HIGH/TOTAL SPINAL ANAESTHESIA IN CARDIAC SURGERY

With this inaugural "Journal Critique" I pondered the question whether to select a single journal article that I found interesting, dissecting it for value, faults and future implications as is always the norm; or to sit down and assess the state of research in the cardiac anaesthesia journals and the relevance of such research to our practice and training in the South African context – should we directly translate it into our practice, is there a need to clearly define our own direction, have we individually or as units and institutions picked up anything that says "perhaps we are different"?

While I was pondering the question and looking up information that was stimulating and checking what the published challenges in cardiac anaesthesia in South Africa were, I came across an expert opinion article from one of my colleagues who had just been on an overseas trip where this particular expert had presented the lecture entitled "Neuraxial anesthesia for cardiac surgery: thoracic epidural and high spinal anesthesia - why is it different?" by R. Kowalewski, D. Seal, T. Tang, C. Prusinkiewicz and D. Ha in HSR Proceedings in Intensive Care and Cardiovascular Anesthesia 2011; 3(1): 25-28.

When my colleague initially verbally presented the presentation to me, I used my preconceived perceptions of what “spinal anaesthesia” in cardiac surgery should mean and imply, based on my exposure in our institution and the South African context – my initial response was one of disgust and an immediate dismissal of the “agent” who would make such a presentation as a charlatan and antirevolutionary.

He informed me that the presenter actually came from a reputable institution in Calgary, Canada and that they had been using this “High/Total Spinal with Local Anaesthesia” technique in Cardiac Surgery in over 10 000 patients without major morbidity. I asked him to forward me the article so that I could validate its authenticity and what support it has received in the wider cardiac anaesthesia community.

I read the article which notwithstanding its shortcomings as an “expert opinion” document (probably the weakest grade of evidence available), stimulated me enough to want to explore this technique in cardiac anaesthesia. I did a number of investigative maneuvers to explore the matter to my utmost satisfaction before presenting it:

- The institution where the authors come from and the reputation it has
- Other reputable researchers the author has published with
- Journals that have been willing to publish the work
- What other research has been done in the field on the same topic
- Contradictory research and criticisms of the technique or the opinions of this data.

To the first three questions I had promising information that made me want to have another look at the implications these authors were challenging all of us with – they are from a reputable institution, have published with some of the preeminent researchers
in Canada (Grocott H, Lee TW, Schwinn and others) and various reputable institutions including the New York School of Regional Anesthesia, Anaesthesiology, JCVA, Can J of anaesthesia and many more have been willing to publish their opinions and work.

To the last two questions, there was unfortunately no significant randomized trials and therefore no metanalyses looking at this particular question for me to accept or cast the suppositions of Kowalewski et al into the ever growing hip of meaningless research that we are continuously bombarded with.

Instead I was left wondering what the implications of this simple apparently safe intervention, conducted in over 10 000 patients in one institution, would be for us as South Africans. Is there a place for it in our practice – severe valvular pathology, CABG patients with poor left ventricles, late presenting primary congenital heart surgeries and 2nd and 3rd stage palliations for complex heart surgeries?

The more robust information I could find looking at regional anaesthesia in cardiac surgery were two metanalyses looking at the two questions of “epidural anaesthesia and spinal opioid analgesia” in cardiac surgery, with no metanalyses looking at total spinal anaesthesia with “buvicaine” specifically:


No prospective randomized trial has fully looked at the question of “intrathecal bupivacaine/high spinal anaesthesia” and outcomes in cardiac surgery. There are numerous case reports on the technique varying from CABG, Aortic stenosis and congenital heart surgery. Retrospective case series of the technique have also been published with promising results, alas we await a prospective randomized trial with enough numbers.

A prospective randomized double-blind trial by the Winnipeg High Spinal Anesthesia Group titled High Spinal Cardiac Anaesthesia - Effects on B-Adrenergic Receptor Function, Stress Response, and Hemodynamics by Lee et al is one bold attempt to describe the effects of high spinal anaesthesia and the possible explanation of the observed benefits. The study conclusions were that “high spinal anaesthesia with bupivacaine combined with general anesthesia resulted in less B-adrenergic dysfunction and less stress response during CABG surgery”.

A survey done by Goldstein et al, of Society of Cardiovascular Anesthesiologists members in 2001 showed that of the responders 7.6% used spinal anaesthesia in adult cardiac surgery, 7% used epidural anaesthesia and 1% used a combination of the techniques. 3974 anaesthesiologists were surveyed with 974 (24%) responders. 892 of the respondents were from centres that performed cardiac surgery.
Kowalewski et al go to pains to explain the confusion made between “total spinal anaesthesia with bupivacaine” and “spinal opioid analgesia/anaesthesia with low dose bupivacaine”; and the possible explanation why the former carries better benefits than the latter:

- Total body sympathectomy from T1 = complete denervation of the surgical site
- Minimal humoral, metabolic, homeostatic alterations and no immunosuppression
- Positive myocardial oxygen balance
- Protection of myocardium and abdominal organs, through blood redistribution and
- Protection against b-receptor down-regulation

They do however contend that the benefits of spinal anaesthesia, although numerous especially for the elderly patient, are however “anecdotal, not studied properly or difficult to study”.

They however make the mistake of extrapolating the benefits of “High Epidural Anaesthesia” to suggest that High Spinal Anaesthesia may have the same effects on outcomes. As we have learnt in major vascular surgery that the benefits of Thoracic Epidural Anaesthesia in that setting are derived from the maintenance of the epidural for longer than 24hrs and when used in patients at high risk for major cardiac events and mortality peri-operatively, it makes one wonder how High Spinal Anaesthesia which lasts for a shorter duration of time can have the same beneficial effects.

The intrathecal solution they use consists of:
- 20-40mg of 0.5% hyperbaric bupivacaine
- 0.3-0.4mg morphine
- 10-20ug sufentanil
- Mixed to a total volume of 4-6ml
- Use of a 27G spinal needle prior to heparinization

To the uninitiated they make a startling claim that: "there is an unjustified perception that high level regional anesthesia with total body sympatectomy could cause profound hypotension and bradycardia leading to cardiovascular disaster. While it is true that some degree of hypotension is often present, both bradycardia and hypotension are easily managed by the use of the Trendelenburg position and/or by using small doses of vasoactive drugs such as dopamine or phenylephrine".

This statement goes against all thus far taught manoeuvres, at least in our institution, for managing spinal anaesthesia ie. avoiding high spinal anaesthesia and not using Trendelenburg. They also state that they do not routinely use pre-spinal fluid boluses, but this may be explained by the relatively less potential for elderly arteriolar vasodilation with regional anaesthesia.

They also claim to recently use this technique routinely in most cardiac surgeries including patients with high grade left main coronary artery disease, aortic stenosis and
in patients with left ventricular dysfunction undergoing complex procedures. Thus far no major morbidities have been reported, in particular no spinal hematoma or deaths related to the technique.

CONCLUSION

High/Total Spinal Anaesthesia (TSA) in cardiac surgery will probably remain controversial for a while. It is a pity that the group from Calgary with such vast and long-standing experience in the technique have not endeavoured to publish a prospective randomized trial looking at all outcomes using the technique against standard practice.

If indeed the benefits are as predicted, then such a simple and cheap technique may be one revolutionary step that cardiac anaesthesia could benefit from. It is however still difficult to comprehend how this could be, based on our current knowledge of the physiology and pharmacology of the intrathecal space and drugs used in it. The study by Lee TW, Grocott HP and Schwinn D gives us a glimpse though of the possible mechanisms at play.

Until prospective, randomized trials are published, it remains to be seen what the role of this technique would be outside of Canada. Would it be worthwhile for us to explore opportunities that this technique in practice and in research?

REFERENCES


