I read with interest the above mentioned article, the conclusions of which are of great significance to the tea industry.

The researchers [1] have stated in their article that the study was carried out on Norwood Estate located at 2,500 m above sea level. In fact, the highest elevation of Norwood estate is 1,575 m. The researchers have used an altitude adjusted cut-off value of 13.3 g/dL to determine the prevalence of anaemia, which is based on the elevation of 2,500 m above sea level. Using this cut-off value the prevalence of anaemia in non-pregnant estate women in the sample was calculated to be 94.4%. This is high compared to the prevalence found by other studies. The most recent national survey, carried out by the Medical Research Institute [2] shows the prevalence of anaemia as 37.2% in non-pregnant women in the estate sector.

According to the WHO guide to programme managers [3] haemoglobin adjustment for an elevation of 1,500 m is 0.5 g/dL. As such the factor for adjusting Hb values at the elevation of 1,575 m is 0.5 g/dL and the cut-off point for anaemia should be 12.5 g/dL, and not 13.3 g/dL. In their study [1] the blood samples were collected on filter paper and analysed in the University laboratory within a week. The method used should have been validated in the laboratory and the results included under methods. I feel that the authors should present the results using the correct cut-off point.

The study in question has been confined to only one estate from over 450 estates and cannot be generalised. In addition, the daily average tea leaves plucked per plucker depends on many factors such as the condition of the soil of the field, condition of the bushes, number of bushes per hectare, type of the tea plants (i.e. vegetative propagation or seedling), age of the tea bushes, weather, etc. As these agro-climatic conditions vary between fields, divisions, estates and districts, and contribute immensely to the productivity, the article should have indicated the measures taken as control for these variables.

References
I refer to the article published in the CMJ of March 2004 [1]. This article cites a negative reference for the use of intravenous (IV) magnesium sulphate in asthma. However, there are several other papers in the literature which support the use of this drug in acute severe asthma.

In a study of 248 patients in the 18 to 60 year age group with acute severe asthma, administration of 2 g of IV magnesium sulphate improved pulmonary function when used as an adjunct to standard therapy [2]. In a systematic review of IV magnesium sulphate, the conclusion reached was that it appeared to be safe and beneficial for patients with acute severe asthma and that practice guidelines needed to be changed to reflect this [3]. The drug has been shown to be of benefit even in children [4, 5]. The latest Cochrane Review concludes that current evidence does not support routine use of intravenous magnesium sulphate in all patients with acute asthma presenting to the emergency department but adds that it appears to be safe and beneficial in patients who present with acute severe asthma [6]. Clinicians in this country have used intravenous magnesium sulphate in very acute severe asthma and have found it to be of benefit to the patients.

I feel that the report quoted in the Ceylon Medical Journal from the Drug and Therapeutic Bulletin 2003; 41: 79–80, taken in isolation, may deprive some needy patients with very acute severe asthma, the benefit of intravenous magnesium sulphate.

To the Editor:

Intravenous magnesium sulphate in asthma : current recommendations

References

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