Reviewer's report

Title: Are "Village Doctors" in Bangladesh a curse or a blessing?

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Reviewer: Anne Cockcroft

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The topic of health seeking behaviour and use of different sources of health care is important. However, this paper does not add very much to previous reports from Bangladesh on the topic. The authors' interpretation of their findings is misleading in places and some of their conclusions are not justified by the evidence they present.

Major compulsory revisions

1. Aims of the paper

1.1 The authors mention a number of different aims, including to "to assess the role of various healthcare providers in ......service provision", to “discuss realistic options in ensuring health for the rural masses”, to "explore the health-seeking behaviour of villagers", and to look at both personal and care provider characteristics in determining health-seeking patterns. The paper only really attempts to address the first two of these aims; health seeking behaviour is not really explored to any extent (for example, using qualitative methods such as focus group discussions or in-depth interviews), and there is no attempt to examine characteristics of service-providers beyond their categorisation into different types. For example, there is no information on their level of education, their years of experience, or their views or level of knowledge about different conditions. The paper presents no information about the effect of personal characteristics, except for an indicator of household socioeconomic status; for example, there is no information about the age, sex, or educational level of services users. The authors should revise their statements about the questions the paper is intended to answer and confine them to those their study is able to answer.

2. Methods

The methods section includes some confusing repetition and unnecessary subheadings, yet there is important information lacking. The authors need to re-write the methods section so that it flows logically and takes into account the following concerns.

2.1 The sampling method is unclear. In the abstract, the authors mention randomly selected households from 8 unions of Chakaria upazila. Is this the total number of unions in the upazila? The implication in the methods section is that 1000 households were selected entirely at random within the whole upazila, with
no stratification of the sample. However, the methods section also mentions the Chakaria Health and Demographic Surveillance System Area, which includes 7,600 households. What is this entity? How does it relate to the whole population of the upazila? Some reference about the surveillance system would be helpful.

2.2 Presumably some of the household members who were ill in the last 14 days were children. The authors need to give information about who they collected information from when the ill household member was a child.

2.3 There is almost no description of the analysis, apart from a rather detailed description of the factor analysis used for defining the quintiles for socioeconomic status. Even here, however, the authors give no explanation or justification for the particular list of assets they use. What is the evidence for using these particular assets? If there is previous experience with using this list, the authors should cite this. They should explain what is meant by “Almira” on the list of assets.

2.4 The authors need to describe the analysis they undertook. For example, it is apparent from the results section that some of the questions could have more than one response, and they should clarify this. They also need to be clear about whether respondents selected response options from a list or whether the questions were open-ended. It is apparent that respondents answered several different questions about what they did for illness: where they went, and what type of doctor or practitioner they consulted. The authors need to clarify the questions the respondents answered, and to what extent they had overlapping answers. In Bangladesh, many unqualified practitioners put up a name plate describing themselves as “doctors” and it can be difficult for the public to know if a particular practitioner is a qualified doctor or not (apart from the price of treatment, which may be a good guide). Did the authors have any means to check the respondents' reports of the type of practitioner they consulted?

2.5 In the Results section, the authors quote statistical significance levels, but they do not give any information in the methods about what tests of statistical significance they used. They need to include this information.

2.6 The paragraph on health-seeking behaviour is confusing. It is not clear in this paragraph that the random selection of “patients” was among those in the household who reported being ill in the last 14 days. The use of the term “patient” is best avoided in this population based study. Clearly, many of the household members concerned did not view themselves as “patients” as they did not seek care.

2.7 The use of the term “recall bias” in the paragraph on illnesses is incorrect. The authors should revise the sentence.

3. Results

3.1 It is remarkable that the authors provide no information about the age and sex of the household members who reported having an illness in the last 14 days. Age and sex may well influence health seeking behaviour – whether to
seek treatment and who from. It is a serious limitation if the study did not collect this information; if it was collected, it should be reported and used in the analysis. Information about the level of education would also be relevant if it was collected.

3.2 Categorisation of type of illness based on the report of the household members is probably unreliable, other than into very broad categories. The authors should present the findings from this categorisation more cautiously then they do at present. In Table 1, the authors should give numbers as well as percentages.

3.3 One thing that does emerge clearly is that the data collection coincided with an outbreak of feverish colds in the area. The authors do not say when the survey took place; was it during the winter? This illness outbreak explains the very high rates of reported illness in the last 14 days; it also means it is not appropriate to generalise about health seeking behaviours or requirements for medical manpower from the findings (see below).

3.4 The findings presented in the paragraph headed “treatment seeking” are confusing. The authors should give numbers as well as percentages. Were the reasons given for not seeking care in response to an open-ended question? The percentages mentioned add up to more than 100%. Could respondents give more than one response? The authors should pay more attention than they do to the report of half the respondents who did not seek care that they did not feel that they needed treatment – they may well have been right!

3.5 Under “source of treatment” there is confusion about division into public/private sources and into qualified/unqualified providers. In the division into public/private facilities, presumably the private facilities included both qualified practitioners and unqualified practitioners (including village doctors).

3.6 The analysis by type of illness is probably unreliable, especially for those categories with (presumably) only small numbers of reports. Table 2 should include numbers as well as percentages. There is an argument that for numbers less than 100, only the number should be shown.

3.7 In the last paragraph under the heading “source of treatment” the authors state that 73% of the “patients” chose their healthcare provider based on their belief they were getting good quality care. Without numbers being given, it is hard to tell which “patients” are included in this statement. Is it confined to those ill persons who actually consulted some sort of health care provider (thus excluding the 66% who did not seek care)? In the same paragraph the authors say that 38% of the “patients” chose the health care provider because of proximity to their home. The 73% and 38% already come to more than 100%; did the question allow more than one response?

3.8 The only section of the paper that includes statistical analysis is that examining socioeconomic status and health seeking behaviour. The first sentence in this section belongs in the Discussion section.

3.9 The reported analysis compares the lowest quintile with the highest quintile of
scioeconomic status for reported illness and health seeking behaviour. This has the advantage of increasing the contrast, but it excludes the majority (about 60%) of the population. The authors should at least report the findings from all five quintiles, and preferably use a test of trend to examine the trend from lowest to highest quintile. Another option could be to compare the lowest quintile with the rest of the population. The authors could consider both options and perhaps report the findings from both.

3.10 Table 3 is very difficult to interpret as it stands. The authors should show the fractions on which the percentages in Table 3 are based. It is not clear why the numbers in the columns for treatment seeking are so small. Which people are included in this “treatment seeking” analysis? The large numbers in the columns for illness reporting appear to be based on the total number of household members in the surveyed households. But this is inappropriate. The categorisation into socioeconomic status quintiles was by household, not individually. Therefore the analysis should be based on the number of households who had at least one member ill in the last 14 days, not based on individuals.

3.11 The authors should state the tests of statistical significance they used to produce the p values shown in Table 3. The statistical significance of the difference in illness reporting between the lowest and highest quintile is inflated by the much larger numbers included when undertaking the analysis at individual level and including all members of the households; the difference may well not be significant when, as they should, the authors undertake the analysis at household level.

3.12 Later in the section on socioeconomic status and treatment seeking, the authors describe the proportions of “patients” in the lowest and highest quintile consulting qualified and unqualified doctors; we also need to know how many of the qualified doctors were in public (government) facilities and how many were seen privately. The authors then say that although the differences were not significant there was an overall increasing trend in consulting qualified doctors among the higher quintiles. This implies they carried out some sort of statistical test for trend, across all five quintiles. Did they do this? If so, they should describe the test.

4. Discussion and policy implication

4.1 The authors' claim for the required number of qualified doctors to treat patients, based on the findings from the survey, is seriously misleading. First, they ignore the fact that the high prevalence of recent illness was unusual and related to an outbreak of a feverish cold illness in the area (even though they state this earlier in the paper). Second, they assume that all people ill in the last 14 days needed to see a doctor, whereas at least half of them, by their own report, did not need to see a doctor. Third, they assume that although the reported illnesses were during a period of 14 days, all the ill people would go to see the doctor on the same day. Their calculated requirement for qualified doctors is greatly inflated.
4.2 The review of the benefits of task shifting is uncritical and most of the references are in relation to task shifting to cope with the AIDS epidemic in Africa, which may not be relevant to the situation in Bangladesh.

4.3 The authors do not cite a paper from this reviewer published in another BMC journal, which reported, in more detail and from a series of three large representative national surveys in Bangladesh, on the use of qualified and unqualified practitioners and of government and private facilities. The paper also included findings from focus group discussions about the reasons people chose different health care providers.


4.4 When the authors’ suggest incorporating village doctors within the formal health care system, with referrals to qualified doctors etc, they should mention the strong opposition to such a scheme from the doctors’ professional body in Bangladesh (this is described in reference 9 cited by the authors).

Minor essential revisions

1. Language
The paper should be reviewed by someone whose first language is English.

2. Figures
Figure 1 does not really add much and could presumably be incorporated into Table 2. Figure 2 duplicates information given in the text. Either the figure can go, or the information in the text can be removed.

3. References
Several references are included in the text in Harvard style (under “socioeconomic status and health seeking behaviour” in the Results section) rather than in Vancouver style (consecutive numbers in the text). The word “book” needs to be removed from a number of the references at the end of the text.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

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