for osteoarthritis, sensitivity analysis suggested that electroacupuncture might be associated with better outcomes. Furthermore, indirect comparison between electroacupuncture and manual acupuncture also indicates the same tendency. For example, in a study by Dr. Berman et al., both electroacupuncture and manual acupuncture significantly relieved knee osteoarthritis pain between weeks 14 and 26 compared to needle insertion at sham points and nonpenetrating mock electrostimulation, whereas in a study by Dr. Witt et al. manual acupuncture significantly improved pain at 8 weeks but not 26 weeks compared to superficial needling at nonacupuncture points. Although these data are preliminary, they suggest that electroacupuncture might be more effective than manual acupuncture for managing pain. However, more studies that directly compare the effects of these types of acupuncture on pain, and take into consideration pain severity, acupuncture point location (local vs. distant), treatment “dosage,” and follow-up period, are necessary.

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Competing Interests
The authors declare no competing interests.

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The Potency of Different Propofol Formulations

To the Editor:
With great interest I read the study by Le Guen et al. on the comparison of the potency of different propofol formulations that was published in February issue of Anesthesiology. The authors compared the dose of Diprivan® (AstraZeneca, Cheshire, United Kingdom), Propoven® (Fresenius-Kabi AG, Bad Homburg, Germany), and Lipuro® (B-Braun, Melsungen AG, Germany) alone or in combination with lidocaine, which was necessary to achieve induction of general anesthesia, measured by a bispectral index (BIS)–controlled closed-loop system. I have, however, some concerns about the methodology that may undermine the clinical validity of the authors’ conclusions.

The most reliable way to compare pharmacologic potency of different drug formulations is a crossover study with healthy individuals either in a single center or with unified laboratory assessments. Otherwise, interindividual variations in pharmacodynamics might reduce validity of the findings substantially. The authors themselves criticize other studies for “ignoring high interindividual variability of the dose–effect relationship.” Yet, they chose to conduct a multicenter study and included patients ranging from American Society of Anesthesiologists I to III. The resulting interindividual variability in both BIS and propofol sensitivity are confounding factors that influence the closed-loop system. Another point of concern is the dose measurement in multiple centers, which also suffers from a very low sample size. Especially Propoven® with saline was measured only in four patients. Any results based on this sample size are prone to high statistical variability.

Concerning data handling, the authors do not report whether data from patients who had not reached induction at 360 s (which can be seen in figure 2 of the original article) were used for the analysis. Because the primary study outcome was “the dose of propofol given alone or associated with lidocaine until the moment of induction,” this information seems quite relevant.

A patient’s BIS is prone to artifacts, and those can directly influence propofol dose administered by a BIS-controlled closed-loop system. Notably, “gentle manual assistance if Spo2 decreased below 92%” as described in the methods will influence the BIS, and the authors did not state how often and in which group this measure was applied. In addition, since pain delays the time until induction, the effects of formulation potency and pain-induced induction delay cannot be separated in the analysis. In conclusion, I am not convinced that the data presented by Le Guen et al. demonstrate clinically relevant differences in potency between propofol formulations.

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The author declares no competing interests.

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In Reply:

We thank Dr. Goddon for his interest in our recent study published in Anesthesiology1 about the comparison of different propofol formulations during induction of general anesthesia.

First, the aim of our study was not to perform a pharmacokinetic study. Our primary outcome was the required dose of propofol with or without lidocaine to achieve induction of general anesthesia. Induction was defined using bispectral index that indirectly measured the cortical effect of propofol infusion. A secondary outcome was calculated and measured propofol and lidocaine plasma concentrations as indicated in the article. This was performed only in Foch Hospital and not in other centers (for logistic reasons). No stratification was planned in the randomization and this explained an imbalance between the six groups. Our text was extremely cautious: “These results should be guardedly analyzed for several reasons: assays were done on a limited number of patients, blood samples were never taken during a steady-state period because induction is per se an unstable period and because closed-loop propofol administration consisted in several consecutive boluses at short intervals, arteriovenous difference is probably higher during such a period than during a maintenance period, […]”. A cross-over study, suggested by Goddon, cannot be considered in patients in comparison to healthy volunteers. This inevitably induces intervariability difference but represents real life.

The second point underlined by Goddon is the question of data handling when patients did not reach induction at 360 s. We have arbitrarily limited the x-axis of figure 2 (duration of anesthetic induction) to 360 s, but no data were retrieved in the analysis.

The third point is artifacts. Bispectral index has numerous possible artifacts; they were well described by Dahaba.2 Our inclusion criteria considered some of them.1 In the Materials and Method section, we allowed the possibility to “gently” ventilate patients in case of significant drop in oxygen saturation (SpO2 <92%) during induction. This rescue maneuver could modify bispectral index, if painful, but we did not record the number of such interventions.

Finally, we agree with Dr. Goddon: pain and time required for induction cannot be separated. Our article reported the differences for these parameters between formulations of propofol (long-chain triglycerides versus mixture of long- and medium-chain triglycerides) and between formulations mixed with either saline solution or lidocaine 1%.

As a conclusion, our method is probably not perfect but allows a standardization of anesthesia limiting human bias. We also want to draw anesthesiologists’ attention to the fact that there are extremely different formulations for propofol from one country to another and sometimes in the same country.*

Competing Interests

The authors declare no competing interests.

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References


Practice Guidelines for the Perioperative Management of Patients with Obstructive Sleep Apnea: Navigating through Uncertainty

To the Editor:

We read with interest the update of “Practice Guidelines for the Perioperative Management of Patients with Obstructive Sleep Apnea,” by the American Society of Anesthesiologists Task Force on Perioperative

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