



SDI FINAL EVALUATION FORM 1.1

PART 1:

Journal Name:	British Journal of Medicine and Medical Research
Manuscript Number:	2013_BJMMR_4571
Title of the Manuscript:	EFFECT OF THE SAMPURNA HRIDAY SHUDDHIKARAN (SHS) MODEL IN HEART FAILURE PATIENTS IN INDIA: A PROSPECTIVE STUDY

PART 2:

FINAL EVALUATOR’S comments on revised paper (if any)	Authors’ response to final evaluator’s comments
The overall quality of the paper has been significantly improved. Unsubstantiated claims have been removed, overall improving the scientific impact of the paper. Abstract: overall improved in clarity but would still merit from editorial attention. The results and conclusion sections are missing verbs and have undefined acronyms. Suggest rounding 6MWT to nearest meter or at most 1 decimal place as 2 decimal place accuracy suggest centimetre accuracy in the test measurement. Similar considerations could be made for EF. Background – the socioeconomic health introduction nicely places the importance of Ayurvedic practices in context. Methods: I am afraid I remain unclear as to the population included and the methodology. Were these patients really inpatients? If so, what was the reason for admission? If only NYHA class 2, why were they in hospital? Do the authors maybe mean that these were stable symptomatic HF patients admitted from an outpatient setting, but hospitalized for the 6 day duration of SHS? In this case, what was the outpatient setting within which they were identified? Were these patients on any pharmacotherapy for their HF prior to the commencement of SHS? (i.e. beta blockers, ACEi etc.?) If so, what was the therapy? I understand from the authors reply that “no other intervention was performed”, but were the patients continued on any prior pharmacotherapy? Please clarify. Hrid Dhara – there was no previous mention of dough in this therapy. Please explain. The timing of the assessment is now easier to understand. The conclusion is much more succinct and appropriate. I would be hesitant to proclaim that “the current study confirms the beneficial action of SHS in heart failure patients” but it is certainly intriguing, hypothesis generating and confirms the need for further study of this potentially life improving therapy. As suggested by the authors, the lack of a control group is a major limitation of this study. A future well designed randomized trial would be most enlightening. At this stage, I would suggest toning the conclusion statement down a little more to: “Herbal treatment procedure SHS is associated with improvements in ejection fraction.... etc.”, particularly given the lack of a control group. Furthermore, the study does not illustrate the link between short-term structural changes in the LV and long-term HF treatment efficiency. Structural changes may be deemed conventient surrogate markers that may (or may not) reflect a therapy’s long-term efficiency. The manuscript would be acceptable with minor changes.	We totally agreed with the reviewer’s comment following changes are done. Line 24 & 27 with missing verbs are corrected. Acronyms are defined (Line 26 & 37). Rounding of parameters also done. All patients were inpatients. And conventional sampling method was used where out of all patients admitted in the hospital within one month period & willing to participate in the study were included. SHS procedure was done twice in a day for 6 consecutive days for its optimum benefit to the heart failure patients. Heart failure patients of NYHA class II & Class III were under close medical supervision round the clock. This assures doctors & patients for treating any emergency if at all occurs during treatment period. Yes, these were stable symptomatic HF patients admitted from an outpatient setting of the same hospital but hospitalized for the 6 day duration of SHS. Patients were not taking any pharmacological therapy during treatment. Dough is used to restrict warm water on precordial area. In the conclusion Line 31, 32 & 172 are corrected. Line 165-167 & Line 173-175 are deleted.