FIGURE Circuit to provide CPAP to the non-ventilated lung during one-lung anaesthesia. The patient end is connected to the non-ventilated lumen of a double-lumen endobronchial tube.

outflow tubing lies below the surface of the water in the syringe. Individual requirements for CPAP may depend on individual lung mechanics. This circuit permits fine-tuning of the CPAP level by simply adjusting the depth the oxygen outflow lies underwater.

We usually use an oxygen inflow of $1.0 \text{ L} \cdot \text{min}^{-1}$. At flows less than $2.0 \text{ L} \cdot \text{min}^{-1}$ we find the manometer measured pressure in the circuit is the same as the level set by the depth of the underwater outflow line (at similar low oxygen flows we feel it is not strictly necessary to incorporate a manometer in the circuit).

The "pop-off" valve and reservoir bag can be autoclaved or chemically disinfected, the remainder of the circuit is disposable.

We have used this circuit to improve oxygenation in over 100 thoracotomies in the past two years without any problem related to its use. It combines the ability to ventilate with the safety and physical advantages of underwater seal pressure.

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Respiratory complications in patients with upper respiratory tract infections

To the Editor:

The conclusions drawn in the article by Tait and Knight¹ must be viewed with skepticism.

This study was retrospective. Only patients having received a general anaesthetic were therefore included. For patients whose surgery was cancelled, the authors did not identify the criteria which were used for cancellation because of an upper respiratory infection. As a result, it is possible (and even likely) that patients who had the most obvious and serious symptoms of upper respiratory infection were not included. The conclusion that "there is

no increase in intraoperative complications associated with patients presenting with uncomplicated upper respiratory tract infections" is therefore, in my opinion, not justified.

The authors have failed to address the differences in symptomatology and duration which may occur with the various "viral" illnesses. It is well known that viral illnesses may last from 24 hours to several weeks. Viral illnesses also occur in epidemics. It is therefore possible that illnesses which were present in the Ann Arbor community at the time of this study were illnesses which were of longer duration than might be present in other communities at another time. It would have been helpful if the authors had characterized the endemic illnesses at the time of their study.

It is unreasonable to make the generalized statement that "if the anaesthetist decides to postpone elective surgery due to the presence of an upper respiratory infection, the postponement should be for a period exceeding two weeks up to a month or longer." There is no evidence whatsoever in this paper identifying the effects of anaesthetizing patients who had an upper respiratory infection longer than two weeks preceding surgery, and it is therefore particularly important to discount the extension of this generalization to read "up to a month or longer."

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REFERENCE

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REPLY

We are grateful for the opportunity to respond to Dr. Goresky's comments regarding our article.

We are sensitive to the inherent biases of retrospective studies and fully realise that a retrospective diagnosis of an upper respiratory tract infection (URI) is difficult. We are aware, and have stated in the text, that since we were not able to establish the frequency of cancellation due to the presence of severe URI symptoms our population may have suffered some preselection bias with respect to severity. Because of the potential for a preselection bias, we have deliberately and specifically drawn conclusions based on the patient presenting with an uncomplicated URI only and make no reference to the patient who may present with a more severe infection. Uncomplicated by our definition refers to mild URIs with no evidence of lower respiratory involvement or secondary bacterial infection. The statement to which Dr. Goresky takes exception, i.e., "there is

no increase in intraoperative complications associated with patients presenting with uncomplicated upper respiratory tract infections" is therefore perfectly justified given the limitations of this type of study. Incidentally, the frequency of case cancellation due to severe respiratory symptoms observed in a parallel study was less than five per cent, which suggests that any preselection bias was minimal. Obviously, it is difficult to determine symptom severity retrospectively; however, we feel that the symptoms expressed in our population would have been sufficient to warrant concern over potential intraoperative complications.

The clinical diagnosis of an URI is always difficult largely because of the fact that one particular clinical syndrome is not necessarily associated with one particular viral agent. Indeed, all the respiratory pathogens are capable of producing similar clinical expressions. Physicians, therefore, frequently make diagnoses based on the agent that appears most prevalent at the time and hence the term "cold" is often an ubiquitous term for a respiratory illness of uncertain aetiology. Obviously, a diagnosis made retrospectively compounds this problem and as we have stated, it is certainly conceivable that not all of our symptomatic population had true viral infections. Our criteria for URI was selected to encompass the typical symptomatology of the acute uncomplicated upper respiratory tract infection and although we concur that cases of omission and commission conceivably occurred we felt that the symptoms represented those typically seen in anaesthetic practice and which by previously defined criteria would have warranted careful evaluation prior to surgery. With respect to the possibility of an "Ann Arbor epidemic" at the time of study, we have contacted the University of Michigan's School of Public Health and verified that the temporal distribution of respiratory illness in the Ann Arbor area during this time period was consistent with the natural distribution of viral agents which make up the syndrome of the URI. Since it was apparently felt necessary to extrapolate these results to other communities in terms of viral prevalence, one would think that the generalized nature and distribution of viral agents observed in the Ann Arbor community at that time should be fairly representative of the country as a whole.

This study showed that children who were asymptomatic at the time of surgery, but who had a history of URI within the two weeks preceding surgery had a higher prevalence of intraoperative respiratory complications. Since our study evaluated only those patients with a history of URI within two weeks preceding surgery, Dr. Goresky takes exception to our extension statement to include the delay of surgery for "up to a month or longer." Although Dr. Goresky feels that this is unjustified, we included this extension based on observations from other studies. As we clearly stated in the discussion, Empey et al.¹ demonstrated ranging degrees of altered respiratory tract reactivity in a group of adults which lasted for up to six weeks following upper respiratory infection. In addition, McGill et al.² found a significant incidence of perioperative complications in children with a history of URI within one month prior to elective surgery. We feel, therefore, that the extension statement is not unreasonable given the fact that it is not merely our opinion but is supported by a number of other studies.

As Dr. Goresky correctly points out, our study was a retrospective one. In many cases there is a tendency among physicians to dismiss retrospective data because of the inherent potential biases of case selection. This is unfortunate, since controlled retrospective studies, although certainly not defini-

tive, provide an important platform for furthering a hypothesis. Since a study of the effects for anaesthesia on intraoperative complications in patients with URIs posed a potential for increased complications a conservative approach appeared warranted. It was for this reason that a retrospective study was employed rather than a prospective one. We have attempted to limit any bias in this study to a minimum and although we willingly concur that some preselection bias may have occurred, we have limited our conclusions to the patient with uncomplicated infections only. This paper does not serve to dispute dogma, but hopefully represents a basis for further work directed at this perennial clinical problem.

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Continuous epidural infusion of ketamine during labour

To the Editor:

Naguib *et al.* reported the successful use of a small epidural dose of ketamine for postoperative analgesia.¹ Systemic side effects, motor block, vasoplegia or respiratory depression were not observed. Knowing that ketamine is a safe drug for use during labour,² we decided to evaluate the usefulness of a continuous epidural infusion of ketamine for obstetrical pain relief.

Four primiparae, ASA physical status I, requesting pain relief during normal early labour were studied. The study was approved by the University Committee and informed consent was obtained from each patient. In two cases an epidural catheter was inserted at the L₃-L₄ level. A bolus of 30 mg ketamine hydrochloride in 10 ml saline was given, immediately followed by a continuous infusion of 30 mg ketamine/10 ml saline/hour. In the two other cases one catheter was inserted at the L₁-L₂ level and a second at L₄-L₅. A solution of 30 mg ketamine in 3 ml saline was administered through the top catheter, immediately followed by a continuous infusion of 30 mg

ketamine/3 ml saline/hour. The second catheter was intended for use in the second and third stages of labour. Pain scores were obtained using a linear analogue pain scale. Blood pressure, heart rate and respiratory frequency were monitored.

None of the patients reported adequate analgesia at any time during the study. The administration of local anaesthetics was necessary within 45 to 60 minutes, due to insufficient analgesia. Furthermore, all patients complained of blurred vision, felt unpleasant and had mood changes.

Although we did not study a large number of patients, epidural ketamine does not seem to be appropriate for pain relief during labour. Analgesia appears to be insufficient and is accompanied by undesired systemic side effects which seem to be unrelated to the epidurally infused quantity of ketamine.

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REPLY

Thank you for the opportunity to reply.

It has been demonstrated that ketamine interacts with opiate receptors, and that its agonistic action at these receptors is responsible for its analgesic effects.¹⁻³ Since the epidural injection of opiates has proved to be of little utility for the relief of labour pain,⁴⁻⁷ it is not surprising that ketamine was not as effective as well. High doses of morphine (7.2 mg) provided satisfactory analgesia for six hours in 66 per cent of patients only for the first stage of labour in one study,⁴ whereas morphine 2-5 mg was unsatisfactory and did not relieve the pain of labour.⁵ In general, epidural narcotics have proved to be inferior to even dilute concentrations of local anaesthetics. Epidural meperidine 25 mg provided inferior analgesia to 0.125 per cent bupivacaine,⁶ whereas increasing the dose to 50 mg provided good relief in only 50 per cent of patients.⁷

In contrast, epidural morphine was effective in controlling postoperative pain with duration of relief of up to 24 h reported.⁸ The efficacy epidural ketamine in controlling postoperative pain has also been reported.⁹⁻¹¹ Nevertheless, the small number of patients (4) reported by Schneider and Diltor is insufficient to draw any conclusion regarding the efficacy of ketamine, since variability in the duration of analgesia has been observed with