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Educational outreach to improve vaccination uptake

Siriwardena *et al*¹ should be commended on their trial, which attempts to improve influenza and pneumococcal immunisation rates, as this area is a perennial challenge for every general practice. However, their conclusion that, 'this study demonstrates that education delivered to practice teams ... may lead to improved outcomes for patients', is more spin than substance.

First, though not stated, I can only assume that the null hypothesis for the study was that educational outreach has no effect on increasing influenza and pneumococcal immunisation rates in the over-65s in GP practices in the Trent region. With their reported results they found no statistical difference between the intervention and control groups with regard to an increase in influenza vaccination uptake, but there was a statistically significant increase in pneumococcal vaccination rates. Thus, with this adequately powered study, at least for influenza vaccination, they were unable to reject the null hypothesis and so, with respect to their conclusion, at best they can only conclude that 'this study demonstrates that education delivered to practice teams ... may or may not lead to improved outcomes for patients'.

Secondly, the authors point out that a national and local campaign, coupled with financial incentives on influenza vaccination, is the most likely explanation for their inability to find a statistical difference in the uptake of influenza vaccination between the intervention and control groups. They later tell us that there are no such initiatives for pneumococcal vaccination. Surely then, with the current evidence, the conclusion should be that, 'in the absence of a national and local cam-

paigned and financial incentives on vaccination, this study demonstrates that education delivered to practice teams ... may or may not lead to improved outcomes for patients'.

Lastly, the authors were optimistic in expecting a single educational outreach event to result in healthcare workers changing practice. Though they themselves have admitted that the intervention was of short duration, this should have been addressed from the start, as it is already known that attempts to change practice need to be repeated.²

What this paper does is to add our the current knowledge that, in changing clinical practice, there are no quick fixes.

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We note that in the paper by Siriwardena *et al*,¹ as in many others adopting 'educational outreach', details of the technique were ascribed to the paper by Soumerai and Avons.² However, the description in the latter paper is sketchy at best. It seems to us unlikely that a single GP will present precisely the same subject to a number of practices without deviation from a prepared script.

The exercise would seem more cred-

ible if the details of how this intervention was developed and received in pilot studies were included. There is a risk that we might accept lower standards for evaluating an educational intervention than for evaluating pharmacological studies.

Furthermore, despite referencing several papers on the methodology of cluster trials it is most unclear whether the paper by Siriwardena *et al* in fact used correct methodology. Reference is made to Poisson regression, but unless practices are included in the model as a random effect the analysis is likely to underestimate the width of the confidence intervals. If a random effect was included then we need to know whether a marginal or cluster-specific method of estimation has been used; since, for non-linear models, they estimate different population parameters.

It was unclear how baseline rates were included in the model and whether differences in baseline dispensing status had been allowed for, despite having been highlighted in Table 2. Possibly because we were unclear how the results were calculated we found it rather strange to see the results expressed as odd ratios, when Poisson regression results in relative risks.³

Reporting of cluster trials is more difficult than for individually randomised trials, but following a protocol such as the revised CONSORT statements would make the explanation of such trials easier to follow.⁴ The clarity was not helped by the apparent omission in the paper of Table 3, which contained the results.

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Author's response

Wong incorrectly states the conclusion, arrives at the wrong null hypothesis and also fails to understand the message of our study.¹ We used 'may' to mean 'expressing ability, permission, freedom, possibility, etc.' (Chambers' Dictionary), and to qualify our conclusion in the discussion. To add 'or may not' is redundant.

The correct conclusion, which was clearly stated, was that 'Practices where primary care teams received an educational outreach visit demonstrated a significantly greater improvement in uptake in high-risk groups for pneumococcal, but not influenza, vaccination.' The null hypothesis was that an educational outreach visit to volunteer primary healthcare teams has no effect on increasing influenza and pneumococcal vaccination rates in high-risk groups (patients aged 65 years and over and those with coronary heart disease, diabetes or splenectomy). The study was adequately powered for both influenza and pneumococcal vaccination as explained in the sample size calculation. Unfortunately the main body of results (Table 3), which detailed the effects of the study, was omitted due to an editorial error, but can be accessed on the *BJGP* online at www.rcgp.org.uk.

In reply to Wong's second point we think this is too specific. There are many other factors that could swamp an intervention. On his last point we

agree that a short intervention worked and that this may be surprising. But that is what we found. The classic review by Oxman *et al*² supports the use of educational outreach and multifaceted interventions. When a team approach to quality improvement is needed, as in the case of influenza and pneumococcal vaccination, an educational intervention involving the team and using a systems approach is a rational method that we demonstrated could work.

Campbell and Jiwa are correct in stating that the intervention could not have been precisely the same on each occasion. The educational session, while broadly following academic detailing principles described in the paper, enabled teams to identify barriers to change and begin to try and address these within their own practice. The role of the educational outreach visitor was to try and facilitate this, rather than impose an external set of solutions. This approach recognised the professional dominance model of behaviour change and acknowledged that promoting ownership of the process of change among the individual professionals within teams was a key factor in influencing change.³ We seem to have over-compressed the description of the results section. In fact we included the strata as fixed effects rather than as a random effect as this seemed more natural to us. We did indeed include baseline rates as a covariate. We cannot think of any reason why we wrote odds ratio when we clearly meant to write risk ratio and apologise for this.

Complexity theory is one of several models that may explain our approach and its effects.⁴ The notion of systems and organisational changes to improve and sustain quality,⁵ which explains the outcome of the educational outreach in this study, is, we believe, far more attractive than repeated education, which ignores the possibility that practice teams will continue to learn for themselves.⁶

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GPs with special interests

I am appalled to see suggestions that general practitioners could work as semi-specialists.¹ The days of the specialist working on his own ended in England 50 or more years ago: the modern specialist is part of a team, with colleagues and equipment to match. If a GP can spend part of his time as a member of such a team, as I did, he can be a valuable member of it while being a true generalist during the rest of his time: I even composed discharge letters as a hospital doctor to myself as a GP!

There may be conflicts of timetables and monetary problems and the present pace of hospital inpatient medicine is not well suited to part-timers but what we should avoid is having someone who is not a truly specialised specialist. He will be looked down on by the real specialists and will not be sufficient for the GP who needs a second opinion or the use of some special skill.

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Reference

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Chronic fatigue syndrome/ME

In previous correspondence,¹ I challenged the trivialisation of chronic fatigue syndrome (CFS), and the generalisation and speculation in an editorial on chronic fatigue.² The authors dismissed my arguments as, in their opinion, I had not demonstrated that I 'was prepared and able accurately to read and interpret a scientific article'.³ I consider this remark to be unfair and unjustified.

I shall discuss each of their points in turn. First, they denied that by referring to the illness as 'fatigue or its synonyms', they were trivialising 'the suffering of patients with PUPS (persistent unexplained physical symptoms)'. The authors must be aware of the controversy surrounding the word 'fatigue'. As one affected surgeon wrote: 'there is nothing in your experience in medical school, residency, or practice with its gruelling hours and sleep deprivation that even approaches the fatigue you feel with this illness. Fatigue is the most pathetically inadequate term'.⁴ Other writers on the subject recognise this, which is probably why most tend to describe the main symptom as profound, debilitating or disabling fatigue. But this was not the case here. The authors clearly equated CFS with (normal) tiredness and chronic fatigue. Elsewhere, they referred to 'commonplace symptoms' and in their response, again wrote about 'fatigue and its synonyms'.

I referred in my letter to symptoms such as nausea. Their response was to state that I had 'missed the point', because other physical symptoms are also found in patients without fatigue. 'Far from validating CFS ... they undermine the authenticity of CFS as a discrete syndrome'. I did not claim that CFS was a discrete syndrome; I was discussing the trivialisation and misrepresentation of the illness. As for additional symptoms, the current diagnostic criteria require the presence of four symptoms in addition to profound fatigue. These include sore throats and tender cervical or axillary lymph nodes. I referred to nausea because a study concluded that it was one of two symptoms that 'would appear to strengthen the case definition of CFS'.⁵

Also on the issue of trivialisation, the

authors insisted that: 'at no point did we describe the symptoms as "normal functioning"'. However, in their editorial, they wrote that some patients 'may translate physiological manifestations of unhappiness (or even of normal functioning) into symptoms of illness'.

The authors continued: 'careful reading of the editorial will show that Dr Goudsmit is mistaken in her belief that we subscribe to the theory that somatisation of mental illness is the cause of PUPS'. They also challenged my view that they had portrayed patients as 'unhappy, prejudiced attention-seekers' who exaggerate their symptoms.

I concede that the authors did not describe patients with CFS in those exact terms. However, I did not mention the 'somatisation of mental illness', just somatisation. They referred to the translation of physiological manifestations of unhappiness, the 'somatisation of mental anxiety', and depression.

Somatisation is not a clearly defined concept. Some use the term to describe medically unexplained symptoms and functional abnormalities of unknown aetiology. Others, however, perceive it as the somatic expression of distress that 'may serve the individual to attain certain gains; psychological, social or both'.^{6,7} The former is descriptively neutral, but the latter clearly links it to psychological problems. One might argue that in their account of PUPS, the authors were essentially discussing the modern concept of somatisation.⁷ However, some comments suggest that they were also thinking in more traditional psychiatric terms. For example, there were references to the gains involved in adopting victim status, the wish to legitimise symptoms, the role of the media, and the attention-seeking pressure groups.

My interpretation of the editorial was not just influenced by the words and their context, but also by the references. Take the book by Elaine Showalter (their reference 23), which included CFS as an example of 'hysterical epidemics'. I think she would probably recognise my description of these patients, as would Barsky and Borus (their reference 25). They too included CFS and related this to somatisation and the 'amplification' of symptoms. Like Stanley *et al*, they discussed the various gains associated with victimhood and the dubious role

of support groups. In short, the authors used the language of somatisation and alluded to various psychosocial problems. I questioned that.

The authors denied they were referring to a homogeneous population but there was no acknowledgement of the heterogeneity of either CFS or the population reporting unexplained symptoms. In paragraph 4, the authors state that patients with PUPS believe themselves to be ill ... and 'usually, but not always, are depressed and/or anxious'. There was no discussion of the subsets with abnormalities consistent with ongoing disease. No distinctions were made between people reporting fatigue and, say, dyspepsia or headaches. There was an emphasis on shared characteristics; hence my comments.

I was also criticised for describing the author's own research as 'an unpublished study which we cannot evaluate.' I submitted my letter early in July. At that time, the paper was not listed on Medline, nor on the journal's website. In fact, it wasn't published until mid-September. Given that it was not in the public domain when I wrote the letter, neither I, nor others, were able to evaluate the findings. I accept that it would have been more accurate to describe the study as yet to be published, but the wording reflected a lack of linguistic precision, not my competence as a scientist.

Finally, the authors took me to task for describing a review as a 'viewpoint paper'. I concede that the paper was headed 'review' but it was a contentious one. It only discussed evidence supporting a number of hypotheses, not the evidence against. Given this imbalance, I'm not sure my description justifies the critical comments about my inability to read and interpret scientific publications.

I disagreed with the authors. I was not persuaded by their arguments. It's a sad day when authors respond to such criticisms by publicly challenging the competence of fellow professionals.

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Health and safety — in general practice?

We agree with Kennedy *et al*'s¹ conclusion that GPs' knowledge of health and safety legislation is poor.

Recently we conducted a qualitative study exploring GPs' attitudes to managing health and safety in their role as employers. Preliminary analysis of our work indicates that, while participants considered themselves to be good employers concerned about the well-being of their staff, they were unaware of the full scope of their health and safety responsibilities. This was evident in their uncertainties about who had responsibility for managing health and safety and in their reported lack of formal policies and procedures. Some participants stated that one reason for taking part in the research was to learn more about these issues. GPs who were also part-time occupational health physicians were more aware of their legal duties.

The GPs had difficulty in managing employment issues in relation to illness, especially when their employees were their patients. Their attitudes in this area were tempered by their own attitudes to illness in themselves, whereby, as previously reported, they rarely took time off work.²

We suggest that efforts should be made to increase GPs' sense of 'ownership' for health and safety and to ensure that they can access competent support. We welcome the improved access to support arising as a consequence of the recent Department of Health initiative³ and

identify appropriate postgraduate training as being an important factor in addressing these issues.

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Driving under the influence of insulin

The tragic case of a fatal road accident that took the life of a man and injured the diabetic driver of the other vehicle was reported in the national¹ and diabetic² press this summer. I was asked to give evidence, partly because of a special interest in diabetes and partly because the diabetic man was previously registered with me. I had been his GP for about nine years and had also worked in the diabetic clinic at the local hospital where he attended. I was asked: Had I advised him to test his blood glucose before driving? No. I had not been aware that in the nine pages

of advice given on the Diabetes UK website entitled 'Driving and Diabetes',³ steps to avoid hypoglycaemia included a recommendation to test blood glucose before driving. Other measures include carrying a snack in the car and not driving for over two hours without stopping for some food.

Since discovering this, I have been asking those who take insulin if they follow this advice. So far I have yet to find anyone who does. On asking 18 of my local GP colleagues it appears that none of them give this advice. Of the two diabetes special nurses that I asked, one said that such advice was given, but that to test every time would 'be silly'; the other replied that the advice was given when insulin was started as part of the appropriate education. However the case before the court concerned a man who had started insulin for type 1 diabetes, when he was too young to drive.

The minutes of 1 May 2002 of the Meeting of the Honorary Medical Advisory Panel that advises the DVLA,⁴ say that there is awareness of the fact that most diabetics do not test their glucose level prior to driving. Eighty per cent of them would not have enough test strips prescribed to do so. The panel did not feel they could make such testing a legal requirement, especially in view of the fact that diabetics as a group are not especially dangerous drivers. There are also issues about education for both people with diabetes and medical professionals.

So far, I have yet to decide whether I should be advising all my patients who drive and take insulin to test every time before driving. It is sensible, standard advice, so why is it hard to make use of it? Clearly, if testing is going to be done it will have to be before every journey. It would be impossible to predict which journey is going to cause an accident. Testing only when you have symptoms of hypoglycaemia would defeat the purpose of the test, which is to detect unrecognised hypoglycaemia or the risk of such occurring during the journey.

There are some questions that need addressing before we can be sure the advice is essential:

- *Would such testing reduce hypoglycaemic episodes and therefore accidents too?* I know of no

good evidence that personal blood glucose testing results in fewer hypoglycaemic attacks.

- *How should the diabetic person respond to the result?* A blood glucose of 7 mM or over would make the risk of hypoglycaemia seem low. Results of 3 mM or less would require food and not driving until the glucose came up. This would only happen if sufficient mental ability remained to correctly interpret the result and take appropriate action. Levels in the middle, of 4 to 6 mM, are the awkward ones. Taking sugary sweets every time could upset glycaemic control. Not doing so might still risk hypoglycaemia while driving.

In the meantime, it does seem sensible to advise anyone taking insulin to test their glucose level before driving, even if only to address the legal implications. Whether road accidents will be less frequent remains to be seen.

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Prescribing antimicrobials for UTI

In the review article by Eva Hummers-Pradier and Michael M Kochen in the September issue of the *BJGP*,¹ it is stated in the summary on page 752 that urinary tract infections are 'symptomatic infections of the urinary tract, mainly caused by ...' This is a repetition of words and explanation of the main cause of UTI, rather than a definition of UTI. Furthermore, their statement is inaccurate because UTIs are sometimes asymptomatic; for example, in pregnant women.

Hummers-Pradier and Kochen made recommendations on page 755, about who should not be receiving antibiotics

following a urinary dipstick test. The complex issue of prescribing, interaction between the doctor and patient, and other factors (e.g. socio-economic) were ignored. Many patients attend general practice when their own remedies have already failed and they usually want something to be done. Patients were ten times more likely to be given a prescription if the GP perceived that the patient expected one.²

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1. Hummers-Pradier E, Kochen MM. Urinary tract infections in adult general practice patients. *Br J Gen Pract* 2002; **52**: 752-761.
2. Cockburn J, Pit S. Prescribing behaviour in clinical practice: Patients' expectations and doctors' perception of patients' expectation — a questionnaire study. *BMJ* 1997; **315**: 520-523.

Warfarin safety

Our practice has recently been involved in the tragic death of a patient because of an error in warfarin prescribing. A receptionist misread a doctor's handwriting and informed a patient to take too high a dose of warfarin. The patient died of gastrointestinal haemorrhage three weeks later. We had been prescribing warfarin and interpreting results for six years prior to this with no similar errors. The coroner returned a verdict of accidental death.

The coroner found that our practice was no different from many other practices in the country. He informed us he would be writing to the Chief Medical Officer to highlight the potential for errors in warfarin prescribing. He suggested we also write to colleagues, publications, and other health-related organisations to highlight the need for care in warfarin prescribing and outline the changes we have made to ensure safe prescribing of warfarin.

Our new warfarin protocol is as follows:

1. International Normalised Ratio (INR) results are interpreted by a doctor in accordance with our protocol and entered onto the computer in a standardised

format.

2. The doctor informs the patient by telephone the same evening of his/her result, and dosage and interval to next INR.
3. We confirm the above in writing a few days later.

This difficult case for all involved highlighted that a seemingly safe system can go wrong with tragic consequences. We now feel we have a safer system with the added safeguard of confirmation and checking of the INR and dosage in writing.

Name and address supplied

The sovereignty of virtue

I read with interest Diane Reeves' critique of my paper in the August *BJGP*¹ — a title chosen, as many readers will have noticed, in homage to the late Iris Murdoch's wonderful paper, *The Sovereignty of Good*. Clearly, I did not achieve Murdoch's clarity, or Reeves would not have got in such a muddle over the claims of virtue ethics or my own position.

The Enlightenment, Beauchamp and Childress, and the Bristol and Alder Hey scandals occurred in that order. It is therefore incontrovertible that the first two failed to prevent the latter two. Whether an alternative ethical approach would have done so is less certain — my purpose was to explore that possibility.

Reeves thinks that I believe 'all we need to do to be good doctors is develop good character traits, or virtues, and we'll just act rightly "as naturally as breathing"'. While virtuous persons certainly aspire to such an integrated level of virtue, Reeves' 'all' and 'just', implies that I think this is easy. I did not suggest, nor do I believe, this. No-one who has seriously attempted to live a virtuous life would dare to claim that it is not difficult and probably not fully achievable — but surely it is worth striving for.

I thought I had made it clear that 'This does not mean a return to ethics being what good chaps do, or abandoning rational thought about moral issues'. Her discussion of courage suggests, however, that she has overlooked my emphasis on the impor-

tance of practical wisdom as central to the virtues, something which I think all virtue ethicists agree on. It is clear from Aristotle's account of courage (*Ethics*, Book III), for example, how important practical wisdom is in distinguishing courage from cowardice or foolhardiness. I can think of no tenable virtue ethics analysis that would view the Bristol heart surgeons as courageous. Their behaviour was not something one would admire or aspire to, nor did it lead to flourishing, 'eudaimonious' lives, in terms of either the internal or the external goods of their practice. A virtue ethics account explains not merely that they were wrong, (as other approaches do), but also how they might have avoided their errors — by cultivating humility, practical wisdom, empathy, etc.

There are virtue ethicists who hold that virtue is not just central to right behaviour but is the only valid approach to it — St Augustine's suggestion that we should 'love, and do what you will' (though elsewhere he seems to be pretty hot on duties), or perhaps The Beatles' — 'All you need is love'. I did not go this far in suggesting that the exclusive emphasis on duty or consequences is misguided, and that we should see virtue as 'central' and 'at the heart of the moral life'. The relationship between rights, duties, consequences, and virtues seems to me to be a meta-ethical problem to which there is as yet no satisfactory solution.

Reeves, I think, overvalues the Enlightenment — or perhaps, rather, she ignores and undervalues what went before. Her addition of the adjective 'post-Enlightenment' to terms such as philosophical thinking, ethics, and

philosophical skills detracts from, rather than adds, to them. She is surely in error in assuming that deontology was an Enlightenment invention. I am no historian of ideas, but I would think that the Ancient Hebrews could claim a robust deontology that antedated the Enlightenment by more than two thousand years. What is peculiar about Enlightenment deontology, which I challenge, is the attempt to found it solely on reason, rather than on a holistic view of human personhood.

It does not follow from this that teaching the skills of rational analysis of ethical problems is futile (nor was it invented in the Enlightenment — the Medieval scholastics were, I believe, experts in the application of general principles to particular cases): 'It is necessary but not sufficient to ensure right action, which involves many other less rational virtues, such as temperance, courage, and benevolence'.

Certainly it is as she suggests important to 'challenge students to develop new ways of thinking about themselves and their patients', and 'tolerance and understanding' are among the virtues I would hope they would develop. But I would prefer to see this in terms of the cultivation of practical wisdom, wedded to the other elements of virtue — right feeling and right motivation — rather than as a solely intellectual exercise in rights and duties, acts and their consequences.

I agree with Reeves that the ethics of general practice has been insufficiently studied, and we certainly need to clarify the moral basis on which our day-to-day practice rests. Indeed, many of my academic publications have had this as their aim. But I do not feel that she has made a coherent case that these

issues are better explored within a Kantian framework than one addressing the virtues of the doctor — and perhaps the virtues of the patient?

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Reference

1. Toon P. The sovereignty of virtue. *Br J Gen Pract* 2002; **52**: 694-695.

Corrections

In the letter by Aldo Mariotto entitled 'Patient-doctor relationships: duet or duel? A physician's perspective', which was featured in the October issue of the *BJGP* (page 855), we regret the accidental omission of the names of the letter's co-authors. The full list of authors should have read as follows:

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In addition, in the November issue of the *BJGP*, the paper by Robinson *et al* entitled 'Measuring consultation skills in primary care in England: evaluation and development of content of the MAAS scale' (pages 889-893) contained spurious reference citations throughout the text.

We deeply regret these errors, and apologise for any confusion or inconvenience caused. Amended versions of both the letter and the original paper will shortly appear on the *BJGP* website, at www.rcgp.org.uk.