Assessment of response bias is neglected in cross-sectional blindness prevalence surveys: a review of recent surveys in low- and middle-income countries.

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Abstract

Purpose Findings from cross-sectional blindness prevalence surveys are at risk of several biases that cause the study estimate to differ from the 'true' population prevalence. For example, response bias occurs when people who participate ('responders') differ from those who do not ('non-responders') in ways that affect prevalence estimates. This study aimed to assess the extent to which response bias is considered and occurs in blindness prevalence surveys in low- and middle-income countries (LMICs).

Methods We searched MEDLINE, EMBASE and Web of Science for cross-sectional blindness prevalence surveys undertaken in LMICs and published 2009–2017. From included studies we recorded and descriptively analysed details regarding enumeration processes, response, and non-response, including the impact of non-response on results.

Results Most (95%) of the 92 included studies reported a response rate (median 91.7%, inter-quartile range 85.9–95.6%). Approximately half clearly described enumeration processes (49%), and reported at least one strategy to increase the response rate (53%); a quarter (23%) statistically compared responders and non-responders. When differential response was assessed, men were more likely to be non-responders than women. Two-thirds (65%) of the time a sociodemographic difference was found between responders and non-responders, a difference in blindness prevalence was also found. Only 13 studies (14%) commented on implications of non-response on prevalence estimates.

Conclusion Response rates are commonly reported from blindness prevalence surveys, and tend to be high. High response rates reduce—but do not eliminate—the risk of response bias. Assessment and reporting of potential response bias in blindness prevalence surveys could be greatly improved.

Introduction

Estimates of the prevalence and causes of visual impairment and blindness are required to inform local eye care plans,[1] and to calculate global prevalence estimates.[2] In low- and middle-income countries (LMICs) these estimates tend to be generated from cross-sectional surveys, such as the Rapid Assessment of Avoidable Blindness (RAAB).[3] In its latest global action plan the World Health Organization (WHO) has called for more prevalence surveys to be undertaken.[1] When developing eye care plans, decision-makers must be provided a comprehensive summary of findings from these surveys, including the implications for results of any potential sources of bias. Good research attempts to minimize bias, whereby the results of a study deviate systematically from the true value.[4] To aid interpretation of a study's findings, reporting guidelines recommend that authors discuss any potential bias in their results, including the likely direction and magnitude of the bias.[4]

Selection bias is potentially a major cause of bias in cross-sectional surveys. It occurs when the study population selected does not represent the target population and causes the study's prevalence estimate to differ from the 'true' population prevalence.[5] Of particular concern for blindness and visual impairment surveys (hereafter referred to as blindness surveys) is the selection bias known as *response bias.* Response bias occurs if characteristics between those who participate ("responders") and those who are unavailable or decline participation ("non-responders") in a study differ in a way that affects prevalence estimates.[4]

Response bias is of concern in cross-sectional blindness surveys; in many settings people with visual loss may be more likely to be at home and available to participate when enumerators visit, and willing to undergo an eye examination, while those with good vision may be more likely to be away at work or undertaking other activities. If responders are more likely than non-responders to be blind, the study results will over-estimate the true prevalence of blindness in the population.[6,7] To date, there has been no systematic assessment of response bias in blindness surveys. The aims of this study were to assess the extent to which response bias is considered, and the extent to which it exists, in cross-sectional blindness prevalence surveys undertaken in LMICs.

Methods

We sought to identify cross-sectional surveys of visual impairment and/or blindness undertaken in LMICs published after 1 January 2009. We considered studies published from January 2009 onwards as authors of these papers could draw on the STROBE (*Strengthening the Reporting of Observational Studies in Epidemiology*) reporting guidelines that became available in late 2007 and included guidance on reporting response bias.[4] On 1 November 2017 we searched MEDLINE, EMBASE and Web of Science using the

algorithm *'blindness or vis* impairment or low vision'* and *'prevalence or rapid assessment or population-based'*. The titles and abstracts of all citations identified during the initial search were systematically screened by two authors independently to exclude publications that clearly did not meet the inclusion criteria. Any differences were resolved by discussion. In addition, we examined reference lists of all included articles, as well as published review articles of blindness prevalence surveys.[2,8] The full-text article was retrieved for review if the citation was potentially relevant. The selection criteria are shown in Box 1.

Box 1: Selection criteria

Inclusion	• conducted in countries classified by The World Bank as LMIC in 2016[9] • published from 1 January 2009 • presented cross-sectional population-based survey data • provided information on visual impairment based on subjective visual acuity measurement • published in English
Exclusion	• undertaken in specific populations (e.g. in hospitals, 'institutionalised', 'diabetics') • only included children (as recruitment tends to be through key-informants rather than population-based) • only reported disease-specific visual impairment

Data extraction

Data were collated using an Excel spreadsheet (Microsoft Corp, Redmond, Washington) and transferred to Stata 12.0 (StataCorp LP, TX) for analysis. The data were extracted by one researcher (JR) and verified by another (AP or CG). Any discrepancies were resolved by discussion.

From each study, we extracted information on the country and whether the RAAB protocol was used. We recorded available information on the response rate; whether the description of enumeration processes made it clear that the denominator of the response rate included those who were absent (an example is provided from the RAAB Training Manual in File S1); reasons for non-response; and whether strategies to reduce response bias were reported (e.g. returning at a different time; examining those who could not attend examination sites in their homes).

We also extracted data on whether studies reported the following elements recommended for reporting and assessing response bias:[4,10-12]

- Number and characteristics of non-responders;
- Comparison of known characteristics of responders and non-responders;
- Comparison of characteristics of responders with the target population (derived from a census or similar);
- Post-survey adjustment of results (i.e. age- and/or sex-standardisation of prevalence estimates);
- Discussion of potential response bias in the context of the results.

Analysis

Descriptive analysis included the number and proportion of studies reporting aspects of enumeration and response as outlined above (e.g. response rate; reasons for non-response, disaggregation of non-responders, comparison to target population etc). The median response rate and inter-quartile range was calculated. The Kruskall-Wallis test was used to test for a difference between response rates in low, lower-middle and upper-middle income countries. Comparisons between responders and non-responders were summarised.

Results

A total of 3,125 records were identified by the search and ultimately 92 studies (summarised in File S2) met the inclusion criteria for this study. Thirty-eight of the included studies were RAABs (41%), and over half of all studies (57%) were conducted in one of four countries—China (n=20, 22%), India (n=16, 17%), Iran (n=10, 11%) and Nigeria (n=6, 7%). A similar number of surveys were reported from upper-middle (n=41, 45%) and lower-middle income countries (n=40, 43%), with fewer from low income countries (n=11, 12%).

Sampling, enumeration and response

Most studies (n=79; 86%) used a cluster sampling strategy, and sample size calculations were outlined in 63 studies (68%). The number of participants ranged from 175 to 125,641 with a median and inter-quartile range (IQR) of 3,050 (IQR 2,065–4,705).

The target non-response rate most commonly included in sample size calculations was 10% (Table 1). Most studies (n=87; 95%) reported the response rate achieved, which tended to be high, with a median and inter-quartile range of 91.7% (IQR 85.9–95.6%). Seventy-five studies (82%) reported a response rate of at least 80% and 54 studies (59%) reported a response rate of at least 90%. The median rates in low (95.5%; IQR 86.2–96.5%) and lower-middle income countries (94.0%; IQR 89.4–96.5) were slightly higher than in upper-middle income countries (89.7%; IQR 81.7–94.0%) (χ^2=9.373, p=0.0092). Both the target and achieved non-response rate was reported in 55 studies, and in 43 of these studies (78%) the non-response rate achieved was better than the target rate.

Approximately half of the studies (n=45, 49%) clearly described the enumeration process, including how absentees were managed. A similar proportion of studies (n=49, 53%) reported at least one strategy to increase the response rate, most commonly returning to the house of absentees at least once (n=46, 50%), followed by offering to conduct testing at the house for those unable to travel to a central location

(n=10, 11%). When reporting the response rate, just over one-third of studies (n=35, 38%) provided reasons for non-response that quantified those who were absent or refused to participate (Table 1).

Description and comparison between responders and non-responders

Non-responders were disaggregated by at least one sociodemographic characteristic in 36 studies (39%), most commonly sex (n=29, 32%), followed by age (n=27, 29%), education level (n=10, 11%) and place of residence (n=6, 7%).

Twenty-one studies (23%) made at least one statistical comparison of sociodemographic characteristics between responders and non-responders. Across the 21 studies, 43 comparisons were made and 28 of these (65%) found a difference. The median response rate of studies finding at least one difference between responders and non-responders was 87.8% (IQR 80.5–91.8%).

The most commonly assessed difference between responders and non-responders was sex (n=18, 20%), followed by age (n=17, 18%), education (n=5, 5%) and place of residence (n=3, 3%; Table 2). The majority of studies that assessed sex identified a difference (n=15, 83%), with men being more likely to be non-responders in 14 of the 15 studies (93%; Table 2). The age of responders and non-responders differed in 9/17 studies (53%), with no clear pattern of non-responders being older or younger. In the five studies that compared education level, three (60%) found that non-response was more likely among those with lower education (Table 2). In the three studies where place of residence was assessed, one study (33%) found urban dwellers were more likely than rural dwellers to be non-responders.

Of the 28 instances where a difference was found between sociodemographic characteristics of responders and non-responders, 23 also assessed whether there was a difference in blindness prevalence between the same sociodemographic subgroups; a difference was identified 65% of the time (15/23; Table 2). The median response rate of studies finding a sociodemographic difference in response and subgroup differences in blindness prevalence was 88.6% (IQR 84.4–91.7%).

Comparison to target population

Thirty-eight studies (41%) compared the age and/or sex distribution of the survey sample to the target population using a census or similar; the majority were RAABs (28/38, 74%). Most (28/38, 74%) used a table to show the sample and target population disaggregated by age and/or sex; the remaining 10 studies made comment in the text without showing data.

Eight studies (21%; 8/38) referred to the table with disaggregated data but made no comment on similarities or differences between the sample and target population (e.g. “Table x shows the distribution of the sample and target population”). A further fifteen studies (39%; 15/38) said the sample was a “good representation”, “generally similar” or “there was no difference” compared to the target population, without

providing statistical evidence to support the statement. The remaining 15 studies reported differences between the sample and target population, only one of which[13] reported statistical comparisons. The groups most frequently under-represented were younger people and men (both 7/38, 18%).

Visual status of non-responders and assessment of response bias on prevalence estimates

No study reported following up non-responders to assess their visual acuity. One in five studies (n=18) reported asking family or neighbours about the visual status of those absent. Only two of these studies summarised responses to the question—one reported that 9% of non-responders were believed to be blind[14], while the other reported one-third of non-responders were believed to have some form of vision problem.[15] Only one of these reflected on these findings in relation to their prevalence estimates—no analysis was undertaken but the authors thought the likelihood of bias occurring was low.[14]

Almost half of the studies (n=44, 48%) reported prevalence estimates standardised to the age and/or sex composition of the target population.

Implications for non-response on results (response bias)

Thirteen studies (14%) reflected on the implications of non-response on prevalence estimates in the discussion section, only eight of which also reported a statistical test comparing responders to non-responders or the target population. Of these eight, only three described implications of non-response consistent with the results presented (e.g. non-responders were younger, blindness prevalence was lower in younger people, therefore the study estimate may be an overestimate of the true prevalence[13]). The remaining five studies suggested an implication that was not supported by the comparison of responders and non-responders alongside the prevalence estimates of subgroups.

Discussion

Response rates in cross-sectional blindness prevalence surveys included in our review tended to be high, but authors commonly neglected to assess the potential for response bias, and rarely reflected on the implications of potential response bias on prevalence estimates. The only way to rule out response bias is to conduct a follow-up survey of non-responders and compare their results with responders. This is undertaken increasingly in high-income countries[16,17] but remains unfeasible in the LMIC context. In lieu of this approach, a number of strategies are recommended to assess or address *potential* for response bias (listed in methods),[4,10-12] and each of these strategies were reported by less than half of the included studies (Table 1).

The most commonly reported strategies to address potential response bias were adjustment of prevalence estimates to the age and/or sex composition of the target population (48%) and comparison of

age and sex composition between the sample and target populations (41%). Studies rarely reflected on the difference between the adjusted- and sample-prevalence or population compositions, nor on the possibility that data on the target population—usually extracted from the census—are outdated, or from a different geographic area than the survey (e.g. national rather than district). While this potential limitation was overlooked by many studies, there were good examples of these issues being discussed in a way that enhances interpretation of the results.[13,18] A further limitation of adjusting prevalence estimates to a target population is the implicit assumption that blindness prevalence in responders and non-responders is the same. We found no examples of this being tested, but this assumption is conceivably not valid.[6,7]

Studies that used RAAB methodology tended to report more response bias elements than non-RAABs (Table 1). The RAAB manual outlines many of the elements related to survey planning, implementation and analysis, including enumeration,[19] and the RAAB software automatically generates a number of relevant elements (i.e. target population table, age- and sex-adjusted prevalence). However, the manual does not include a section on interpreting and reporting results, and subsequently these elements (e.g. implications of perceived visual status of non-responders, implications of non-response on prevalence estimates) were reported less often in published manuscripts.

The reporting of both RAABs and non-RAABs would be strengthened if a reporting guideline was developed and used specifically for blindness prevalence surveys.[22] Ideally this guideline would draw on the resources available from the EQUATOR Network[23] to develop a blindness-specific STROBE extension. Once developed, the guideline must then be consistently used by authors, reviewers and editors to improve the completeness of reporting of blindness prevalence surveys.[22] More complete reporting would assist decision-makers when planning services based on survey results, enable more informative global estimates of blindness prevalence, and permit a more comprehensive assessment of risk of bias within studies.[20,21]

While the high response rates observed reduce the *risk* of response bias, the bias can exist even when the response rate is high if non-response is strongly correlated with the outcome (i.e. blindness).[11] Our results suggest this potential exists in blindness prevalence surveys. Even with a response rate close to 90%, where response rates differed between sociodemographic subgroups, a difference in blindness prevalence between these subgroups was found almost two-thirds of the time (65%; Table 2). This finding highlights the need for authors to more consistently reflect on the implications of non-response on their prevalence estimates and associated results such as subgroup analysis.

The high response rates reported in included studies, particularly among RAAB surveys, suggest the enumeration and recruitment strategies used in blindness surveys are effective. This result is encouraging, and reflection on the reasons for this success is warranted. Strategies to reduce response bias—more commonly reported in RAAB surveys—included door-to-door enumeration, undertaking surveys at the participants' home or nearby, offering to test at home those unable to travel, returning at

different times to reach absentees, having ‘low burden’ test procedures, and offering referral to services for people identified with an eye problem during the survey.[19]

Another possible contributor to the high response rates observed is if enumerators failed to include all absent individuals in the sample (including from empty houses), which would incorrectly inflate the response rate. This possibility is conceivable, as blindness surveys often take place in underserved areas, where enumerators may face challenges in following the study protocol when met with people in need of eye care.[7] The RAAB training manual has specific advice regarding how to minimise this likelihood (File S1), but elsewhere states non-response should be <10%. While this cut-off is meant to encourage the survey team to return to enumerate absentees, it could also serve as a disincentive to report all non-responders. Arguably, it would be better to recommend a target of <10%, but also emphasize where this is not possible, to accurately measure, report and discuss implications of non-response.

RAAB currently collects data on age and sex. The next version of RAAB will collect more social variables to enable assessment of inequality in outcomes between social subgroups including disability, place of residence and socioeconomic status. In some settings other variables may be relevant, such as language or migrant status. These additional data will also allow researchers to assess differential participation across these social dimensions, and any impact of response bias on prevalence estimates.

The response rates we found across low-, lower-middle and upper-middle countries were similar (range <6%), and all are higher than rates observed in surveys in high-income countries. For example, recent visual impairment surveys in Australia and the United States obtained response rates of 71.1%[24] and 79.2%[25] respectively. In our study, upper-middle income countries tended to have lower response rates than lower-middle and low-income countries which suggests that more robust enumeration strategies will be required in the future as economic development and urbanisation increase. In urban areas and higher income countries, potential participants may be more likely to work further away from home, eye services tend to be more accessible, and there is lower perceived value of free screening, reducing the incentive to participate in a survey.

Strengths of this study include the comprehensive search, and the robust data extraction and verification process. Limitations also exist. We included only studies published in English, so the extent to which response bias is considered in studies published in other languages remains unknown. Further, we wanted to assess whether response differed between surveys conducted in urban and rural areas but were unable to do so as definitions of urban and rural were not provided or inconsistent, or response rates were not disaggregated by place of residence.

Blindness surveys in LMICs tend to achieve high response rates which reduce—but do not eliminate—the risk of response bias. Currently, consideration of potential response bias and its implications in blindness surveys is weak. Improvements are required at all stages of the research process, including during survey

design, training enumerators, monitoring and minimising non-response during survey implementation, and assessing the potential for response bias on the prevalence estimates. There are limitations in comparing response and adjusting prevalence estimates to target populations. Therefore, at a minimum, reports of cross-sectional blindness surveys should quantify and describe the sociodemographic characteristics of non-responders, compare sociodemographic characteristics of responders and non-responders, and reflect on the implications of any differences on the study's prevalence estimates. This information is essential to enable accurate interpretation of survey results by decision-makers responsible for planning eye care.

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Table 1: Reporting of participation in 92 blindness prevalence surveys published 2009–2017

Study characteristic	RAAB†	Non-RAAB	Total	
	n/38 (column %)	n/54 (column %)	n/92	(column %)
Median response rate (inter-quartile range) (%)	95.3 (93.8–96.2)	87.0 (80.1–91.5)	91.7 (85.9–95.6)	
Enumeration and response				
Response rate reported	37 (97)	50 (93)	87	(95)
Enumeration description made it clear absentees were included in the response rate denominator	28 (74)§	17 (31)	45	(49)
Reported at least 1 strategy to increase response rate	26 (68)	23 (43)	49	(53)
Provided reasons for non-response that included refused and absent	26 (68)	9 (17)	35	(38)
Responders vs non-responders				
Non-responders disaggregated by at least one socio-demographic characteristic	12 (32)	24 (44)	36	(39)
At least one statistical comparison made between responders and non-responders	4 (11)	17 (31)	21	(23)
Comparison between sample and target population of at least one of age or sex	28 (74)	10 (19)	38	(41)
Implication of non-response on results				
Reported asking family/neighbours about perceived visual status of non-responders	15 (39)	3 (6)	18	(20)
Reported the perceived visual status of non-responders obtained from family or neighbours	1 (3)	1 (2)	2	(2)
Standardised prevalence estimates to age and/or sex composition of population	26 (68)	18 (33)	44	(48)
Implications of non-response discussed in relation to reported prevalence estimates	3 (8)	10 (19)	13	(14)

†includes 4 Rapid Assessments of Cataract Surgical Services
§10 of these did not mention absentees, but stated that they followed Rapid Assessment of Avoidable Blindness (RAAB) protocol

Table 2: Summary of comparisons made between responders and non-responders in 92 blindness prevalence surveys published 2009–2017

Social group	Studies that tested a comparison	Difference in response between subgroups			Difference in blindness between subgroups*			
	n/92 (%)	No difference n (row %)	Difference n (row %)	Subgroup with higher non-response (n)	Did not test n (row %)	No difference n (row %)	Difference n (row %)	Subgroup with higher blindness
Sex	18 (20)	3/18 (17)	15/18 (83)	Men 14; Women 1	4/15 (27)	6/15 (40)	5/15 (33)	Women 3; Men 2
Age	17 (18)	8/17(47)	9/17 (53)	Older 3; Younger 3; Mixed** 3	1/9 (11)	—	8/9 (89)	Older 8
Education	5 (5)	2/5 (40)	3/5 (60)	Lower education 3	—	1/3 (33)	2/3 (67)	Lower education 2
Place of residence	3 (3)	2/3 (67)	1/3 (33)	Urban 1	—	1 (100)	—	
Total	43 comparisons in 21 studies		28 differences in 18 studies	—	5	8	15 differences in 10 studies	—

**The subset of studies finding a difference in response between subgroups*

***Older and younger age groups had higher non-response than age groups in the middle*