Determinants of acclimatisation in high altitude

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Dear Editor,

I read, with great interest, the paper 'Determinants of acclimatisation in high altitude' by Syed VS, et al, *MJAFI* 2010;66:261–265. The authors deserve to be congratulated for bringing to focus an important lacuna in the recommendations regarding acclimatisation to high altitude. This lacuna is, of course, the absence of guidelines to help identify those who are not acclimatised. The DG Memorandum 140 of 1997, published many years ago, says little in this matter. But now, after so many years and so much more experience, it is time that definite guidelines are available. The authors have made an important move in this direction.

However, the paper raises more questions than it answers. Even though the 'ends' are commendable the 'means' lack credibility. I would like to highlight a few important deficiencies in the study design and the way data has been handled.

STUDY DESIGN

1. The study is apparently trying to test the validity of the yardstick that was used for assessing acclimatisation status at the end of the 6-day period of stage I acclimatisation. A good test of validity would have been a comparison of the numbers of high altitude illnesses reported in those found 'fit' vs in those found 'unfit' using the selected yardstick. Could the authors tell us whether all subjects who were declared fit remained asymptomatic at that altitude? And how many of each cohort did develop any of the high altitude maladies during the period of the study or follow-up? These data would have indicated whether the cut-off values of pulse rate, blood pressure (BP) and SpO₂ used by the authors were effective in discriminating between those who had acclimatised well from those who had not.

METHODOLOGY

1. Why did the authors choose the particular values of their yardstick?
2. The authors used the cut-off level of BP as 140/90 mmHg. It is not clear where persons with BP of, say, 136/94 mmHg

or 144/88 mmHg were placed—in the 'fit' or 'unfit' category? Also mean BP is always conventionally reported as a single number and is calculated as the sum of diastolic pressure and one-third of pulse pressure. Statements such as 'mean BP of 145/100 mmHg' are therefore inaccurate. If the authors believed that values of systolic BP (SBP) and diastolic BP (DBP) are important by themselves then they should have been reported and analysed separately.

3. The paper makes no mention of the type of equipment used to measure BP and other variables or whether measurements were done by one or more observers. These are aspects relevant to the validity of data collection.

RESULTS AND DATA

1. It would have been very interesting to know the mean heart rate, mean SBP, mean DBP and mean SpO₂ in all 278 first entry subjects and all 244 re-entry subjects on the first and also on the 6th day. This data would have helped readers to get an idea of how different was the group who were found unfit on the 6th day.
2. Table 6 presents the statistical comparison apparently between subjects found unfit in the re-entry group on the first day of acclimatisation vs subjects found unfit on the last day of acclimatisation. Since, there were only two subjects in the latter group the statistical analysis is not likely to be meaningful and hence I consider this comparison invalid.
3. At no point in the paper are standard deviations of calculated averages mentioned. Very important information is thus hidden from the reader.

Thus, I feel that even though the yardstick used may actually have been valid yet the methodology and data analysis do not help the reader to arrive at any conclusion about its usefulness.

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(The authors have not replied to the above Letter to the Editor despite numerous reminders therefore the letters to editor are published without author reply.)

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