heart failure model, created by micro-embolization of the coronary artery, was used in their study, and they clearly demonstrated that the impaired cardiac function made workload control easier.

Pirbodaghi and colleagues¹ evaluated coronary flow in response to rotational speed change. However, previous studies⁴ have already reported that coronary flow is diminished as left ventricular assist device support is increased, and the increased rotational speed in the diastolic phase optimizes coronary flow. As Pirbodaghi and colleagues mentioned, the coronary arteries have an autoregulatory system that maintains optimal coronary flow according to oxygen demand. Therefore, the coronary flow with the pulsatile control system should be examined in ischemic heart models when considering practical clinical settings.

Pirbodaghi and colleagues¹ discuss that unloading of the left ventricle became a method to promote cardiac recovery. In their study, they were able to control cardiac workload by changing the rotational speed of a rotary blood pump via synchronization with the electrocardiogram. We believe that unloading is not the only method of cardiac functional recovery. In fact, excessive unloading of the left ventricle might lead to cardiac disuse atrophy.⁵ Instead, favorable work for the native heart in each phase of cardiac recovery is extremely important to obtain the best improvement of cardiac function.

In summary, we were greatly interested in Pirbodaghi and colleagues'¹ idea of rotational speed change with waveform modulation for controlling native cardiac load and hemodynamics, and look forward to these authors' further reports. However, we feel compelled to point out that the control system of the rotational speed change in synchronization with the native cardiac cycle described by Pirbodaghi and colleagues¹ is similar to previous studies²⁻⁴ and the authors are remiss in failing to mention these other articles. We also need to point out

that the impaired cardiac function model, which is very important for investigating the efficacy of this novel pump control method in actual clinical settings, may lead to different results, as shown by Umeki and colleagues.³

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<http://dx.doi.org/10.1016/j.jtcvs.2012.12.081>

Reply to the Editor:

We appreciate the comments and concerns expressed by Arakawa and colleagues regarding our article, titled "Pulsatile control of rotary blood pumps: Does the modulation waveform matter?"¹

Unfortunately, we have to disagree with Arakawa and colleagues. As is

obvious from the title of our article, it investigates the effect of different waveforms on the heart-device interaction. In contrast to the authors' claim, this is the first article in the literature that uses basic waveforms (sine, triangle, saw tooth, and rectangular) with different phase shifts to examine their impact on left ventricular unloading. The previous publications²⁻⁴ just varied the pump speed during systole and diastole, which was first reported by Bearnson and associates⁵ in 1996, and studied its effect on aortic pressure, coronary flow, and end-diastolic volume. We should mention that dp/dtmax is a load-sensitive parameter of contractility and not representative for the degree of unloading. Moreover, none of the aforementioned reports has studied mechanical unloading and in particular the stroke work of the left ventricle. Our method is unique because we do not just alternate between high and low speed but have accurate control of the waveform because of the direct drive system of Levitronix Technologies LLC (Waltham, Mass) and a custom-developed pump controller.

Without referring, Arakawa and associates state "several previous studies have already reported the coronary flow diminishes as the left ventricular assist device support increases." It should be noted that all the waveforms used in our study have 2000 rpm average value with 1000 rpm amplitude, which is not an excessive speed for the Centri-Mag rotary pump (Levitronix) to collapse the ventricle and diminish the coronary flow.

We agree with Arakawa and co-workers that there is a need for a heart failure model to come to more relevant results with respect to clinical expectations. However, we have explored many existing models, including species and breeds that have a native proneness to cardiomyopathy, but all of them differ from the genetic presentation in humans. We certainly do not believe that the use of microembolization, in which the coronary circulation is impaired by the injection of

microspheres, would form a good model from which to draw conclusions about coronary flow change under different loading conditions. A model would be needed in which either an infarct is created to mimic ischemic heart failure or the coronary circulation remains untouched to simulate, for instance, dilated cardiomyopathy. Furthermore, in discussion we clearly mention that “lack of heart failure is a major limitation of our study.”

We also believe that unloading is not the only factor of the cardiac functional recovery, and an excessive unloading of the left ventricle might lead to cardiac tissue atrophy. Therefore, in our article we mention that control of the level of cardiac unloading by assist devices has been suggested as a mechanical tool to promote recovery, and more studies are required to find better strategies for the speed modulation of rotary pumps and to achieve an optimal heart load control to enhance myocardial recovery.

Finally, there are many publications about pulsing rotary blood pumps and it was impossible to include them all. We preferred to reference some of the earlier basic works such as an original research by Bearnsen and coworkers⁵ and another article published by our group,⁶ which is more relevant.

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RADIAL ARTERY OR SAPHENOUS VEIN: A GRAFT OF SECOND CHOICE IN CORONARY ARTERY BYPASS SURGERY

To the Editor:

With great interest we have read a recent meta-analysis from Cao and colleagues¹ on patency outcomes comparing the radial artery (RA) and saphenous vein (SV) after coronary artery bypass graft (CABG) surgery. The extracted data suggest superiority of the RA compared with the SV at midterm angiographic follow-up, while considering the increased incidence of string sign associated with the RA as a potential clinical concern. We wish to point out a major issue that needs to be taken into consideration when comparing RA and SV patency rates.

Intraoperative graft management is a crucial determinant for the long-term results of SV patency. We believe the “no-touch” harvesting technique of the SV graft imposes the pivotal role in its patency. This method provides a pedicled graft that has little similarity with a venous graft harvested conventionally, but a patency rate comparable with the internal thoracic artery.² It preserves normal vessel architecture with intact adventitia, preserves vasa vasorum, and maintains

medial blood flow and endothelial integrity. The perivascular fat provides a cushion support that protects the vein against arterial hemodynamics and kinking, as well as providing a source of factors beneficial to graft performance. Superior long-term patency rate can be explained by a slower progression of atherosclerosis in these vein grafts.³ The conventional harvesting technique damages vein structure. Early vein graft failure is associated with distention-induced endothelial denudation. The damage of the outermost layers has adverse long-term effects on graft performance and its patency. Complete “bedside to bench” situations of mechanisms underlying the improved performance of “no-touch” SV graft are reported in a recent review.⁴

Despite the benefits clearly shown by the “no-touch” technique, its use is still limited to only a few centers worldwide, as is often the case with all new interventional techniques. What should be of a real concern, particularly in light of unequivocal scientific evidence on graft quality obtained in such manner,⁵ is the ever-increasing popularity of the SV grafts harvested endoscopically. This contributes to a substantial heterogeneity of the extracted data with questionable comparability. We consider that the information regarding the prevalence of SV grafts harvested endoscopically is of great importance when assessing SV patency.

The existing evidence on “no-touch” SV makes us question why this technique should have been excluded from this article, or at least discussed. An ongoing multicenter randomized controlled clinical trial (SUPERIOR SVG Trial, NCT01047449) aims to provide strong evidence whether the new technique of a pedicled SV graft improves its patency in CABG.

The study results favor midterm patency rates of the RA over the SV in CABG. Although the scientific data are inconsistent on the matter, we believe that the preservation of normal