarism are largely based on studies conducted in high-income countries.“ This is true, but not completely true as there were studies about occurence (for example https://www.ncbi.nlm.nih.gov/pubmed/22994914) in Africa. You could mention the studies that detect plagiarism in submitted manuscripts (You have listed 2 in the reference list: Taylor and Zhang, there are also 2 more: one American (Higgins et al. Research Integrity and Peer Review (2016) 1:13) and one is mine, Croatian (in 2010 Croatia was a middle income country, and it is the country with the lowest income in the EU now (Baždarić K, Bilić-Zulle L, Brumini G, Petrovečki M. Prevalence of	Thank you for your suggestion. We have included the two references and have amended the section in the introduction, as well as the one in the discussion to include the suggested references.

Plagiarism in Recent Submissions to the Croatian Medical Journal. Sci Eng Ethics. 2012;18(2): 223- 9.)).	
p.4., l 51 - we selected published articles published in 2016 as – delete published once	Thanks – we have corrected this.

p. 4., l 54: please explain in more details the randomization, did you use Excel or a randomizer or something else	We used Excel and have clarified this in the manuscript
p14. on the protocol I see that you have similarity of 10% as a cut off value? why did you use it? (it was used in some other studies). Turnitin has 25%. Did you record the exact OSI for each manuscript?	The study was done in stages. In the first round, we manually reviewed any articles that had an OSI above 10%. However, we found this arbitrary cut-off unhelpful, and were afraid of false negatives, so we decided to check articles with an OSI below 10% using our framework. Therefore, we recorded the exact OSI for all 495 articles and then manually verified the presence and extent of plagiarism using our framework.
Did you manually verify all manuscripts or only some of them?	Yes, we manually reviewed all articles, regardless of OSI, using our framework. We describe this in the methods section: “We submitted the PDFs of all articles to Turnitin text-matching software. Turnitin generated a similarity report containing the overall similarity index (OSI), expressed as the percentage of matching text,[15] excluding quotations and references. We manually reviewed all similarity reports with the plagiarism framework (Table 1).”

did you exclude references before manual verification? I have found several cases of plagiarism only on references similarity.	Yes, references were excluded, as reported in the manuscript: "Turnitin generated a similarity report containing the overall similarity index (OSI), expressed as the percentage of matching text,[15] excluding quotations and references."
If you have found similarity in the methods did you check for salami slice or redundant publication?	Each article was checked for plagiarism and redundancy (copying of one's own work). None of the manuscripts were entirely copied or published in duplicate.
Results p.6. l.7. - Only 16 journals said they – Avoid comments in the results and therefore I think it would be more neutral to state: There are 16 (xx%) journals that state the usage of text matching software and of these...	We have amended the sentence.
Table 2 – reference to plagirism software – please delete the word plagiarism	We have changed this to "text-matching software".
p.7. l 24. Overall similarity index (OSI) of included articles – please give a median of the OSI with minimum, maximum and CI	We have added the following to the results section: "The median OSI was 15%, with a minimum OSI of 0% and a maximum of 68%."
Table 4 – there is a % sign were it is not needed (5th row). It could be a figure instead of table.	Thanks. We have removed the redundant % sign. We discussed having a figure instead of a table but prefer keeping a table as it provides more details.

p.8. l 27 - The most important factor that appeared to influence plagiarism was whether the journal referred to textmatching software or not. – this is an explanation for the discussion section, not results. Also, this is only a hypothesis, we cannot be sure that it is	Thank you for your comment. We explored numerous characteristics as reported in Supplementary file 6. As we agree with your point not to explain the findings in the results section, we have decided to include Supplementary file 6 as a Table in

an influence, it can be associated. p. 8. l 33 – see comment above p. 8. l 34 – the most striking – also, see previous comment	the manuscript. We have also amended the text in the results section.
Discussion p. 9 , l 28 – you mention Taylor and Zhang. These studies indicate the OSI, but there is a big difference and you should comment that also. They are done in submitted manuscripts and the once you have analyzed are published. That means that the actual frequency of plagiarism in submitted manuscripts in African journals is much higher than reported here. Your study could methodologically be compared to Higgins study because they use criteria of one sentence also. You could also compare the occurence of plagairism and redundancy as in the Croatian Medical Journal study, it is pretty much the same and I think the main reason is the language. This situation does not happen in the analysis where majority of the authors are native speakers. The section analysis has shown the same pattern.	We have amended the relevant paragraph in the discussion section.
p.9 l 30 – Zhang found 23% of plagiarism and redundancy, not only plagiarism. a quarter of 23% (39 papers out of 662) was high level, so not 23% but 6% (read on page 9 in abstract).	Thanks for pointing this out. We have amended the sentence as follows: "Zhang (2010) used text-matching software to screen manuscripts submitted to a Chinese journal for plagiarism [19] and found that 23% contained plagiarism or redundancy, of which a quarter contained high levels of plagiarism. However, it is not clear how plagiarism was defined."

Also, the Zhang used CrossRef, Taylor also and you used Turnitin, it is not the same.	See comment above regarding Crossref similarity check and Turnitin.
p.9. l 49 – technical plagiarism – it was said many times before you can cite Roig M (Biochem Med 2010; 20(3): 295-300)	Thanks for this suggestion. We have added the reference to the discussion.
Reviewer: 3	
1. The title “Plagiarism in research: a survey of regional medical journals” would be more accurate if it says that it is a survey of African medical journals.	We agree with your suggestion and have amended the title accordingly.
2. It is great that AJOL exists and, as potential reader, I would like to know about something this database. The AJOL website explains the requirements for inclusion and I suggest adding some words about this.	We have added an additional sentence to the discussion section to describe AJOL.

3. In the Abstract conclusion, authors say that plagiarism “can rapidly be eliminated if journal editors implement screening strategies”. However, no studies have explored whether this is true (proportion of plagiarism before and after implementation of screening strategies). “May” or "could" is better than “can”.	We have changed ‘can’ to ‘could’.
FORMATTING AMENDMENTS (if any)	
- Kindly re-upload figure 1 with at least 300 dpi resolution and at least 90mm x 90mm of width in either TIFF or JPG format.	We have uploaded Figure 1 in JPEG format with 300dpi.
- Patient and Public Involvement: Authors must include a statement in the methods section of the manuscript under the sub-heading 'Patient and Public Involvement'.	We have added the following to the methods section: “We did not involve patients or the public in this study.”

This should provide a brief response to the following questions: How was the development of the research question and outcome measures informed by patients' priorities, experience, and preferences? How did you involve patients in the design of this study? Were patients involved in the recruitment to and conduct of the study? How will the results be disseminated to study participants? For randomised controlled trials, was the burden of the intervention assessed by patients themselves? Patient advisers should also be thanked in the contributorship statement/acknowledgements. If patients and or public were not involved please state this.	

VERSION 2 – REVIEW

REVIEWER	Sonia Vasconcelos, Associate Professor Science Education Program, Institute of Medical Biochemistry Leopoldo de Meis, Federal University of Rio de Janeiro (UFRJ)
REVIEW RETURNED	09-Sep-2018

GENERAL COMMENTS	The authors have responded appropriately to the issues raised.

REVIEWER	Ksenija Bazdaric Rijeka University, Faculty of Medicine, Department of Medical Informatics
REVIEW RETURNED	11-Sep-2018

GENERAL COMMENTS	Dear authors, I have read your answers to all reviewer's comments and the article and I agree with your comments and revisions made, especially in the discussion section. I recommend acceptance of the manuscript.