While we could not individually separate the three major non-size-based criteria (visceral pleural invasion, hilar atelectasis, and obstructive pneumonitis), Jones et al (reference 16 in our article) have clearly indicated that hilar atelectasis and obstructive pneumonitis rarely are the only criteria for staging non-small cell lung cancer as stage 1B.

Hung et al raised the possibility that our finding that T2P tumors > 3 cm in size is a poor independent prognostic factor for survival because these patients may have T2S tumors sizes much > 3 cm. From our patient database, the mean tumor size for patients with T2P tumors > 3 cm was 5.15 cm (median size, 4.5 cm; 95% confidence interval, 3 to 9 cm) and the mean tumor size of patients with T2S tumors > 3 cm was 5.07 cm (median size, 4.5 cm; 95% confidence interval, 3.2 to 8.5 cm; p = 0.4959 [nonparametric t test]). Thus, it is unlikely that the independent unfavorable prognostic significance of T2P tumors > 3 cm in size in our study is due to larger tumor size.

Another finding in our study was that patients with T2P tumors ≤ 3 cm in size had similar survival times to those with T2S tumors ≤ 3 cm and T2P tumors > 3 cm in size was an independent favorable prognostic factor, thus raising another question about whether the designation T2P should even be applied to tumors ≤ 3 cm. As stated by Travis et al (page 1387) on behalf of the International Association for the Study of Lung Cancer (IASLC) International Staging Committee (ISC) “the question of the effect of tumor size on the impact of visceral pleural invasion remains to be determined in studies of larger numbers of cases such as the prospective IASLC database” and “Study of larger numbers of cases with careful documentation of visceral pleural invasion will hopefully provide an answer to the question whether size has any impact on the significance of this T factor . . . . Hopefully these issues will be resolved in the prospective IASLC lung cancer staging project (page 1389).” Thus, it is not only worthwhile to perform an analysis to determine the prognostic significance of the T2P designation according to the newly proposed T factor sizes > 3 and ≤ 5 cm (T2a), > 5 and ≤ 7 cm (T2b), and > 7 cm (T3), but is also worthwhile to conduct such an analysis for tumor sizes ≤ 2 cm (T1a), > 2 cm, and ≤ 3 cm (T1b).

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Clinical Guidelines and Clinicians’ Intentions in End-Of-Life Care

To the Editor:

Guidelines that broadly interpret the principle of double effect can enable the practice of consensus and nonconsensual euthanasia (ie, physician-assisted death) under the premise of palliation. Kuschner and colleagues implemented guidelines following alleged euthanasia in four ICU patients so as to avoid wide-ranging interpretations about end-of-life care. However, these guidelines can inherently generate more confusion about clinicians’ intentions and actions when the recovery of transplantable organs is added as a treatment goal in patients dying in ICU. Such guidelines are recommended as good palliative care, although transplantable organs may be recovered from donors before fulfilling the legal definition of death.

Furthermore, Kuschner and colleagues state that “Opioids or benzodiazepines used to treat [discomfort] after withdrawal of ventilator support do not appear to hasten death. The important principle is that opioids and sedating medications should be titrated to achieve the desired effect of [comfort].” Titrating continuous infusions of opioids and/or sedatives for subjective symptoms such as “discomfort” and/or achieving “comfort” allow broad interpretations by clinicians of the desired effect and dosage.

One argument has been made to invoke the morally distinct action of “the devil’s choice” for those venturing into practices conflating euthanasia and physician-assisted death (an intended death) with the practice of palliation (a foreseen death). The use of continuous (vs intermittent) infusions of opioids and sedatives can also cause ambiguities and uncertainties regarding intentions and causations allowing for psychological acceptance of euthanasia as palliation.

However, many religions and cultures condemn intentionally hastening death. Clinicians are reminded that:

*The principle of double effect is at home in a tradition of morality which takes seriously the moral psychology of the one who acts. Therefore, a focus on intention (among other things) is included in any moral appraisal of human action … other things also matter, such as the moral nature or moral kind of an act (whether it is an act of deception or of honesty, of empathy or manipulation), the intention with which an act is performed (whether to alleviate pain or to end a person’s life, to teach or to misguide), the motive with which an act is performed (whether out of kindness or contempt, generosity or selfishness), and the kind of person we become when we act in one way or another (a healer or a killer, a teacher or a liar).

Intentions are private and often undisclosed. Neither the law nor practice guidelines can regulate the true intentions and safeguard the integrity of actions. Only bedside clinicians can.
Response

To the Editor:

Rady and colleagues1 have raised concerns about the implementation of our guidelines2 and clinicians’ intentions in end-of-life care. We acknowledge that our guidelines do not eliminate moral confusion about end-of-life care in the ICU, and we agree that a focus on intention should be included in any moral appraisal of human action. While our guidelines, template physician note, and order set do not regulate the integrity of clinicians’ actions, they do provide a normative basis around which consensus regarding best practices can develop. That is, they provide a standard framework for developing and communicating the goals of care and support consistency in efforts to achieve them.

Guidelines for the withdrawal of life-sustaining treatment and other palliative measures of last resort, such as palliative sedation, make a distinction between palliation and euthanasia not only on the basis of clinician intention (symptom relief vs patient death), but also on the basis of methods (use of sedative medications sufficient to relieve symptoms vs administration of lethal medications) and the definition of successful outcomes (removal of treatments that are no longer desired or do not provide comfort vs patient death).3,4 Moreover, the rationale for permitting patients and their surrogate decision makers to stop life support is based not only on clinician intention, but also on patient autonomy and informed consent, and the principle of proportionality.4,5 Miller and Truog6 have observed that this proportionate response in desperate circumstances without a more desirable and achievable outcome. Finally, it continues to be our impression that our guidelines, template physician note, and order set support patient autonomy at the end of life, as well as strengthen understanding about palliative care practices in our ICU, reduce ethical conflicts, and improve patient care.

The authors have no conflicts of interest to disclose.

The editors welcome letters to the Editor on topics of interest to members of the American College of Chest Physicians. Correspondence should be sent to: David A. Grueneivald, MD, Veterans Affairs Puget Sound Health Care System, 1660 S Columbia Way, S-182-GE, Seattle, WA 98108; e-mail: david.grueneivald@va.gov.

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Autosomal-Dominant Polycystic Kidney Disease

To the Editor:

I read with interest the article in a recent issue of CHEST (January 2009) by Li and colleagues.1 However, some points need to be clarified. First, the abbreviation ADPKD stands for autosomal-dominant polycystic kidney disease. Second, the authors think of ADPKD as being unlikely based on the absence of a family history.