PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Older patients' perception to deprescribing in resource limited settings: A cross sectional study in an Ethiopia University Hospital.
AUTHORS	Tegegn, Henok; Tefera, Yonas; Erku, Daniel; Haile, Kaleab; Abebe, Tamrat; Chekol, Fasil; azanaw, yonas; Ayele, Asnakew

VERSION 1 – REVIEW

REVIEWER	Emily Reeve University of Sydney, Australia
REVIEW RETURNED	02-Dec-2017

GENERAL COMMENTS	I commend the authors on the research undertaken. This paper presents new results on patients' attitudes towards deprescribing in a population which has not yet been studied (those in a resource limited setting). However, I have several major concerns with the manuscript. In particular, the way the results of the rPATD have been analysed and limited linking of results with their discussion and other relevant research. I have provided more details on these below. I recommend that the study itself is suitable for publication, however, significant revisions, including redoing analysis is required before publication. I hope the authors find my comments helpful. Introduction: The PATD has been used in two other countries not mentioned in this manuscript – I suggest that these are read and mentioned in the manuscript: Ng WL, Tan MZW, Koh EYL, Tan NC. Deprescribing: What are the views and factors influencing this concept among patients with chronic diseases in a developed Asian community? Proc Singapore Healthc. March 2017:1-8. doi:10.1177/2010105817699633. Sirois C, Ouellet N, Reeve E. Community-dwelling older people's attitudes towards deprescribing in Canada. Res Soc Adm Pharm. 2017;13(4):864-870. doi:10.1016/j.sapharm.2016.08.006. Methods: More clarity is needed in how the rPATD was altered for use in this setting. The method of translation is mentioned, but it would be good to know if any alterations to the wording were required for this different culture/setting. Additionally, it should be noted that the rPATD was developed for self-administration while in this study it was done as interviewer administered. This study used a 4 point Likert scale while the validated rPATD uses a 5-point. Please provide discussion/rationale for these alterations. Page 5, line 38-48: "The rPATD questionnaire has four major factors including Burden factor (4 items); Appropriateness factor (5 items); Concerns about stopping factor (4 items); Involvement factor

	(4items) and additional two global questions are also considered." The rPATD has 5 questions in each of the 4 factors (not 4 in some as reported) – in the tables all 5 questions are reported? While statistical analysis is presented, it is not clear what the normality testing was applied to, nor which tests were used to compare means as reported in the results. Page 6-7: The operational definition of poor attitude towards deprescribing – I have a few concerns about this, and therefore all the analysis related to this variable. The results of the rPATD have not previously been treated in this way. While the four factors are all related to attitudes towards deprescribing and proposed to relate to willingness to deprescribe, we do not yet know how these factors interact with each other: e.g. if they are additive, synergistic or should have different weights. Individual questions on their own have variable strength to the overall message of 'poor attitude towards deprescribing' – such as 'Overall, I am satisfied with my current medicines' – was agreement to this question considered a poor or a good attitude towards deprescribing? I am not convinced that this is an appropriate way to combine the results of the rPATD, and don't see the clinical relevance of doing it as such. Additionally, here 19 Qns is mentioned, though again 22 questions are mentioned later? Results: Figure 1: what does the % related to? % of participants who had a mean score >2.5? Additionally, please justify the combining of the two general questions? Page 9 and Table 2: The mean and SD are presented for each question – please justify this analysis – the data can not be considered continuous in nature as it comes from Likert questions which are ordinal. Was normality tested for each of the questions? What test was used to compare means? Table 3: In this table the questions are presented in English – these do not match the original English version of the rPATD. Are these the results of the back-translation? Some appear to have a slightly different meaning. I suggest either using the original English wording (with a note about the back translation/slightly altered wording) – or explain why the wording is different. Page 11: As discussed previously, I don't think that the analysis presented about 'good attitude towards deprescribing' is appropriate. Discussion: Overall while the discussion includes several important points – I was confused by how several of the statements related to the results and/or previous research. A few examples are below: "The mean number of daily medications of elderly patients in this study was 3.43 ± 1.50. Findings from Kalogianis MJ et al. study conducted in Australia shows total number of medications per patient was 14.6 ± 5.3, which is a lot higher than our result" – why is this single study mentioned here as contrast – what message/results does this relate to? "Two third of patients believe that they spent a lot of money to medicines; one of the single most indicator to patients medications burden." – why is it the single greatest indicator to medication burden? Care needs to be taken with wording – the rPATD has not yet been established to have predictive ability, i.e. how an individual responds to the rPATD may or may not mean that they would actually stop a medicine/be willing to deprescribe. "Majority of the respondents feel that they took at least one of the

	medicines neither needed longer nor working anymore. Despite of this perception, elderly patients are solely dependent on the physician decision and look to hear the likelihood of stopping the medicine from the doctor side [11].” – I think that this paragraph is trying to highlight the interaction of patient’s beliefs and the trust in their doctor, I would suggest revising this section as it is not clear. The limitations of this study have not been adequately discussed, in particular the limitations of translating a validated tool, issues of response bias and generalisability. Some of the dot points presented as ‘Strengths and limitations of this study’ do not appear to be strengths or limitations (i.e. dot point 3 and 5). Conclusion: “Elderly patients are found to be exclusively dependent on the clinician’s decision to deprescribing.” – what result in your study does this relate to? I would recommend an English language revision for this manuscript. Minor points • Please be consistent with the spelling of deprescribing versus de-prescribing (deprescribing is the more commonly used variation) • Page 4 line 35 – “Several studies conducted in developed countries regarding older people’s attitudes toward deprescribing reported a higher rate of willingness to discontinue their medications ranging from 40.5% to 90% [10-12].” I don’t believe that the figure of 40.5% is correct – which reference does this relate to? I also wondered why Ref 23 wasn’t mentioned here? • Page 6, line 7: I presume this is meant to be ≥5 medications (not ≤5 medications) • While the STROBE statement is included – only Y/NA are provided – please include the page numbers of where it can be found. E.g. they have reported Yes to indicating number of participants with missing data but this is not found in the manuscript.

REVIEWER	Amy Page Monash University
REVIEW RETURNED	11-Dec-2017

GENERAL COMMENTS	Thank you for the opportunity to review this article. The qualitative barriers and enablers to deprescribing have been well described in developed nations (particularly Australia and Canada), but there has been limited research into these issues in developing countries. Therefore, this paper contributes important information. I have some comments for the authors to consider to strengthen their manuscript. Emphasis: Although the title emphasises the developing country issue, it is more or less ignored throughout the abstract. It should be emphasised as it is this aspect that makes the study novel. English writing: 1. Tense. The article swaps from using the past tense to the present and even the future tense. The authors should rewrite the artlcle to ensure that it is consistently written in the past tense.

	2. Hyphenation. Deprescribing is, by convention, written without the hyphen. 3. There are a number of sentences with grammatical errors. It should be re-written and proof read to insure it is grammatical. e.g. In spite of these, majority of elderly patients do not have good knowledge of their medicines and merely dependent on the health care providers to initiate decisions about their medications. (Please note: this is not the only sentence, I just provide it as an illustration.) Introduction: 1. The authors appear to have paraphrased their definiton of deprescribing from Scott et al. I suggest that they quote directly to maintain the consistency of the definition. 2. The authors assert that it "has a number of benefits to the patient including reducing costs associated with medicines". The pharmaco-economic data has not been, as yet, ascertained. Further research such as RCT data is needed before this informatino can be ascertained, and it is, as yet, undetermined whether deprescribing has an effect on overall costs. I suggest that the authors focus on the endpoints associated with deprescribing, or that they cite the outcomes from either Potter 2016 or Page 2016 (full citations below). Potter K, Flicker L, Page A, Etherton-Beer C. Deprescribing in frail older people: a randomised controlled trial. PLoS one. 2016;11(3):e0149984. Page AT, Clifford RM, Potter K, Schwartz D, Etherton-Beer CD. The feasibility and the effect of deprescribing in older adults on mortality and health: A systematic review. British Journal of Clinical Pharmacology. 2016; 82(3):583-623 3. I would like to see one paragraph to frame polypharmacy in the context of developing countries or of Ethiopa. Some ideas for the context may be to include: What degree of polypharmacy is there locally? Are chronic diseases and multimorbidity common locally? Does the government provide medications at a subsidised cost, or are they affordable, or are they viewed as very expensive and thus that it is a privilege to use medications? 4. Terminology is inconsistent. Reconded to use older people in preference to other terminology such as elderly. Methods: 1. You state that 'The investigators were properly trained on the instrument and ways of approaching the patients and securing their permission for interview prior to the data collection process' Can you please explain what the 'proper' training is? 2. It is good to see polypharmacy defined. However, I think you may have used the less than symbol when you meant greater than. 3. Were there enough participants using 10+ medications to include them as a seperate group? 4. What was the purpose of using the Beliefs about medicines questionaire as well as the rPATD?

	5. Please reference SPSS. 6. You have re-defined deprescribing and polypharmacy. This second definition is repetitive, but it also is slightly inconsistent. Consider using these definitions to replace the definitions used earlier in the text. Results: 1. You correctly state that "Most of the participants were men (173 individuals, 54.92 %)". Consider leaving this out, because at this level it appears to be roughly even, so it is not particularly important. Discussion: 1. The discussion is a bit over-reaching at the moment. For example, the results do not support the assertion that trust is a subtheme. 2. The authors should re-situate their study in the context of the developing ocuntry to highlight the novelty of this study. 3. This paragraph is unclear and needs to be rewritten for clarity: Majority of the respondents feel that they took at least one of the medicines neither needed longer nor working anymore. Despite of this perception, elderly patients are solely dependent on the physician decision and look to hear the likelihood of stopping the medicine from the doctor side 4. In the paragraph about how to deprescribe, may I please recommend referencing either one or both of Page 2017 or Potter 2017 as these articles describe how the available tools to deprescribe, and how to deprescribe respectively. Page A, Potter K, Clifford R, Etherton-Beer C. Deprescribing in older people. Maturitas. 2016;91:115-134. Potter K, Page A, Clifford R, Etherton-Beer C. Deprescribing: A guide for medication reviews. Journal of Pharmacy Practice and Research. 2016;46(4): 358–367 References: The referencing style is inconsistent. For example, reference 22 is truncated after three authors to et al, while reference 23 lists 8 authors by name. Not all references are correct for the authors names. For example, reference 26 should be Page AT, Etherton-Beer CD, Clifford RM, Burrows S, Eames M, Potter K (not as it is written currently which is Page AT, Christopher D. Beer E, Rhonda MR, Sally B, Eames M, Potter K). Some references use surname, first name, initials etc. e.g. Emily Reeve is referred to as Reeve E and Emily Reeve in the reference list.

VERSION 1 – AUTHOR RESPONSE

Date: 1/1/2018
Dear Editor,BMJ Open

We appreciate your feedback and the referee's input on our paper“Deprescribing and reduction of inappropriate polypharmacy in resource limited settings: Older patients’ perception in an Ethiopia University Hospital; Submission ID: bmjoppen-2017-020590”, we hope this render our paper to be accepted for publication. We did incorporate most of the feedback provided, which we believe has strengthened our paper. Below is a list of issues we addressed:
As recommended, we made all the changes accepting the reviewer comments. Below acknowledged and clarified further point-to-point answer for the reviewer comments.

Editorial Corrections requested
Please revise your title to state the research question, study design, and setting (location). This is the preferred format for the journal.
Authors reply: The title has been revised accordingly
Please provide specific page and line numbers for each item in the STROBE checklist.
Authors reply: page and line numbers have been added in the STROBE checklist
- Please work to improve the quality of English throughout the manuscript, either with the help of a native speaking colleague or with the assistance of a professional copyediting agency.
Authors reply: The manuscript was copyedited and revised for any grammatical and/or typo error by someone who is fluent in English (Akshaya Srikanth Bhagavathula).

Reviewer 1:
I commend the authors on the research undertaken. This paper presents new results on patients’ attitudes towards deprescribing in a population which has not yet been studied (those in a resource limited setting). However, I have several major concerns with the manuscript. In particular, the way the results of the rPATD have been analysed and limited linking of results with their discussion and other relevant research. I have provided more details on these below. I recommend that the study itself is suitable for publication, however, significant revisions, including redoing analysis is required before publication. I hope the authors find my comments helpful.
Authors reply: We really feel grateful for the reviewers’ positive suggestions and comments. Being from resource-limited country, we tried to provide valuable scientific information related older patients’ perception regarding deprescribing and reduction of inappropriate polypharmacy in Ethiopia through this research.
Introduction:
The PATD has been used in two other countries not mentioned in this manuscript – I suggest that these are read and mentioned in the manuscript:
Ng WL, Tan MZW, Koh EYL, Tan NC. Deprescribing: What are the views and factors influencing this concept among patients with chronic diseases in a developed Asian community? Proc Singapore Healthc. March 2017:1-8. doi:10.1177/2010105817699633.
Sirois C, Ouellet N, Reeve E. Community-dwelling older people’s attitudes towards deprescribing in Canada. Res Soc Adm Pharm. 2017;13(4):864-870. doi:10.1016/j.sapharm.2016.08.006.
Authors reply: Both References have been included in the introduction part
Methods:
More clarity is needed in how the rPATD was altered for use in this setting. The method of translation is mentioned, but it would be good to know if any alterations to the wording were required for this different culture/setting.
Authors reply: No alteration was made to the original items while collecting data. Unfortunately we included words taken from the SPSS labeling. Now we have made corrections and kept the original items.
Additionally, it should be noted that the rPATD was developed for self-administration while in this study it was done as interviewer administered.
Authors reply: Due to low literacy level in our setting, we were forced to interview most of the patients to clarify the questions.

This study used a 4 point Likert scale while the validated rPATD uses a 5-point. Please provide discussion/rationale for these alterations.
Authors reply: We have preferred a 4 point likert scale because it can allow the participants to be more discriminating and avoid misinterpretation of mid points. This has been explained in the methodology as suggested by the reviewer.
Page 5, line 38-48: "The rPATD questionnaire has four major factors including Burden factor (4 items); Appropriateness factor (5 items); Concerns about stopping factor (4 items); Involvement factor (4items) and additional two global questions are also considered." The rPATD has 5 questions in each of the 4 factors (not 4 in some as reported) – in the tables all 5 questions are reported?
Authors reply: This has been corrected
While statistical analysis is presented, it is not clear what the normality testing was applied to, nor which tests were used to compare means as reported in the results.
Authors reply: Both the Shapiro–Wilk and Kolmogorov-Smirnov was employed to the normality testing. Median has been used in the non parametric data in the modified result parts.
Page 6-7: The operational definition of poor attitude towards deprescribing – I have a few concerns about this, and therefore all the analysis related to this variable. The results of the rPATD have not previously been treated in this way. While the four factors are all related to attitudes towards deprescribing and proposed to relate to willingness to deprescribe, we do not yet know how these factors interact with each other: e.g. if they are additive, synergistic or should have different weights. Individual questions on their own have variable strength to the overall message of 'poor attitude towards deprescribing' – such as 'Overall, I am satisfied with my current medicines' – was agreement to this question considered a poor or a good attitude towards deprescribing? I am not convinced that this is an appropriate way to combine the results of the rPATD, and don't see the clinical relevance of doing it as such.
Authors reply: We are really thankful for very important comment of the reviewer. We have made a major modification to the analysis to be consistent to other studies and can scientifically sound. Therefore, we omitted the previous overall attitude merging all four factors rather we separately do analysis of correlations to the individual survey items such as Patients' perception of side effect from their medicines, the patients' willingness to discontinue their medications if advised by doctor, and Overall satisfaction of patients with their medication taken.
Additionally, here 19 Qns is mentioned, though again 22 questions are mentioned later?
Authors reply: This was incorrectly written and omitted from the manuscript now.

Results:
Figure 1: what does the % related to? % of participants who had a mean score >2.5? Additionally, please justify the combining of the two general questions?
Authors reply: As per to reviewer's suggestion, after changing the analysis, this figure has been omitted from the result part.
Page 9 and Table 2: The mean and SD are presented for each question – please justify this analysis – the data cannot be considered continuous in nature as it comes from Likert questions which are ordinal. Was normality tested for each of the questions? What test was used to compare means?
Authors reply: Initially we used mean and SD as the Likert scale was previously treated as continuous variable by giving weight for each individual scale. However, we have now changed those into frequency and median for non parametric and nominal data.
Table 3: In this table the questions are presented in English – these do not match the original English version of the rPATD. Are these the results of the back-translation? Some appear to have a slightly different meaning. I suggest either using the original English wording (with a note about the back translation/slightly altered wording) – or explain why the wording is different.
Authors reply: We have used the same original English version of the rPATD in the data collection part. However, the short versions of wording filled in the SPSS were displayed in the result part. We have corrected as suggested by the reviewer.

Page 11: As discussed previously, I don't think that the analysis presented about 'good attitude towards deprescribing' is appropriate.
Authors reply: This has been omitted and modified as as suggested by the reviewer

Discussion:
Overall while the discussion includes several important points – I was confused by how several of the statements related to the results and/or previous research. A few examples are below:
"The mean number of daily medications of elderly patients in this study was 3.43 ± 1.50. Findings from Kalogianis MJ et al. study conducted in Australia shows total number of medications per patient was 14.6 ± 5.3, which is a lot higher than our result" – why is this single study mentioned here as contrast – what message/results does this relate to?
Authors reply: Other studies have been also included now and we tried to mention the low median number of our study as compared to others because majority (81.6%) of the patients still agreed to stop one of the medicines even if they don't have large number of medications. This has been further explained based on reviewers comment.
"Two third of patients believe that they spent a lot of money to medicines; one of the single most indicator to patients medications burden." – why is it the single greatest indicator to medication burden? Care needs to be taken with wording – the rPATD has not yet been established to have predictive ability, i.e. how an individual responds to the rPATD may or may not mean that they would actually stop a medicine/be willing to deprescribe.
Authors reply: "one of the single most indicator to patients medications burden" has been deleted to avoid confusion and modified as suggested by the reviewer
"Majority of the respondents feel that they took at least one of the medicines neither needed longer nor working anymore. Despite of this perception, elderly patients are solely dependent on the physician decision and look to hear the likelihood of stopping the medicine from the doctor side [11]." – I think that this paragraph is trying to highlight the interaction of patient's beliefs and the trust in their doctor, I would suggest revising this section as it is not clear.
Authors reply: As per to the reviewer comment and major the change in the analysis, this statement is invalid and have been omitted from the discussion part.
The limitations of this study have not been adequately discussed, in particular the limitations of translating a validated tool, issues of response bias and generalisability. Some of the dot points presented as 'Strengths and limitations of this study' do not appear to be strengths or limitations (i.e. dot point 3 and 5).
Authors reply: Corrected as per to the reviewer comments.
Conclusion:
"Elderly patients are found to be exclusively dependent on the clinician's decision to deprescribing." – what result in your study does this relate to?
Authors reply: omitted as the analysis is also a bit changed.
I would recommend an English language revision for this manuscript.
Authors reply: The manuscript was copyedited and revised for any grammatical and/or typo error by someone who is fluent in English
Minor points
• Please be consistent with the spelling of deprescribing versus de-prescribing (deprescribing is the more commonly used variation)
Authors reply: corrected
• Page 4 line 35 – "Several studies conducted in developed countries regarding older people's attitudes toward deprescribing reported a higher rate of willingness to discontinue their medications ranging from 40.5% to 90% [10-12]." I don't believe that the figure of 40.5% is correct – which reference does this relate to? I also wondered why Ref 23 wasn't mentioned here?
Authors reply: corrected
• Page 6, line 7: I presume this is meant to be ≥5 medications (not ≤5 medications)
Authors reply: corrected

• While the STROBE statement is included – only Y/NA are provided – please include the page numbers of where it can be found. E.g. they have reported Yes to indicating number of participants with missing data but this is not found in the manuscript.
Authors reply: corrected
Reviewer 2:
1. Tense. The article swaps from using the past tense to the present and even the future tense. The authors should rewrite the artlcle to ensure that it is consistently written in the past tense.
2. Hyphenation. Deprescribing is, by convention, written without the hyphen.
3. There are a number of sentences with grammatical errors. It should be re-written and proof read to insure it is grammatical. e.g. In spite of these, majority of
elderly patients do not have good knowledge of their medicines and merely dependent on the health care providers to initiate decisions about their medications. (Please note: this is not the only sentence, I just provide it as an illustration.)
Authors reply:The authors are thankful. We endorse your valuable comments regarding the language issues and resolved in our revised manuscript. The paper was also proofread by someone who is fluent in English to improve the quality of the manuscript.
Introduction:
1. The authors appear to have paraphrased their definiton of deprescribing from Scott et al. I suggest that they quote directly to maintain the consistency of the definition.

2. The authors assert that it "has a number of benefitsto the patient including reducing costs associated with medicines". The pharmaco-economic data has not been, as yet, ascertained. Further research such as RCT data is needed before this informatino can be ascertained, and it is, as yet, undetermined whether deprescribing has an effect on overall costs. I suggest that the authors focus on the endpoints associated with deprescribing, or that they cite the outcomes from either Potter 2016 or Page 2016 (full citations below).
Potter K, Flicker L, Page A, Etherton-Beer C. Deprescribing in frail older people: a randomised controlled trial. PLoS one. 2016;11(3):e0149984.
Page AT, Clifford RM, Potter K, Schwartz D, Etherton-Beer CD. The feasibility and the effect of deprescribing in older adults on mortality and health: A systematic review. British Journal of Clinical Pharmacology. 2016; 82(3):583-623
Authors reply: The authors are grateful for the comments. We re-write this paragraph in light of the comments raised, and we incorporated the reference by Page etal. Kindly check the changes.

3. I would like to see one paragraph to frame polypharmacy in the context of developing countries or of Ethiopa. Some ideas for the context may be to include: What degree of polypharmacy is there locally? Are chronic diseases and multimorbidity common locally? Does the government provide medications at a subsidised cost, or are they affordable, or are they viewed as very expensive and thus that it is a privilege to use medications?
Authors reply: The authors are thankful for the recommendation. We tried to assess the prevalence of multimorbidity in Ethiopia (no study has been published so far regarding polypharmacy in Ethiopia). Kindly check the revised manuscript for the amendments made.

4. Terminology is inconsistent. Reconded to use older people in preference to other terminology such as elderly.
Methods:
1. You state that
'The investigators were properly trained on the instrument and ways of approaching the patients and securing their permission for interview prior to the data collection process'
Can you please explain what the 'proper' training is?
Authors reply: The authors are thankful for the questions. We mean by ‘proper training’ is a training conducted in the actual place of study setting to see if the investigators could manage data collection

efficiently with adequate background knowledge about the study, working independently; and good communication skills.
2. It is good to see polypharmacy defined. However, I think you may have used the less than symbol when you meant greater than.
Authors reply: The authors are thankful. It has been corrected
3. Were there enough participants using 10+ medications to include them as a seperate group?
Authors reply: The authors are thankful for the comment. Unfortunately, we had only one patient taking 11 medications.
4. What was the purpose of using the Beliefs about medicines questionnaire as well as the rPATD?
Authors reply: Because both Beliefs about medicines questionnaire and the rPATD questionnaire enabled us to address our research question. They both are validated to assess older patients belief about their medications and willingness for deprescribing as applications in clinical practice and research.
5. Please reference SPSS.
Authors reply: The authors are thankful for the comment. Reference has been added
6. You have re-defined deprescribing and polypharmacy. This second definition is repetitive, but it also is slightly inconsistent. Consider using these definitions to replace the definitions used earlier in the text.
Authors reply: The authors are grateful for the recommendation. We have modified and corrected the definition to be consistent throughout the document.
Results:
1. You correctly state that "Most of the participants were men (173 individuals, 54.92 %)". Consider leaving this out, because at this level it appears to be roughly even, so it is not particularly important.
Authors reply: The authors are grateful for the recommendation. We have omitted this result as suggested by the reviewer.
Discussion:
1. The discussion is a bit over-reaching at the moment. For example, the results do not support the assertion that trust is a subtheme.
Authors reply: The authors are grateful for the comments. We have modified the discussion points to go in line with our result part and any changes made in the discussion have been highlighted in color.
2. The authors should re-situate their study in the context of the developing country to highlight the novelty of this study.
Authors reply: The authors are grateful for the comments. We tried to consider that in the discussion despite the fact that no studies are available in developing countries.
3. This paragraph is unclear and needs to be rewritten for clarity:
Majority of the respondents feel that they took at least one of the medicines neither needed longer nor working anymore. Despite of this perception, elderly patients are solely dependent on the physician decision and look to hear the likelihood of stopping the medicine from the doctor side.
Authors reply: As per to the reviewer comment and major the change in the analysis, this statement is invalid and have been omitted from the discussion part.
4. In the paragraph about how to deprescribe, may I please recommend referencing either one or both of Page 2017 or Potter 2017 as these articles describe how the available tools to deprescribe, and how to deprescribed respectively.
Page A, Potter K, Clifford R, Etherton-Beer C. Deprescribing in older people. Maturitas. 2016;91:115-134.
Potter K, Page A, Clifford R, Etherton-Beer C. Deprescribing: A guide for medication reviews. Journal of Pharmacy Practice and Research. 2016;46(4): 358–367

Authors reply: The authors are grateful for the comments. We have found both articles important and relevant. Therefore, we have cited both articles in the manuscript.

References: The referencing style is inconsistent. For example, reference 22 is truncated after three authors to et al, while reference 23 lists 8 authors by name.
Not all references are correct for the authors names. For example, reference 26 should be Page AT, EthertonBeer CD, Clifford RM, Burrows S, Eames M, Potter K (not as it is written currently which is Page AT, Christopher D. Beer E, Rhonda MR, Sally B, Eames M, Potter K).
Some references use surname, first name, initials etc. e.g. Emily Reeve is referred to as Reeve E and Emily Reeve in the reference list.
Authors reply: The authors are grateful for the valuable comments. We have checked all the reference and corrected the references.

VERSION 2 – REVIEW

REVIEWER	Emily Reeve University of Sydney, Australia
REVIEW RETURNED	23-Jan-2018

GENERAL COMMENTS	The authors have made several changes to the analysis and in particular have addressed the concerns that I had with how the results had been grouped and analysed by taking this out of the analysis. However, the quality of the English throughout the manuscript is not acceptable for publication. The discussion section does not do the job of placing the results within the context of their setting, comparing to previous literature and how the results would influence practice. Points are brought up in the discussion, but I found them lacking in depth (which may also be a result of the issues with language). Despite several positive revisions I still have major concerns with the manuscript which I have outlined below). Recommend using the term 'older adults' rather than elderly (used in abstract), additionally I suggest using the term 'participant' rather than 'patient' when discussing individuals involved in the study. Abstract: Objective is reported to assess socio demographic and clinical data – but this doesn't match the results presented in this manuscript "Most of the participants (81.6%) were still willing to reduce one or more of their medications if advised by their doctor." – Why 'still' – suggest removing this word, also the question asks 'if their doctor said it was possible' not 'if advised by their doctor' – I suggest reviewing this and consider keeping the wording close to the original for the conclusions as to not alter the meaning. – Later in the manuscript the terms 'ordered by the doctor' is used (pg 33, line 17) – I suggest revising this also as this may have a slightly different meaning than 'if their doctor said it was possible' "Belief in Medicine use Questionnaire" – the name of this is Beliefs about Medicines Questionnaire – the results of this are not reported in this manuscript? "Binary logistic regression was performed to assess the association between willingness to stop medication if advised by doctors and predictor variables." – please clarify if you did this by grouping responses to strongly agree and agree versus strongly disagree and disagree (i.e. did you convert the 4 point Likert data to binary variable?) – it is reported that none of the variable were associated – suggest including this information in supplementary material – which variables were looked at? The operational definitions section can be removed as both of these terms have been defined in the introduction. Why is both a mean and a median number of medications presented? Was medication data normally distributed? (You have

	described normality testing in your methods but not what variables it was applied to and whether they were normally distributed.) "The most common reason of hospital visit in polypharmacy groups were diabetes mellitus (DM), hypertension, and Rheumatic heart Disease (RHD). While in non – polypharmacy study participants, hypertension, DM and heart failure were more prominent." – how can hypertension be more prominent in non-polypharmacy participants when it is listed for polypharmacy participants? This is another example of the need for further English language revision. The authors provided the reason for interviewer administration (rather than self administration) in their response to my previous comment but did not include this in the manuscript – I think it should be added as this would be of interest to readers. "This is also consistent with previous studies conducted by Mona et al [12], Reeve et al [13] and Alessandro et al [27] having 80% , 68%, and 89% of patients' willingness rate to stop their medicines, respectively." – The result of 68% for reference 13 is incorrect and relates to a different question in the PATD – please revise this sentence. Cost is mentioned in the discussion however, your study did not find an association between these two questions – can you explain this further – especially the implications for the low-resource setting. "In the current study, patient were willing to stop one or more of medications if advised by doctor even if they were taking few medications, didn't perceive side effects from one or more of their medications, were not reluctant to stop a medicine taken for a long time and had overall satisfaction with their medications" – this discussion does not appear to be representing the results adequately – the analysis was for associations, suggest revising.

VERSION 2 – AUTHOR RESPONSE

Editorial Requirements:
- Please work to improve the quality of English throughout the manuscript with the assistance of a professional copyediting agency.
  - Dear Editor, thank you very much. we are trying to get the editing of the English from the native speaker
- Please ensure that all your previous responses to the reviewer's comments have been incorporated into the manuscript. For example, we note that you have not explained how the investigators were trained, in response to the previous comments of reviewer 2.
  - Dear Editor, Thank you. We have made sure that all reviewers' comments are incorporated in the main manuscript.

Reviewer(s)' Comments to Author:

Reviewer: 1
Reviewer Name: Emily Reeve
Institution and Country: University of Sydney, Australia
Please state any competing interests: None declared
  - Dear reviewer, the competing interest declared, all authors do not have any competing interest.

Please leave your comments for the authors below

The authors have made several changes to the analysis and in particular have addressed the concerns that I had with how the results had been grouped and analysed by taking this out of the

analysis. However, the quality of the English throughout the manuscript is not acceptable for publication.

- Dear reviewer. We have tried to improve the quality of English and edit with the help of native speaker.

The discussion section does not do the job of placing the results within the context of their setting, comparing to previous literature and how the results would influence practice.

- Dear reviewer, we are addressing the discussion with the context of the result and adding the literatures to compare and elaborate our findings.

Points are brought up in the discussion, but I found them lacking in depth (which may also be a result of the issues with language).

- Dear Reviewer, we are trying to get the English language editing from the native speaker to address the language concern.

Despite several positive revisions I still have major concerns with the manuscript which I have outlined below)

- Dear reviewer, thank you very much for raising very important points throughout the review of this manuscript. We found it helpful and constructive to best refinement of the manuscript. We are trying to address all the possible comments one by one.

Recommend using the term 'older adults' rather than elderly (used in abstract), additionally I suggest using the term 'participant' rather than 'patient' when discussing individuals involved in the study.

- Dear reviewer, we replaced the terms as per the comment throughout the manuscript except at some parts of the manuscript due to altered meaning of word substitution.

Abstract:
Objective is reported to assess socio demographic and clinical data – but this doesn't match the results presented in this manuscript

- Dear reviewer, Thank you very much. The objective of this study aimed at assessing older patients' attitude towards deprescribing. And this has been changed now.

"Most of the participants (81.6%) were still willing to reduce one or more of their medications if advised by their doctor." – Why 'still' – suggest removing this word, also the question asks 'if their doctor said it was possible' not 'if advised by their doctor' – I suggest reviewing this and consider keeping the wording close to the original for the conclusions as to not alter the meaning.

- Dear reviewer, we removed the wording "still" and replacing "if advised by their doctor" with "if their doctor said it was possible" throughout the manuscript while not changing the meanings and original conclusion.

– Later in the manuscript the terms 'ordered by the doctor' is used (pg 33, line 17) – I suggest revising this also as this may have a slightly different meaning than 'if their doctor said it was possible'

- Dear reviewer, we revised accordingly "ordered by their doctor" replaced with "if their doctor said it was possible"

" Believes in Medicine Use Questionnaire" – the name of this is Beliefs about Medicines Questionnaire – the results of this are not reported in this manuscript?

- Dear reviewer, We are so thankful for the reviewer. It was entirely written differently. It was not to mean that BMQ has been used in our study but to explain the original rPATD has utilized BMQ for validation to show as one of the quality of the tool. And we have modified the statement in the manuscript as the following

  "The previously validated tool -Belief in Medicine use Questionnaire (BMQ)-Overuse [18] was utilized for comparison and validation of the rPATD questionnaire."

"Binary logistic regression was performed to assess the association between willingness to stop

medication if advised by doctors and predictor variables." – please clarify if you did this by grouping responses to strongly agree and agree versus strongly disagree and disagree (i.e. did you convert the 4 point Likert data to binary variable?) – it is reported that none of the variable were associated – suggest including this information in supplementary material – which variables were looked at?

- Dear reviewer. We are grateful for the reviewer. It is very important to clarify this more clearly and we have described this in the methodology part of the manuscript and removed from the result as it is already there in the supplementary material now. It was corrected as the following
  -Binary logistic regression was performed after dichotomizing the two item questions (willingness to deprescribe one or more of their regular medications if the doctor said it was possible and overall satisfaction with their prescribed medications) by grouping the 4 Likert responses to "strongly agree and agree" versus "strongly disagree and disagree" as shown in the supplementary table.

  The variables analyzed in the Univariate analysis are included in the supplementary material as per to your suggestion and described as the following

  -"In Univariate analysis, Sociodemographic and clinical variables like Age, sex, education, charlson comorbidity index (CCI) and reason of hospital visit (chief complaint) didn't fit final model according to the Hosmer-Lemeshow assumption, all having p >0.2. The level of statistical significance was defined as $p<0.05$ and all tests were two-tailed.

The operational definitions section can be removed as both of these terms have been defined in the introduction.

- Dear reviewer, the operational definitions removed as per the suggestion

Why is both a mean and a median number of medications presented? Was medication data normally distributed? (You have described normality testing in your methods but not what variables it was applied to and whether they were normally distributed.)

- Dear reviewer. Thank you very much for this valuable comment. We added mean data in addition to median despite the normality test applied to age variable and number of medications revealed it is not normally distributed and as result we should use median instead of mean. We added mean to compare it with other literatures. However, we have omitted the mean for non normally distributed data to get rid of outlier data as per to reviewer suggestions.

"The most common reason of hospital visit in polypharmacy groups were diabetes mellitus (DM), hypertension, and Rheumatic heart Disease (RHD). While in non – polypharmacy study participants, hypertension, DM and heart failure were more prominent." – how can hypertension be more prominent in non-polypharmacy participants when it is listed for polypharmacy participants? This is another example of the need for further English language revision.

- Dear reviewer, we understand the genuine concern and revised it as "The commonest reason of hospital visits both in polypharmacy and non-polypharmacy groups were hypertension and diabetes mellitus (DM)." Since in both groups Hypertension and diabetes mellitus are the common cause of hospital visits.

The authors provided the reason for interviewer administration (rather than self administration) in their response to my previous comment but did not include this in the manuscript – I think it should be added as this would be of interest to readers.

- Dear reviewer, Thank you. We have included this statement in the method (Survey Instrument) section of the manuscript.

"This is also consistent with previous studies conducted by Mona et al [12], Reeve et al [13] and Alessandro et al [27] having 80% , 68%, and 89% of patients' willingness rate to

stop their medicines, respectively." – The result of 68% for reference 13 is incorrect and relates to a different question in the PATD – please revise this sentence.

- Dear reviewer, the sentence is revised and the incorrect result of that reference is addressed… but we used it for our comparison from PATD questionnaire since it is similar to measure with our question intention…willingness to stop their medicines if their doctor said possible.

Cost is mentioned in the discussion however; your study did not find an association between these two questions – can you explain this further – especially the implications for the low-resource setting.

- Dear reviewer, though no association was found by our study between these two variables, majority of the patients agreed they spent a lot of money to their medicines and we tried to discuss the cost implications as supported by other studies

"In the current study, patient were willing to stop one or more of medications if advised by doctor even if they were taking few medications, didn't perceive side effects from one or more of their medications, were not reluctant to stop a medicine taken for a long time and had overall satisfaction with their medications" – this discussion does not appear to be representing the results adequately – the analysis was for associations, suggest revising.

- Dear reviewer. Thank you for the comment. The statement look confusing while interpreting the correlation described in Table 3, However, It was meant to explain that one of the questions item "**patient willing to stop one or more of medications if the doctor said, it was possible"** were negatively correlated with others item questions such as "Sometimes I think I take too many medicines", "perceiving of side effects from one or more of their medications" and "being reluctant to stop a medicine taken for a long time. However, it was positively associated with "overall satisfaction with their medications" in other words, patients were willing to stop one or more of medications if the doctor said, it was possible, even if they had overall satisfaction with their medications.
  Therefore, this statement has been modified in the manuscript to be clearer for the readers.