thesiologists from staying in the magnet room during MRI scans. We have not found any studies of MRI-induced injury to healthcare personnel from long-term exposure to EMFs or any studies correlating exposure levels to disease. Anesthesia personnel who provide limited or occasional care in the MRI environment run a risk of exposure to EMFs.[3,4] Anesthesia providers should carefully consider their anesthetic technique to minimize the time spent in the MRI magnet room. In the future, exposure limits to EMFs should be recorded by anesthesia personnel to facilitate future epidemiologic studies to determine EMF exposure rates. More research is required in developing anesthetic techniques to minimize the EMF exposure limits.

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In Reply:

We appreciate the comments from Bryan *et al*. regarding our article[1] that is related to electromagnetic fields (EMFs) in operating rooms, but magnetic resonance imaging (MRI) magnetic rooms. The anesthesiologists have been exposed to a large amount of EMFs in MRI magnetic rooms because of the recent lack of EMF-safe monitors and machines in an MRI environment. However, there is no specific study about the amount of EMFs in MRI magnetic rooms related to the anesthesiologist and long-term effects of EMFs to the anesthesiologist in an MRI environment. We agree with your opinion that anesthesiologists should consider minimizing the time spent in the MRI magnetic room and should start an epidemiological study for the anesthesiologists working in an MRI environment.

European directive 2004/40/EC on occupational exposure to EMFs was to be implemented in the Member States of the European Union by 2008. Because of some unexpected problems, the deadline was postponed until 2012.[2] Now is the time, we think, for all anesthesiologists to be interested in their working environment, especially EMFs in operating rooms, MRI magnetic rooms, and intensive care units.

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Implicit Memory Phenomena under Anesthesia Are Not Spurious

To the Editor:

I read with great interest the article by Hadzidiakos *et al.*[1] in the August issue of ANESTHESIOLOGY. These investigators conducted a study of memory function under anesthesia using the process dissociation procedure (PDP), a method that my colleagues and I have used in the same context in the past.[2–4] In contrast to our studies, Hadzidiakos *et al.* report no evidence of memory function in terms of word stem completion test performance, a discrepancy for which the authors provide plausible explanations such as the depth of anesthesia and midazolam premedication. However, notwithstanding their null finding, one of the PDP models—the original—produced parameters suggesting the presence of controlled (explicit) and automatic (implicit) memory processes. By extending the model to include guessing parameters, the authors go on to show that the original model produces faulty estimates and that other published results using the original model are faulty. That is, Hadzidiakos *et al.* find no evidence of any memory processes in three of the four inspected studies when the extended measurement model is applied. They conclude that in these studies there was no contribution (*i.e.*, evidence) of memory at all and that past findings are spurious.

I take issue with this conclusion for several reasons. Foremost, a model that generates discrepant parameters depending on its assumptions or underlying structure should not invalidate the behavioral findings it attempts to model. When significant differences are found in patients' postoperative behavioral responses to old material presented under anesthesia *versus* new material not presented before, this difference is real and evidences memory for old material regardless of how the underlying process is labeled. Dismissing these behavioral observations ignores an overwhelming body of evidence in favor of implicit memory ("priming") phenomena in the cognitive psychology and neurology literature and surely cannot have been the intent of Hadzidiakos *et al.*

Although their critique of the original PDP model may be warranted in that the modeling heavily depends on (controversial) assumptions and proper test instructions,[5] the evidence for or against memory function under anesthesia is based on actual response data and not on models. In many of the anesthesia studies cited by Hadzidiakos *et al.*, and many more, response tendencies demonstrated memory for material presented under anesthesia, and the quest for understanding this phenomenon continues.[6] Therefore, it would be wrong to imply or believe that memory function under anesthesia is a spurious phenomenon. Second, the authors failed to include studies that used the extended PDP model and found evidence of automatic memory processes.[2,7] Although one study may not have properly implemented the PDP methodology and produced skewed estimates as a result,[5] another found robust evidence of implicit memory function under seemingly adequate levels of anesthesia based on patient response data and Bucher's PDP model.[2] It is not clear why this evidence was disregarded.

I commend the authors on undertaking their study and welcome their critical examination of a popular yet tricky methodological approach but regret their simplified argument and failure to distinguish between modeled and actual reality.

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In Reply:

In the reply to our article,[1] Prof. Kerssens makes the central assertion that "Foremost, a model that generates discrepant parameters depending on its assumptions or underlying structure should not invalidate the behavioral findings it attempts to model." This statement contains two common misconceptions.

The first misconception is that "behavioral findings" should be counted as somehow more direct and thus less problematic evidence of something compared with model-based analyses. However, this view fails to take into account the fact that there is no such thing as a model-free measurement. Every analysis makes assumptions about data. The set of these assumptions forms what is called a measurement model. Thus, every analysis is necessarily model based, and the measurement models may be more or less adequate. For instance, the analysis mentioned explicitly by Kerssens on the difference between responses to old material presented under anesthesia *versus* new material not presented before implies a measurement model comprising the assumptions (1) that responses to new words are exclusively based on guessing, (2) that responses to "old" words are exclusively based on memory and guessing, (3) that in the latter case, memory and guessing processes are strictly additive, and which implies (4) that the assumed underlying distribution of the evidence variable is rectangular.[2] All these assumptions may be inadequate. For instance, assumption (1) precludes strategic processing such as generating unusual words, which, however, has been observed before.[3] Assumptions (3) and (4) imply deviations from signal detection theory that have been criticized.[2] Another problem of this model is that "memory" is assumed to be a single homogeneous process that cannot be decomposed further, an assumption that obviously need not be adequate, and one that would not even allow for the simple distinction between automatic and controlled memory processes.

Second, it is not correct to state that we used one model, which generates parameters that depend on the assumptions of the model. Rather, we applied two different measurement models for the process-dissociation procedure, one of which has been shown to be more adequate than the other.[4] Obviously, the better of the two models should be used for data analysis, which is what our analyses clearly confirm. The use of an inadequate model has led researchers (and would have led us) to conclude that there was memory for intraoperative events, which in fact was not there.

A further point is that Kerssens wonders why we did not include studies that used the extended measurement model for the process-dissociation procedure and found evidence of automatic memory processes.[3,5] This point is well taken. We did not include these studies for several reasons. First, these studies were not instances of the point we wished to make, that is, using inadequate measurement models may lead to inadequate conclusions. Second, although we do mention studies that found evidence of memory for intraoperative events (see p. 301 in our target article[1]), it must be realized that our article was not meant to be a meta-analysis in which every single study on this issue was to be included. For instance, we did not include a study by Kerssens *et al.*[6] in which no evidence of memory for intraoperative events was found