Reviewer’s report

**Title:** A retrospective cohort study of Laparoscopic Subtotal Colectomy with Antiperistaltic Cecorectal Anastomosis in Treating Adult Slow Transit Constipation: 81 cases

**Version:** 2  
**Date:** 7 January 2014

**Reviewer:** Renato Costi

**Reviewer’s report:**

**Major compulsory revisions**

The paper is definitely about outcome of surgery, and surgery is not described in detail.

Rationale and ethics of the paper are debatable: as, according to authors, in the literature a 5-7cm-long cecal reservoir has better outcome of 10-15cm-long cecal reservoir, they decide to perform a 2-3-cm-long cecal reservoir. This latter means just to let the ileocecal valve. In the literature, although the rationale of cecorectal anastomosis is to avoid the postoperative diarrhoea typical of ileorectal anastomosis, still, diarrhoea is definitely a severe and intractable complication. Has any preclinical trial been performed? Has any consent been signed by patients before undergoing such an innovative procedure?

The paper is not well written and should be extensively re-edited. English needs revision.

**Minor essential revisions**

**Title**

Actually the main purpose of the study is not explained by the title which should be revised: “A retrospective comparison of short term results and functional recovery after subtotal colectomy and cecorectal anastomosis with short colonic reservoir vs. long colonic reservoir” should sound more appropriated.

**Abstract**

There are several typing errors.

In the Results section the sentence “at each time point, the outcomes...” should be replaced with real results (numbers).

**Introduction**

The first sentence needs references.

The reported background of present surgical management of constipation is poor.

Total colectomy followed by ileorectal anastomosis is the most performed operation worldwide for constipation, thus it may be deduced that has some success, whereas it is described by the authors only with its main drawbacks.
Subtotal colectomy followed by cecorectal anastomosis is described only with its theoretical advantages, whereas some complications have been described. Moreover, several other technical options have been described, as side-to-end cecorectal anastomosis, or isoperistaltic anastomosis by cecum translational rotation in the left iliac fossa, thus avoiding any vascular torsion. Finally, the laparoscopic approach is not reported.

Methods

Methods section is poor.

The sentences relative to patients lost at follow up should follow patients’ selection description.

Patient selection section is poorly described: what is “sure” diagnosis (is there any unsure diagnosis?)? What is “repeated” GTT? Why repeated? How many times? Which are symptoms of outlet obstruction? What is conservative treatment? Inclusion and exclusion criteria are mixed in a confused way. Is there any minimum age for surgery? Preoperative work up should be clearly reported before a clear list of inclusion and exclusion criteria.

The “Surgical procedure” is poorly described. Considering that the authors are comparing the outcomes of two surgical procedures, five lines is not enough, especially if we consider that a laparoscopic subtotal colectomy is definitely a technically demanding procedure. Trocar position deserves to be described as well as the main operating phases of the procedure. The figures 1 to 4 propose drawings of poor quality. Some of operative data are reported intable 1, but there is not any link in the text. Hospital stay is 13 days after both procedures, which is a lot, and deserve a commentary from the authors. Postoperative complications are poorly reported.

In the Patient data acquisition section, the sentence beginning with “We designated..” needs references unless it is an arbitrary action of the authors. In any case it is unclear what is the APFS/ABS score: is it the sum? If it is not, what happens if a patient have APFS=0 and ABS=3?

Reference no. 25 refers to a paper from Todd and coworkers.

In the Comparative Method paragraph, the “improved value” is the preop value less the observed value. This way to mesure improvement needs references. If it is not validated by other papers, it should be stated and discussed, since it is debatable that a fraction preop value/observed postop value is not more appropriated.

Stats need review by a statistician. In tables some other stats are reported than those listed in the dedicated text section.

Results

Surgical outcome description is poor.

It is unclear what “transferred to open operation” means. Conversion to laparotomy? Revision? Second procedure? Re-do?

Morbidity description is poor. The meaning of “incomplete ileus” and “pulmonary
"infection" is unclear and should be better explained in a scientific journal.
In tables 2 and 3 the absence of the preoperative values make the whole tables difficult to read.

The Wexner Incontinence Score (WIS) at 6 months is virtually higher for control group than observation group (p=0.05). This datum deserve some discussion since the control group have much longer cecal stump (10-15 cm. in length vs. 2-3), thus bigger reservoir capacity and theoretically higher water absorption activity.

Four decimals after the comma are too many (one is sufficient).

Discussion is not focused.
The authors compare their laparoscopic cases vs. those operated on with open access by the Italian group, and it is not clear why. Moreover, they maintain to have a lower morbidity, which is not, as, in small series, 11% may be considered consistent with 13% reported by Marchesi and co.
The authors refer to six patients in control group with poor results of surgery, but they do not present them in the results and refer to those results not in details (when they did have pain? It seems perioperatively, since they apparently had long treatment in hospital, etc.). If it is, it is unclear why the hospital stay of the two groups is the same (13 days) if the results in control group were poorer.
The period (7 lines) beginning with “We observed considerable excrements..” is not scientifically acceptable, not proved by results, without any reference, and is therefore a personal opinion.

How the authors may observe anything in the research of Jiang?....

**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:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**
I have not competing interests