



SDI Review Form 1.6

Journal Name:	British Journal of Applied Science & Technology
Manuscript Number:	Ms_BJAST_31829
Title of the Manuscript:	Evaluation of Stem Biomass and Carbon density of four deciduous tree species in Northern plains of India using Regression Modeling
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.

To know the complete guideline for Peer Review process, reviewers are requested to visit this link:

(<http://www.sciencedomain.org/page.php?id=sdi-general-editorial-policy#Peer-Review-Guideline>)



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PART 1: Review Comments

	Reviewer's comment	Author's comment <i>(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)</i>
<u>Compulsory</u> REVISION comments	The research is highly relevant as the authors' state it. The results have potential for use in forest carbon projects in general. However, the research needs deep revision to comply with the scientific requirements and use of methods. The most issues are: 1. Need to review and align the concepts, especially those of "carbon sequestration", "carbon stock", and "carbon density", which are used indiscriminately as if they were the same thing. In fact, the article promises to study "carbon sequestration" (see the last paragraph of the Introduction section, page 2) but there is no attempt to do this at least. At the end, the conclusion is provided for "carbon stock". 2. A very serious confusion is given when the authors set up the methods and say "the volume of each species was calculated using equations 1 and 2" (see page 2). At the same time, the very same equations were fitted using regression techniques. This is at least strange. One or other thing should be done, but not both. I mean, if regression was to be fitted, then, other form of volume estimation/calculation should be used. 3. The abstract promises to analyse assumptions such as the use of the "50% of the dry weight to carbon conversion factor". However, the study does not even attempt to estimate this value or at least analyse it. 4. Following on previous numbers, all research results are deemed inconsistent, since all the rest of analysis derives from these calculations. 5. Additional issues that need consideration are editorial: a. Abbreviations must be defined the first time they are used (e.g. DBH), and these must be maintained and not confounded throughout the article (e.g. AGB	



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	instead of AGB – see page 4) b. Tables must be referred in sequence (e.g. Table 2 is mentioned before Table 1) c. The first paragraph of “Results and discussion” (page 4) does not belong to this section. It should either go to introduction or literature review section. d. Be consistent with the terms. The introduction and methods use the term “AGB”, but the results never mention this, and just say “biomass” instead. e. Need to follow the editorial rules, such as to italicize the Latin names of the plant species; the first time you mention the species need to indicate the Authority [e.g. <i>Vachellia nilotica</i> (L.) P.J.H.Hurter & Mabb.]	
<u>Minor</u> REVISION comments	Consider using the current name for <i>Acacia nilotica</i> (new name is <i>Vachellia nilotica</i>) Please check typing mistakes (e.g. repetition of “more accurate” in Conclusions – page 6) Table 2: consider including the age of the plantation, the DBH range and the Height range Table 3: consider using AGB instead of biomass Table 3: It is not clear what carbon density is, it is not clear what carbon sequestration is as well. The provided figures are not consistent with the equations provided as the basis for these calculations.	
<u>Optional/General</u> comments		

Reviewer Details:

Name:	<i>Almeida Siteo</i>
Department, University & Country	<i>Department of Forestry, Eduardo Mondlane University, Maputo, Mozambique</i>