



SDI Review Form 1.6

Journal Name:	International Journal of Biochemistry Research & Review
Manuscript Number:	2013_IJBcRR_7264
Title of the Manuscript:	Can Total Antioxidant Capacity Be A Surrogate Marker In Chronic Obstructive Pulmonary Disease ?
Type of the Article	Original Research Article

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

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PART 1: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
<u>Compulsory</u> REVISION comments		
<u>Minor</u> REVISION comments		
<u>Optional/General</u> comments	This study investigates Total Antioxidant Capacity (TAC) in COPD versus controls in serum. They report, the well known reduction of TAC in COPD. In addition they investigate various antioxidant agents such as bilirubin, albumin, urate, CP and found no correlation with TAC. It is obvious that the interpretation of their results were wrong! They had to use the multi regration analysis to verify the contribution of any of each individual antioxidant to TAC. In addition the study has a number of other methodological problems. The authors did not discuss: 1. Ex smokers 2. If the controls and COPD had similar smoking intensity 3. If they had controlled for equivalent air pollution condition in both groups ect, ect The paper is badly written with a lot of tables and unnecessary Figures. Parts of the discussion should be methods (eg the whole page 9) or results (lines 172-188). Figures 3-4 are methods and should be OMITED as well as Figure 1 and Figure 2 should only be discuss in the text, although according to previous major comment are using the wrong methodology (multi-regration is needed).	I agree with the reviewer regarding methodological problems. Statistical section is now completely revised, which has been meticulously done with help of another scientist in biostatistics who did MSc statistics with specialisation in operation research from IIT, Powai, Mumbai. ANOVA plus , post -hoc BONFERRONI has been applied. As there was no appreciable correlation of bilirubin, albumin, urate, and CP with TAC multi regression analysis was not done Dividing the smokers group of 43 subjects as ex-smokers and current smokers further makes the group small and thus compromising the statistical relevance. I fully agree with the reviewer . Article is rewritten. Relevant tables have been incorporated and some figures have been deleted. As suggested, referred portion from



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	Table 3 is without p values?? What was the cause for COPD in no smokers? Biomass fluid ??	discussion has been shifted to method section. English wherever required has been corrected and polished as suggested. P values earlier also were given but it was given in a common multi comparison table (Table 4) However new tables have been incorporated now. Refer article of my friend: The Lancet, Volume 374, Issue 9691, Pages 733 - 743, 29 August 2009. Chronic obstructive pulmonary disease in non-smokers. Finally, I sincerely thank the reviewer for meticulously going through the manuscript and giving extremely valuable suggestions
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