Reviewer’s report

Title: Predictors of dying at home in patients receiving care from visiting nurses in Japan: Comparison of cancer and non-cancer patients

Version: 1 Date: 4 August 2010

Reviewer: Catherine Walshe

Reviewer’s report:

I have used the STROBE statement – Checklist of items that should be included in reports of case-control studies to guide this review. Detailed comments follow regarding each section of the paper, but the main issues are detailed immediately below:

Major Compulsory Revisions

1. This paper needs to be situated much more clearly in the context of what we already know about the determinants of place of death. This issue affects the background section, the discussion of choice of variables, and the discussion and interpretation of the research.

2. The paper needs to be amended for an international audience – in particular the VNS service needs to be clearly described, as well as other aspects of the Japanese health service.

3. Ethical issues regarding this research need to be described and addressed.

4. The conflation of scales and variables needs to be more clearly considered, and possibly removed. The possible impact on place of death of patients of variables not measured in this study needs to be considered. The issue of when data were collected (with reference to admission to the VNS service and/or date of death and/or date of hospitalisation) needs to be addressed.

5. The limitations of the study need to be more clearly articulated.

Minor Essential Revisions

1. There are a number of small spelling mistakes – ‘severe’ is misspelt on two occasions.

2. The title and key words could be re-visited.

3. The objectives of the study need stating more clearly.

4. The structure of the report needs revisiting, especially the methods section, to enhance clarity for the reader.

Detailed comments now follow.

In general, this paper addresses an area of interest, as the impact of nursing services on outcomes of interest (such as place of death) of patients is an under-explored area. However, there are issues with the paper as presented:
Title and abstract:

The title accurately reflects the content of the study, but might be more informative if it also made reference to the study methods. The term ‘visiting nurses’ is not in common usage, and may require amendment (see comments below).

I would recommend re-visiting the key words used, as they are not necessarily standard terms, for example they are not all MESH headings, nor necessarily reflect the most common terms used in this field. This may hamper the retrieval of this article by those interested in its contents.

In the abstract, the introduction could more clearly reflect the rationale for the study, and place it in an international context. The methods could be more succinct, and more clearly describe the setting and timescale of the study. In the conclusion, it is not clear that the third sentence can be concluded from the findings.

Introduction:

Paragraph 1. This paragraph does not seem to reflect the breadth of knowledge about place of death and its determinants. For example, there are omitted population studies which are not focussed on cancer patients using palliative care services, and cancer focused studies and reviews e.g. 1-15. This background paragraph is critical to determining a clear rationale for the presented study, and should be expanded to more clearly explore and critique the current knowledge and gaps in this field.

Paragraph 2. For an international readership, the information on place of death for those in Japan should be contextualised/compared with data on place of death in other countries. Why does the Japanese government wish to increase the percentages of those dying at home? There is much literature debating issues of home death that could be exploited to discuss these points e.g. 16-21. The section on insurance changes is confusing in this section, and should be moved to a section contextualising these services (i.e. paragraph 4). In particular, the service is referred to as VNS - it would be preferable not to abbreviate an unfamiliar term.

Paragraph 3. This section does not belong here, but would be more appropriate within the methods section.

Paragraph 4. This section does not fully explain the role of visiting nurse stations. This is not a term that is familiar to me, and I wanted to know more about the role and function of these nurses, particularly as they are described as providing ‘medical management’ rather than nursing care. Are VNS individual nurses or a team/nursing service? More information is needed to compare these services to services that readers from other countries might be familiar with such as district nurses, home nursing services, home hospice etc.22. What is the definition of ‘terminal phase’ used in this paragraph?
Paragraph 5. A clearer articulation of the gap in knowledge that this study seeks to fill is required here. Here, or in the methods section, a clear statement of the objective(s) of the study is required.

Methods:

I found following the key elements of the study design a little confusing, and some of these issues may be better disaggregated into different sections, perhaps those suggested by STROBE such as study design, setting, participants, variables, data sources/management, bias, study size, quantitative variables, and statistical methods. It is unclear whether research ethics committee/institutional approvals were required or obtained for this study.

Overall design: this needs to be clearer – in particular there is potential for confusion about whether the focus of the questionnaire is the VNS service or the patients receiving this service. For example this section refers to ‘VNS services to be surveyed’ and inclusion criteria as ‘patients served by the VNS’. The process of collecting patient level data via a VNS needs to be explored in more detail. Although consent of the VNS can be implied by the return of the questionnaire, what about the issue of patient consent? Although these are deceased patients, what processes were followed to gain consent to use data about them?

Setting: Are VNS services found across Japan? How was the stratification by post code (of VNS?) done? Why was stratification by postcode done rather than a random selection of VNS? Was recruitment only in Dec 05, or were there follow up questionnaires. If no follow up, why?

Participants: Does there need to be description of eligibility for both elements of the study – the selection of the VNS, and then the selection of eligible patients? Was the questionnaire per patient or per VNS? If the VNS had more than one patient to report on, how was this structured and achieved?

A rationale for the patient inclusion criteria needs to be given. The wording of the inclusion criteria is unclear – for example does the ‘within one month after admission’ refer to admission to the VNS service, or admission to hospital? Why the within one month criteria? Why excluded if under 20? How was the presence of psychiatric disorders determined? What is meant by psychiatric disorders – for example we know that many people at the end of life are depressed, is this classed as a psychiatric disorder? How many were excluded using this categorisation?

How do the VNS nurses become aware of hospital deaths, and do we know if their information on place of death is accurate? A general statement on the accuracy of the nursing records is required – much record keeping is inaccurate, and if asking for data on patients who had died up to 6 months before the survey, there is real potential for recall bias that needs to be discussed and addressed.

Variables: There needs to be more discussion here or elsewhere about the
limitations of the study when it can only use variables routinely collected by the VNS. In particular the ‘gaps’ in variables between those that could be measured in this study, and those identified as important in other studies need to be identified – may there be un-measured variables impacting on place of death?

The data on patient and family preferences demands more discussion. It is clearly known that patient/family preferences for place of death can change over the course of an illness, and they are not always congruent\textsuperscript{21;23}. How did the VNS determine preferences, and at what point in the illness? How many patients had a change in preferences recorded?

The re-categorisation of variables is concerning – why did the authors feel they needed to re-categorise a mobility scale from 4 levels to 3? In what way, conceptually, are home bound and nearly bedbound the same? I have the same concerns about the re-categorisation of the cognitive function scale. Who did the re-categorisation – the VNS completing the questionnaire or the researchers?

Why were the variables not all coded at the same time – some were coded at the beginning of the month, others when the patient was hospitalised. Surely this means that the hospitalised patients data was potentially collected at a different time to those who died at home, and so cannot be compared? If at the beginning of the month – this does not tell us how close to death the patient was, as the patient could have died at any point during the month? Surely then this affects some of the variables which are likely to change as death approaches? If date of death is known, would it be possible to determine how close to death the patient was when the data was captured? However, it is not clear that date of death was captured in this study? I have concerns that these issues with data collection seriously undermine this study.

Bias: There is no discussion of any efforts to address potential sources of bias.

Study size: There is no explanation of how the study size was determined.

Statistical methods: I am not a statistician, and cannot comment on the appropriateness of the statistical analysis. It is not clear how missing data were addressed.

Response rate and representativeness of the sample: Again, this appears to conflate issues of VNS response and patient response? The discussion of ‘ownership’ needs contextualising for an international readership, as I am not sure what this means. What is a prefecture?

Results:
A flow diagram might be helpful here to describe the numbers at each stage of the study.

Table 1 could be enhanced with some of the additional information (i.e. diagnosis) that is within the paragraph describing these data on page 9. There is a tendency in these paragraphs on page 9 and 10 to repeat data from the table, this duplication is unnecessary and should be removed.
What is the rationale for creating combined variables regarding functioning and preferences described on page 10? Would it be more appropriate to report these percentages at the bottom of table 1 with reference to the missing data (where up to 34% of preferences are unknown)?

Again, some of the data from table 2 and 3 is purely repeated in the text, this should be avoided.

Discussion:

For the reader, it would be preferable if the main findings from tables 1, 2 and 3 were summarised here, rather than the repetition of most of the findings in the tables in the above section.

The interpretation of the study needs to be done more clearly in the light of what is already known about what influences place of death, to draw out explicitly what this study adds to existing knowledge. Findings which support existing studies should be noted, and those which are novel highlighted. For example, the finding related to length of time on a VNS caseload is not discussed in detail, but is something which is not well understood.

This should be cautiously done, without over-interpreting the findings. For example on page 14, paragraph 1, there is an extrapolation of the findings about preference concurrence regarding stress and health care professionals which does not necessarily follow from the findings, this tendency is repeated when discussing the role of nurses etc.

Limitations: There are other limitations to this study, many highlighted above, which need to be discussed in this section.

Reference List


(6) Brazil K, Bedard M, Willison K. Factors associated with home death for individuals who receive home support services: a retrospective cohort study. BMC Palliative Care 2002; 1(2).


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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests