



SDI Review Form 1.6

Journal Name:	<u>International Journal of Biochemistry Research & Review</u>
Manuscript Number:	Ms_IJBCRR_24489
Title of the Manuscript:	Disruption of disulfide bonds as a cause of insulin resistance in D.M 2
Type of the 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 (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	The manuscript is poorly written and unclear. It compiles several information about the main subject but it does not show a clear connection among them. -The manuscript depicts bold statements about insulin resistance in the abstract section that are not explored or properly referenced in the body of the article ("The exogenously administered insulin restores the normal carbohydrate metabolism, by reversing the conditions that resulted in switch over to Beta oxidation, thus restoring insulin sensitivity in treated diabetics") and it does not show the connection with the title. -The introduction is too long and it does not state the aim or importance of the article (e. g. It mentions about the effectiveness of exogenous insulin in contrast to the endogenous insulin – no reference is mentioned and no connection with disulfide bond disruption is made). -The conclusion is lacking. - Moreover it presents major grammatical, spelling (heteromear-line 55, glycolysis-line 65 etc) and punctuation mistakes which often lead to difficulties in understanding. The term theory does not apply to the subject of the article, it would be better to use the term	Unsubstantiated comments are ignored. Personal feelings are respected. Need not necessarily be assuaged. Typing errors are exaggerated and blown out of proportion as to uncharitably call the script poor. Yet the opinion is respected but not accepted. All such typing errors including the bunch pointed by the reviewers are taken care of. It is unfortunate that the contents are not clear and the connection could not be seen by the reviewer. Viewed as personal problem and respected. However if the reviewer is genuine in his comments I welcome him to my email address and I am confident of dispelling the misunderstanding about subject matter if any. It is my offer in order to avoid any acrimonious debate on a forum which is not appropriate for the same(such as this). Hope the suggestion is acceptable. Regarding bold statements...of which a mention is made, "statements are always bold as long as they are on firm footing". The contemptuous use of the word is applicable to both sides if one's contentions raised are proved to be hollow and shallow. Permit me to give an abridged reply to the doubt expressed about exogenously administered insulin....It is an universally accepted fact that exogenously administered insulin is effective in DM2 patients ,which is also practically experienced



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	hypothesis. -It lacks consistency in the expression of scientific terms (e. g. D. M2/ DM2/ DM 2; sub unit/ subunit etc) and in the reference style (see authors statements, spelling mistakes in the article titles etc)	in day to day practice, in spite of insulin resistance{ which is only to the endogenously produced insulin). Just to take care of a ny doubt that might arise to the reader as to why the exogenously administered insulin does not meet the same fate as the endogenous insulin, under the same prevailing conditions of beta oxidation of fats as source of energy, still persisting. But in view of clinical effectiveness of exogenous insulin, it is logical that the energy metabolism shifts back to one based on carbohydrate as otherwise its effectiveness can not be explained. The mechanism of such a shift is still an enigma, and any speculation about the same is avoided. This possible shift is also corroborated by the improvement of glycaemic status on institution of insulin therapy. This lends support to the hypothesis that disruption of the disulfide bond formation (which makes the difference between both energy metabolisms referred to)is what is causing insulin resistance in DM2. However, the reference to exogenous insulin is sacrificed by deleting from the conclusion part of the abstract, not as a step of retreat, or in acceptance of the accusation of having made bold statements, but for the simple reason that if the same confuses the reviewer, what would be the fate of an average reader. Since the bone of contention is removed,, the reviewer is assumed/expected to be satisfied. The comment that the introduction is too long is rectified by introducing the missing <u>sub heading-ie, " brief recapitulation of relevant literature"</u> immediately after the short introduction part.
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