problem of stratification and selection for admission to those units.

Finally, it is worth emphasising that not all investigations are of unproved value and that effective strategies for managing high risk patients do exist. We can avoid surgery altogether, and, indeed, quantification of risk may be particularly important in those operations, such as carotid endarterectomy, in which the benefit of surgery disappears at a given (and low) rate of perioperative complications.⁵

As we stated in our last paragraph, further studies are urgently needed; these should not only assess methods of stratifying risk but also evaluate their effectiveness in reducing morbidity. Until such evidence is forthcoming, our article summarises the best methods available.

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Treatment of acute mountain sickness

EDITOR,—Hans-Rudolf Keller and colleagues recommend descent as the treatment of choice for acute mountain sickness but show that both treatment in a portable hyperbaric chamber and administration of dexamethasone (which was the superior treatment) may facilitate descent by improving symptoms.¹ Pressurisation produced only short term benefit. A previous controlled trial of treatment in a portable hyperbaric chamber by the same group also failed to show any long term benefit of pressurisation in people with acute mountain sickness.² Spending longer in the pressure bag has not been shown to confer additional benefit.³

Despite the lack of long term efficacy the use of portable hyperbaric chambers has become widespread, and much has been made in the lay press⁴ of the anecdotal reports of symptomatic relief to which Keller and colleagues refer.¹ In Britain this popularisation of "the life saving pressure bag" has resulted in trekking agencies feeling under pressure to make sure that these portable hyperbaric chambers are available on package holiday treks and to state that they are in their advertising to potential clients. Unfortunately, there is confusion among the public about the therapeutic role of these portable hyperbaric chambers. At least one potentially avoidable death has occurred. In 1991 a European trekker died at 5100 m on the Mera La in eastern Nepal after three days of intermittent treatment in a portable hyperbaric chamber without an attempt at descent (personal observation). Descent might have been life saving.

Some 20 000 people travel from Britain to Nepal each year on trekking holidays, and many more go to India, Pakistan, South America, and east Africa, where they may trek to a considerable altitude. General practitioners, who provide travel immunisation for their patients, should familiarise themselves with the recommended treatments for altitude illness and ensure that their patients are informed about mountain sickness and its manage-

ment. Trekking groups that carry portable hyperbaric chambers should use them only to facilitate descent and not as an alternative to descent.

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Management of genital candidiasis

Review omitted issues on recurrent thrush

EDITOR,—The British Society for Medical Mycology's review on the management of genital candidiasis fails to bring out some important points.¹

Firstly, the vulval vestibulitis syndrome is not included in the section on differential diagnosis. This, however, is the commonest misdiagnosis in our clinic for patients with recurrent thrush and must be considered in any patient who complains of superficial dyspareunia.²

Secondly, the authors suggest that "recommended regimens are not clearly related to difference between drugs." Neither are they clearly related to differences between patients. Unfortunately, almost all comparative treatment studies have been in unselected women. In the section on management of recurrent disease the authors mention that recurrence may be due to sequestration of fungal hyphae and spores in the deeper layers of the vaginal epithelium, which then regrow when conditions are favourable. This "vaginal relapse" hypothesis implies that it may be possible to prevent further attacks by intensive antifungal treatment. This needs to be tested in prospective comparative studies specifically in women with recurrent disease.

Evidence suggests that changing the contraceptive method to medroxyprogesterone acetate may be beneficial in recurrent disease.³ In our experience, medroxyprogesterone acetate is better than suppressive antifungal treatment in selected women. This is currently the subject of prospective study in our department. Requests for support from the pharmaceutical industry for both this and other studies of hormonal treatments for recurrent vaginal candidiasis have been successful.

The controversy over whether sexual transmission causes relapse in recurrent disease is discussed at some length in the article. The authors do not mention that sexual intercourse may predispose to the development of symptoms by causing epithelial damage. This is indicated by the common clinical entity of "honeymoon thrush" and the association between frequency of intercourse and symptomatic attacks of vaginal candidiasis.⁴ In women with recurrent vaginal candidiasis it is important to emphasise that intercourse should be slow and unhurried, with extra lubrication being applied to the penis. The possibility of sexual transmission should be "de-emphasised" so that further damage does not occur in an already strained relationship.

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Chlamydial and gonococcal infections are rare causes of vaginal discharge

EDITOR,—It is unfortunate that the British Society for Medical Mycology lists chlamydial and gonococcal infections as part of the differential diagnosis to be considered in the management of vaginal discharge. While both may rarely cause frank cervicitis and thus be a direct cause of discharge, patients are much more likely to have associated bacterial vaginosis (or, less commonly, trichomoniasis). It is frustrating that this list, with the implication that gonorrhoea or chlamydial infection is a common direct cause of vaginal discharge, constantly appears in articles and textbooks. This misconception leads to requests for investigations that may be unnecessary and to inappropriate antimicrobial treatment.

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Role of viral infections in exacerbations of asthma

Study supports results of audit by General Practitioners in Asthma Group

EDITOR,—Sebastian L Johnston and colleagues¹ highlight what patients with asthma have been saying for years: that asthma gets worse with colds. The General Practitioners in Asthma Group carried out its second national audit of attacks of asthma and looked at 2332 attacks in patients of all ages. Factors that precipitated or preceded an attack were upper respiratory tract infection (1511 attacks), non-compliance with preventive treatment (151), "stress" (113), and allergy (103). This supports the view that upper respiratory tract infection is the dominant precipitating factor in asthma and deserves more attention.

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Allergy must also be a factor

EDITOR,—Sebastian L Johnston and colleagues from Southampton report impressive data supporting an association between viral infections (predominantly rhinoviruses) and exacerbations of asthma among children of school age.¹ The association they observed was so strong that it is

not surprising that they imply that there could be a causal relation between viral infection and exacerbations of asthma. They recognise that the argument that rhinoviruses could cause exacerbations of asthma is not new. They do not, however, mention that there have been multiple unsuccessful attempts in the past to show experimentally that these viruses can induce attacks of asthma (D A Tyrrel, W Busse, C Reed, J Gwaltney; separate personal communications). Indeed, the only model in which rhinovirus has been consistently shown to produce features of asthma in the lung is one combining viral infection with allergen challenge.^{2,3}

Johnston and colleagues do not state which of the children were allergic at the time of the study. Studies at Poole General Hospital (64 km from Southampton), however, showed that being allergic to dust mites was the strongest risk factor for developing asthma⁴ and for being admitted to hospital.⁵ Indeed, it has been reported that atopy in the cohort studied was associated with "lower FEV₁ [forced expiratory volume in one second], greater within day and between day variation in PEF [peak expiratory flow] and greater severity of respiratory symptoms."⁶

For at least two reasons it seems inevitable that allergy to common inhalant allergens and rhinovirus infection are cofactors for attacks of asthma in childhood—because of the results of the experimental studies and because these two risk factors are too common to be distinct. In our study of children presenting to the University of Virginia's paediatric emergency room with acute episodes of wheezing we found that the combination of rhinovirus culture and IgE antibody to common allergens was the strongest risk factor of wheezing.⁷ More recently, we have confirmed this association by using the polymerase chain reaction to detect rhinovirus.⁸ Thus, although the reported association between exacerbations of asthma and viral infections is striking, the group from Southampton should recognise that the children almost certainly have to be primed to respond in this way. The commonest well recognised cause of priming is exposure to a foreign protein to which the children have made an immediate hypersensitivity response, and this is the only model for which there is convincing experimental evidence.

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Sexual health, GU medicine, and a non-descriptive title are all appropriate names

EDITOR,—C Sonnex and colleagues voice concern that the use of the title sexual health clinic might further increase the stigma of genitourinary medicine clinics.¹ Although this is important to question, the reverse may equally be true.

Their survey of a small sample of clients already attending their clinic has limited potential to discriminate which of these possibilities is actually the case. It is not surprising that most clients attending a clinic called clinic 1A prefer it to have this title: after all, that is the clinic they have already chosen to attend. Also, as the survey includes neither demographic data nor information about sexual health needs, it is not possible to know whether the study group represents a population to whom we wish to target genitourinary medicine services. It is important to know about the preferences of those who are not already using these services as well as those who are. This is especially true for target groups.

The authors rightly suggest that terms such as "sex" and "genito" may cause clients embarrassment when they attend a clinic, but their survey showed that abbreviating "genitourinary" to "GU" made little difference to patients' choice. Furthermore, the nature of the potential stigma attached to each term may be different and will depend on culture and title—for instance, the word "sex" is far less taboo than it was just 10 years ago.

The search for an ideal title for genitourinary medicine clinics will be difficult as some degree of taboo will always be attached to such clinics. The terms "sexual health" and "genitourinary medicine" are not mutually exclusive. Wise providers will exploit each term to its best advantage—for instance, a clinic may use the term "sexual health" in its advertising, "genitourinary medicine" for liaison with other medical specialties, and a non-descriptive title on site.

The *Health of the Nation* placed sexual health firmly on the purchaser-provider map; genitourinary medicine would be wise to remember that it is not the only provider vying for its slice of the sexual health pie.

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Male partners of women with pelvic infection should be traced

EDITOR,—Minerva asks "Should the male partners of women with pelvic infections be referred for contact tracing?" and concludes that, although the concept "may seem a good idea," it is impractical.¹ We believe that, however difficult, it must be tackled.

The evidence is unquestionable. Kinghorn *et al* showed that, of 34 male contacts of women with acute salpingitis, 30 had urethritis.² They concluded that male contacts should be routinely investigated irrespective of the presence of symptoms or of the microbiological findings in women. Jacob *et al* referred to male partners as "a forgotten factor in pelvic inflammatory disease."³ In their study 46 of 58 men identified as partners of women with pelvic inflammatory disease had nongonococcal urethritis, of whom 36 were asymptomatic. Kamwendo *et al* reported an incidence of sexually transmitted infections of 59.7% in 200 male partners of 196 women with pelvic inflamma-

tory disease.⁴ No symptoms of urethritis (dysuria, urethral itching, or discharge) were volunteered by 96 of the 117 infected men.

To make a true impact on the epidemic of pelvic infection and the resultant morbidity we must examine and treat the male partner. The question is how to achieve this. Firstly, all clinicians who have a role in the management of women presumed to have pelvic infection must assume greater responsibility for detecting asymptomatic sexually transmitted disease in the male partners. Secondly, a public awareness campaign acknowledging that pelvic infection may be a complication of sexually transmitted diseases and addressing the asymptomatic nature of some of these diseases is long overdue. Sexually active men should understand that infection with *Chlamydia trachomatis*, the commonest identifiable cause of pelvic infection in the developed world, is asymptomatic in over half of men carrying the bacterium.⁵

Others have risen to the challenge. In Sweden an intensive education programme in which the importance of contact tracing was emphasised reduced the incidence of salpingitis by 40% over 10 years. In Britain the infrastructure (in the form of the genitourinary medicine service) already exists to screen for sexually transmitted diseases and to manage partners of men and women who might be infected. Clinicians should use it to its full potential.

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Open herniorrhaphy is safer and cheaper than laparoscopic surgery

EDITOR,—R S Taylor and T M Loosemore write that I failed to make a case against laparoscopic inguinal hernia repair.¹ I maintain that we do not need the endoscopic method when we have an open mesh technique that can be performed under local anaesthesia, is suitable for patients with considerable comorbidity, and has a rapid recovery rate and the lowest recurrence rate reported. The patient is awake and feels safe during and after the procedure. What more would a patient wish for?

I am not against comparative trials, but there are sufficient data suggesting that prospective comparative trials might be unnecessary. Common sense is still needed in surgery. The most recent multicentre report of elective laparoscopic hernia repair in 3229 carefully selected patients, reports a rate of major complications of 1%; two deaths (due to myocardial infarction and liver failure); one case of vascular injury, 53 cases of neuralgia, 37 of urinary retention, 84 of haematoma, one of injury to the colon, five of small bowel obstruction, two of bladder injury, and two of hernia at the site of the trocar; and a recurrence rate of 2% (mean time to recurrence 22 months).² These are appalling results for elective correction of a benign condition. The long term risks of intestinal obstruction