The results of this study suggested superiority of CABG over PCI in elderly patients, this is counterintuitive and these results may not be generalizable to the majority of patients requiring coronary revascularization. Those patients with a higher risk profile were likely to be excluded from randomization because of procedural risks associated with CABG. The advantage of PCI in the elderly patients could therefore not be identified in this pooled analysis.

Furthermore, long-term survival of young patients with more complex coronary artery disease is best realized through surgical revascularization with a left internal mammary artery (IMA) to the left anterior descending artery and additional arterial grafts (preferably the right IMA) to other major coronaries. This will optimize long-term survival due to excellent graft patency, which is critical, especially in young patients with a relatively long life expectancy. Young patients who undergo PCI will have a high risk of multiple repeat revascularizations and are susceptible to the associated procedural risks.

The ancillary benefit of PCI over CABG is its lesser invasiveness and shorter initial hospitalization. However, in younger, fitter patients, CABG is appealing because of low complication rates, short lengths of stay, and little time needed to resume normal activities of daily living. The benefit of PCI over CABG in younger patients may therefore be small, whereas long-term efficacy is clearly superior in the majority of young patients; the treatment of choice should therefore be CABG.

REFERENCES


Correspondence

Prognostic Value of Multidetector Coronary Computed Tomography Angiography

Cho et al. (1) should be commended for their recent attempt to answer an important question in an ongoing debate on the prognostic value of coronary computed tomography angiography (CCTA). In their retrospective cohort study from a single center, they concluded that CCTA provided improved discrimination for future major adverse cardiovascular events over the exercise stress test (1). However, it should be noted that:

1. Results of the current study were predominantly based on a clinician-driven outcome—revascularization, which is more amenable to change and should be interpreted with great caution. The difference in prognostic value of the exercise stress test and CCTA (for both negative and positive tests) failed to achieve statistical significance for more relevant clinical outcomes—cardiac death and nonfatal myocardial infarction. Thus, the CCTA-based approach led to a higher rate of revascularization, but it remains unclear whether the CCTA-guided therapeutic decision-making process led to improvement in outcomes in terms of hard clinical endpoints.

2. The researchers censored the outcomes by excluding revascularizations that occurred <90 days after the index test to avoid a confounding effect of CCTA driving the study endpoint. However, such selective removal of patients creates treatment selection bias and results in greater observed risk reduction among patients with obstructions as compared with those without obstructions.

Despite the high radiation exposure, higher cost, unproven clinical benefits, and inability to provide useful clinical information in the settings of high heart rate, coronary calcification, and obesity—which are rampant among patients with coronary artery disease (2)—should we really advocate CCTA as a first-line test for more than 5 million Americans who present to the emergency department every year with chest pain (3)?

REFERENCES


Relationship of Cardiac Output to Respiratory Pattern and Pressures in Patients With Fontan Circulation

We congratulate Shafer et al. (1) on their recent publication. We would like to raise some points and would be grateful to the researchers for clarification could be provided to better understand their methodology and results. With regards to the patient population, we were somewhat surprised to see that young patients, seemingly undergoing operations in the last decade or so, all underwent atriopulmonary Fontan. Total cavopulmonary connection has been the surgery of choice in the last 2 decades because it offers a better hemodynamic profile and reduces the risk of arrhythmia generated by an enlarging right atrium.

The results of this study were obtained in young and asymptomatic patients a few years post-Fontan operation. We wonder whether the conclusions can be generalized to the whole population of patients with Fontan circulation, including those with very large right atria and those with a “failing Fontan.”

The inert gas rebreathing method is indeed one of the most accurate methods for noninvasive assessment of cardiac output. This method requires appropriate and strict adjustment of respiratory pattern. A rebreathing bag of a volume of 1.5 to 2.5 liters should be used for several breaths and should be emptied at each breath for the technique to provide accurate results (2). Such adjustment of breathing pattern during exercise produces little change in cardiac output in healthy individuals but could significantly affect cardiac output in patients with Fontan circulation. In fact, an optimal respiratory breathing pattern during exercise has been suggested in Fontan patients, resulting in the most efficient pulmonary augmentation of blood flow (3). The lack of a significant increase in cardiac index from “exercise” to “exercise plus inspiratory load,” as seen in this study, would appear to contradict previous data on the beneficial effects of negative inspiratory pressure in Fontan patients (4). We wonder whether this relates to the technique used for measuring cardiac output.

Furthermore, it appears unclear to us whether the increase in stroke volume in these patients was accounted for entirely by the skeletal muscle and ventilatory pumps. Complete separation of the effects of the muscle and ventilatory pumps is difficult to achieve, even with such a carefully designed protocol such as the one used in this paper, as demonstrated by the change in minute ventilation in both patients and controls on “zero-resistance cycling” (Fig. 4 of their paper [1]). The latter proved to be statistically nonsignificant, but we wonder whether this was due to the Bonferroni post hoc adjustment for multiple comparisons, which is known to inflate type II errors.

We appreciate the interest and thoughtful comments regarding our recently published paper and are pleased to take this opportunity to discuss our techniques and data further (1).

As stated, our study was performed exclusively in patients with the atriopulmonary connection (APC) Fontan. Although this is not the more contemporary type of Fontan circulation, our design did allow for study of a relatively homogenous population, limiting the amount of unmeasured confounding. Although there is evidence of less efficient flow dynamics at rest and lower effective pulmonary blood flow at peak exercise in the APC Fontan versus the total cavopulmonary connection (TCPC), the changes during exercise as they would apply to our findings are unknown (2,3). We agree that there is a need for additional investigation of our results in the TCPC Fontan and hope to do so in the future.

In part because of the work of Shekerdemian et al. (4), the inspiratory load was included in our study and designed to impose an enhancing and inhibitory (from the expiratory load) stimulus on the ventilatory pump. However, it is worth highlighting a few key differences between the Shekerdemian et al. (4) study and ours. First, their patients were intubated and paralyzed and thus by definition without thoracic or skeletal muscle pump activity. Their study showed that negative-pressure ventilation was able to counteract some of the untoward effects of positive-pressure ventilation in the absence of respiratory muscle pump activity. It is important to emphasize that we showed a decline in stroke volume even in the presence of muscle pump activity, highlighting the need for caution when using this type of ventilatory support. Although our study failed to show significant additional benefit of negative intrathoracic pressure during exercise above the increase in stroke volume due to the muscle pump, we agree that the design did not evaluate the ventilatory pump without simultaneous action of the muscle pump. If we had added an isocapnic hyperpnea condition in the absence of zero resistance cycling to our study, we could have commented more fully on this concept. With our data, we would suggest that unless antagonized, the effects of change in intrathoracic pressure are less critical than the muscle pump in maintaining the stroke volume in the nonparalyzed patient with Fontan circulation.

REFERENCES


*Adult Congenital Heart Centre
Royal Brompton and Harefield NHS Foundation Trust
Sydney Street
London SW3 6NP
United Kingdom
E-mail: a.kempny@rbht.nhs.uk

http://dx.doi.org/10.1016/j.jacc.2013.01.081

Reply

We appreciate the interest and thoughtful comments regarding our recently published paper and are pleased to take this opportunity to discuss our techniques and data further (1).

As stated, our study was performed exclusively in patients with the atriopulmonary connection (APC) Fontan. Although this is not the more contemporary type of Fontan circulation, our design did allow for study of a relatively homogenous population, limiting the amount of unmeasured confounding. Although there is evidence of less efficient flow dynamics at rest and lower effective pulmonary blood flow at peak exercise in the APC Fontan versus the total cavopulmonary connection (TCPC), the changes during exercise as they would apply to our findings are unknown (2,3). We agree that there is a need for additional investigation of our results in the TCPC Fontan and hope to do so in the future.

In part because of the work of Shekerdemian et al. (4), the inspiratory load was included in our study and designed to impose an enhancing and inhibitory (from the expiratory load) stimulus on the ventilatory pump. However, it is worth highlighting a few key differences between the Shekerdemian et al. (4) study and ours. First, their patients were intubated and paralyzed and thus by definition without thoracic or skeletal muscle pump activity. Their study showed that negative-pressure ventilation was able to counteract some of the untoward effects of positive-pressure ventilation in the absence of respiratory muscle pump activity. It is important to emphasize that we showed a decline in stroke volume even in the presence of muscle pump activity, highlighting the need for caution when using this type of ventilatory support. Although our study failed to show significant additional benefit of negative intrathoracic pressure during exercise above the increase in stroke volume due to the muscle pump, we agree that the design did not evaluate the ventilatory pump without simultaneous action of the muscle pump. If we had added an isocapnic hyperpnea condition in the absence of zero resistance cycling to our study, we could have commented more fully on this concept. With our data, we would suggest that unless antagonized, the effects of change in intrathoracic pressure are less critical than the muscle pump in maintaining the stroke volume in the nonparalyzed patient with Fontan circulation.