PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	How do general practitioners manage patients with cancer symptoms? A video-vignette study.
AUTHORS	Jiwa, Professor; Meng, Xingqiong; O'Shea, Carolyn; Magin, Parker; Dadich, Ann; Pillai, Vinita

VERSION 1 - REVIEW

REVIEWER	Willie Hamilton University of Exeter, UK
REVIEW RETURNED	04-Jun-2015

GENERAL COMMENTS	I'm really not sure what to make of this paper. In part this is because the study is part of a larger trial, and as such the precise hypothesis/rationale is unclear. The paper represents a vast piece of work - with over 1,000 vignette-GP dyads, and is perfectly well written up. I would only quibble methodologically with the three way classification of investigation outcome. It is simpler, and arguably more accurate, to have 'the doctor activity is likely to have uncovered cancer if it were present vs the doctor activity is unlikely.... But given the methods, I am staggered by the results. Indeed, I'm so staggered, I begin to question the methods. On the face of it there's some appalling legally actionable primary care practice in Australia (sure NICE guidelines are not Holy Writ, but they do provide a benchmark) OR this study is biassing GPs towards a 'wrong' answer. My problem with the latter view is that usually 'unreal' studies pus one towards 'correct' clinical behaviour. So, I'm left with a very well conducted, very well written up paper, but with a nebulous rationale, and unbelievable (or nearly unbelievable) results. What a curate's egg!

REVIEWER	Tim Stokes ELAINE GURR PROFESSOR OF GENERAL PRACTICE DEPARTMENT OF GENERAL PRACTICE & RURAL HEALTH / Te Tari Mātauranga Rata Whānau me te Hauora Taiwhenua DUNEDIN SCHOOL OF MEDICINE • UNIVERSITY OF OTAGO • PO BOX 56, DUNEDIN 9054, NEW ZEALAND
REVIEW RETURNED	16-Jun-2015

GENERAL COMMENTS	This study reports how a sample of Australian GPs manage patients with cancer symptoms using video-vignettes. The vignettes were designed so that each vignette met the UK's NICE clinical guideline on referral for suspected cancer criteria for referral to a specialist. It

	was conducted as part of an intervention trial of a referral proforma. The key finding is that in more than 20% of cases the patient was not investigated or referred. I have two major concerns with the study. 1. it is unclear from the results how the statement that "in more than one in 5 cases the patient was not investigated or referred" is calculated. The authors state p.6, lines 31-32 and in table 3 that pre-intervention only a small minority of patients were inappropriately managed (by prescription only). They then state that post-intervention (for reasons not stated in this paper) this figure (inappropriate use of prescription only) increased to 1 in 5 (21.5%) of patients. I am unclear why - if the fact that the results are pre or post intervention is not relevant (p. 3, lines 38-40)- the summary statistic of pre and post intervention is not reported. 2. the results presented in table 4 and presented in the abstract are difficult to interpret and it is difficult to assess their clinical importance. The authors have chosen their comparator as colorectal cancer - and all other cancer actions are reported as more or less likely when compared to colorectal cancer. If this is a meaningful comparison then it is important that the rationale for choice of colorectal cancer as the comparator is explicitly stated. Other comments: 3. Abstract - l. 9-10. Worded incorrectly. According to the background text it should state "warranted a referral to a specialist". not "urgent investigation" 4. Abstract l. 14. should read "referral proforma". 5. Abstract l. 19-29. It should state: Compared with colorectal cancer .... participants were less likely to .... 6. Introduction l17-18. This sentence is unclear in meaning and needs re-wording. 7. 41-43. The correct abbreviation for the UK's National Institute for Health and Care Excellence is NICE. 8. Page 4, l.52-53. Please define why "investigation only" was considered the base outcome 9. Table 2 P. 5. Needs editing. Please ensure all % are inserted correctly 10. Page 6. l. 35-36. It is unclear what the statement "clinic remoteness was the only demographic factor significantly associated with the management of the patient" means. 11. p. 7 l31-33. "participants in remote/very remote locations ... more likely to manage the patient with an investigation only". This finding is not shown in the RRR for this group in table 4. These are all less than 1. 12. a key limitation of this study, which makes interpretation of the findings difficult, is the fact that the study design did not examine the reasons for the GP decision. This is mentioned in the discussion section but needs to be emphasised in the strengths and weaknesses section.

REVIEWER	Paresh Dawda Visiting Fellow, ANU, Australia Adjunct Associate Professor, University of Canberra, Australia Regional Medical Director, Ochre Health
REVIEW RETURNED	21-Jun-2015

GENERAL COMMENTS	In the section on strengths and limitations I do not understand why the authors have stated "as the participants differed from GPs who practice in Australia, the generalisability of the findings is limited". Table 2 seems to suggest all the participants are Australian GPs. Did the authors mean that the study participants are not necessarily representative of all Australian GPs. A discussion in the strengths and limitations of the small number of participants (n=4, remote and very remote area) in remote areas and the impact that may have on the results and findings with respect to remoteness would be helpful. I feel it would be helpful for authors to review and reference the following publication: Delayed diagnosis in cancer: Thematic review. NPSA (2010) available at: http://www.nrls.npsa.nhs.uk/resources/?EntryId45=69894

VERSION 1 – AUTHOR RESPONSE

Review Rejoinder
Prof. Willie Hamilton
• I would only quibble methodologically with the three way classification of investigation outcome. It is simpler, and arguably more accurate, to have 'the doctor activity is likely to have uncovered cancer if it were present vs the doctor activity is unlikely.... Although the authors appreciate the suggestion, for the purpose of this analysis, the three-way classification of investigation outcome was purposefully chosen to address the aim of this study.
• given the methods, I am staggered by the results. Indeed, I'm so staggered, I begin to question the methods. On the face of it there's some appalling legally actionable primary care practice in Australia (sure NICE guidelines are not Holy Writ, but they do provide a benchmark) OR this study is biassing GPs towards a 'wrong' answer. My problem with the latter view is that usually 'unreal' studies pus one towards 'correct' clinical behaviour Although the results offer an insightful contribution to extant literature, the authors have duly noted the limitations associated with this study. These include the non-representative sample – as such, the results do not necessarily reflect the practices of all Australian GPs.
• I'm left with a very well conducted, very well written up paper, but with a nebulous rationale, and unbelievable (or nearly unbelievable) results. What a curate's egg Following the reviewer's reflection, the manuscript was revised accordingly and now reads as follows:

Furthermore, a recent report on delayed cancer diagnoses noted a 'lack of reporting culture in primary care compared with acute hospitals… [As such] any analysis will show only a small proportion of incidents in primary care, and from general practice in particular'. (23) This may explain the limited literature on potential delays to cancer diagnosis within primary care. The data presented here suggest a substantial risk of delay. The review also concluded that some of the factors that contribute to practitioner delay included: symptom misattribution and/or no examination or investigation of malignancy. The data presented in this paper support these conclusions.
Prof. Tim Stokes
• it is unclear from the results how the statement that "in more than one in 5 cases the patient was not investigated or referred" is calculated. The authors state p.6, lines 31-32 and in table 3 that pre-intervention only a small minority of patients were inappropriately managed (by prescription only).

They then state that post-intervention (for reasons not stated in this paper) this figure (inappropriate use of prescription only) increased to 1 in 5 (21.5%) of patients. I am unclear why - if the fact that the results are pre or post intervention is not relevant (p. 3, lines 38-40)- the summary statistic of pre and post intervention is not reported. The aim of the intervention tested in the original trial – namely, a referral pro forma – was not to guide GP referral, investigation, or prescribing. As such, the intervention was not deemed to be germane to the focus of this study – that is, whether the patient was referred, investigated, or offered a prescription. The original trial is now published and in the public domain (please see reference 12). Although the difference in the outcome of this study offers an informative contribution to extant literature, the purpose of this review is to summarise patient management, encompassing all phases of the trial. This has been duly clarified at the end of the introduction, which now reads as follows:

As part of a larger pre-post, randomised control trial of an interactive online referral pro forma (12), the review of data reported here focused on how Australian GPs manage patients with cancer symptoms. The intervention tested in the original trial did not aim to guide GP referral, investigation, or prescribing practices – as such, its focus is not germane to the focus of this review, which encompasses data from both phases (12).

• the results presented in table 4 and presented in the abstract are difficult to interpret and it is difficult to assess their clinical importance. The authors have chosen their comparator as colorectal cancer - and all other cancer actions are reported as more or less likely when compared to colorectal cancer. If this is a meaningful comparison then it is important that the rationale for choice of colorectal cancer as the comparator is explicitly stated. In accordance with the reviewer's comment, the manuscript has been revised accordingly and now reads as follows:

For two key reasons, colorectal cancer was the chosen referent category. First, it represents a prevalent type of cancer. Second, in this study, colorectal cancer symptoms were managed in a similar proportion of option – that is, prescription, referral, or investigation.

• Abstract - l. 9-10. Worded incorrectly. According to the background text it should state "warranted a referral to a specialist or further investigation". not "urgent investigation" In accordance with the reviewer's comment, the manuscript has been revised accordingly. However, as per the NICE guidelines, these patients require investigation within two weeks, hence the use of the term, 'urgent'.

• Abstract l. 14. should read "referral proforma". In accordance with the reviewer's comment, the manuscript has been revised accordingly.

• Abstract l. 19-29. It should state: Compared with colorectal cancer .... participants were less likely to .... In accordance with the reviewer's comment, the manuscript has been revised accordingly.

• Introduction l17-18. This sentence is unclear in meaning and needs re-wording. In accordance with the reviewer's comment, the manuscript has been revised accordingly and now reads as follows:

GP access to diagnostic tests is particularly helpful in cancer care (5). It can optimise the timely receipt of appropriate treatment and as such reduce, if not avert the personal, social, and economic costs of cancer (6-8). Given the complexity of health systems, it can be difficult (if not impossible) to isolate definitive causal relationships between GP diagnostic tests and cancer outcomes (9, 10). However, GP access to diagnostic tests is likely to help to identify those patients who require urgent care (11).

• 41-43. The correct abbreviation for the UK's National Institute for Health and Care Excellence is NICE. In accordance with the reviewer's comment, the manuscript has been revised accordingly.

• Page 4, l.52-53. Please define why "investigation only" was considered the base outcome 9. In accordance with the reviewer's comment, the manuscript has been revised to clarify that investigation only was 'selected' as the base outcome.

• Table 2 P. 5. Needs editing. Please ensure all % are inserted correctly 10. In accordance with the reviewer's comment, the manuscript has been revised accordingly.

• Page 6. l. 35-36. It is unclear what the statement "clinic remoteness was the only demographic factor

significantly associated with the management of the patient" means For clarity, the manuscript has been revised accordingly and now reads as follows:

Of all the demographic data pertaining to the doctors, the only factor that appeared to influence their decisions was the geographical location of their practice (p<.001 of overall Wald test after regression).
• p. 7 l31-33. "participants in remote/very remote locations ... more likely to manage the patient with an investigation only". This finding is not shown in the RRR for this group in table 4. These are all less than 1. The authors appreciate this constructive observation. The rrr for investigations was 1 as we are comparing the other options (prescription or referral) with 'investigations'.
• a key limitation of this study, which makes interpretation of the findings difficult, is the fact that the study design did not examine the reasons for the GP decision. This is mentioned in the discussion section but needs to be emphasised in the strengths and weaknesses section. This limitation has been duly noted in the revised manuscript, which now reads as follows:

Finally, data were not collected on participants' reasons for their selected patient management strategy.
A/Prof. Paresh Dawda
• A discussion in the strengths and limitations of the small number of participants (n=4, remote and very remote area) in remote areas and the impact that may have on the results and findings with respect to remoteness would be helpful. This limitation has been duly noted in the revised manuscript, which now reads as follows:

Third, as the participants differed from GPs who practice in Australia, the generalisability of the findings is limited. Similarly, the number of participants from very remote areas was limited to four participants.
• I feel it would be helpful for authors to review and reference the following publication: Delayed diagnosis in cancer: Thematic review. NPSA (2010) available at:
http://www.nrls.npsa.nhs.uk/resources/?EntryId45=69894
The authors appreciate this constructive observation and have revised the manuscript accordingly.

VERSION 2 - REVIEW

REVIEWER	Willie Hamilton University of Exeter, UK
REVIEW RETURNED	01-Jul-2015

GENERAL COMMENTS	My only tiny suggestion is that reference to the NICE guidelines is changed to the NICE 2005 guidelines, as we've just published the updated ones.