CORRESPONDENCE

Solubility of Diethyl Ether

To the Editor.—In reviewing the literature prior to our recent studies on ether solubility (ANESTHESIOLOGY 24: 676, 1963), we found no reference to such solubility other than the classic papers of Haggard (J. Biol. Chem. 55: 131, 1923) and of Shaffer and Ronzoni (J. Biol. Chem. 57: 741, 1923) both of which gave blood/gas partition coeffiicents of about 15. Dr. C. P. Larson has called to my attention two other papers (Jones *et al.*: ANESTHESIOLOGY 14: 490, 1953 and Hattox *et al.*: ANESTHESIOLOGY 14: 584, 1953) on the same subject. These investigators using the mass spectrometer found a partition coefficient of 12 which is confirmed by our work. Recently, Lowe (personal communication) found a coefficient of 10.5 to 11 with flame ionization after separation of gas chromatography. These more recent findings of coefficients of 11 or 12 are obtained with a variety of techniques and, I believe, substantiate these values as opposed to the older one of 15.

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Tourniquet Pain

To the Editor.—A recent paper in ANESTHESIOLOGY, "Theoretical aspects of pain: bizarre pain phenomena during low spinal anesthesia," by Drs. de Jong and Cullen (Sept.–Oct. 1963, pages 628–635) challenge the interpretation of data which Dr. Deas and I published last year (ANESTHESIOLOGY 23: 287, 1962). Dr. de Jong also criticized our conclusions previously (Current Comment: ANESTHESIOLOGY 23: 881, 1962). I must comment upon these two critiques.

They mention our high incidence of tourniquet pain, quoting 63.7 per cent incidence which we reduced to 33.3 per cent by increasing the dose of tetracaine in the spinal injection. As they suggest, the incidence is lower than either of these figures; in our own experience in the Navy, we promptly lowered the incidence by adding epinephrine, by administering doses of tetracaine more proportional to the heights of the patients, and by keeping the level of the spinal around tenth thoracic. Nevertheless, the reason we did the study was that the incidence was appreciable; we cannot consider a phenomenon rare simply because no one has recorded its incidence. Every anesthetist who has given spinal anesthesia has seen this.

Drs. de Jong and Cullen believe that the pain is carried over small fibers which travel "around" the area of spinal block; this of course is the opposite of our theory, namely, that tourniquet pain goes "through" the block carried by fibers which are large and therefore become excitable before pin-prick sensation returns. Certainly, Dr. Deas and I did not prove that the sensation which the patients called pain went through the spinal. But one must note that our patients, at the time they perceived pain, still had analgesia to pin-prick to fifth thoracic level. We are forced to conclude either: (1) that touniquet pain enters the cord below fifth thoracic level upon nerve fibers larger than those transmitting pin-prick, or, (2) that it enters above fifth thoracic level on smaller fibers.

No one has demonstrated the existence of nerve fibers travelling along the sympathetic chain from the legs to enter the cord above fifth thoracic level. Kuntz (South. Med. J. 44: 673, 1951) was only able to find fibers from the legs entering the cord in the lower thoracic and lumbar regions. However, Gasser (Proc. Assoc. Res. Nerv. Ment. Dis. 23: 44, 1943) has reported that fast pain resists cocainization more than slow pain. Also, Arrowood and Sarnoff (ANESTHESIOLOGY 9: 614, 1948) have demonstrated convincingly the relief of pain other than pin-prick type by increasing the dose of local anesthetic. We have merely chosen the theory best supported by data. We do not deny that pain is transmitted through the sympathetic chain, but no one has shown that it travels so far.

If our patients who developed pain had