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Tom Archer

Comments on Greggory DeVore paper:

“Assessing maternal cardiac function by obstetricians: technique and reference ranges.”

Tom Archer comments:

1) This is an impressive, extremely useful and ground-breaking paper in its wealth of data and comprehensive approach.

2) The paper gives a viable road map for enabling OBGYN and MFM doctors to perform relevant hemodynamic profiles on their own patients for both immediate patient care and further research.

3) DeVore’s proposed “empowerment” of OBGYNs and MFMs is entirely appropriate given their already-established experience with ultrasound techniques

4) I have the following highly respectful (and hopefully constructive) comments about the paper and its conclusions:

A) The patients—ranging from 11 to 39 weeks of gestation-- were all studied IN THE SUPINE POSITION with PW and CW Doppler of the descending aorta to obtain the VTI, SV and CO measurements. Is it not possible—or even probable-- that much of the data obtained from supine patients in later pregnancy (after 20 weeks?) reflects the maternal circulation when the gravid uterus is obstructing the inferior vena cava and uterine venous return? This positional venous obstruction in the supine position would tend to reduce CO compared to what would have been observed in a lateral position in which the inferior vena cava and uterine venous return were not obstructed. I have read in other studies of pregnancy hemodynamics the speculation that the decrease in maternal CO observed after mid-pregnancy may be due to obstruction of the inferior vena cava, and the DeVore data seem consistent with that possibility.

B) The study in question was performed in 2023. Why were the measurements obtained in the supine position? Could the measurements have been obtained in both supine and lateral positions? Dr. DeVore obviously did exactly what I mention here in his 2025, which he also sent us and which I comment on.

C) Comments A) and B) would seem to make relevant my recommendations to systematically look for great positional variation in maternal CO (what I have called high “positionality” of CO) as 1) a source of confusion in the data, and 2) a possible way of identifying patients at high risk of maternal or fetal injury due to chronic and recurrent positional obstruction of uterine venous return.

D) Both Dr. DeVore and others have used the phrase (or similar phrases) of “cardiovascular maladaptation” to describe unfavorable hemodynamic situations during pregnancy. This phrase would seem to “blame” the cardiovascular system in some way for not adapting well to pregnancy, whereas I have been trying to emphasize that obstruction of venous return by the gravid uterus (a preventable event) may set in motion a “counterproductive compensatory response” on the part of the CV system to a forcibly reduced venous return and cardiac output. What I am encouraging is that we broaden the concept of “cardiovascular maladaptation” to include the concept of “counterproductive compensation” by the CV system for a reduced venous return and CO **which have a known cause, namely obstruction of the IVC by the gravid uterus.** This distinction

may seem trivial or “semantic” but I would argue that the advantage of the term “counterproductive compensation” emphasizes the fact that there may be a **correctable cause** of that compensation or “maladaptation” (namely obstruction of the IVC and OUVR.) Of course, this comment does not deny the fact that other more “intrinsic” defects of the maternal CV system may exist in some patients, which have nothing to do with obstruction of the IVC.

E) **This may be a useful conceptual model:**

1) CO and SVR measured WITHOUT IVC obstruction (e.g. LLD, although in 25% of patients CO is higher in the RLD position than in the LLD) may reflect the “native” or “intrinsic” or “locked in” or “fixed” maternal CV system.

2) CO and SVR measured WITH IVC obstruction (e.g. supine) may reflect short-term “compensatory” behavior of the CV system and the sympathetic nervous system (SNS) to maintain acceptable maternal blood pressure in the face of an acute reduction in venous return and CO.

F) The paper represents a lot of very valuable work, but it does not mention one aspect of the hemodynamic situation that I have tried to emphasize in my writing. This aspect is as follows:

1) Obstruction of the inferior vena cava and concomitant obstruction of uterine venous return (OUVR) not only decreases maternal CO but also increases uterine venous pressure—and this increased uterine venous pressure may cause more problems than the decrease in CO per se.

2) A corollary of this point is that positionally decreased maternal CO may serve as a marker for a dangerous hemodynamic situation characterized not only by decreased CO but also by increased uterine venous pressure, and it is not clear which of these factors might be dominant in causing maternal or fetal injury.

G) The inability of the investigators to obtain the ascending aorta VTI with their Doppler probe is very interesting, since the USCOM Doppler probe apparently is able to obtain the VTI from both the ascending and descending aorta. How do the investigators explain their inability to insonate the ascending aorta with the probe they were using? **This question is not meant to lessen the value of the study. But it is an interesting apparent difference between the capability of the USCOM device and the ultrasound device which DeVore et al used in their excellent study being discussed.**

H) It is very helpful and enlightening that DeVore et al show the difference between descending aorta VTI (and hence SV) as obtained with CW versus PW Doppler using the same Doppler probe (which was not the USCOM device probe). The VTI obtained with their probe in the CW Doppler mode is somewhat higher than that obtained with the same probe in the PW Doppler mode. This is a very interesting finding which educated me as to the fact that the VTI can be obtained with both PW and CW ultrasound modalities but that the VTIs may differ somewhat between the two modalities! It remains to be understood why the USCOM probe is able to insonate both the ascending and descending aorta, but the probe which DeVore et al used was only able to insonate the descending aorta.

I) DeVore provides a wonderful educational service in his paper of pointing out the agreement between his study and Vinayagam’s work measuring the CO in the ascending aorta. DeVore points out that the CO in the descending aorta which his group measured is approximately 28% less than that measured in the ascending aorta by Vinayagam, with the difference being due to the perfusion of the upper body, including the head. It is very satisfying and confidence-building to

see this compatibility between the two data sets from the two investigators.

J) Nothing in this review is meant to detract from the valuable of this work, which is a very important source of information and a very important roadmap for progress in improving pregnancy outcomes.