

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Promotion of couples' voluntary HIV counseling and testing in Lusaka, Zambia by influence network leaders and agents |
| AUTHORS | Wall, Kristin ; Kilembe, William; Nizam, Azhar; Vwalika, Cheswa; Kautzman, Michelle; Chomba, Elwyn; Tichacek, Amanda; Sardar, Gurkiran; Casanova, Deborah; Henderson, Faith; Mulenga, Joseph; Kleinbaum, David; Allen, Susan |

VERSION 1 - REVIEW

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| REVIEWER | Knut Fylkesnes, Professor, Centre for international Health, University of Bergen, Norway No competing interests |
| REVIEW RETURNED | 08-May-2012 |

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| THE STUDY | <p>A major concern: this paper is not giving the readers a chance to understand the principles employed when training agents. On page 7 first paragraph: agents received 4 days training – and “...successful door-to-door promotional strategies”. This can be any kind of strategy like selling soaps, books etc. But in this kind of promotional effort (VCT) there are ethical guidelines. This is a critical element – so the authors should be clearly communicating their principles. Also relevant for the CVCT (which is a bit better described – but need more explanations of principals.</p> <p>Another concern relates to the use of incentives: apparently this became a bit problematic since reducing payments per invitation. My major concern is the payment/couple attending (5.25 USD). This might be seen as unethical when considering the possibility of side effects like questionable methods used by agents to influence couples. For many agents the amount of 5.25 USD is a substantial amount of money. At least, the authors should have discussed this properly.</p> <p>Exclusion of 70 INAs with low success: this is very problematic. At least the authors should also give the success level when including them – and discuss the implications.</p> <p>Statistical methods: Association INA-level characteristics and successful invitations were estimated. The descriptions on how this actually was handled need more details, particularly the handling of the different levels (Couple-INA-INL).</p> |
| RESULTS & CONCLUSIONS | The relative low success level by faith-based INAs: this is a |

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| | <p>borderline finding and should not be presented as conclusive (consider the stratified multivariate findings again).</p> <p>Limitations of the study are not discussed, and the issue of cost-effectiveness seems not to have been considered.</p> <p>First paragraph is using a questionable expression of “success”: 100 couples/month to seek CVCT is not saying much about “impact” – success level (rate) is far much better (includes a denominator).</p> <p>There is no clear consensus between what is rightly stated in the final paragraph in the discussion (that one should be cautious in the interpretation of most aORs since the magnitude of association is rather low and borderline significance) – and the statements in the conclusions. This should be reconsidered.</p> |
| GENERAL COMMENTS | <p>This topic of this paper is highly relevant and important employing an innovative method promoting couple counseling. However, there are some concerns and some suggestions.</p> <p>A major concern: this paper is not giving the readers a chance to understand the principles employed when training agents. On page 7 first paragraph: agents received 4 days training – and “...successful door-to-door promotional strategies”. This can be any kind of strategy like selling soaps, books etc. But in this kind of promotional effort (VCT) there are ethical guidelines. This is a critical element – so the authors should be clearly communicating their principles. Also relevant for the CVCT (which is a bit better described – but need more explanations of principals.</p> <p>Another concern relates to the use of incentives: apparently this became a bit problematic since reducing payments per invitation. My major concern is the payment/couple attending (5.25 USD). This might be seen as unethical when considering the possibility of side effects like questionable methods used by agents to influence couples. For many agents the amount of 5.25 USD is a substantial amount of money. At least, the authors should have discussed this properly.</p> |

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| REVIEWER | <p>Heidi Reynolds Research Associate Carolina Population Center University of North Carolina-Chapel Hill USA</p> <p>I declare no competing interests</p> |
| REVIEW RETURNED | 24-May-2012 |

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| GENERAL COMMENTS | <p>The objectives of the study and study methods are strong. In light of clinical evidence, the timing for studying approaches with the potential for increasing couples update of HIV testing is good. I think the study will make a contribution to the literature. But I encourage the authors to consider some of these suggestions:</p> <p>My main comment with the study is that the success of the intervention seems overstated, particularly in the abstract (“The study demonstrated the ability of influential people to promote CVCT...”). I think your main recommendation is what you say in the</p> |
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| | <p>discussion: now you know characteristics associated with INAs and invitations these results can be used to help strengthen the intervention. A 6% uptake of CVTC <i>after</i> excluding of INAs with <1.5% seems weak (also, the fact that you had to exclude so many INAs is problematic, see next paragraph). Your argument that the intervention was successful would be strengthened if it is effective or a cost effective way of getting couples tested (how does the cost per HIV test per couple compared with other strategies to increase uptake?). If I did the math right, the cost/couple tested in ~\$7. Granted, the costs are only those paid to INAs and doesn't include the cost of the service, training, etc., or the costs paid to the INAs with <1.5% success. However, that may be a reasonable amount to pay to get two new people in for testing, especially in a high prevalence area. Similarly, you say in the discussion that the "program...prompted approximately 100 couples/month to seek CVCT". How many couples were getting VCT prior to the intervention? The interpretation of success of the program would be very different if that answer is 100 or 10,000. But, I think you need to make a recommendation that subsequent studies need to consider the incremental cost-effectiveness or at least the cost comparability of such a strategy given alternative strategies to increase couple uptake.</p> <p>Regarding the exclusion of INAs with <1.5% success, there were a relatively large number excluded (70 of 390 or 18% if I understand correctly). I can understand that returning fraudulent receipts could be an issue, but how do you know these INAs just weren't just bad at their job? If they were doing their job and not engaging in fraudulent activity, then it would probably change the results about characteristics associated with testing uptake, not to mention decrease any effectiveness/cost-effectiveness. Where these INAs with <1.5% success more concentrated in the time period before the incentive structure was changed? Do you have any other way to justify their exclusion?</p> <p>Another comment is that there are lots of different results being reported, e.g., characteristics of INA/INLs, couples, and invitations associated with testing, etc. It's hard for the reader to keep track of all those results. Can a table be used to summarize the main findings?</p> <p>I don't see the value of stratifying results by cohabiting and non-cohabiting. Interesting to note that cohabiting couples were more likely to test, but that's about all.</p> <p>Even though you report the INL and INA recruitment in another publication elsewhere, I wanted more information in the introduction (p.7) about why INLs and INA were affiliated with NGO, health networks, private network, etc. Also what is a "health network" or "private network" in this context?</p> <p>P.7 You mention that the CVCT promotional activities took place in two of three communities. Where are the results from the 3rd</p> |
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| | <p>community? Is there a reason they are not presented as a comparison group? This should be addressed or just omit discussion of the 3rd community.</p> <p>You mention in the conclusion that the cost of transportation in Lusaka is high which might partially explain the lower than expected uptake of testing. However, you said that you reimbursed for transportation on p. 8. Can you elaborate, then, why cost of transport might still be a barrier? It also seems contradictory to the finding that the mobile VCT was not associated with increased uptake of testing. If transport cost was an issue, people might be more willing use a mobile VCT center.</p> <p>In the Discussion section you say that “inviting the couple together versus either partner alone, thereby removing pressure for one partner to propose testing....” seems to be too strong of a conclusion. You could hypothesize that this was the reason, but you don’t know.</p> <p>Small stuff:</p> <p>Spell out CVCT the first time used in the abstract.</p> <p>Spell out INA and INL in the tables (or foot note)</p> |
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VERSION 1 – AUTHOR RESPONSE

Reviewer: Knut Fylkesnes, Professor, Centre for international Health, University of Bergen, Norway

No competing interests

Exclusion of 70 INAs with low success: this is very problematic. At least the authors should also give the success level when including them – and discuss the implications.

We agree that the exclusion of the 70 INAs who did not achieve 1.50% success may affect the generalizability of the results, but feel that the exclusion was necessary in order to determine the INA-level predictors of successful invitations among INAs not returning fraudulent invitation receipts. The average success of these 70 INAs was 0.57%, and when adding these INAs to those included in the analysis, the overall success was 4.97%. This information has been added to the manuscript. Additionally, a discussion of the lack of generalizability of our findings to INAs returning fraudulent receipts has been added to the discussion.

Statistical methods: Association INA-level characteristics and successful invitations were estimated. The descriptions on how this actually was handled need more details, particularly the handling of the different levels (Couple-INA-INL).

The following modeling details have been added: “We fit the marginal multilevel logistic regression model using PROC GENMOD. GEE analysis methods with an exchangeable correlation structure accounted for two-level clustering of couple and invitation level characteristics within individual INAs and INLs. We hypothesized a priori that an exchangeable correlation structure would be appropriate since couples within a cluster should not be increasingly/decreasingly correlated. We also considered other correlation structures, such as unstructured.”

The relative low success level by faith-based INAs: this is a borderline finding and should not be presented as conclusive (consider the stratified multivariate findings again).

We have modified the language regarding the low success of faith-based INAs to indicate that this was a marginal association.

Limitations of the study are not discussed, and the issue of cost-effectiveness seems not to have been considered.

The lack of generalizability due to the exclusion of INAs with <1.5% success has been added to the discussion as a limitation. We agree that cost-effectiveness is a critical aspect of this work and are preparing a detailed manuscript addressing this. Given the complexity of cost-effectiveness calculations, we do not feel we can adequately include it in this manuscript.

First paragraph is using a questionable expression of "success": 100 couples/month to seek CVCT is not saying much about "impact" – success level (rate) is far much better (includes a denominator). Our measure of success was % of invites resulting in couples seeking testing (i.e., number of couples invited who tested with the denominator being the number of invites distributed).

There is no clear consensus between what is rightly stated in the final paragraph in the discussion (that one should be cautious in the interpretation of most aORs since the magnitude of association is rather low and borderline significance) – and the statements in the conclusions. This should be reconsidered.

The language in the conclusion has been modified to indicate that the factors we identified are potential predictors, which can be used as a framework in other locales.

This topic of this paper is highly relevant and important employing an innovative method promoting couple counseling. However, there are some concerns and some suggestions.

A major concern: this paper is not giving the readers a chance to understand the principles employed when training agents. On page 7 first paragraph: agents received 4 days training – and "...successful door-to-door promotional strategies". This can be any kind of strategy like selling soaps, books etc. But in this kind of promotional effort (VCT) there are ethical guidelines. This is a critical element – so the authors should be clearly communicating their principles. Also relevant for the CVCT (which is a bit better described – but need more explanations of principals).

We agree this is a critical aspect of the work and deserves in-depth presentation. In addition to the descriptions provided in prior publications (referenced on page 7 in Methods), we are preparing a separate manuscript detailing the training procedures for both INLs and INAs, and an analysis of the relationships and interactions between these two levels of promotional agents. INLs and INAs signed IRB-approved informed consents and participated in training sessions at the outset as well as monthly meetings covering important aspects of the work. This information has been added to the methods section.

Another concern relates to the use of incentives: apparently this became a bit problematic since reducing payments per invitation. My major concern is the payment/couple attending (5.25 USD). This might be seen as unethical when considering the possibility of side effects like questionable methods used by agents to influence couples. For many agents the amount of 5.25 USD is a substantial amount of money. At least, the authors should have discussed this properly.

CVCT had not received any publicity at the time this study was initiated, and the level of knowledge about CVCT was poor in the target population [Kelley et al]. A critical goal of INL and INA promotions was to provide basic information about discordancy and inform couples of the importance of CVCT, and then alert them to where and when the services could be obtained. This was difficult ground-breaking work: to interest influential people, adequate compensation was required. If only one

invitation in 16 resulted in a couple seeking testing, and each invitation required 15 minutes of interaction, an INA would spend 240 minutes – 4 hours – to earn \$5.25 for the CVCT couple plus \$0.11 x 16=\$1.76 for the invitation distribution. Given the relationships between INAs and invited couples – friends, family, colleagues, neighbors, etc – it is difficult to see how coercion might be brought to bear in this context. An INA might overemphasize the monetary gain couples would receive for attending CVCT, but this would be provided to couples whether or not they chose to test. If the motivation to attend CVCT was entirely financial, at worst couples would be paid for the time they spend listening to an educational talk.

Reviewer: Heidi Reynolds
Research Associate
Carolina Population Center
University of North Carolina-Chapel Hill
USA

I declare no competing interests

The objectives of the study and study methods are strong. In light of clinical evidence, the timing for studying approaches with the potential for increasing couples update of HIV testing is good. I think the study will make a contribution to the literature. But I encourage the authors to consider some of these suggestions:

My main comment with the study is that the success of the intervention seems overstated, particularly in the abstract ("The study demonstrated the ability of influential people to promote CVCT..."). I think your main recommendation is what you say in the discussion: now you know characteristics associated with INAs and invitations these results can be used to help strengthen the intervention. The language in the abstract and conclusion has been modified to indicate that the factors we identified are potential predictors, which can be used as a framework in other locales.

A 6% uptake of CVTC after excluding of INAs with <1.5% seems weak (also, the fact that you had to exclude so many INAs is problematic, see next paragraph). Your argument that the intervention was successful would be strengthened if it is effective or a cost effective way of getting couples tested (how does the cost per HIV test per couple compared with other strategies to increase uptake?). If I did the math right, the cost/couple tested in ~\$7. Granted, the costs are only those paid to INAs and doesn't include the cost of the service, training, etc., or the costs paid to the INAs with <1.5% success. However, that may be a reasonable amount to pay to get two new people in for testing, especially in a high prevalence area. Similarly, you say in the discussion that the "program...prompted approximately 100 couples/month to seek CVCT". How many couples were getting VCT prior to the intervention? The interpretation of success of the program would be very different if that answer is 100 or 10,000. But, I think you need to make a recommendation that subsequent studies need to consider the incremental cost-effectiveness or at least the cost comparability of such a strategy given alternative strategies to increase couple uptake.

The issues of exclusion by INA success rate and the concerns related to I would cost-effectiveness have been addressed above.

Regarding the exclusion of INAs with <1.5% success, there were a relatively large number excluded (70 of 390 or 18% if I understand correctly). I can understand that returning fraudulent receipts could be an issue, but how do you know these INAs just weren't just bad at their job? If they were doing their job and not engaging in fraudulent activity, then it would probably change the results about characteristics associated with testing uptake, not to mention decrease any effectiveness/cost-effectiveness. Where these INAs with <1.5% success more concentrated in the time period before the

incentive structure was changed? Do you have any other way to justify their exclusion?

This question is in line with a similar question from reviewer 1: after careful review of the data, we concluded that the likelihood is that the majority of these INAs fraudulently completed invitation receipts without actually distributing invitations. In so doing, they were inventing the demographic and contextual responses on the invitation receipts, which we did not want to include for fear of diluting the legitimate data. We acknowledge that the exclusion of these 70 INAs may potentially have excluded a few INAs who were simply bad at their job (these are in fact linked-INAs who did not succeed in promoting CVCT likely resorted to completing fraudulent receipts) and reduce generalizability of our findings. A discussion of this limitation has been added to the discussion section.

Another comment is that there are lots of different results being reported, e.g., characteristics of INA/INLs, couples, and invitations associated with testing, etc. It's hard for the reader to keep track of all those results. Can a table be used to summarize the main findings?

The predictors of CVCT uptake included: recruiting INAs who have tested with partners, focusing invitations on INA acquaintances, issuing invitations to couples and in a discreet location, and utilizing INAs from non-governmental and health networks. This information is presented as a "Key Message" bullet point in the article summary table.

I don't see the value of stratifying results by cohabiting and non-cohabiting. Interesting to note that cohabiting couples were more likely to test, but that's about all.

We stratified the results because couple cohabitation status was found to be a significant effect measure modifier, indicating that the results are significantly different for different levels of the cohabitation variable. As such, stratified results must be presented. For example, among non-cohabiting couples, INAs who could read English were more successful (aOR=2.1, $p = 0.001$) whereas among cohabiting couples this association was not found ($p = 0.44$) (Table 2).

Even though you report the INL and INA recruitment in another publication elsewhere, I wanted more information in the introduction (p.7) about why INLs and INA were affiliated with NGO, health networks, private network, etc.

INLs were recruited through network (faith-based, health, private, and CBO/NGO) referrals, and INLs in turn referred INA candidates. INLs and INAs designate themselves to a network category that best describes their role when promoting CVCT. This information has been added to the methods section.

Also what is a "health network" or "private network" in this context?

Health networks included clinical officers, nurses, home healthcare visitors, community health workers, neighborhood health committee members, and traditional birth attendants. The private sector included individuals who were self-employed or those involved in providing the public with goods or services. The definition of the "private network" has been added to the manuscript.

P.7 You mention that the CVCT promotional activities took place in two of three communities. Where are the results from the 3rd community? Is there a reason they are not presented as a comparison group? This should be addressed or just omit discussion of the 3rd community.

We have removed the third neighborhood from the paper. We had intended to use data reported from government clinics in the 3 neighborhoods as an indicator of background uptake, but this information was not collected in government clinics during this time so we were not able to use this benchmark.

You mention in the conclusion that the cost of transportation in Lusaka is high which might partially explain the lower than expected uptake of testing. However, you said that you reimbursed for transportation on p. 8. Can you elaborate, then, why cost of transport might still be a barrier? It also seems contradictory to the finding that the mobile VCT was not associated with increased uptake of testing. If transport cost was an issue, people might be more willing use a mobile VCT center.

Cost of transportation is high in Lusaka, which is why transportation was reimbursed. We did not

intend to make the argument that cost of transportation is therefore a barrier to testing in this manuscript.

We were also surprised that the mobile unit was not predictive of testing, as it was in Rwanda, not because of mitigated transportation costs, which were reimbursed, but because of the increased convenience and decreased time commitments engendered by mobile testing. This has been added to the discussion.

In the Discussion section you say that “inviting the couple together versus either partner alone, thereby removing pressure for one partner to propose testing....” seems to be too strong of a conclusion. You could hypothesize that this was the reason, but you don’t know.

The strength of the language regarding this conclusion has been changed to indicate that it is a hypothesis.

Small stuff:

Spell out CVCT the first time used in the abstract.

This change has been made.

Spell out INA and INL in the tables (or foot note)

These changes have been made.

VERSION 2 – REVIEW

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| REVIEWER | Heidi Reynolds Research Associate Carolina Population Center Univ. of North Carolina-Chapel Hill USA |
| REVIEW RETURNED | 18-Jul-2012 |

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| THE STUDY | The supplemental documents do not raise questions about the work. |
| GENERAL COMMENTS | The authors dealt with all the issues raised by reviewers. The only remaining comment is about the presentation of results stratified by cohabiting vs. not. I understand that that cohabitation status is an effect modifier. However, I still think that it would be a clearer presentation of the results if this aspect (stratifying by cohabiting v not) is dropped. The study seeks to identify those factors associated with testing uptake, and cohabiting is important and the most public health relevant since >80% of couples in the study are cohabiting. |