CORRESPONDENCE

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Euthanasia in the Intensive Care Unit

To the Editor:—In their article, Truog and Berde[1] adopted the subtle distinction between "nonvoluntary" euthanasia (according to a surrogate's decision) and "involuntary" euthanasia (without a competent person's consent). They further stated that involuntary euthanasia has no ethical merit. Though everyone is (and should be) concerned about possible abuses, I personally find these clear-cut statements too short, especially when they apply to the incompetent, terminally ill patient.

In the intensive care unit (ICU), the decision to forgo treatment often involves patients who are comatose because of extensive brain damage or diffuse encephalopathy. In these circumstances, the distinction between the withdrawal of life support and/or the administration of large doses of sedative agents generally becomes tenuous. As an example, rather than wean from the respirator a patient who cannot breathe spontaneously (terminal weaning) and then administer opioids to relieve discomfort, is it not more appropriate medically to administer the medications while maintaining mechanical ventilation, so as to avoid any possible discomfort? This could be a matter of personal opinion.

An inquiry among European intensivists revealed that 36% (87 of 242) of the responders practiced euthanasia in patients with no real chance of recovering a meaningful life. Euthanasia was practiced more commonly by younger doctors; it was practiced less commonly in the South of Europe (and, in general, by Catholic rather than by non-Catholic physicians); but it was not more common in the Netherlands than in other European countries.[2]

Thus, euthanasia is practiced regularly in many ICUs for patients who did not explicitly request it. An important question is whether the surrogates should be involved in these decisions. Many European intensivists would say no, because the surrogates lack the appropriate medical knowledge and expertise. Moreover, these decisions may raise deep feelings at a later time. Many believe that the physician can represent the patient's best interests without seeking a surrogate's approval.[3]

May I emphasize that what American courts decide as acceptable is not necessarily what is universally appropriate on ethical grounds. In more general terms, legal issues and ethical issues should be kept distinct.

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In Reply:—Our purpose was to write a balanced critique of the euthanasia movement in the United States and to emphasize that greater involvement by anesthesiologists in pain management could diminish the perceived need for legalized euthanasia. We agree with Barnette, Wendling, and Heath that legalization of assisted suicide or euthanasia would present many serious problems. We are troubled by the intolerant and dogmatic tone of these letters, however. Good arguments and good reasons exist on both sides of this debate. Patients are legitimately concerned about the lack of attention that medicine has paid to comforting the dying. They are asking for more control over decision-making than the medical profession has been willing to give them. It is one thing to believe, as we do, that this is not the time to legalize euthanasia. It is quite another to refuse to acknowledge the reasonableness of those who believe otherwise. To do so is

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to risk the misunderstandings and polarization that have characterized the abortion issue.

Vincent points out that the distinction between active and passive euthanasia is fuzzy. As others have emphasized,[1] perhaps the fundamental question in the euthanasia debate is not whether our actions are "killing" (active) *versus* "allowing to die" (passive) but, rather, who is making the choice. As we move down the slippery slope from individuals choosing death for themselves to others choosing death for them, the ethical justifications become more difficult. Vincent shows just how far the Europeans have progressed down that slope.

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Pitfalls in Performing Meta-analysis: I

To the Editor:—We read with interest the meta-analysis by Sorenson and Pace[1] comparing the effects of regional and general anesthesia on morbidity and mortality in patients undergoing surgical repair of a femoral neck fracture. We have some concerns regarding the meta-analysis of 246 cases evaluated for the presence of deep venous thrombosis (DVT). The conclusions were based on data in the three articles by Davis *et al.*,[2] Davis and Laurenson,[3] and McKenzie *et al.*[4] Unfortunately, the two studies by Davis *et al.* appear to contain overlapping DVT data, as is stated in the methods section of the 1981 paper.[3] Also, although 132 patients were entered into the study, only 90 received labelled fibrinogen and only 76 produced "adequate scanning data" for analysis. Thus, only 116 cases (76 from Davis and Laurenson and 40 from McKenzie *et al.*) are available for analysis, not 246. If the 116 cases in the studies by Davis and Laurenson and Mackenzie *et al.* are pooled, DVT was diagnosed by ^{125}I-fibrinogen scanning or venography in 46 of 59 general anesthetic patients (78%) and 25 of 57 subarachnoid block patients (43%). This may prove to be statistically significant, and we would be interested in the results of a reanalysis of the data.

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