

Medical education research in obstetrics and gynecology



TO THE EDITORS: The recently published article “Implementation and Validation of a Retroperitoneal Dissection Curriculum”¹ describes the design and evaluation of a surgical curriculum in gynecology. The authors should be congratulated on an innovative method for teaching a difficult subject aiming to improve patient care. From a scientific point of view, however, there are several concerns about the publication of this type of research.

First, there is no generalizable concept that is being tested or explored and no underlying theoretical framework is used to explain what constituted the active ingredients that caused the observed effects. This makes replication highly difficult and limits the application of study results in other contexts significantly. In other words, evaluation of courses and curricula may be useful and of local interest but they rarely add anything to our existing knowledge base.^{2,3}

Second, as a scientific community we need to carefully evaluate study methods regardless of the topic being explored. The present study, for example, used a pretest posttest design to conclude that participants learned something when being taught. While the obvious nature of this conclusion may seem intuitive, it begs the question: is there any type of teaching and training that would fail to improve trainees’ knowledge and skills over time? And does this design allow us to make causal inferences?⁴

We should ask ourselves if we would accept the same conclusions in clinical research using the same study design and outcome measures as those used in the present study (and in many other education studies published in clinical journals). For example, would we accept a study that concluded that a new operative method results in improved outcomes based on differences before and after the intervention? Probably not, just as we should not accept the use of self-assessments, which repeatedly have proven to be poor indicators of actual skills or knowledge.⁵ In other words, if the journal and our community wish to take medical education research seriously, we need to apply the same standards as expected in clinical research to medical education research, while emphasizing the role of improving our conceptual understanding of a subject rather than concluding that training works or that trainees loved the course. ■

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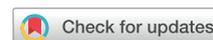
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REPLY



We thank Dr Tolsgaard for his thoughtful letter regarding our article “Implementation and Validation of a Retroperitoneal Dissection Curriculum.”¹ Although residency programs understand the need for simulation-based training, there are limited evidence-based curricula available, particularly in gynecology. Our study presented a comprehensive, multi-modal teaching approach to laparoscopic retroperitoneal dissection.

Dr Tolsgaard raised a concern regarding the lack of a theoretical framework guiding our research. Although not explicitly stated, we followed the framework of Zevin et al² for curriculum design and validation, including both cognitive and technical skills components. This framework draws on the theory of cognitive integration, explaining how transfer is improved when training incorporates both conceptual *and* procedural knowledge.³ Our curriculum began with a video-based lecture regarding *Why* (conceptual) task elements and technical model practice for *How* (procedural) task elements.

To Dr Tolsgaard’s second point about the assumption that teaching in any form leads to improvement in knowledge and skills, we contend this has yet to be proven in surgical

training. If Dr Tolsgaard truly believes this assumption, then what is the value of education research as a field? We argue that an important step in any study design is ensuring the validity of our assumptions. In our case, we tested the assumption that simulation improves knowledge and procedural skill and that those improvements correlate with improved real-world performance. This assumption is crucial in the context of simulation-based training and evaluation in the era of competency-based training.⁴

We agree with Dr Tolsgaard's final point about study design in educational research. Like pregnant patients, trainees are a vulnerable study population. We recognize the value of randomized double-blinded placebo-controlled trials; however, feasibility prevents this approach with medical trainees. We agree that self-assessment and learner satisfaction should not be primary outcomes of education research, and thus we designed our study with a primary outcome of the validated measurement of surgical skills transfer. As educators, we have to be cognizant of the effects of training and assessment on trainee wellness and engagement, so we believe that trainee satisfaction is a necessary component of programmatic assessment.

Finally, a genuine problem in health professions education research is the ability to reach the clinical audience involved in day-to-day trainee teaching. Most clinicians are unfamiliar with the concept of theoretical frameworks. As the *American Journal of Obstetrics and Gynecology* is a clinical journal, we deliberately left out the use of medical education jargon to make our article more accessible to those on the front lines. ■

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You can never be too prepared: ECMO for MCA



TO THE EDITORS: We have read with great interest the clinical review by Zelop et al.¹ These topics were very informative and important for many clinicians. From the perspective of the cardiologist, we have commented on the extracorporeal membrane oxygenation (ECMO) strategy.

As Zelop et al¹ mentioned, even if exceeding 4 minutes after collapsed, perimortem cesarean delivery (PMCD) should be considered for maternal cardiac arrest (MCA). At the same time, we recommend that physicians consider the enactment of ECMO as soon as possible. Recent American Heart Association guidelines admitted ECMO as one of the options for cardiopulmonary resuscitation (CPR), called extracorporeal membrane oxygenation-assisted cardiopulmonary resuscitation (ECPR). ECPR is usually suggested after conventional CPR has

been performed for more than 10 minutes. Considering the fact that the time to establish the system of veno-arterial extracorporeal membrane oxygenation (VA-ECMO) is 10–20 minutes, we should consider ECMO and PMCD decisions for out-of-hospital MCA patients as early as possible.

Obviously although many MCA cases occur in hospital settings and one fourth of those cases were related to anesthetic problems, we should select ECMO candidates promptly. For example, 18.6% of MCA cases (11 of 59) were due to venous and thromboembolic causes according to the Cardiac Arrhythmia Pilot Study (CAPS) trial, and unfortunately, many of them died (10 of 11, 90.9%).² In Japan, a national survey revealed that about 6% of MCA cases were related to a pulmonary embolism. Furthermore, a CHEER