Fig. 2 – (A) A small tick holding on to the fourth web space of left hand. (B) Legs of the tick seen tucked underneath.

patient did not report for follow-up. The tick specimen was identified as *Hyalomma aegyptium*. *Hyalomma* is a common genus of hard tick encountered in India. However, *Hyalomma anatolicum* is the commoner species seen.²

Many patients are unaware of ticks biting them. They either notice it as a “boil” or a new mole³ which is asymptomatic or associated with pain. The ticks are missed unless they are big enough. The key point is to observe the tiny legs tucked away under the “object”. History of contact with animals or brushing through foliage is important. Ixodid ticks attach firmly to the skin up to a week or more and get engorged. They are not dislodged easily. Agasid ticks feed briefly and are not usually encountered.

Ticks have to be removed early, since the risk of transmission of disease increases beyond 24 hours of attachment. The common method is using an angulated forceps with steady traction, with care to keep mouthparts intact.⁴ After removal, the patient needs to be observed for at least 30 days for signs and symptoms of tick borne disease. Prophylaxis with a single oral dose of Doxycycline 200 mg following a tick bite in a hyperendemic region proved effective but controversies exist.⁵

The aim of this letter is to sensitise dermatology residents and other clinicians about the possibility of missing a small retained tick unless one looks for the legs carefully, which are invariably tucked underneath.

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Letter to the Editor

Dear Editor,

As a researcher in the field of high altitude with published body of work and currently posted to High altitude area, I read with considerable interest, the original article titled “Incidence of

high altitude pulmonary edema in low-landers during re-exposure to high altitude after a sojourn in the plains”, by Apte et al. published in *Med J Armed Forces India* 2015;71:214–220.¹

Planning and conduct of a prospective cohort study is fraught with various challenges due to inherent limitations of our data management techniques. Moreover, data collection in

any longitudinal study involves rigorous quality assurance at all levels and follow-up.² It is commendable that in spite of these challenges, the authors have been able to conduct this longitudinal study.

However, I would like to mention few issues with the design and conduct of this particular study, as well as the fallacious interpretation drawn by the authors.

This study has estimated the incidence in the entire sample over a period of five years in which individuals were followed up for a variable period of time, as the tenure in this sector is 24 months. Thus the study does not qualify as a classical cohort study. In fact, it may be classified as an 'open cohort' with participants entering the cohort at different time periods; hence, there are design issues and the incidence per person years contributed needs to be given. The analysis of this type of design with variable times of entry and follow-up should have been done by using the survival analysis technique rather than the simple analysis using proportions.

Moreover, the main finding of the study that "there was no statistically significant difference between the incidence of HAPE in the air entry First Entry (FE) cohort and Repeat Entry (RE) cohort" is totally fallacious as the study was not powered enough to estimate this difference. Calculations for cohort study, to estimate the difference in incidence found in this study with power of 80% at 95% alpha, have been shown in the table below.³ Thus, it is evident that the main finding of the study is erroneous and conveys something that may or may not be the reality. This fallacy has crept in the study because it was not designed as a cohort study with appropriate sample size and power calculations.

Cohorts	Ratio FE:RE :: 1:2.81 (as in the study)		Ratio FE:RE :: 1:1 (if planned)	
	Kelsey	Fleiss	Kelsey	Fleiss
First entry (FE)	10,238	9351	13,047	13,045
Repeat entry (RE)	28,820	26,322	13,047	13,045
Total	39,058	35,673	26,094	26,090

Thus, the important issues are: inability to understand the actual design of their study; fallacies in planning, design, and conduct; and inappropriate statistical analysis. The study itself enumerates a large number of limitations suggesting compromise on the validity and reliability of results. The observations mentioned in preceding paragraphs coupled with the limitations mentioned by the authors need to be accounted for before any conclusions are drawn by the readers of this study.

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Reply

Dear Editor,

We are pleased to receive such an early response to our paper comparing the incidence of high altitude pulmonary edema (HAPE) after the first exposure and after re-exposure to high altitude.¹ The letter gives us an opportunity to delve deeper and educate ourselves about finer points (for us) in epidemiology. It also commends our efforts in conducting this longitudinal study, and we express our thanks for this. At the same time the letter raises relevant issues regarding design, analysis, and interpretation and suggests that readers be cautious.

The first issue is about study design. We are in total agreement that conducting a prospective cohort study is beset with several challenges. We do not claim in the article that our study was a classical cohort study. In fact, we have used the word 'cohort' in its general sense of a 'group' sharing certain

characteristics. In analytic epidemiology, a 'cohort study' classically consists of one group that was 'exposed' and another group that was 'unexposed'. More importantly, risk of disease-occurrence increases with time. For instance, incidence of cancer in a cohort will be directly proportional to the duration of follow-up. This is not true for HAPE. Irrespective of the entry-type (first or re-exposure), the risk of developing HAPE is limited to the short period immediately after exposure to high altitude. Beyond that the risk is non-existent, if the person remains at the same altitude. For these reasons, the concepts of 'incidence rate' and 'incidence per person-time' do not apply to our situation, and comparing the 'incidence proportions' is sufficient to answer our research question.²

The second issue is about the technique of data analysis. The letter states that 'survival analysis' should have been used. We are not in agreement with this viewpoint. The underlying idea