PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Characterization of the rural indigent population in Burkina Faso: a screening tool for setting priority health care services in sub-Saharan Africa
AUTHORS	Ouédraogo, Samiratou; Ridde, Valéry; Atchessi, Nicole; Souares, Aurélia; Koulidiati, Jean-Louis; Stoeffler, Quentin; Zunzunegui, Maria-Victoria

VERSION 1 - REVIEW

REVIEWER	Dr Daprim Ogaji University of Manchester, United Kingdom
REVIEW RETURNED	24-Aug-2016

GENERAL COMMENTS	Review of Article from BMJ Thanks for requesting my review of this article submitted to your journal. A few of my comments on the article are stated below: Title: In Sub-Saharan African countries, priority health services should be expanded to unmarried middle aged and older adults: example from Burkina Faso Authors may wish to consider a revision of the title to be more concise, captivating without compromising any relevant element of the study Page 2, line 46 – since universal coverage is what the local health system intends to achieved, the statement should read “Burkina Faso should extend access to high priority … Page 3, line 4 – delete ‘and by’and also the repetition ‘approved the study’ at the end of the line Authors should be consistent with the use of the word healthcare/health care Page 4 Authors may need to make the focus clearer The article appears to be focused on testing a new method of identifying indigent population in local communities Page 4, line 25 – I think the authors are referring to coverage of priority health services. If so, statement should be rephrased to capture this Line 37 – first study that characterised …. Line 41 – using both primary and secondary data from….. (if this is right) Line 43 – should these characteristics be specific? i.e. socio-demographic Line 44 – statement not clear. Maybe authors meant, this/our

	approach could identify indigents…… Line 50 – More research is needed Note sure how specificity and sensitivity were calculated as the (quasi) gold standard was not clearly explained in the methods section. I suspect it is the traditional community based targeting This article may also benefit from a review by a statistician Discussion Authors should adequately discuss the limitations of the study

REVIEWER	Peter Lloyd-Sherlock University of East Anglia, UK
REVIEW RETURNED	07-Sep-2016

GENERAL COMMENTS	This is a robust piece of analysis, which reveals some potentially valuable insights for health policy in sub-Saharan Africa. That said, a number of points still require work: Other studies have called into account the validity of CBT for free access to health services, claiming it can be a subjective, unsystematic and political process. The authors need to provide a stronger defence of this approach in B Faso. As part of this, they might show how indigents were distributed across the 20 villages and explain this distribution. Additional data (including qualitative) to demonstrate the validity of this CBT process might be used. The authors need to say a lot more about the relationship between the 2007 survey and the 2014, and about Riddle's definition (justifying its use). At one point, they say they applied a very open approach, but later they refer to Riddle's definition -this must be clarified. The authors rightly note that the study does not include urban B Faso. But to what extent are the districts selected representative of all rural BF? More information is needed on study variables, especially those related to health and socio-economic status. To what extent is self-reported health a useful measurement of actual need? it may say more about health awareness, health expectations and overall life satisfaction than about health per se, especially in a poor population with little engagement with services. The discussion at the end is quite useful, but the authors might make more use of other sources, such as P.Lloyd-Sherlock, B.Corso and N.Minicuci (2015) “Widowhood, Socio-Economic Status, Health and Wellbeing in Low and Middle-Income Countries” Journal of Development Studies 51(10): 1374-88. The discussion of health priorities should say more about what is actually feasible in such a poor setting and might suggest particular interventions of relevance to indigent groups. It may be more helpful to think in terms of interventions (eg hypertension treatment for all older women) than trying to target particular population groups.

REVIEWER	Susanna Cramb Cancer Council Queensland, Australia
REVIEW RETURNED	14-Oct-2016

GENERAL COMMENTS	This is a well-written, interesting and potentially useful study. The statistical analyses are described well, and overall I have only minor suggestions for improvement. 1. I am rather intrigued as to how such a high proportion of incomplete questionnaires were returned, given these were administered by trained data collectors. 2. Were the subjects with incomplete questionnaires similar in characteristics to the completed questionnaires? It would be helpful if you are able to supply any brief additional detail on the subjects with excluded questionnaires. Similarly, given CART analyses are able to handle missing data, did you try analysing all questionnaires (even those with missing values) to see if results were consistent? 3. Additional details on the cohort would be useful, especially if you could supply a diagram detailing the numbers contacted, numbers who refused consent, numbers who did not fully complete questionnaires etc. 4. Please double-check the references are correct. For instance, reference 6 is Hanson et al (and there is no apparent mention of Ridde within this chapter), yet in the manuscript text it is placed beside Ridde et al. 5. On page 10, it mentions 315 people had difficulties walking 400 m – according to Table 1, this should be 246 people, and 9.4%. Also, it says 15% perceived their health as poor – suggest providing to one decimal point for consistency with other percentages (15.5%), or else rounding up to 16%. 6. Figure 1 did not appear to have a caption. 7. If possible, a map showing the country and the two regions sampled from might be helpful for international readers. Even if a map cannot be supplied, it would be interesting to know how many villages there are in total in these two regions. Are villages generally of a similar size within each region? 8. Overall, the English is very good, but there are a couple of places some minor edits are needed (E.g. in the Ethics consideration section of the abstract, the article summary (limitations of this study paragraph), and on page 14, change “have had not children” to “have had no children”.)

REVIEWER	Jake Morgan Boston University, US
REVIEW RETURNED	14-Oct-2016

GENERAL COMMENTS	The conclusions are overstated in this paper. On the one had, the authors aim to describe the indigent population, which they do. The authors then conclude that because many indigents are unmarried and over 45, Burkina Faso should extend universal coverage to those who are unmarried and over 45. However, there is no discussion of whether 78% sensitivity and 81% specificity are good enough to make this recommendation. Because non-indigents outnumber indigents, the misclassification would result in many non-indigents receiving publicly funded healthcare, which may be costly. The authors need to consider budget impact and/or cost-effectiveness analyses in order to confirm that their policy

	suggestions are supported by evidence. This would also allow the researchers to better characterize the tradeoffs in sensitivity and specificity in their various algorithms to define indigence. They present a variety of combinations, but are not able to justify why one algorithm is preferred. The last line of the conclusion is confusing. Indigent people reported poorer health - why does that imply that patient-centered care for indigent people must take into account age and gender? The demographic composition of a group such as indigent people does not relate to patient-centeredness. There were various editorial mistakes through out. For example, page 4, line 50 "researches are" should be "research is." A main result of the analysis was an algorithm to identify indigents that traded off between sensitivity and specificity depending on the variables included. If this is to remain a main focus of the manuscript the authors must provide some way to justify their selection of a particular algorithm. If authors want to suggest policy changes they must be able to demonstrate how the policy would affect costs and outcomes on the population against the status quo.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1
Authors should be consistent with the use of the word healthcare/health care
Response to the reviewer: We replaced “healthcare” by “health care” throughout the manuscript.

Reviewer 1 comments on Page 4 (Article summary)
Authors may need to make the focus clearer. The article appears to be focused on testing a new method of identifying indigent population in local communities
Authors may need to make the focus clearer. The article appears to be focused on testing a new method of identifying indigent population in local communities
line 25 – I think the authors are referring to coverage of priority health services. If so, statement should be rephrased to capture this
Line 37 – first study that characterised ….
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Line 43 – should these characteristics be specific? i.e. socio-demographic
Line 44 – statement not clear. Maybe authors meant, this/our approach could identify indigents……
Line 50 – More research is needed

Response to the reviewer: As recommended by the Editor, we have suppressed the section “Article summary” which included the article’s focus. And we have changed the article title to reflect the focus of the article.

Reviewers 1 and 3 on the study limitations
Authors should adequately discuss the limitations of the study
Line 50 – More research is needed
Response to the reviewers: Two sentences related to the study limitations in the has been included in section “strengths and limitations” (Page 4): This study was limited to certain rural areas in Burkina Faso, further research is needed to assess if these results could be generalize to urban areas. Moreover, due to lack of data, we could not perform comparative analyses of the characteristics of people with and without missing data in the study.

2- Study title

The editor and the reviewer 1 suggested changing the study title
The title shouldn't be declarative. Please also include the study design in the title.
Authors may wish to consider a revision of the title to be more concise, captivating without compromising any relevant element of the study

Response
Original Title: “In Sub-Saharan African countries, priority health services should be expanded to unmarried middle aged and older adults: example from Burkina Faso”
New Title: “Characterisation of rural indigent population in Burkina Faso: A helpful screening tool for setting priority health care services in Sub-Saharan African”

3- Abstract
Reviewer 1
Page 3, line 4 – delete ‘and by’and also the repetition ‘approved the study’ at the end of the line
Response to the reviewer: As recommended by the Reviewer 1, in Page 2, we removed “universal” after “Burkina Faso should extend” in the first sentence of the conclusion.

4- Introduction

Reviewer 2
Other studies have called into account the validity of CBT for free access to health services, claiming it can be a subjective, unsystematic and political process. The authors need to provide a stronger defence of this approach in B Faso. As part of this, they might show how indigents were distributed across the 20 villages and explain this distribution. Additional data (including qualitative) to demonstrate the validity of this CBT process might be used. The authors need to say a lot more about the relationship between the 2007 survey and the 2014, and about Riddle's definition (justifying its use). At one point, they say they applied a very open approach, but later they refer to Riddle's definition -this must be clarified.

Response to the reviewer: The validity and the effectiveness of the CBT process have been documented by many researches particularly in Burkina Faso.1-3 Recently, Schleicher et al (http://linkis.com/ub.uni-heidelberg.de/y3gZu) who compared Decentralized versus Statistical Targeting of Anti-Poverty Programs in Burkina Faso concluded that the community-based assessment targets a similar share of consumption-poor households as the best-performing statistical procedures which are not calibrated with household-level consumption data. In a cost-benefit analysis they found that in the sub-Saharan African context community-based targeting is far more cost-effective than any statistical procedure for common amounts of welfare program benefits.
In Burkina Faso, community-based targeting consists of a process for selecting the worst-off by the community members, a village selection committee with a gender balance and designated by the village health committee. The Community-Based targeting approach to identity the indigents for access to many services have been adopted by the local authorities in Burkina Faso. To avoid any capture of local elite,4-6 the members of the selection committees cannot be administrative officers, village chiefs or members of the health committee. Village selection committees produce lists of indigents whom they selected based on a consensual definition and with no pre-determined criteria: ‘‘someone who is extremely disadvantaged socially and economically, unable to look after him/herself, and devoid of internal or external resources’’. The process and this definition were introduced and validated by Ridde et al in 2007.7-8 The 2014 survey used the selection process and

the definition. In each village the selection committee was given the entire responsibility and autonomy to select the worst off within their community. It was part of a state led intervention for performance-based financing of health services in two districts of Burkina Faso.

5- Population and Methods

Reviewer 1
Note sure how specificity and sensitivity were calculated as the (quasi) gold standard was not clearly explained in the methods section. I suspect it is the traditional community based targeting
Response to the reviewer: To make it clear that the community-based selection was used to identify the indigents and as a gold standard to assess the diagnostic performance of the test tree in the study, we have included 2 sentences in the method section: one at the beginning of the second paragraph of the section “setting” (Page 6, paragraph 2) and another in the last sentence of section “statistical methods” (Page 9).

Reviewer 2
The authors rightly note that the study does not include urban Burkina Faso. But to what extent are the districts selected representative of all rural BF?
Response to the reviewer: In Burkina Faso, about 15 million of people live in rural areas. Our study population represent more than 2% of people living in rural areas in Burkina Faso. Diebougou (127,857 inhabitants) in the southwest and Gourcy (208,740 inhabitants) in the north. Moreover, these districts are different in term of agricultural practices, weather conditions and ethnic composition. They represent the diversity of the rural context in Burkina Faso. A study with a larger sample would have been much more expensive and difficult to achieve.

Reviewer 2
More information is needed on study variables, especially those related to health and socio-economic status. To what extent is self-reported health a useful measurement of actual need? it may say more about health awareness, health expectations and overall life satisfaction than about health per se, especially in a poor population with little engagement with services.

Response to the reviewer: The costs and difficulties associated with assessing the health of a population have led to an ongoing search for indicators of health status that can be readily collected from large numbers of individuals with minimal expenditure of resources, including time, money, training, and logistics. Many previous studies have reported and commented on the use of Self-reported health status.9-12 In a study, How well Self-Reported and Observed Indicators Measure Health and Predict Mortality in Bengladesh, Kuhn et al13 reported that individuals can effectively assess their own health status even in settings of poor education and low levels of interaction with modern health care systems. They suggested including a range of health indicators in data collection—in a way that permits research on indicators in this region given the urgent need to assess health and track its trends over time in sub-Saharan Africa.
Due to the lack of information regarding our study population’s health, we have collected variables selected based on health determinants (demographic and socio-economic characteristics, health, physical functioning) reported in previous studies in Africa.9,10,13 We have considered Andersen and Newman’s14 model of societal and individual determinants of medical care utilization. We agree with the reviewer that self-reported health may say more about health awareness, health expectations and overall life satisfaction than about health per se, especially in a poor population with little engagement with services. More research is needed on the topic. We have included this as a limitation section in the discussion, page 15, paragraph 2.

Reviewer 3

This is a well-written, interesting and potentially useful study. The statistical analyses are described well, and overall I have only minor suggestions for improvement.
1. I am rather intrigued as to how such a high proportion of incomplete questionnaires were returned, given these were administered by trained data collectors.
2. Were the subjects with incomplete questionnaires similar in characteristics to the completed questionnaires? It would be helpful if you are able to supply any brief additional detail on the subjects with excluded questionnaires. Similarly, given CART analyses are able to handle missing data, did you try analysing all questionnaires (even those with missing values) to see if results were consistent?

Response to the reviewer: We didn't have any information on the people with missing data. Therefore, we couldn't perform any comparative analysis of their characteristic with those for whom data have been collected. We have included this as a bullet point in the limitation section of the article (page 4). As the reviewer is probably aware, Data collection in this part of Africa is extremely difficult especially when data collection involves interviewing very vulnerable persons with low educational attainment
Reviewer 3
Additional details on the cohort would be useful, especially if you could supply a diagram detailing the numbers contacted, numbers who refused consent, numbers who did not fully complete questionnaires etc.

We provide the table bellow to inform the reviewer 3 on the cohort of indigents. We don't have access to the details regarding the cohort of non-indigents. Please see below for each district, the details of the number of indigents with complete data, those with incomplete data, those absent, sick or too old to respond to the survey.

	Gourcy		Diébougou	
	Numbers	%	Numbers	%
Complete data	281	80.5	548	83.3
Incomplete data	10	2.8	20	3.3
Absent	27	7.8	31	4.7
Sick	1	0.3	12	1.8
Too old to respond	5	1.4	17	2.6
Disabilities	22	6.3	27	4.1
Dead	3	0.9	5	0.8

Table: Information regarding the indigents contacted during the survey

6- Results
Reviewer 3
On page 10, it mentions 315 people had difficulties walking 400 m – according to Table 1, this should be 246 people, and 9.4%. Also, it says 15% perceived their health as poor – suggest providing to one decimal point for consistency with other percentages (15.5%), or else rounding up to 16%.

Response to the reviewer: We have corrected the section presenting the general characteristics of the study population (page10): Of the total sample population, 1,433 (54.9%) lived in Gourcy, 1,555 (59.5%) were women, and 574 (22.0%) were aged 60 years and over. Most were illiterate (2,312, 88.5%); more than one-third (1004, 38.4%) had difficulties satisfying food needs; 9.4% (246) had difficulties walking 400 metres, and 15.5% (406) perceived their health as poor.

Reviewer 3
Figure 1 did not appear to have a caption.

Response to the reviewer: The Figure 1 appear on page 28

Reviewer3
If possible, a map showing the country and the two regions sampled from might be helpful for international readers. Even if a map cannot be supplied, it would be interesting to know how many villages there are in total in these two regions. Are villages generally of a similar size within each region?

Response to the reviewer: We don't know the precise size of the villages included in the study but these were chosen because they had more than twenty indigents within their population.
In the two regions included in the study, we used data from 20 villages. This detail is mentioned on page 6, paragraph 2 (section "setting").

7- Discussion

Reviewer 2
The discussion at the end is quite useful, but the authors might make more use of other sources, such as P.Lloyd-Sherlock, B.Corso and N.Minicuci (2015) "Widowhood, Socio-Economic Status, Health and Wellbeing in Low and Middle-Income Countries" Journal of Development Studies 51(10): 1374-88.

Response to the reviewer: As recommended by the reviewer, we included Lloyd-Sherlock et al?'s study results in our discussion (page 14): In a study of Widowhood, Socio-Economic Status, Health and Wellbeing in Low and Middle-Income Countries, Lloyd-Sherlock et al found that the association between widowhood and being in the poorest household wealth quintile was most consistent across countries (China, Ghana, India, the Russian Federation and South Africa).

Reviewer 2
The discussion of health priorities should say more about what is actually feasible in such a poor setting and might suggest particular interventions of relevance to indigent groups. It may be more helpful to think in terms of interventions (eg hypertension treatment for all older women) than trying to target particular population groups.

Response to the reviewer: We thank the reviewer for recommendation that we write more about particular interventions of relevance to indigent groups. We will be considering this question in our future research. There is limited research on the topic in this deprived setting. We intend to develop and implement successful interventions targeting this population in rural Burkina Faso. The details and results of these interventions will be the topic of future papers. Our recommendations in this paper aimed at guiding health authorities in Burkina Faso in particular, and Sub-Saharan Africa more generally, who have already committed to implement Universal Health coverage for their population. We are mentioning the necessity for some group of the population - the most likely to be indigent according to the community, and the less targeted by health care programmes and interventions - to have access to the health care center and coverage of their basic health care needs in the in-between period before universal health care coverage is rolled out.
Moreover, Burkina Faso is considering the introduction of a cash transfer program targeting the indigents. Identifying the vulnerable population as we did in our study will enable this type of economic intervention as well. Community-based targeting can ensure that indigents benefit from interventions' advantages.

Reviewer 3
Overall, the English is very good, but there are a couple of places some minor edits are needed (E.g. in the Ethics consideration section of the abstract, the article summary (limitations of this study

paragraph), and on page 14, change “have had not children” to “have had no children”.)

Response to the reviewer: Page 14, paragraph 2, the following sentence has been corrected: women victims of such violence and discrimination are mostly seniors, have had no children or only girls, have emigrated, or their children have not "succeeded".

Reviewer 4
The conclusions are overstated in this paper. On the one hand, the authors aim to describe the indigent population, which they do. The authors then conclude that because many indigents are unmarried and over 45, Burkina Faso should extend universal coverage to those who are unmarried and over 45. However, there is no discussion of whether 78% sensitivity and 81% specificity are good enough to make this recommendation. Because non-indigents outnumber indigents, the misclassification would result in many non-indigents receiving publicly funded healthcare, which may be costly. The authors need to consider budget impact and/or cost-effectiveness analyses in order to confirm that their policy suggestions are supported by evidence. This would also allow the researchers to better characterize the tradeoffs in sensitivity and specificity in their various algorithms to define indigence. They present a variety of combinations, but are not able to justify why one algorithm is preferred.
The last line of the conclusion is confusing. Indigent people reported poorer health - why does that imply that patient-centered care for indigent people must take into account age and gender? The demographic composition of a group such as indigent people does not relate to patient-centeredness.
A main result of the analysis was an algorithm to identify indigents that traded off between sensitivity and specificity depending on the variables included. If this is to remain a main focus of the manuscript the authors must provide some way to justify their selection of a particular algorithm. If authors want to suggest policy changes they must be able to demonstrate how the policy would affect costs and outcomes on the population against the status quo.

Response to the reviewer: We thank the reviewer for their? (do you know their gender?- comment on the paper. We agree with the reviewer that budget impact and/or cost-effectiveness analyses are need to confirm that our policy suggestions are supported by evidence. We have included this comment in the conclusion of our manuscript.
Research on indigents are rare in sub-Saharan Africa. To our knowledge, this paper is the first to attempt characterising the group of population that constitute the rural indigent population in a Sub-Saharan country. A budget impact and/or cost-effectiveness analyses were beyond the scope of the present paper.
As we explained above in response to the reviewer 2 comment, our recommendations in this paper aimed at guiding health authorities in Burkina Faso in particular and Sub-Saharan Africa in general which have committed to implement Universal Health coverage for their population. We are mentioning the necessity for a targeted group of the population - those most likely to be indigent according to the community, and those less targeted by health care programmes and interventions - to have access to the health care center and coverage of their basic health care needs.
We expect that this paper will stimulate and serve as a guide to further in-depth research on the topic. As an example, a future study could analyse the cost of an intervention that aims to provide free access to health care to the group identified in our study, with a sensitivity analysis considering the cost of misclassification.

8- References

Reviewer 3
Please double-check the references are correct. For instance, reference 6 is Hanson et al (and there is no apparent mention of Ridde within this chapter), yet in the manuscript text it is placed beside

Ridde et al.

Response to the reviewer: We have checked and reviewed the reference in all the manuscript

VERSION 2 – REVIEW

REVIEWER	Daprim Ogaji University of Port Harcourt Nigeria
REVIEW RETURNED	13-Jan-2017

GENERAL COMMENTS	Authors have attended to the areas of concerns in the previous review

REVIEWER	Susanna Cramb Cancer Council Queensland, Australia
REVIEW RETURNED	16-Jan-2017

GENERAL COMMENTS	Overall, I am satisfied with the majority of changes the authors' have made. However, having seen the additional numbers supplied, describing 180 indigent questionnaires as 'incomplete' on page 10, lines 10-13 is potentially misleading, as is stating that '1,009 indigents were interviewed' (lines 8-10, page 7). It appears that 30 indigent questionnaires were incomplete, and a further 150 were not administered. The manuscript should be revised accordingly. It would be very helpful to have this kind of breakdown within the manuscript for the non-indigent population too – are there really no details on which non-indigent questionnaires had no responses and partial responses?

REVIEWER	Jake Morgan Boston Medical Center Boston, MA 02118 United States
REVIEW RETURNED	10-Jan-2017

GENERAL COMMENTS	I commend the authors for their substantial revision. However, there were some reviewer comments that were not addressed - in general I believe it a reviewer raises an issue, it is likely to occur in the minds of other scientific readers, and so should be included in the text. I review these instances below: Reviewer 2 pointed to questions about CBT in the literature. The authors dispute this and cite some counter-arguments. Part of this discussion should be in the manuscript The 'gold standard' measure was questioned by two reviewers, and while the authors clarified that they used community selection as the gold standard there was no discussion as to why. I think part of the confusion is the use of the word 'gold standard' which usually denotes a widely accepted metric. The authors should provide clear rationale as to the use of this standard and consider not referring to it as a gold standard, perhaps just the base-case standard or something similar. Also, while the trade-offs between sensitivity and

	specificity might be discussed in a later manuscript, the authors should describe what these trade-offs might imply, otherwise it is useless to describe them - for example, "an algorithm with high SE might be preferred in X scenario while high SP might be preferred if Y. Further research should explore this more" or something like that Reviewer 3 asked for more details about regarding the regions sampled. The authors respond that they have no data. Certainly there is some way to find out more about the numbers of villages in each region and relative sizes of villages. Perhaps the authors can contact the study team who conducted the original research (the source of their data) or perhaps even the local government has online/phone resources to contact. The readers is going to expect this basic information Reviewer 4 advised including budget impact and cost-effectiveness analysis. While this is beyond the scope of the research, the authors did not address this well enough. In their addition, authors noted that "Impact and/or cost-effectiveness analyses are needed to confirm...." First, the reviewer mentioned "Budget Impact" analysis which is a specific kind of analysis, rather than "impact analysis." Second, these analyses do not confirm whether policies can be implemented. Rather, they evaluate the value of implementing certain policies and the trade-off with other policies given scarce resources. As is the manuscript has no path of future research that would translate this research to practice which requires some sort of valuation.

VERSION 2 – AUTHOR RESPONSE

Reviewer 3 comments

Overall, I am satisfied with the majority of changes the authors' have made. However, having seen the additional numbers supplied, describing 180 indigent questionnaires as 'incomplete' on page 10, lines 10-13 is potentially misleading, as is stating that '1,009 indigents were interviewed' (lines 8-10, page 7). It appears that 30 indigent questionnaires were incomplete, and a further 150 were not administered. The manuscript should be revised accordingly. It would be very helpful to have this kind of breakdown within the manuscript for the non-indigent population too – are there really no details on which non-indigent questionnaires had no responses and partial responses?

Response to the reviewer: As suggested by Reviewer 3, we revised the section "results" lines 2-9, page 10: We identified 2,077 non-indigents and 1,009 indigents for the study in the two districts. A total of 1,783 (85.8%) non-indigents and 829 (82.2%) indigents aged 18 years and over with complete questionnaires were considered in the present analysis. A total of 294 non-indigents' questionnaires were incomplete and could not be used for the analyses. Indeed, during the period of the interview, 59 indigents were absent from their house, 13 were sick, 22 were too old to respond to the questionnaire, 49 were disabled, 30 did not complete their interview, and 8 indigents had died. Unfortunately, we did not have details on the missing questionnaires for the non-indigents.

Reviewer 4 Comments

I commend the authors for their substantial revision. However, there were some reviewer comments that were not addressed - in general I believe it a reviewer raises an issue, it is likely to occur in the minds of other scientific readers, and so should be included in the text. I review these instances

below:
Reviewer 2 pointed to questions about CBT in the literature. The authors dispute this and cite some counter-arguments. Part of this discussion should be in the manuscript

Response to the reviewer: As suggested by Reviewer 4, we included points about CBT in the Discussion (page 14, lines 11-20): In the present study, we considered CBT as a base-case standard for the classification of indigents and non-indigents. According to Conning and Kenave(47), CBT may lead to increased conflict and division within the community and places high time costs on community leaders. Program goals may be subverted to serve elite interests, or local targeting preferences might differ substantially from national or donor preferences. However, the social acceptability, validity, and effectiveness of the CBT process have been documented in Burkina Faso.(48-50) Schleicher et al.(11), who compared decentralized versus statistical targeting of anti-poverty programs, found that in the sub-Saharan African context community-based targeting is far more cost-effective than any statistical targeting procedure for welfare program benefits.

We also, modified the Introduction (page 5, lines 13 to 22): In Burkina Faso, CBT consists of a process by which the worst-off are selected by a gender-balanced village selection committee of community members appointed by the village health committee. To avoid any capture of local elite,(9-11) the selection committees members cannot be administrative officers, village chiefs, or health committee members. Village selection committees produce lists of indigents whom they select based on a consensual definition and with no pre-determined criteria: ‘‘Someone who is extremely disadvantaged socially and economically, unable to look after him/herself, and devoid of internal or external resources’’. The process and the definition were introduced and validated by Ridde et al. in 2007.(12-14).

The 'gold standard' measure was questioned by two reviewers, and while the authors clarified that they used community selection as the gold standard there was no discussion as to why. I think part of the confusion is the use of the word 'gold standard' which usually denotes a widely-accepted metric. The authors should provide clear rationale as to the use of this standard and consider not referring to it as a gold standard, perhaps just the base-case standard or something similar. Also, while the trade-offs between sensitivity and specificity might be discussed in a later manuscript, the authors should describe what these trade-offs might imply, otherwise it is useless to describe them - for example, "an algorithm with high SE might be preferred in X scenario while high SP might be preferred if Y. Further research should explore this more" or something like that

Response to the reviewer: We thank the reviewer for this comment. We have replaced the term ‘gold standard’ with ‘base-case standard’. We chose CBT as a base-case standard because this selection process was adopted by the authorities in Burkina Faso. This has been clarified on page 9 lines 6-9: We also assessed screening performance of the test tree, using CBT as a base-case standard for classification of indigents and non-indigents, since this approach to identifying indigents for access to services was adopted by the authorities in Burkina Faso.

The Government of Burkina Faso has committed to providing universal health coverage for all the population. In that context, when screening to identify indigents, missing a case could have consequences for that person. It is therefore essential to maximize the number of true positives. Sensitivity could be improved with a compromise in specificity by including the orange nodes in the screening criteria. We excluded the description of the sensitivity and specificity including the “orange nodes” from the results (page 12, lines 11-20) and the table 2 (page 27). Because, in the orange nodes, for those under 45, we need information on upper limb strength. If we want to suggest an expansion of free health care to this category of the population, additional costs will be incurred to collect and measure this physical functioning. The trade-offs between sensitivity and specificity might be discussed in a later manuscript.

In our policy suggestion, we also specified that impact analyses should consider exploring the trade-offs between sensitivity and specificity in the classification of indigents and non-indigents (page 15, lines 11-13).

Reviewer 3 asked for more details about regarding the regions sampled. The authors respond that they have no data. Certainly, there is some way to find out more about the numbers of villages in each region and relative sizes of villages. Perhaps the authors can contact the study team who conducted the original research (the source of their data) or perhaps even the local government has online/phone resources to contact. The readers is going to expect this basic information

Response to the reviewer: As requested by Reviewer 3, we added a table presenting the total resident population in each village and the numbers of indigents and non-indigents included in the study (table1).

Reviewer 4 advised including budget impact and cost-effectiveness analysis. While this is beyond the scope of the research, the authors did not address this well enough. In their addition, authors noted that "Impact and/or cost-effectiveness analyses are needed to confirm...." First, the reviewer mentioned "Budget Impact" analysis which is a specific kind of analysis, rather than "impact analysis." Second, these analyses do not confirm whether policies can be implemented. Rather, they evaluate the value of implementing certain policies and the trade-off with other policies given scarce resources. As is the manuscript has no path of future research that would translate this research to practice which requires some sort of valuation.

Response to the reviewer: We thank the reviewer for these clarifications, and we have corrected the sentence accordingly (page 15, lines 7–13):
Given scarce resources in Burkina Faso, a budget impact analysis is needed to estimate the financial consequences of extending access to free health care services to other sub-groups of populations. The government’s ability and willingness to support and sustain these programs must be assessed. These analyses should also explore the trade-offs between sensitivity and specificity in the classification of indigents and non-indigents and investigate the consequences of including these groups.

Additional changes
General:
We corrected some typo in the manuscript and the references.

Abstract
We added to the first sentence of the Conclusion: In its progress toward universal health coverage, Burkina Faso should extend free access to priority health care services to widow(er)s under 45, unmarried people aged 45 years and over, and married women aged 60 years and over, and services should be adapted to their health needs.

Methods
We added a sentence at the end of the section related to the setting: In table 1, we present the total population of the villages in 2011 as well as the numbers of non-indigents and indigents included in the present study.

Statistical methods
We added a sentence at the end of the section related to statistical methods (page 9): We also assessed the screening performance of the test tree using CBT as a base-case standard for classification of indigents and non-indigents, since this was the approach adopted by the authorities in Burkina Faso to identify indigents for access to many services.

VERSION 3 – REVIEW

REVIEWER	Susanna Cramb Cancer Council Queensland, Australia
REVIEW RETURNED	26-Apr-2017

GENERAL COMMENTS	Thank you for the additional changes you made to the manuscript. I am satisfied with this version. My only comment is that 1009 identified indigents minus the 829 indigents whose responses were analysed is 180, whereas summing together the 59,13,22,49,30 and 8 gives 181.

REVIEWER	Jake R Morgan Boston Medical Center, Boston, MA, USA
REVIEW RETURNED	19-Apr-2017

GENERAL COMMENTS	The authors provided a good response to substantial reviewer comments in the first and second reviews. My only comment on the latest revision would be a suggestion to eliminate the last line of the conclusion. Patient-centered care has a very specific meaning in Health Services Research and related fields. Given you do not talk about this elsewhere, and are not using it as commonly understood, the manuscript would be improved by avoiding the word. I believe the revised manuscript to be suitable for publication in BMJ open.

VERSION 3 – AUTHOR RESPONSE

Reviewer 4 comments
The authors provided a good response to substantial reviewer comments in the first and second reviews. My only comment on the latest revision would be a suggestion to eliminate the last line of the conclusion. Patient-centered care has a very specific meaning in Health Services Research and related fields. Given you do not talk about this elsewhere, and are not using it as commonly understood, the manuscript would be improved by avoiding the word. I believe the revised manuscript to be suitable for publication in BMJ open.

Response to the reviewer: We thank Dr. Jake R Morgan for his comment and for recommending our manuscript for publication in BMJ open. As suggested by Dr Morgan, we revised the section “conclusion” last line, page 15: It reads now: “Indigent people reported poorer health, chronic disease, and limitations in physical functioning. This implies that free priority health care services for indigent people must take into account age and gender, as well as the management of chronic conditions”

Reviewer 3 Comments
Thank you for the additional changes you made to the manuscript. I am satisfied with this version. My only comment is that 1009 identified indigents minus the 829 indigents whose responses were analysed is 180, whereas summing together the 59,13,22,49,30 and 8 gives 181.

Response to the reviewer: We thank Dr Susanna Cramb for notifying this mistake. The number of indigents absent from their home was 58 not 59.