Predictive Factors for Conversion of Laparoscopic Cholecystectomy

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Dear Sir,

I read with interest the article by Drs. Atmaram DC and Lakshman K on predictive factors for conversion of laparoscopic cholecystectomy [1].

I have certain doubts and observations regarding this study.

1. To form group 2, authors have selected 14 random patients from the nonconversion group. Why? They could have taken all the 512 patients and incidence could have been compared by applying statistical tests of significance. More the number better will be the reliability. There is no rule that two groups to be compared should have the same number.
2. Age and sex ratio of the groups are different and authors have not mentioned whether it is statistically significant (age 62.43 vs 47.8; male–female ratio 3:4 vs 1:1). When groups differ, is it really prudent to compare?
3. Authors have mentioned 4 cases of acute cholecystectomy in group 1 under results but not mentioned how many were there in group 2. We all know that in acute cholecystitis, chance of Calot's triangle inflammation obscuring the anatomy is more increasing the risk of conversion.
4. Under discussion authors have mentioned a number of factors affecting the conversion rate, but considered

only ultrasound findings and LFT. Why other factors are not mentioned in the article though they are simple and easily documentable such as duration of symptoms, jaundice, and body mass index. If these data are not available and documented because they are retrospective, are they worth studying?

5. In spite of all these exercises, authors could not come to any conclusions and conclude by stating “it is difficult to preoperatively predict factors responsible for conversion.” They should have stated “difficult to predict based on LFT and ultrasound findings” because these are the only factors studied.

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