Author's response to reviews

Title: Mortality Registration and Surveillance in China: History, Current Situation and Challenges.

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Author's response to reviews: see over
Dear Dr Mathers,

Re: Mortality Registration and Surveillance in China: History, Current Situation and Challenges

Thank you for your email communication dated 24th December, concerning our paper on Mortality Registration and Surveillance in China. We have now revised the paper on the basis of the comments of the two referees, and have attached a copy of the revised paper.

We list below our detailed responses (in bold font) to individual comments from the two reviewers.

Responses to comments from Dr Judith Banister

This is a useful paper that will be publishable, though it still needs corrections as described below. The paper focuses on two sources of information on causes of death in China, the death registration area that is strongly biased toward lower mortality cities and more developed rural areas (VR), and the DSP system. The paper barely mentions two other sources of mortality data that probably give more usable, complete, and accurate data on mortality levels and trends in China than the VR or DSP systems, which are (1) the series of censuses (1982, 1990, and 2000), and (2) the child mortality surveillance system (See Banister & Hill 2004). The authors probably ignore those other mortality data sources because the censuses and the CMSS do not record and report causes of death; CMSS is, however, the best source of maternal mortality data.

See revisions to Introduction on pgs 4-5. Other data sources are now mentioned as advised by reviewer, and clarification provided regarding focus on VR in this paper.

p. 4—China’s population is closer to 1.3 billion than to 1.2 billion now. Suggest you say 1.3 billion or look up current estimate with one more decimal place (UN Pop Div for example).

Done (see pg 4)

pp. 4-7—This paper implies that the Ministry of Health vital registration system is not useful for anything, but does not say so. I have been conducting demographic research on the PRC for over 3 decades and have never been able to use the MOH vital registration system results for any purpose, though I have tried. China’s official annual death rates come from the National Bureau of Statistics annual survey of population change, not from the MOH vital registration system. All usable cause-of-death data come from the DSP system. I suggest that authors admit more clearly on pp. 4-7 that the unrepresentative MOH vital registration system is a waste of resources and could be abandoned without any loss. Expanding the system to China’s whole population is not going to happen for at least several decades, I would guess, so why use precious resources on it in the meantime?

Annual survey of population change now mentioned as a data source for all cause mortality in the Introduction (pg 4).
We disagree, however, with the suggestion to abandon the VR system, given that VR is the gold standard for mortality statistics, and should remain an ideal to be strived for, and should be promoted to complete coverage over the next few decades, rather than closure. While the system is undoubtedly biased and incomplete, it represents the basis for further expansion of vital registration in China. Moreover, the sheer size of the system (registering approximately 700,000 deaths each year) means that it provides a useful complement to the more representative DSP system to assess cause-specific mortality trends.

p. 6—Is it necessarily true that the overall cancer death rate per 100,000 population will always steadily rise during all stages of the epidemiological transition? In China (esp. rural China) during the last quarter century, age-specific mortality rates have been steadily and strongly dropping. Even if the proportion of deaths due to cancer in each age group has been steadily rising, might the net result be a flat cancer death rate per 100,000 population during this part of the demographic/epidemiological transition?

Amended this statement as suggested (pg 7).

Table 1—This table is problematic in its current form, and needs to be completely redone. I am assuming that the authors are giving central death rates \(m(0)\) in this table, not infant mortality rates (IMR). I suggest that the table have an \(m(0)\) column and an IMR column for each relevant year so that the data can be attributed to the correct years and so that the best IMR data can be shown in the table. Census infant death data refer to 1989-90 (not 1991) and 1999-2000; authors please go back to the original reported death data from the 2 censuses and recalculate those numbers; I think they are both incorrect (but I am in US right now and don’t have census volumes handy). The table should include IMR data from the CMSS, which has higher and more complete IMR data than any of these sources. Authors check if the DSP rural \(m(0)\) numbers you give here have already been adjusted—it appears so but I do not have the sources handy. I think it is misleading to compare adjusted data from one source with unadjusted data from all other sources, if that is what is happening.

As suggested by the reviewer, we have reformatted the table to enable easier comparisons. (see Revised Table 1)

Since we cannot obtain data on live births from the Census (or the VR), we have restricted the table to show only central death rates \(m(0)\) (and not IMRs), as correctly inferred by the reviewer. The overall conclusions about the comparative completeness of the various sources remain valid, however, with the infant death rates.

Also, as pointed out by the reviewer, we have corrected the DSP death rates to show only the unadjusted death rates from each source.

To clarify, the reason for including this table is to show the shortcomings in routine registration systems (DSP and MOH-VR) in accurately capturing levels of infant mortality, in part due to lack of sufficient attention to birth registration. We acknowledge that the National Maternal and Child Health Surveillance system is probably the most accurate source of data on infant mortality, but that is not the point of providing the information in the table.

Figure 2—age-standardized on what population of what year? Why compare VR 1999 with DSP 1996-98? Use the same year or years—both systems cover all the years of late 1990s. I don’t believe that the CDR from unadjusted VR data is 7.0, even after age standardizing, unless you used a non-China age standard. Recalculate Figure 2 using the unadjusted data from both sources, the same year(s) of data, and a good China age standard. Then see if your discussion of Figure 2 on p. 7 still holds.

We used the WHO standard population for 2000, and have provided a footnote to Figure 2 along with a reference.
DSP data were pooled over three years due to much smaller numbers of deaths than MOH-VR (even pooling three years of DSP data produces only 25% of deaths in one year of MOH-VR). Hence, more stable age, sex and broad cause group rates were obtained by pooling data, to improve comparability.

We recomputed the age standardized death rates for DSP 1997-1999, since these are the latest years for which we have the data. This period now overlaps with the year used for VR (1999).

Table 2—are the GDP data for the neighborhoods and townships, or for larger units than the DSP sites? From what source? The other data come from Census 2000 but not the GDP data. (Should say “million RMB”.)

Source of GDP data and method for deriving the estimates now provided as a footnote to Table 2.

Corrected “Million RBM” to “Million RMB.”

Figure 4 is very confusing. What purpose does it serve?

As suggested by the reviewer, we have deleted this figure, and provided additional detail in text (pgs 14-15).

p. 12—you say that “The DSP appears to be reporting the highest values for IMRs, after correction for underregistration of infant deaths.” Please compare the adjusted IMRs from DSP with the unadjusted (or adjusted) IMRs from CMSS to see if CMSS is still more complete than DSP.

See response to earlier comments on Table 1.

Besides, this positive statement about DSP IMR data is misleading because the unadjusted data are grossly underreported and only these data include causes of infant deaths. We are missing cause-of-death information for one-fifth to one-third of infant deaths, right? (Table 4)

Agreed. Now clarified in response to comments on Table 1.

p. 12—What I see from Table 4 is that by 1998 DSP coverage of infant deaths had improved. Percent underreported of deaths at all ages changed very little—13% in 1992 and 14% in 1995 & 1998, too minor a change to support the statement that “the extent of undercount appears to be deteriorating at the national level.” Table 4 says that overall completeness in 1998 was 86%, not 85%.

Agreed. Statement changed from ‘deteriorating’ to ‘no signs of improvement’(pg 13).

Overall completeness figure corrected to 86%, as pointed out by reviewer (pg 13)

Why has DSP conducted no completeness survey since 1998? Is funding being cut for the system? If there are such problems, the article should say so.

Details relevant to restructuring of death registration systems in the new millennium have been added to the Discussion section.(pg 15).

p. 13—I would say that the DSP system is very useful indeed for cause-of-death data, but to use the word “remarkable” twice at the top of p. 13 is being too enthusiastic. There are problems with the DSP system—for example, most of the results and discussion of the results are not available in English; if you want some information from DSP that does not happen to be published, you cannot get it from MOH or CAPM unless you happen to be WHO and are funding the DSP system; and the completeness surveys seem to have been abandoned.

Amended tone of remarks as suggested by reviewer.

Middle of page—should be “approaches”.

Done
Here again the authors say “mortality data” when they mean “cause-of-death data.” For data on China’s overall mortality levels and trends; age-specific mortality levels and trends; mortality by sex and age-sex groups; province-specific, provincial urban and rural, county-level, and city-level mortality data, the best sources are the 1973-75 nationwide mortality survey and the censuses of 1982, 1990, and 2000. For data on under-5 mortality, infant mortality, mortality ages 1-4, and maternal mortality, the best source is the CMSS for 1991 to the present. These sources are “novel, affordable, and sustainable approaches to data collection on mortality that is representative of” the PRC population. DSP is, however, the best source for cause-of-death data.

Agreed. Changed ‘mortality’ to ‘causes of death’.

We feel that routine registration systems that yield information on both levels and causes of mortality are desirable, rather than surveys, censuses etc. Hence, we consider that the DSP system is more appropriate for this purpose, and needs to be strengthened to improve completeness of registration and cause attribution, and have reflected this in the discussion section of the paper (pgs 14-15)

Figure 5 may mean something to the project managers of the evaluation project, but it is unintelligible to me, and therefore I would suppose to most readers of this paper. Deleted figure as suggested by reviewer. Aims, methods and expected outcomes of evaluation project explained briefly (pg 15).

Responses to comments from Dr Chen Jie

In general this is a well written and valuable paper on an important subject. This article made a good contribution about how to establish a cost-effective system of statistical data collection, especially on Mortality and cause of death as scientific evidences for policy makers.

In addition, I have some comments or suggestion as follows:

1) The title of paper covers three aspects: history, current situation and challenges, but most of the content in the paper described 'history' and some 'current situation', few dealt with 'challenges'. Actually there were many challenges ahead: impact of the administration decentralization on DSP; financing and sustaining of DSP after using up the loan from the World Bank; there were no qualified medical staff at some township hospitals and adequate equipment in some poor or less developed rural areas for identifying the cause of the death; and so on.

Agreed. Added points on challenges ahead in the discussion section viz: need for capacity building and coordination of activities (pgs 14-15)

2) It will be much better if the authors could make some kind of comparison with the Vital Registration and DSP.

A comparison of mortality patterns from the DSP and MOH-VR is a detailed topic, and has been the focus of a separate research paper submitted to the Bulletin of the WHO, which is currently being revised based on comments from reviewers. We are happy to provide a cross reference to that paper, if required. Figure 2 provides an (admittedly broad) comparison of death rates from the two systems.
3) There is a well organized and financed family planning services network, maybe can use this system or together with health or other sectors to set up complete vital registration system?

This is beyond the scope of the paper, but could be the focus of future activities, based on the outcomes of the evaluation project described in this paper. We have raised this possibility in the discussion (pg 15).

4) There are some paragraphs of this paper missing references to support the described point, such as the following:

   P6: A good test to assess the quality of the vital registration data is to examine trends in cause-specific death rates (added Ref).

   P13: Tanzania and elsewhere suggests that the data are extremely useful for determining the need for priority health programs (added Ref)

5) Figure 1: Trends in reported cancer mortality in rural areas of China, 1970 – 2000

   rural areas -> urban and rural areas

   Amended title of table as pointed out by reviewer.

   6) Figure 2: The meaning of the figures is not clear.

   The Figure compares age standardized mortality rates for broad cause groups from the two systems, in order to demonstrate likely biases in the reporting of causes of death in the different statistical systems in China. We have added additional terms to the legend in the Figure to clarify this.

   7) Table 1 and 3 source: When?

   Added sources and dates as suggested by reviewer.

We trust that we have now adequately addressed the concerns of the reviewers in the revised paper, and look forward to hearing from you.

With very best wishes.

Yours sincerely,

Alan Lopez
Professor of Medical Statistics and Population Health
Head, School of Population Health