tunity, in that many ex-trainees are now principals in local practices; clearly, their referral patterns have changed.

Frequently, busy general practitioners will spend two or three minutes phoning the department to refer patients or request an immediate appointment, and this is always 'rewarded' (subject to the patient's informed consent) with a detailed report of management, microbiological diagnosis, and (where appropriate) whether a partner has been treated simultaneously. This has been found to be a very effective means of communication, while protecting the patient's absolute rights of confidentiality, which are defined by parliamentary statutes.

The key issue in communication is understanding and a commitment to share. Perhaps if more vocational training schemes had the opportunity for SHO appointments in genitourinary medicine, a wider perception might be achieved. This would increase the understanding of the importance of preventing the re-infection of gynaecological infection, and of contact-tracing in disease control in populations. There could be a much greater awareness of the very large numbers of patients with dangerous genital tract infections who have no apparent symptoms.

Such communication and understanding may increase the benefit to both the individual patient and the young adult population. Should this be achieved, surely it would produce a significant contribution to the *Health of the Nation* targets.[2]

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The methodological quality of megatrials

Sir,

In his discussion of the methodological quality of megatrials, Bruce Charlton (July *Journal* p 429) should have taken more time to study the content of our paper on the type and quality of randomized controlled trails (RCTs)[1] before dismissing it as an example of a 'generalized interpretative approach based on statistical criteria and checklists'.[2] It is a far more robust approach than he suggests.

Empirical studies have demonstrated that bias (systematic error) is greatest when inadequate randomization occurs, in the form of defective or unclear concealment of treatment allocation.[3] The consequence is that poorly randomized trials yield exaggerated treatment effects of between 30% and 40% when compared with properly randomized trials. These less rigorous studies are more likely to result in 'false positive' or exaggerated claims of efficacy (concluding that there is an effect when in fact there is not).[3]

In contrast, Charlton cites his own, non-empirical work when suggesting a 'knowledge of the identity and importance of specific interfering causes' in the interpretation of an RCT. He presumes that it is always possible to identify such causes when in clinical settings this is frequently not possible.[4] For this reason, criteria have been developed as a check against selection, performance, exclusion and detection biases in RCTs.[5]

While acknowledging that there is no perfect method in the assessment of RCT quality, it seems that the method adopted by the Cochrane Collaboration and used in our study has the benefit of being valid, clear and straightforward.[5]

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Is there evidence that megatrials are based on a methodological mistake?

Sir,

In his discussion paper (July *Journal,* p 249) Bruce Charlton advises us of the substantial problems with megatrials. There may well be such problems, but we are concerned about the lack of evidence he uses to support his argument. In particular, Charlton provides no evidence to support his notion that the conduct (and presumably reporting) of megatrials are somehow worse than for smaller trials. We are not aware of any evidence to support this viewpoint. Furthermore, the paper takes up almost three pages and nowhere is there a definition of a megatrial. Does Charlton define a megatrial as one involving 500, 1000 or 3000 patients?

Although it is important to air viewpoints, there is evidence to indicate that opinion can lag behind the evidence by up to 10 years.[1] We wonder whether this is another example.

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Summative assessment

Sir,

I read with interest Campbell's and Murray's article on summative assessment (July *Journal*),[1] yet I cannot see how they came to the conclusion that registrars who failed were not competent to enter general practice. If this assessment is to identify registrars who fall below the standard for minimal competence, then there needs to be a definition of what minimal competence is. The authors obviously failed to do this with the MCQ paper from the Royal Australian College of General Practitioners, despite the use of the Angof