

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The ethics of unlinked anonymous testing of blood: views from in-depth interviews with key informants in four countries
AUTHORS	Kessel, Anthony; Datta, Jessica; Wellings, Kaye; Perman, Sarah

VERSION 1 - REVIEW

REVIEWER	Marloes A van Bokhoven MD PhD. GP and lecturer. CAPHRI School for Public Health and Primary Care, department of General Practice, Maastricht University. The Netherlands. I have no competing interests.
REVIEW RETURNED	02-Jul-2012

THE STUDY	<p>The study has 2 objectives: improved understanding about the development of unlinked anonymous testing (UAT) over time and to use UAT as an instrument to gain understanding of ethical tensions between surveillance and patients' right. Both are ambitious and the authors do not translate these objectives into research questions. The authors choose qualitative interviews with stakeholders as the design for the study. I wonder if for the first objective a study from official document regarding decisions concerning UAT in the past years would not be more appropriate here. In this way, longitudinal analysis would be possible and recall bias by respondents would be avoided. The second objective could be addressed with a literature review. The authors show in the discussion section that several papers are available to give answers to the second objective. Without a summary of this literature in the background section of the manuscript, the additional value of their data does not become clear.</p> <p>Several aspects that are in my opinion essential in manuscripts reporting qualitative research, remain unclear in the description of the study:</p> <ul style="list-style-type: none"> - stakeholders from 4 countries have been chosen. Though the authors report their professions, they do not report why these interviewees are the appropriate people with expertise about the policy making regarding UAT in their respective countries. They also do not report the amount of experience in the field while they do state that eg in the USA UAT surveys stopped in 1996. In addition 12 participants come from the UK and only 3 from The Netherlands. It does not become clear why these different numbers are necessary per country. - The authors do not report in which language(s) interviews have been conducted. Language might have been a barrier for non native English speaking interviewees. - Interviews lasted between 20 minutes and 1 hour. 20 minutes is very short regarding the topics that have been discussed, according to the authors. - 2 of 21 interviews have not been recorded and transcribed. The
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	<p>authors do not describe how they analysed the results from these interviews.</p> <ul style="list-style-type: none"> - The authors do not describe who exactly coded and analysed the interviews (with initials). They describe that a second researcher assisted, but they do not describe the procedure. E.g. they do not describe their consensus procedure. - I find no description of the method that has been used to analyse the content nor the coding structure (open coding? based on an already known model such as known ethical paradigms?) - The authors do not describe a triangulation procedure - The authors do not report if data saturation was reached. - I find it very difficult to understand how the findings are related to the literature. Where are they in contrast and where do they come to similar findings. <p>I do not understand the remark in the second paragraph of the discussion section that a representative sample was not intended. That seems to me self-evident in a qualitative study and needs not to be described as a sort of strength or weakness.</p> <p>I have some concerns of the structure of the manuscript:</p> <ul style="list-style-type: none"> - at the beginning of the background and discussion section introductions are written. I did not see this before. E.g. a discussion section usually starts with a summary of the main findings.
RESULTS & CONCLUSIONS	<p>Regarding the contents of the results:</p> <p>The relevance of the themes mentioned is often not clear in relation to the study's objectives. E.g. page 8; 'several respondents suggested that a review of UAT would be timely'</p> <ul style="list-style-type: none"> - The authors use very broad and superficial codes, both in their objectives, e.g. 'ethical issues' and in the results section, e.g. 'value of UAT' and 'a particular theme was whether the public has a duty to participate...'. Regarding the latter the authors do not formulate the arguments pro and contra, which is necessary to respond to objective 2. Another example is 'UK6 discussed how advances in testing... might alter the questions asked...' the authors do not mention which details are, nor provide a citation. This is, in my opinion, essential to answer the objectives. <p>When I read the citations I often come to different conclusions. e.g. page 7-8 analysis: 'respondents cited the continuing infection rate... as justification', corresponding citation: 'more feeling of public health emergency....'</p> <ul style="list-style-type: none"> - The authors do not make clear distinctions between interviewees' responses, analysis of the responses and their personal opinions. - many interviewees' responses seem to be their opinions regarding how HIV surveillance should be organised, instead of answers to the study's aims. <p>The structure of the discussion section does not become clear to me. The authors state that they first summarise the findings from the interviews and subsequently discuss them in relation to the literature. However, in the lengthy summary of the findings they frequently add literature references as numbers. In this way it is not clear what the authors mean: is this study finding supported by the literature? If yes, this should be stated and discussed in the subsequent section of the discussion. Does this finding come from the literature only? If yes, this should be stated and not reported as a finding from the interviews.</p>
REPORTING & ETHICS	<p>I do not have concerns about any fraud. However, throughout the manuscript I get the impression that the authors already had an opinion regarding this topic, namely that the ethics of the current UK</p>

	<p>procedure are questionable (see e.g. page 8) beforehand. If I understand it correctly from the abstract, the study was meant to come up with findings to inform health policy. The paper gives me the impression that the interviews' main topic has been what kind of UAT, if any, should be carried out and not the objectives that the authors state. Probably an editorial would be more appropriate to bring this message across?</p>
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REVIEWER	<p>A.M. Viens Research Fellow Institute for Medical Ethics & History of Medicine Ruhr-University Bochum Germany</p> <p>Competing interests: AMV will be co-editing a volume on criminal law, philosophy and public health practice with AK.</p>
REVIEW RETURNED	07-Jul-2012

THE STUDY	<p>No supplemental documents contain information that should be better reported in the manuscript, nor raise questions about the work.</p>
GENERAL COMMENTS	<p>1. Page 3, paragraph 4. Suggest changing "recent" to "dominant" – attention to individual rights and autonomy has been dominant for decades within the health(care) context.</p> <p>2. Page 9, paragraph 1. Change "of" to "or", so it reads "... verbal or written)."</p> <p>3. Page 10, paragraph 2. Authors switch between "MSM" and "gay men" – if you don't mean to distinguish two different populations here, I would be consistent with terminology. Authors also switch between "US" and "USA" in a number of places in manuscript – pick one and be consistent.</p> <p>4. Page 13, paragraph 5. Singular/plural agreement - remove "the" and make it: '... against "fundamental legal principles" ...'</p> <p>5. Page 19, paragraph 2. The fact that the CDC did not use it in the US, but did abroad in international research settings would also be something to further illustrate an apparent attitude of moral relativism, as it speaks to a claim whereby "UAT is not right for us, but it is right for them".</p> <p>6. Page 19, paragraph 2. Why is Buehler et al. 1994 cited in the text? Be consistent with referencing system.</p> <p>7. Page 20, paragraph 1. Context, necessarily, changes with time and place! What you must mean is that policy/positions/attitudes/etc within a jurisdiction can change or can differ between jurisdictions.</p> <p>8. Page 20-1. There is not too much about rights in the final section of the paper, though the section title gives the impression that the discussion will be about the putative tension between rights and the public good. Perhaps the better distinction to draw for this section would be between individual and community, or individual good and public good. The same points can be made under such headings. Nevertheless, it is worthwhile to say more about rights here, especially because of what was found from examining the American</p>

	<p>context with respect to how constitutional structures can affect which public health programs are taken up (or people's beliefs about how likely they would be taken up given the legal structures). This further underpins the larger point concerning how a jurisdiction's legal culture contributes to context. Canada, for instance, given its different constitutional structure will have a different context with respect to rights, viz. rights can be justifiably infringed when they are balanced with the public good. So greater attention to rights and how they can come into conflict with other rights, as well as with the public good, and how that conflict is resolved is still worth touching on more than is currently done in the section that is supposed to be about rights.</p> <p>9. Page 21. Why is the stewardship relationship Kantian? A stewardship model need not be Kantian in nature, nor have the authors argued for why we should, if we do use a stewardship model, employ a Kantian model. Suggest deleting "Kantian".</p> <p>10. Page 22, note 5. For what it's worth, I'm not sure how much can really be made of this. In the discourse, paternalism is generally seen as negative and the term "pater" refers to father in Latin. So male terms in this realm can be both positive and negative, even if nanny has a negative connotation and steward a positive connotation.</p> <p>11. Throughout the article, a number of acronyms are used (e.g., GUM, IDU, UAT, CDC, etc.) but are done inconsistently. There are many times where the authors, for example, with use "GUM" and then later say "genitourinary medicine" making the use of the earlier acronym redundant. Read through the paper again – I saw about a dozen instances where this was done with different terms/phrases.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Marloes A van Bokhoven

2) The study has 2 objectives: improved understanding about the development of unlinked anonymous testing (UAT) over time and to use UAT as an instrument to gain understanding of ethical tensions between surveillance and patients' right. Both are ambitious and the authors do not translate these objectives into research questions.

The authors choose qualitative interviews with stakeholders as the design for the study. I wonder if for the first objective a study from official document regarding decisions concerning UAT in the past years would not be more appropriate here. In this way, longitudinal analysis would be possible and recall bias by respondents would be avoided. The second objective could be addressed with a literature review. The authors show in the discussion section that several papers are available to give answers to the second objective. Without a summary of this literature in the background section of the manuscript, the additional value of their data does not become clear.

Our response

The reviewer is asking for a different kind of study and, at this stage, we are unable to do a different study. We believe, nevertheless, that our study has interesting, valid and important findings. We would like to point out that on the research team we had a highly experienced professor of social science (KW) as well as other expert input on social science methodology on the steering group (e.g. GH). Also, the research protocol was peer reviewed as part of the grant application for the eminent Economic and Social Research Council prior to funding being awarded; and the ethics committees provided further external review on qualitative aspects.

With regard to the literature sources, we have reviewed the literature extensively, and this is evidenced in the background section (history of unlinked anonymous testing in different countries) as well as in the discussion section. A number of official documents are already referenced, for instance those from the Department of Health in the UK. A comprehensive review of official documentation (by this we assume the reviewer means government and related publications) in all countries was beyond the scope of this project – and we feel not essential to its aims and objectives.

3) Several aspects that are in my opinion essential in manuscripts reporting qualitative research, remain unclear in the description of the study:

- stakeholders from 4 countries have been chosen. Though the authors report their professions, they do not report why these interviewees are the appropriate people with expertise about the policy making regarding UAT in their respective countries. They also do not report the amount of experience in the field while they do state that eg in the USA UAT surveys stopped in 1996. In addition 12 participants come from the UK and only 3 from The Netherlands. It does not become clear why these different numbers are necessary per country.

Our response

We have added sentences (p6) to explain that one criterion was appropriate knowledge/expertise; and that a pragmatic approach meant that more informants were from the UK than other countries (this was the basis of the ESRC funding, which had limits on the number of possible interviewees).

4) - The authors do not report in which language(s) interviews have been conducted. Language might have been a barrier for non native English speaking interviewees.

- Interviews lasted between 20 minutes and 1 hour. 20 minutes is very short regarding the topics that have been discussed, according to the authors.
- 2 of 21 interviews have not been recorded and transcribed. The authors do not describe how they analysed the results from these interviews.

Our response

We have added wording on p6 to clarify that interviews were carried out in English, without any obvious language barrier issues.

We have added a phrase on p7 to explain that shorter interviews were with people who had less to contribute.

We have added a phrase on p6 state that notes were taken for unrecorded interviews.

5) - The authors do not describe who exactly coded and analysed the interviews (with initials). They describe that a second researcher assisted, but they do not describe the procedure. E.g. they do not describe their consensus procedure.

- I find no description of the method that has been used to analyse the content nor the coding structure (open coding? based on an already known model such as known ethical paradigms?)
- The authors do not describe a triangulation procedure
- The authors do not report if data saturation was reached.
- I find it very difficult to understand how the findings are related to the literature. Where are they in contrast and where do they come to similar findings.

Our response

These are useful points from the reviewer on coding and analysis. We have elaborated on detail on these aspects in the final paragraph of the methods section on p7.

Triangulation. We were trying to get two things from the interviews: an understanding of the history and current state of sero-surveillance in the countries; and participants' views on different approaches. We did triangulate the information that informants gave on the former by reviewing the literature and other data sources – and have added a sentence on p7 to explain that. It was not appropriate or relevant to triangulate informants' views.

In relation to data saturation, this was a small study, which accounts for the relatively few interviews (especially non-UK), and data saturation was not relevant in that regard. We could add a further sentence on this, though felt it was clearer with the changes now made.

6) I do not understand the remark in the second paragraph of the discussion section that a representative sample was not intended. That seems to me self-evident in a qualitative study and needs not to be described as a sort of strength or weakness.

Our response

We have removed this phrase on p16.

7) I have some concerns of the structure of the manuscript:

- at the beginning of the background and discussion section introductions are written. I did not see this before. E.g. a discussion section usually starts with a summary of the main findings.

Our response

We have significantly reduced (as well as re-aligned) the opening parts to the discussion section. There is now just one paragraph on p16 at the start of the discussion before we summarise the main findings.

8) Regarding the contents of the results:

The relevance of the themes mentioned is often not clear in relation to the study's objectives. E.g. page 8; 'several respondents suggested that a review of UAT would be timely'

- The authors use very broad and superficial codes, both in their objectives, e.g. 'ethical issues' and in the results section, e.g. 'value of UAT' and 'a particular theme was whether the public has a duty to participate...'. Regarding the latter the authors do not formulate the arguments pro and contra, which is necessary to respond to objective 2. Another example is 'UK6 discussed how advances in testing... might alter the questions asked...' the authors do not mention which the details are, nor provide a citation. This is, in my opinion essential to answer the objectives.

When I read the citations I often come to different conclusions. e.g. page 7-8 analysis: 'respondents cited the continuing infection rate... as justification', corresponding citation: 'more feeling of public health emergency....'

- The authors do not make clear distinctions between interviewees' responses, analysis of the responses and their personal opinions.

- many interviewees' responses seem to be their opinions regarding how HIV surveillance should be organised, instead of answers to the study's aims.

Our response

We have added sentences at the bottom of p7 (beginning of results section) to reiterate that the results section represents the views and perceptions of the informants, and that the labels applied (by us) reflect the themes that emerged. To clarify, within the section what is presented is informants' views (and our thematic analysis of those) but does not reflect any additional interpretive layer that we have undertaken. We think this is now clearer (in terms of distinctions between interviewees' responses, analysis and our opinions) but could add further if still felt to be necessary.

9) The structure of the discussion section does not become clear to me. The authors state that they first summarise the findings from the interviews and subsequently discuss them in relation to the literature. However, in the lengthy summary of the findings the frequently add literature references as numbers. In this way it is not clear what the authors mean: is this study finding supported by the literature? If yes, the should be stated and discuss in the subsequent section of the discussion. Does this finding come from the literature only? If yes, this should be stated and not reported as a finding from the interviews.

Our response

We have altered, and significantly reduced, the opening part of the discussion to improve the structure and make it clearer (p16-17). We have added a sentence (p16) to indicate that presentation of key findings is in the context of historical development of UAT in different countries as well as what is already known of the subject.

10) I do not have concerns about any fraud. However, throughout the manuscript I get the impression that the authors already had an opinion regarding this topic, namely that the ethics of the current UK procedure are questionable (see e.g. page 8) beforehand. If I understand it correctly from the abstract, the study was meant to come up with findings to inform health policy. The paper gives me the impression that the interviews' main topic has been what kind of UAT, if any, should be carried out and not the objectives that the authors state. Probably an editorial would be more appropriate to bring this message across?

Our response

We completely reject this assertion. It is inevitable that researchers (as human beings) will have opinions, but any such opinions in no way interfered with the study, which was undertaken with from an appropriately objective scientific standpoint. We had a highly experienced research team, from an international school of public health, and had high quality social science and ethics expertise. As mentioned earlier, the research protocol was peer reviewed as part of the grant application for the eminent Economic and Social Research Council prior to funding being awarded; and the ethics committees provided further external review.

Reviewer 2: A.M. Viens

11) Page 3, paragraph 4. Suggest changing “recent” to “dominant” – attention to individual rights and autonomy has been dominant for decades within the health(care) context.
This has been changed

12) Page 9, paragraph 1. Change “of” to “or”, so it reads “... verbal or written).”

This has been changed

13) Page 10, paragraph 2. Authors switch between “MSM” and “gay men” – if you don’t mean to distinguish two different populations here, I would be consistent with terminology. Authors also switch between “US” and “USA” in a number of places in manuscript – pick one and be consistent.

We have changed US to USA for consistency (except for the title of the US Centre for Communicable Diseases, which is a title). We could not locate the MSM switch (we note we use gay), but they are terms used differently (MSM being classificatory, and not generally used in conversation).

14) Page 13, paragraph 5. Singular/plural agreement - remove “the” and make it: ‘... against

"fundamental legal principles"..."

This has been changed

15) Page 19, paragraph 2. The fact that the CDC did not use it in the US, but did abroad in international research settings would also be something to further illustrate an apparent attitude of moral relativism, as it speaks to a claim whereby "UAT is not right for us, but it is right for them".

We have added a sentence on p19 to incorporate this valid point.

16) Page 19, paragraph 2. Why is Buehler et al. 1994 cited in the text? Be consistent with referencing system.

This has been removed.

17) Page 20, paragraph 1. Context, necessarily, changes with time and place! What you must mean is that policy/positions/attitudes/etc within a jurisdiction can change or can differ between jurisdictions.

This is an important point and we have changed this sentence.

18) Page 20-1. There is not too much about rights in the final section of the paper, though the section title gives the impression that the discussion will be about the putative tension between rights and the public good. Perhaps the better distinction to draw for this section would be between individual and community, or individual good and public good. The same points can be made under such headings. Nevertheless, it is worthwhile to say more about rights here, especially because of what was found from examining the American context with respect to how constitutional structures can affect which public health programs are taken up (or people's beliefs about how likely they would be taken up given the legal structures). This further underpins the larger point concerning how a jurisdiction's legal culture contributes to context. Canada, for instance, given its different constitutional structure will have a different context with respect to rights, viz. rights can be justifiably infringed when they are balanced with the public good. So greater attention to rights and how they can come into conflict with other rights, as well as with the public good, and how that conflict is resolved is still worth touching on more than is currently done in the section that is supposed to be about rights.

We have changed the title of this section, as suggested by the reviewer, to reflect what we believe is important in relation to the findings and the discussion. We have not added more about rights because we believe that the section now holds well with the changed title, and because this is already quite a long article. We can, however, add more should the editor wish.

19) Page 21. Why is the stewardship relationship Kantian? A stewardship model need not be Kantian in nature, nor have the authors argued for why we should, if we do use a stewardship model, employ a Kantian model. Suggest deleting "Kantian".

We have deleted this.

20) Page 22, note 5. For what it's worth, I'm not sure how much can really be made of this. In the discourse, paternalism is generally seen as negative and the term "pater" refers to father in Latin. So male terms in this realm can be both positive and negative, even if nanny has a negative connotation and steward a positive connotation.

We have removed this

21) Throughout the article, a number of acronyms are used (e.g., GUM, IDU, UAT, CDC, etc.) but are done inconsistently. There are many times where the authors, for example, with use “GUM” and then later say “genitourinary medicine” making the use of the earlier acronym redundant. Read through the paper again – I saw about a dozen instances where this was done with different terms/phrases.

We have read through and tightened up.

VERSION 2 – REVIEW

REVIEWER	M A van Bokhoven Maastricht University CAPHRI School of Public Health and Primary Care Department of General Practice I have no competing interests.
REVIEW RETURNED	03-Oct-2012

THE STUDY	<p>I agree with the authors that it is not possible to come up with a different design for this study as the study has been done already. I leave it up to the editors if they think that the design is appropriate to answer the study questions and thus to publish the paper.</p> <p>In my opinion the authors repeatedly respond with statements that key methodological issues are not appropriate or relevant in this case. However, explanations are lacking (e.g. points 2,3,7,8 below).</p> <p>1. They repeatedly state that they are highly experienced. I do not want to argue that at all, but I am trying to give honest feedback based on the manuscript I received and on the methodological 'rules' that have been set for the design that was used. In my opinion the fact that authors have excellent track records in itself does not guarantee that all their manuscripts are of sufficient quality.</p> <p>2 they 'used a pragmatic approach' --> a critical reflection in the discussion section on the limitations of this choice and the implications regarding the bias this may have caused is lacking. Furthermore: lack of financial means was used as a reason for the pragmatic choice of not including more foreign key-informants. However, the authors state elsewhere that they did interviews by phone in several instances. I can not imagine that this method was too costly to include more foreign key informants.</p> <p>3 'participants have appropriate knowledge or expertise' --> by not providing any further details this statement can not be checked by the reader. Insight in the background of the participants is necessary to interpret the results</p> <p>4 'notes have been taken' from 2 unrecorded interviews --> this too diminishes the possibility to check the analysis done.</p>
RESULTS & CONCLUSIONS	<p>From 2 interviews no recordings or verbatims are available. This limits in-depth analysis, as re-reading the original statements of the informants is not possible.</p> <p>5 20 minutes was sufficient for some interviews --> it is doubtful if the correct key-informants have been chosen if this short duration was sufficient, given the broad nature of the research questions</p> <p>6 no discrepancies were found between 2 independent researchers</p>

	<p>while open coding was used --> this is highly unusual</p> <p>7 authors state that it was not appropriate to triangulate. They triangulated some factual information with the literature. In my opinion it is questionable if collecting factual information needs a qualitative design. For another part informants gave opinions and the authors state that triangulation was not appropriate. However, they do not clarify why while it is customary to do so in qualitative research for reasons of scientific rigour.</p> <p>8 authors state that they consider adding a remark that data saturation has not been reached not relevant. Here too they do not explain why. In my opinion it is relevant. It can be compared with an underpowered study in quantitative designs.</p> <p>9. I do not see a reaction on my concerns regarding the depth of the analysis (point 8 in my previous comments)</p> <p>10. The extra sentence in the introduction of the discussion section is helpful. However, in the text of the discussion still the nature of the results (findings from the current study or findings from the literature) can not be recognised. The text itself has not been adapted. The heading that the section is a summary of key findings from interviews is somewhat misleading here.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer 1: Marloes A van Bokhoven

I agree with the authors that it is not possible to come up with a different design for this study as the study has been done already. I leave it up to the editors if they think that the design is appropriate to answer the study questions and thus to publish the paper.

In my opinion the authors repeatedly respond with statements that key methodological issues are not appropriate or relevant in this case. However, explanations are lacking (e.g. points 2,3,7,8 below).

Our response

We have no response to these general comments but address specific points later.

1. They repeatedly state that they are highly experienced. I do not want to argue that at all, but I am trying to give honest feedback based on the manuscript I received and on the methodological 'rules' that have been set for the design that was used. In my opinion the fact that authors have excellent track records in itself does not guarantee that all their manuscripts are of sufficient quality.

Our response

We acknowledge this point. We are not sure if BMJ Open has shared the authors' details (and details of those on the advisory group) with the reviewer but are happy for the editor to do so if appropriate.

2. They 'used a pragmatic approach' --> a critical reflection in the discussion section on the limitations of this choice and the implications regarding the bias this may have caused is lacking. Furthermore: lack of financial means was used as a reason for the pragmatic choice of not including more foreign key-informants. However, the authors state elsewhere that they did interviews by phone in several instances. I can not imagine that this method was too costly to include more foreign key informants.

Our response

A balance was required in the study in terms of what was manageable and achievable – both in the context of the constraints of the study and also in relation to what was necessary to complete a useful and sound piece of research. We have added some wording in a new paragraph in the discussion section to reflect some of the limitations in this regard.

3 'participants have appropriate knowledge or expertise' --> by not providing any further details this statement can not be checked by the reader. Insight in the background of the participants is necessary to interpret the results.

Our response

We feel unable to provide further details on the participants because interviews were confidential. We believe we have provided sufficient detail on their backgrounds (in the first paragraph of the methods section), and any more detail may risk inadvertent identification of the interviewees.

4. 'notes have been taken' from 2 unrecorded interviews --> this too diminishes the possibility to check the analysis done. From 2 interviews no recordings or verbatims are available. This limits in-depth analysis, as re-reading the original statements of the informants is not possible.

Our response

We have added further phrasing in the third paragraph of the methods section to clarify that notes were made during or immediately after those particular interviews, and are therefore available for checking if needed. We do not believe that having no recording of two of the interviews affected the analysis, but have added wording in a new paragraph of the discussion section to capture this as a possible limitation.

5. 20 minutes was sufficient for some interviews --> it is doubtful if the correct key-informants have been chosen if this short duration was sufficient, given the broad nature of the research questions.

Our response

The fact that there was variation in interview length reflected that some interviewees had less to say or were less expansive. We do not believe it was necessarily a reflection on whether they were 'key' (correct) informants or not, and we believe that their views were valid.

6. No discrepancies were found between 2 independent researchers while open coding was used --> this is highly unusual.

Our response

We recognise that this may appear unusual, but this was the case between the two researchers in relation to open coding. We feel that it is not necessarily so surprising given the content and nature of interviewee data in this particular study (a combination of factual information and views).

7. Authors state that it was not appropriate to triangulate. They triangulated some factual information with the literature. In my opinion it is questionable if collecting factual information needs a qualitative design. For another part informants gave opinions and the authors state that triangulation was not appropriate. However, they do not clarify why while it is customary to do so in qualitative research for reasons of scientific rigour.

Our response

We maintain that it was reasonable to triangulate 'factual' data from informants (e.g. the history or current state of UAT testing in a country) with literature sources for the purpose of the discussion section; but that it was not appropriate to triangulate data reflecting informants' opinions or perceptions of UAT. We have tried to make this distinction clearer by amending the sentence on p7 (last paragraph of methods section). We also mention this in the new paragraph in the discussion.

8. Authors state that they consider adding a remark that data saturation has not been reached not relevant. Here too they do not explain why. In my opinion it is relevant. It can be compared with an underpowered study in quantitative designs.

Our response

We believe that our previous response is apt, but have included a new sentence in the penultimate paragraph of the methods (p7) to state this more clearly.

9. I do not see a reaction on my concerns regarding the depth of the analysis (point 8 in my previous comments)

Our response

If we understand correctly what this refers back to, we believe that the (already) strengthened fourth paragraph in the methods section does attend to details about the analysis, including the approach used, initials of two researchers who did the analysis, and open coding procedure. We are not sure what further 'depth' to provide about the analysis, given that we adopted a thematic approach rather than a grounded theory approach.

10. The extra sentence in the introduction of the discussion section is helpful. However, in the text of the discussion still the nature of the results (findings from the current study or findings from the literature) can not be recognised. The text itself has not been adapted. The heading that the section is a summary of key findings from interviews is somewhat misleading here.

Our response

Although we can see what the reviewer is getting at here, we have endeavoured to produce a discussion that is interesting to the reader and weaves together findings from the interviews with what is known of the subject. We believe that we have referenced, in general, where there is existing knowledge/information and, conversely, have made it clear in the text when the views of informants are being presented (with phrases to indicate this). We have, however, changed the title of the section to make it clear that this section is a presentation of the summary findings in the context of what is known.