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

being examined during survival analysis is the time-to-event.³ For the purposes of our study, it was not important when HAPE would develop. We were not looking at the time-to-event but at only the number of events. Hence survival analysis would not have been appropriate in our situation.

The third issue raised is about the lack of statistical power in the study due to insufficient number of participants. The Table in the letter does suggest that the power in the study was inadequate. We too would have agreed to this view but are prevented from doing so by the likely fundamental fallacy in a post hoc calculation of power,^{4–6} yet we do realise that had the numbers of participants been larger, the strength of our conclusions would have been greater.

Nevertheless, we would like readers to focus attention on a more fundamental aspect. Our study essentially wished to examine, whether re-exposure to high altitude after a short-term absence is really associated with a greater likelihood of suffering HAPE. This belief is based on a cross-sectional viewpoint that sees, in hospital wards and other collections of HAPE patients, a larger number of those re-exposed to high altitude. But this could also simply be due to the fact that population-at-risk of re-exposed is always greater than the population-at-risk of first exposures to high altitude. Readers, who may want to believe that our epidemiological evidence is flawed or weak may also like to remember that there is no epidemiological evidence in favour of the belief that re-exposure carries a higher risk for development of HAPE.

From all the above it follows that more evidence needs to be gathered before this academic point can be settled. A much larger epidemiological study is mandated and only then can one come nearer to the truth. We do hope the challenge will be accepted by those presently involved in this field. We once again wish to thank the author of the letter for its stimulating contents and for adding to the academic debate.

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Letter to the Editor

Cutaneous pili migrans: A creeping eruption like condition easy to diagnose and cure

Dear Editor,

Cutaneous pili migrans (CPM) is a hair shaft or hair fragment embedded in superficial skin and sometimes in dermis also. It appears like a black colored, thread-like eruption, closely mimicking cutaneous larva migrans.¹ It is a rare condition and since 1957 only 26 cases have been reported.^{1,2}

Most of the cases of CPM are reported from Asia; however, it has been rarely reported from India; one of the authors has

reported a pediatric case of CPM. To the best of our knowledge, these are the first reported cases of CPM in the adult population, in India.

Case 1

A 30-year-old female patient, fashion designer by occupation, presented with asymptomatic black colored, thread-like