Reviewer's report

Title: Determinants of public trust in complementary and alternative medicine

Version: 1 Date: 16 October 2009

Reviewer: Fuschia M. Sirois

Reviewer's report:

This paper presents the findings from a large survey of health care users regarding their trust in CAM. The large data set is a strength of the research. There is, however, a need for a more thorough and critical presentation of the current relevant CAM literature, knowledge, and accepted standards, and how the current study and hypotheses are related to this existing body of knowledge. This is the most significant weakness of the paper and one which significantly limits its contribution to the literature. Below are a number of specific areas which are for the most part related to this issue, and which require attention.

Major Compulsory Revisions

1) On page 3 there are several statements made about the types of public trust, definitions of public trust, and expectations about how public trust may be used as a performance indicator. However, there are no references given. Do these statements reflect the authors’ opinions or are there any references to support these statements?

2) On page 4 an anecdotal example is given as proof that negative media attention can impact trust in CAM. However, there are other sources which have not and should be presented – see Weeks & Strudsholm, 2008 BMC CAM for a review).

3) It would be useful for the reader if a brief synopsis of the previously developed model could be presented so that he factors suggested to influence public trust can be better understood. Simply presenting the factors without the context of the model leaves the reader to take the author’s word about the model rather than allowing the reader to evaluate the suitability of applying this model to the current inquiry. Numerous models have been applied to understand CAM use and if the authors have a new model that might be useful for further research then it should be presented here.

4) On page 6 the authors present hypotheses regarding the influence of social networks on trust in CAM, but only cite one source which appears to have no relation to CAM use. The hypotheses are stated in a way to suggest that the impact of social networks on CAM trust vis a vis CAM use has not been previously investigated. This is not the case and there have been a number of studies that have investigated the role of social networks on trust and therefore use of CAM, most notably Robinson & Cooper’s 2007 paper which specifically examined the use of trusted sources of information when people contemplated
using CAM. This research should be reviewed and presented here as it 1) generally provides support for the hypotheses, and 2) not presenting it misleads the reader by not accurately presenting the current state of knowledge regarding the role of social networks in CAM use. Some papers that come to mind include Caspi, Koithan, & Criddle, 2004; Öhlén, Balneaves, Bottorff, & Brazier, 2006; Robinson & Cooper, 2007; Sirois & Purc-Stephenson, 2008. However, a much more thorough review of this segment of the CAM literature would reveal several others as well, and all require mention here as word limits are not an issue.

5) Similarly on page 7, the authors make speculations about the role of personal experiences with CAM and how this may influence trust in CAM, without referencing previous research that has suggested the same conclusions. This is another oversight that requires attention as it is again misleading as to the state of current knowledge regarding CAM use. Some studies that need mention include Sirois, 2008 BMC CAM; Luff & Thomas, 2000; but again a more thorough search should be conducted to present an more accurate account of the current state of knowledge.

6) Again, on page 7 the hypotheses regarding the role of personal beliefs and philosophy have not been adequately grounded in the current literature and presented as if they are a new insight that will be tested for the first time in this study. Later in the discussion the authors refer to this as the “philosophical congruence hypothesis” and state that a longitudinal study is needed to confirm their results. The indicators used to test this idea that philosophical similarity with CAM (which by the way has been examined by numerous researchers over the past decade) are hardly reflective of overall life philosophy (religious and political views), and the authors have not provided sufficient justification for operationalizing these variables as true indicators of philosophy or beliefs. Moreover, the research that has been conducted to date has not been included. Much of this research has focused on holistic views of health as a key philosophical determinant of CAM use, but again there is no mention of this other research. The authors have referenced a small systematic review regarding beliefs in general and their role in CAM, but no other mention of more recent research on the role of beliefs in CAM use or the fact that there has been a fair bit of research conducted on this topic.

7) On page 8, regarding the issue of trust in regular health care and CAM use, the authors have not presented a balanced view of the topic, and left out much of the relevant literature. The issue of dissatisfaction with regular health care and CAM use is controversial and may be dependent upon when the issue is examined. Some recent research suggests that the reasons for CAM use have shifted in the past decade or so from negative reasons (i.e., dissatisfaction with CAM) to more positive reasons that have less to do with dissatisfaction (Sirois, 2008, BMC-CAM). Also, dissatisfaction may play a role in initial CAM use but not continued CAM use (see Sirois & Purc-Stephenson, 2008 CHPR for a full discussion of this issue). But if trust in CAM develops from greater experience with CAM then what role if any does dissatisfaction in regular health care play in trust in CAM? A more nuanced presentation and discussion of this issue is
needed if the argument for dissatisfaction as a factor predicting trust is going to
be made. Again, the current research on this topic has not been fully covered
and presented and therefore misrepresents the current state of knowledge
regarding CAM. A more thorough, balanced presentation of this topic is required
as there is much recent evidence to suggest that dissatisfaction with regular
health care may not have that much influence on CAM use.

8) It appears that the data was collected in 2001. What are the implications
regarding the interpretation of the findings today from data that is 8 years old?
Again, I refer the authors to the BMC CAM paper which found that the reasons
for CAM use can significantly shift within an 8 year period (Sirois, 2008).

9) One major issue with this study is the lack of information provided regarding
how CAM was defined. In the results it appears that paranormal therapy was
included as a CAM. I believe that most CAM providers would find this inclusion
offensive and delegitimizing. How was CAM defined? Which definitions informed
the types of CAM listed in the survey? And if the categories described as CAM
are different from what other researchers would included as CAM, such as those
provided by NCCAM and other national agencies, is this because of accepted
standards for CAM categorization in the Netherlands? A strong rationale for
including certain therapies as CAM needs to be provided and presented to justify
the very broad inclusion criteria that were apparently used for the current study.

10) Given previous research suggesting that the amount of experience with CAM
influences beliefs and judgements about CAM and its use, and the authors own
hypotheses regarding the role of experience on trust, it is surprising that no
mention is made of the limitation imposed by only assessing CAM use versus no
use in the current study and the implications of this for the conclusions drawn.
Some mention of this is needed. Although the authors mention that trust
develops over time and that longitudinal work may be needed, even a cross
sectional analysis based on different levels of CAM experience would have been
useful for examining the role of experience in the development of trust. This
limitation warrants mention as do the following previous studies that have
employed this approach: Sirois & Gick, 2002; Sirois & Purc-Stephenson, 2008;
Shumay, Maskarinec, Gotay, Heiby, & Kakai, 2002.

11) The last paragraph of the discussion regarding active seeking of information
and trust in CAM is a bit confusing. CAM users are noted to be active seekers of
information in comparison to non-users so the authors’ point regarding a further
research needed here is a bit unclear. Is the basic level of trust referred to similar
to a personality trait? There has been at least one study that I am aware of that
found that people who score high on the trait of Agreeableness are more likely to
use CAM to a greater extent (Sirois & Purc-Stephenson, 2008, JACM).
Agreeableness is marked by a general trust in other people, and therefore would
support this assertion.

12) Given the gaps in the literature review presented in the introduction and the
lack of situating the current study amidst other relevant previous work, the
discussion requires a much more thoughtful consideration of the conclusions
made and their similarities and differences with previous work. Along the same lines, some of the statements regarding what can be concluded from the basis of the current findings should be toned down and stated with respect to the numerous study limitations. The findings are relevant for data collected in 2001, among the Dutch population, and in the context of a very liberal definition of CAM use, that has been dichotomized to omit information regarding actual experience with CAM – one time CAM users and those who may have used CAM for years are treated as a homogenous group with respect to the conclusions made regarding trust in CAM, when in fact there is compelling prior research to suggest that CAM consumers are a diverse group with diverse beliefs and needs. The current discussion does not reflect an acknowledgment of these important points.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

'I declare that I have no competing interests'