Reviewer's report

Title: Use of Biological Based Therapy in Patients with Cardiovascular Diseases in University-hospital Setting in New York City

Version: 2 Date: 22 July 2004

Reviewer: Glen Pearson

Reviewer's report:

General

The authors have attempted to revise the manuscript based upon the two reviews that they received. Unfortunately, some of the revisions made were only superficial, resulting in only a marginal improvement in the overall manuscript. Grammar continues to be a problem in this manuscript.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. As previously stated in the general comments in my original review (but not yet addressed by the authors), the amount of data in this manuscript is excessive and contributes to the overall lack of focus in the manuscript. The authors must reconsider the volume of data presented in the results section (~6 pages of verbal review of results and 6 tables, comprising 8 pages in the manuscript) and present these results in a more organized, focused and concise manner.

2. As previously stated in the general comments in my original review (but not yet appropriately addressed by the authors), the authors ambitiously and naively identify that their study had 5 primary objectives! However, their sample size calculation was based on only one outcome, specifically difference between users and non-users of BBT in terms factors which determine (the more appropriate term to use would have been "contribute to") the use of BBT (such as education level). Consequently, most of the data analysed and presented in the manuscript addressed what should be referred to as secondary objectives. There should be a primary objective and then there can be as many secondary objectives as they would like.

3. Discussion section needs to be reworked and focused a little better. The review of previous research in this area which was previously included in the introduction has simply been moved to the beginning part of the discussion section -- 5 paragraphs over one and one-half pages and is not related to their study results until later in the discussion.

4. Page 16/Paragraphs 1 and 2: While the issues of (a) herbal drug product or BBT consistency and contamination and (b) documented BBT-prescription drug interactions are intriguing and somewhat relevant, I do not think that these 2 paragraphs reviewing those 2 particular papers are necessary. They should be deleted.

5. Page 16/Paragraph 3: The authors comment that "....certain studies looked at only one CV disease while the current study includes all CV risk factors and CV diseases." This is an overzealous attempt to demonstrate the uniqueness of their particular study --- however, I would suggest that a total of 9 patients (4.5%) surveyed with only CV risk(s) makes this claim invalid. In addition, the authors have still not clarified what they define to be a CV risk in this survey -- it obviously excludes DM, dyslipidemia, HTN, renal disease, etc. since these are listed separately in Table 2. Do they
mean patients with non-traditional risk factors such as elevated CRP, elevated Lp(a), elevated Apo-B, LVH, etc...? This must be clarified.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. Page 12/Paragraph 3: The authors comment that "only 33% of users reported that they were asked about BBT use during history/physical examination by....a physician, nurse or PA." Given that this is a pharmacist conducted study, they should include some statement about the percentage of BBT users who reported that they were asked about their BBT use during medication history assessment by pharmacists!

2. Page 19/Paragraph 1/last 2 sentences: These two sentences were added to this version of the manuscript and are representative of the poor grammar in this paper. These need to be corrected.

3. Page 20/Paragraph 3/Knowledge Scores of Users of BBT: This data seems to offer very limited insight and not all that valuable without some context in which to evaluate it? For example, these findings would interesting and useful if the authors had evaluated the knowledge of prescription drugs for these patients. Does this population know as little about the potential side-effects and potential interacting foods for the prescription products that they take? It is highly likely that this is the case. Given that this data misleadingly inferences that there is some degree of greater risk to patients who take BBT products because of a lack of knowledge about their potential side-effects or food interactions, compared to conventional prescription products, needs to be clarified in the discussion.

This is the exact comment that made in the previous version of the manuscript. In their response to this comment from my previous review, the authors indicated that they added comments to the limitation section of the manuscript. I do not believe that this has been adequately addressed. The limitation of this data about BBT knowledge scores is that there is no context in which to evaluate it? My point about prescription knowledge is that I believe that the authors would have found a similar lack of knowledge in these patients about their prescribed medications despite the numerous encounters with health professionals (physicians, pharmacists, etc.) in obtaining them. Consequently, I belive that this information is useless and inappropriately infers that because these patients have poor knowledge about the BBT's that they use, they are at an increased risk. This particular issues requires clarification -- it can either be deleted or it must be justified.

4. Table 1: Footnote 1 still refers to alternative pharmacotherapy -- change to consistent terminolgy used in the manuscript.

5. Tables 2/3: Excessively detailed. Footnotes contain an excessive amount of addtion detail as well! These should be reworked to be more concise.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

None.