Reviewer's report

Title: Cervical cancer screening program in Thimphu, Bhutan: population coverage and characteristics associated with screening attendance

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Reviewer: Lauro Bucchi

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The manuscript of Baussano et al. (Cervical cancer screening program in Thimphu, Bhutan: population coverage and characteristics associated with screening attendance) reports an interesting study. Here are some suggestions to the Authors that I believe can improve the manuscript.

Discretionary Revisions

Page 9, lines 6-15.
The authors state that the current study was similar in methods (and biases) to previous IARC HPV surveys. How do they explain the relatively high prevalence of screening in Thimphu compared with other disadvantaged areas of the world? Do they have some hypotheses on this?

Page 9, lines 16-18.
Perhaps the Authors may want to comment on the effect of age in Table 1. The fact that cumulative screening prevalence is greater among women aged 35-44 years than among older ones suggests that a birth-cohort-dependent increase in total (all-age) prevalence will be observed in the future years. To some extent, the comparatively low figure among women 25-34 years of age is expected, because cumulative prevalence can only increase with increasing age. However, it is curious to note that a problem of declining participation of younger women has been recently observed in the UK NHS Cervical Screening Programme. The data are available at (page 15):

Page 10, lines 8-16.
It is unclear to me why the Authors discuss the problem of poor screening prevalence specifically among university graduates. Apart from this, this is a very surprising and disappointing situation. I wonder whether this has something to do with the fact that the education level has a favourable effect only on women above the age of 45 years (Figure 1). The ecological association between poor screening prevalence among university graduates and data in Figure 1 is compatible with the conclusion that ‘current’ university attendance has a detrimental influence on behaviour. This is difficult to explain, because the study is from Bhutan, not from Italy ...
Table 2.
I suggest replacing “Yes + No” with “Total”. Note: I have not checked all of the numbers in the Results section against those in the Tables. I invite the authors to do it.

Minor Essential Revisions

Page 3, lines 12-18.
The national screening programme is described. Without some clarifications, the term ‘programme’ may be misleading for potential European readers. I suggest that the sentence “Women are invited … on media” (page 10, lines 19-21) be moved from the Discussion and placed here. It needs to be made clear since the Introduction that the programme is not based on an active call-recall system and population (or census) lists. I also suggest that the popular term ‘opportunistic’ be used to indicate the screening model implemented in Bhutan. This term is mentioned only in the Abstract section.

Page 3, lines 24-25.
The study has two primary objectives, that are indicated at the bottom of page 3. Both of these are based on the analysis of previous attendance for screening. I think that there is at least one secondary objective that required a separate analysis and that needs to be pointed out more clearly, that is, the effectiveness of home visit in recruiting never-screened women. Besides, the finding that is invitation at home “… did not work very well …” is mentioned among the conclusions of the study (page 12).

Page 5, lines 6-10.
Women were recruited in two ways and, for each of these, the Authors give the percent participation rate, i.e., 36% and 100%. I suggest to move these data to the Results section and – more important – to provide the numerator and the denominator of both rates. In this part of the Methods section, it would be useful to explain more clearly whether home visits were or (more likely) were not based on a population list (census list). In page 10, lines 17-18, it reads that census in the capital of Bhutan is incomplete. This is not a marginal detail, and I think that the reader should be informed of it in the context of presentation of study Methods.

Page 5, lines 15-17.
The Authors give the number of women seen in each of the two study hospitals. This is probably due to the fact that they need to separate women from Thimphy from women seen in Lungthenphu (see next point). However, an equally important information is the following: how many of women aged 25 years or older (the core study population) were enrolled at home, and how many at the two hospitals?

Major Compulsory Revisions
Page 4, lines 1-5.
If I understand correctly, the Authors have interviewed all women (age 18-69 years) participating to a study (the main study) that was carried out to assess the baseline HPV prevalence in the general adult female population of Bhutan before the effect of vaccination of younger women becomes visible. In other words, the current study was an ancillary component of a larger study that was based on Pap and HPV testing. This explains why patients who refused to be tested were not interviewed. The following problems need to be clarified. First, for the purposes of the main study, what was the rationale of recruiting the study population from two sources, and particularly from sources that were so different from each other? Second, have the Authors assessed whether there is heterogeneity of results between the two subpopulations?

Page 5, lines 17-21.
This part of the Methods section is confused. First, the term “In addition ...” sounds misleading to me. The 500 women whose screening history was taken from the invitation registry did not “... add ...” to the 1620 women who formed the basis of primary analysis (Table 1). Rather, they were included in a separate analysis (Table 2) (see above: Page 3, lines 24-25). This needs to be clarified. Second, why was this separate analysis restricted to women of Thimphu? If this is not made clear, one could argue that the statement “Information on previous screening was therefore available for 1503 women from Thimphu” is wrong, because it seems to me that information on previous screening was also available for the 617 women from Lungthenphu. Also, what information was recorded in the ‘invitation registry’? I strongly invite the Authors to describe their methods in greater detail.

Page 9, lines 20-22.
The Author state that perinatal care and family planning clinics are insufficient to reach the whole target population. However, have they collected information on whether women attending these centres (before the study) were actually offered screening in a systematic manner? And, if not, why not? In those Western Countries that are only targeted by opportunistic screening, there is a major problem of ‘missed screening opportunities’.

**Level of interest:** An article of importance in its field

**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.