Author’s response to reviews

Title: QUALITY of life, satisfaction and outcomes after ministernotomy versus full sternotomy isolated Aortic Valve Replacement (QUALITY-AVR): study protocol for a randomised controlled trial

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Version: 1 Date: 29 Nov 2017

Author’s response to reviews:
Thank you for your really careful review of the manuscript, and for the suggestions to improve his quality. After major changes performed, with over 90% of suggestions accepted, we hope that it can be accepted for publication in the TRIALS JOURNAL.

All questions/doubts were answered below one by one.

Sincerely yours.
Reviewer reports:

Reviewer #1: Traditionally aortic valve surgery is done through a full sternotomy that is easy to perform. This incision is characterized by the aesthetic problem, leading to the search for technical alternatives such as the one proposed by the present investigation. The QUALITY-AVR Trial is a single-blind, single-center, independent, pragmatic randomized clinical trial comparing ministernotomy ("J" shaped upper hemisternotomy toward right 4th intercostal space) versus full sternotomy in patients with isolated severe aortic stenosis scheduled for elective aortic valve replacement. This is a simple but relevant proposal, as well as having the profile of TRIAL. It may be necessary to improve the quality of the figures.

Accepted recommendation. Figures now with 300 dpi.

Reviewer #2: hReviewer(s)' Comments to Author:

Major Compulsory Revisions:

1. The study design was a single-blind, single center, randomized clinical trial. I am wondering if it is a real single-blind clinical trial as surgeon and patient would realize the FS vs. MS after surgery and masking of the surgeon and patients would be difficult.

Blinded patient until discharge. Other thing in a surgery cohort it’s no possible.

2. Sample size was determined for primary end point with an alpha error of 0.05 and with a power of 90% in detecting differences between intervention groups ≥0.10 points in change from baseline quality of life Questionnaire EuroQOL-index (EQ-5D-5L®), measured at 1, 6 or 12 months. The first concern is which time point would be considered as for the measurement of comparison? Two groups of 48 patients are necessary for a minimum of n=96 patients. In view of possible losses to follow-up, 100 patients will be randomized. The potential loss of follow-up or drop is less 10%, of which is relatively low than the average loss in clinical trials. In addition, several secondary points were clearer than the primary end point as the determination of sample size and might be better when compared to the primary end point as its clinical importance.

Added in the text.

Previous studies of our team showed only 1% loss to follow up after AVR surgery across last 5 years.
No QOL measured in MS v FS, TAVI shows better QOL at 1 month. If MS provides similar QOL outcomes, MS would be the control for future comparisons. If positive results, this trial could encourage to perform a large multicentric trial with mortality as primary endpoint (n needed around 2500 pts)

3. Both groups of patients will receive clinical follow-up and complete the EQ-5D-5L® quality of life questionnaire at 1, 6 and 12 months, and SATISCORE® at 1 and 6 months. Why did not add an additional 12-month of SATISCORE® questionnaire assessment?

Satisfaction with surgery wont improve after six months in previous reports (ref 26), its recommended by the creator 2 measurements (1-6 months)

4. The primary endpoint measure is to detect differences between the two intervention groups greater than or equal to 0.10 points change from the baseline questionnaire EQ-5D-5L® Index, at 1, 6 or 12 months after the surgery. The 0.10 point change is really less different and significant? In addition, authors used the quality of life as the major determination as it would be hard to actually reflect the superiority of MS to FS in clinical practice.

QOL is a very important outcome for patients. If safety and outcomes were quite similar in previous trials, QOL could be the difference.

If 0.08 points are enough to consider a relevant benefit in QOL after a stroke, 0.10 seems to play for MS.

5. Authors anticipate this trial could modify the surgical "Gold-Standard" for aortic stenosis surgery, and subsequently the need to change the control group in transcatheter aortic valve implantation trials. Please describe in more details how this trial would change the control group in transcatheter aortic valve implantation trials as it might be considerably different viewpoints in the study design between two clinical trials.

Patient quality of life reported outcomes was selected to be the primary endpoint because currently cardiologist argue that TAVI are better because with same results in complications in the short term, the recovery time is lesser and better with improved quality of life. All data comes from secondary analysis, not primary endpoints.

We add: QOL matters a lot to patients nowadays, and is usually better and statistically significant in the first month in favor of TAVI.
6. In abstract section: to date, few clinical trials have been conducted that compare AVR surgery using MS versus FS, and no significant differences have been found, due to inadequate design, a lack of statistical power or a sample size too small for the primary endpoint of mortality, although significant differences did exist in morbidity on the side of MS (lower rates of pain, transfusions, bleeding, mechanical ventilation time, stay in intensive care and hospital...etc.). However, the current study design still not yet provide the most important information or elements to readers or even probably lead to misunderstanding regarding the role between FS and MS under the current study design.

We think is well explained. Most important information of the design is to achieve a difference in QOL measures, because it is powered for.

In each previous trial, there seemed to be a benefit in the MS, but excepting less bleeding, each study indicated a different one, so it had not been confirmed.

The role of MS is: is similar QOL as TAVI is demonstrated, MS provides the capacity of better and durable valves, so, try it in low and intermediate risk patients taken the non-proven long term durability of TAVI valves.

Reviewer #3: An interesting trial about the quality of life, satisfaction and outcomes after ministernotomy versus full sternotomy access in isolated aortic valve replacement procedures.

The Trial is well designed. However, some English language editing is needed for improvement of the presentation.

* Background

This section could be rewritten to provide more details regarding the primary outcome and less details regarding secondary outcomes such as Mortality.

We add more information.

There are no prospective studies that measure patient quality of life (QOL) and compare the techniques of MS versus FS for AVR. Only one retrospective study exists, with results that are not statistically significant[16], and two clinical trials have compared TAVI versus FS (the PARTNER[17] and the Core Valve PIVOTAL trials[18]) but did not analyse MS. Nowadays QOL matters a lot to patients, and is usually better and statistically significant in the first month in favor of TAVI [17,18]. If similar QOL as TAVI’s is demonstrated, MS provides the capacity of implantation of more durable valves, so, it would be used in low and intermediate risk patients taken the non-proven long term durability of TAVI valves above 5 years.
To date, however, there has been no specifically QOL designed study. For all of these reasons, we designed this clinical trial to compare the QOL of MS versus FS. Surgery satisfaction and morbimortality outcomes are already measured.

* Design

This study is single-blind. However, no details regarding the blinding process is given. A separate section for blinding could be added, delineating the different details of blinding.


* Secondary Outcome measures

It is mentioned that the AKIN criteria was used here to define acute renal failure. However, the KDIGO criteria is the most recent update of this definition. Please provide in-text a brief statement explaining the rationale for using the AKIN criteria.

AKIN was used in VARC 2 criteria according to ESC/EACTS societies, and its the way to compare TAVI vs MS.

* Sample Size

It is mentioned that a sample size of 100 patients will be used to account for any loss to follow-up. Please provide detail regarding the plan for any loss or withdrawals from the trial.

Added, including missing data management. Previous studies of our team showed only 1% loss to follow up after AVR surgery across last 5 years.

* Ministernotomy

"Use of CO2 via drain at surgeon's criterion" This statement could be rewritten to clarify the meaning.

Rewritten. CO2 was used in all MS.

* Finally, please provide a SPIRIT checklist and Figure to accompany the manuscript.

We add SPIRIT figure and checklist.
Reviewer #4: Overall impression:

The current trial protocol investigates a clinically relevant problem in patients undergoing major cardiovascular surgery. The minimally invasive MS strategy is compared to the FS standard approach which even in light of the TAVI procedure is a relevant question. However, the manuscript in the current form lack important information and structure that needs to be provided. The authors could benefit from following the SPIRIT guidelines to the letter.

Title

The current title is too long and repeats itself unnecessarily. The acronym (QUALITY-AVR) should be moved before the :, and it should be mentioned the study is a protocol. A possible title could read:

"QUALITY of life, satisfaction and outcomes after ministernotomy versus full sternotomy isolated Aortic Valve Replacement (QUALITY-AVR): a protocol for a randomized controlled trial."

Title changed: QUALITY of life, satisfaction and outcomes after ministernotomy versus full sternotomy isolated Aortic Valve Replacement (QUALITY-AVR): Study protocol for a randomized controlled trial.

Abstract

The abstract is overly detailed regarding the sample size calculation which should be addressed in the method section.

The sentence line 15-17: ("J" shaped upper hemisternotomy toward right 4th intercostal space) should be deleted.

We disagree, this definition is very important in the abstract as it is the simple size calculation.

Drop the notation Major secondary endpoints. The sentence describing secondary endpoints is cryptic and should be rewritten.

The first sentence in the abstract discussion describes several aims, which should be broken into separate sentences. The wording a "positive" result is unfortunate and should be rephrased.

Major issues

Accepted and changed.
BACKGROUND

* In all a well written background. It is clearly argued that MS is superior to FS in many aspects; however, the reason to choose a patient reported outcome as the primary outcome is not clear. It could be argued that the proper trial to preform would be the trial with mortality as an outcome. As the background reads now, it seems the patient reported approach was chosen in light of the lower sample size and feasibility to perform a trial in a single centre setup.

A patient centred approach is highly relevant, however, this choice needs to be argued in the background.

Patient quality of life reported outcomes was selected to be the primary endpoint because currently cardiologist argue that TAVI are better because with same results in complications in the short term, the recovery time is lesser and better with improved quality of life. All data comes from secondary analysis, not primary endpoints.

We add: QOL matters a lot to patients nowadays, and is usually better and statistically significant in the first month in favor of TAVI.

* Page 4 line 9-16 is one long sentence and needs to be broken into two. Furthermore, it is unclear from the sentence if MS was apart of the PARTNER or PIVOTAL trials. This needs to be clear.

It is explained that MS was never compared in a trial with TAVI. We divide sentences.

* Again "quality of life, satisfaction and outcomes " are all mixed into one. These needs to be described as separate specific aims.

Accepted

METHODS

* The structure of the method section should be rearranged so it follows the SPIRIT statement.

All items were checked and are presents except Participant timeline (Added)

* No SPIRIT figure is provided which is essential to follow the SPIRIT statement which is required by the Trials Journal.
We add a SPIRIT figure and checklist.

* The method section mentions a pragmatic and independent design. How is this and why is it relevant for the current trial? Does the authors mean researcher initiated or?

A pragmatic trial is described in the presented reference 19 (Ford I, Norrie J. Pragmatic Trials. N Engl J Med 2016; 375:454-463.) Pragmatic trials means a trial designed to show the real-world effectiveness of the intervention in all comor population

Independent: no industry funding.

* The authors have a tendency to write to long sentences, which makes the manuscript tedious to read. Throughout the manuscript this needs to be addressed.

Adressed as posible as we can.

* The "EuroQoL 5" was chosen as it was tested in a Spanish population. Is this the same as a validated translation. Please give references in this regard. The current reference is internet reference to a Spanish website, which is irrelevant to the reader.

It is the only reference, in the National Spanish Ministry of Health webpage, so not irrelevant.

* No reference is given to the SATISCORE®. Furthermore it is unclear, how the total score is 100 when the text mentions 20 statements with 6 possible outcomes (120 total). More specific information needs to be provided.

Ref nº 26 was SATISCORE ref. answers are from 0 (no answer) and 1-5. Total 100 points. It is in the text.

* Multiple assessment are performed on multiple outcomes, however, the timing is unclear from the current manuscripts. Produce a spirit figure and reference it actively to help the reader instead of confusing the reader.

Added
* In the "selection of patients" section: "current clinical practice and meet the inclusion and exclusion criteria" drop "the inclusion and" as they are mentioned in the text just before.

   Accepted.

* Block randomization is mentioned. Why was this chosen in a single centre study? Do the authors mean stratification on a given variable instead?

   Is one of the types of randomisation independently of unicentric or multicentric studies. No stratification, simple in block randomisation.

* AleatorMetod is mentioned as a computer randomization program. Give a reference or delete if not relevant.

   Deleted.

* Allocation concealment is briefly described as performed by an administrative officer. Who are the medical personnel? Is the patient intended to be blinded? If so describe in detail how this is achieved. The study is described as single-blind, however, no follow-up description is given. Is the outcome assessor blinded and who assess the different outcomes? Provide sufficient detail so the study could be replicated.

   Accepted and written

* The study endpoint is written in bold text. Why is this? The description is of two interventions, however, the current trial test MS (active treatment) against FS (control/standard of care) write this instead.

   Accepted and written.

* Several combined endpoints regarding complications are mentioned. Furthermore, the MAC are nested within the other. This constitutes a reporting issue and should be discouraged. Composite outcomes are problematic as the direction of interventions effect could balance each other out. So separate incidences should be reported.
* Nosocomial infections are mentioned, however, the time period is one month postoperatively. Does the authors expect the patients to be hospitalized for an entire month. Are infections at home not a relevant complication?

Nosocomial infections are infections acquired during the hospitalization, or even up to 12 months in case of early endocarditis according to VARC criteria.

* Postoperative stay in total and in the intensive care unit are mentioned as outcomes. The correct terminology is length of stay (LOS) and should be changes accordingly. When reporting hospital stay it is vital to report if the unit had ERAS protocols in the department and what were the discharge criteria. This could be provided as supplementary material.

Accepted and added. Discharge depends on Surgeon’s patient criteria. Not relevant for protocol.

STATISTICAL ANALYSIS and sample size

* As the authors also correctly address the SD of 0.15 and MID of 0.10 are arbitrarily chosen on the best available information resulting in a 2 x 48 setup and planned inclusion of 100 patient. I.e. expected drop-out of 4 patients over 12 month period.

Such a low drop-out rate is overly optimistic and will only likely result in an underpowered trial. Will additional patients be recruited.

In previous studies with aortic valve surgery previously published by our group, loss to follow up was 1%. If more than 4% was achieved, is planned in the whole protocol to recruit patients to achieve 96 in total at least.

* The authors have chosen a repeated measure design (1, 6, and 12 months), however, not specified a repeated measure analysis for the primary outcome. T-test will be used (depending on distribution) to test group differences, however, a mixed model analysis would be a more appropriate analysis model.

Is yet written and planned, line 42 page 10 (ANOVA model for repeated measures)

* Distribution of data will be tested using the Kolmogorov-Smirnov test. Why not use the histograms or QQ plots to asses distribution. If relevant do the authors intend to use transformation of data?
KS, histograms or Q-Q plot are 3 ways to detect normality or not. The preferred is irrelevant for the study if normality is well tested, instead our biostatisticians check the 3 in all cases. No transformation, we will use non parametric tests if precise.

* No information regarding how "missing data" will be handled is provided. This needs to be described in detail.

Intention to treat analyses can improve management of missing data. Nevertheless we add to the paper:

Differences between MS and control FS group scores at each follow-up time point will be estimated with longitudinal random-effect growth curve models that will be fit to the repeated measurements for each health status outcome. These longitudinal analyses will use all available quality-of-life data, including data from patients who subsequently will die, withdrew, or will be lost to follow-up, and accommodate missing data under the missing at random assumption. Variables included in the models were treatment assignment, prespecified patient characteristics (age, sex, EuroScore), follow-up time and interactions between treatment and time.

These models were used to calculate mean between-group differences for the EQ-5D index score and the individual subscales at each follow-up time point, as well as the associated 95% confidence intervals (CIs) and P values.

To examine the potential impact of missing data, which, given the illness severity of the trial population, would most likely not be missing at random, we will repeat growth curve analyses after imputing missing scores among surviving patients as the lowest reported score among respondents within each treatment group for each respective time point. We also examined the magnitude of potential survivor bias by comparing the mean baseline scores between treatment groups for the subgroups of patients with available quality-of-life data at each successive follow-up time point.

* "Effectiveness of treatment" is described in the end of the statistics section, however, no measure is given.

Effectiveness means surgery success regarding morbimortality.

* Description of the intention to treat and per protocol is insufficiently described. This needs to be detailed and related to the planned analysis.
We added: The intention to treat population is defined to include all randomized subjects. This population will be used for endpoint analyses. The as treated population (“per protocol”) is defined to include all subjects actually undergoing the index procedure. This population will be used for the analysis of adverse event analyses.

* The statistical section could greatly benefit from a consultation from a statistician and be described in a detailed analysis plan.

Statistical Plan was practically copied from PARTNER 2A Trial (TAVI vs FS-AVR) for posterior comparison and yet explained and revised by a statistician.

SUMMARY AND TRIAL STATUS

* No details regarding data-sharing policy is described in the current protocol. This needs to be described in detail.

We add at the end: Data-Sharing Policy will be under request to the sponsor and publicly shown in clinicaltrials.gov.

Sincerely yours.