The following full text is a publisher's version.

For additional information about this publication click this link.
http://hdl.handle.net/2066/48835

Please be advised that this information was generated on 2017-10-02 and may be subject to change.
Adolescent development

Advice in ABC of adolescence is potentially misleading

Editor—Christie and Viner say that delayed puberty in boys can be quite distressing but is almost always a normal variant. They say that boys aged 15 or over with a testicular volume of 4 ml or more can be reassured that puberty is beginning and, by inference, do not require referral to a specialist. This advice is potentially misleading.

For all that it is a variant of normality, constitutional delay in growth and puberty can have adverse psychosocial and skeletal consequences. 1–3 To deny an apubertal teenager the opportunity to choose low dose androgen treatment until he is into his 16th stage is potentially misleading, as 4 ml testes are within the range found in adult men with hypogonadotrophic hypogonadism and therefore by no means necessarily indicates hypogonadotrophic hypogonadism and its potential consequences.

Our advice is appropriate for boys who present to general practitioners. The absence of any signs of puberty, or lack of further progression through puberty over the next six months should, of course, be viewed with suspicion and merit referral to a paediatric endocrinologist service for full evaluation, including consideration of conditions such as hypogonadotrophic hypogonadism.

Quinton thinks that even the normal variant of constitutional delay in growth and puberty can have adverse psychosocial consequences. 4 However, newer studies have shown that boys who are small or whose adolescence is delayed are not psychologically disadvantaged. 5 Rather than overtreating numbers of normal boys, we focus on improving the communication and understanding of adolescents at risk of chronic fatigue syndrome.

Therapists regularly in the intervention arm show that the specific treatment benefit was carried forward without regular contacts with the therapists. A cautious approach is essential in inferring direct benefit from cognitive behaviour therapy in the intervention arm (as opposed to short term benefit from close contact with therapists).

The level of activity in some of their participants whom the authors considered to be passive remained unclear.

In their summary points the authors claim that cognitive behaviour therapy was effective by challenging patients’ belief that activity aggravated symptoms. Epidemiological data, however, confirm that fatigue made worse by exercise is a characteristic feature of adolescents at risk of chronic fatigue syndrome. Encouraging activity in disabled patients is entirely different from challenging an accepted feature of the disease. A rhetorical approach towards a physically and emotionally challenging condition does not help recovery and only encourages therapeutic failure.

Abhijit Chaudhuri
Senior lecturer in clinical neuropsychology
University of Glasgow, Institute of Neurological Sciences, Glasgow G51 1TF
ac54p@udcf.gla.ac.uk

Competing interests: None declared.


Cognitive behaviour therapy for adolescents with chronic fatigue syndrome

Data are insufficient and conclusion inappropriate

Editor—I have concerns about the design and interpretation of the study reported by Stulmeneier et al on cognitive behaviour therapy for adolescents with chronic fatigue syndrome. 1 The trial arms were not matched for the number of contacts with healthcare professionals. Experience from larger and more carefully controlled randomised interventional trials of patients with chronic fatigue syndrome has clearly shown that short term improvement in symptoms is related directly to the maintenance of regular contacts with healthcare professionals rather than the therapeutic effect of the intervention itself and consequently, the improvement is not sustained once the contact is lost.

The authors did not offer patients in their waiting list the opportunity to meet therapists regularly for five months but without having cognitive behaviour therapy. Few follow up data on patients in the intervention arm show that the specific treatment benefit was carried forward without regular contacts with the therapists. A cautious approach is essential in inferring direct benefit from cognitive behaviour therapy in the intervention arm (as opposed to short term benefit from close contact with therapists).

The level of activity in some of their participants whom the authors considered to be passive remained unclear.

In their summary points the authors claim that cognitive behaviour therapy was effective by challenging patients’ belief that activity aggravated symptoms. Epidemiological data, however, confirm that fatigue made worse by exercise is a characteristic feature of adolescents at risk of chronic fatigue syndrome. Encouraging activity in disabled patients is entirely different from challenging an accepted feature of the disease. A rhetorical approach towards a physically and emotionally challenging condition does not help recovery and only encourages therapeutic failure.

Abhijit Chaudhuri
Senior lecturer in clinical neuropsychology
University of Glasgow, Institute of Neurological Sciences, Glasgow G51 1TF
ac54p@udcf.gla.ac.uk

Competing interests: None declared.

1 Stulmeneier M, de Jong LWAM, Fiehler TJW, Hoogveld SWB, Blenting GP. Cognitive behaviour therapy for adolescents with chronic fatigue syndrome: randomised controlled trial. BMJ 2005;330:14-8 (1 January)
Increasing caesarean rate, meta-analysis shows

Editor—The study reported by Mayor in her news item uses the term “neuropsychal agnosia” and claims that early epidurals do not increase the rate of caesarean deliveries.1 This is confusing as the study was not of early epidurals and first analgesia but of early epidurals and first analgesia. In addition, the study was not randomised controlled trial, but a non-randomised study.2

The claim that women need not worry because “early epidurals increase caesarean rate, meta-analysis shows” is false.3 This trial was about two methods of helping women with pain in early labour. In the so-called epidural arm, on their first request for analgesia, women received intrathecal fentanyl, and in the narcotic arm, hydomorphine. On their second request, almost two thirds of women in both arms were 4 cm or more dilated. In the intrathecal “epidural” arm, they received low dose epidurals; in the narcotic arm, hydomorphine.

This trial, as others that have contributed to the Cochrane meta-analysis,4 showed no increase in caesareans in the presence of epidural analgesia, but does not acknowledge that most women were in active labour at randomisation, while many will do well. Wong et al,4 like Sharma et al, the major contributors to the Cochrane meta-analysis,4 have shown only that when women’s pain in the latent phase is managed with intrathecal, narcotic, or other pharmacological or non-pharmacological means, an epidural in the active phase of labour does not increase the rate of caesareans.

The role of an early epidural in contributing to increases in caesarean rates has yet to be studied in a randomised controlled trial, but the sensitivity analysis in the Cochrane meta-analysis, after removing late randomisation studies, shows that early epidurals to more than double caesarean rates.

Michael C Klein
Physician
BC Women’s Hospital, Vancouver, British Columbia, Canada V6H 3VJ
mklein@interchange.ubc.ca

Competing interests: None declared.

1 Mayor S. Epidurals do not lead to more caesarean sections, study shows. BMJ 2005;330:395-6 (19 February).
description of the syndrome includes four primary features: autonomic lability, hyperthermia (pyrexia) without other cause, extrapyramidal syndrome (cog-wheel or lead pipe rigidity), and encephalopathy.\(^1\) Despite superficial clinical similarities between neuroleptic malignant syndrome and serotonin syndrome, they are usually easily differentiated on the basis of careful neurological examination. Neuroleptic malignant syndrome is associated with lead pipe rigidity, bradykinesia, and other extrapyramidal features.\(^1\) Conversely in serotonin syndrome there is hyperkinesia, hyperreflexia, and clonus.\(^1\)

Descriptions of adverse reactions to psychotropic drugs need detailed clinical descriptions of neuromuscular, central, and autonomic features. Using ambiguous or non-specific criteria to label adverse reactions as a particular syndrome while ignoring the pharmacology of the implicated drug may lead to false associations between particular drugs and clinical syndromes and to inappropriate treatment.

\textbf{Authors' reply—}Clomipramine is not a neuroleptic and it has no clinical evidence of rigidity was based on our article.\(^1\) We described in this patient an earlier diagnosed episode of serotonin syndrome, and no clinical evidence of rigidity was found on that occasion.

In view of the action at dopamine sites of clomipramine, and the statement in the BNF from the BMA and the Royal Pharmaceutical Society of Great Britain, we would continue to support our diagnosis of neuroleptic malignant syndrome in this informative case.

\textbf{Alison Haddow consult}ant in old age psychiatry

Royal Cornhill Hospital, Aberdeen AB25 ZH

alison.haddow@gpct.grampian.scot.nhs.uk

Martin Wilson clinical lecturer

Department of Medicine for the Elderly, University of Aberdeen, Aberdeen AB25 2ZD

Competing interests: None declared.

\textbf{Notes}


\textbf{Editors—}We agree that we should have made greater attention to the long-acting nature of clomipramine, but we did not have any case reports that were available to us.

\textbf{Letters}

\textbf{Risks of gene therapy should be weighed against lack of alternatives for many diseases}

\textbf{Need for expertise based randomised controlled trials}

\textbf{Expertise based design has shortfalls}

\textbf{Editor—}Devereaux et al discussed the need for expertise based randomised controlled trials for surgical procedures.\(^1\)

Firstly, the use of expertise based designs does not necessarily enhance the validity of a surgical trial. Surgical outcome does not depend solely on the operation; other factors that influence the results of an operation are heterogeneous and immeasurable (postoperative management, the surgical team, equipment). A different bias is introduced by the expertise based design, the influence of the overall performance of surgeon A \(\times\) B, and in this regard, expertise based design is not necessarily a more valid comparison of operation A \(\times\) B. Secondly, the use of expertise based designs does not necessarily enhance the applicability of a surgical trial. The expertise based design assumes that an operation will only be performed by a select few. This is rarely the case, and hence the results will not reflect the true performance of an operation introduced to the general public (performed by a variety of surgeons).

Moreover, the results of expertise based design trials do not take into account any learning curve that exists when a new operation is introduced. The initial rates of adverse outcomes are higher when a surgeon refines an existing operative technique, never mind a new one.

A solution is to perform a randomised trial that has a balanced surgical expertise in...
Letters

both arms in proportions reflective of the population that will perform the operations. Academics can analyse the "expertise" subgroups, while the rest of us can look at the overall results to determine how an operation will really perform.

Eric Lim specialist registrar Papworth Hospital, Papworth Everard, Cambridge CB3 8RE ericlim@cvnet.org

Competing interests: None declared.


Surgical research shares many similarities with psychotherapy research

Editor—Of course the expertise based randomised trial, mooted for surgical procedures, is also the norm in psychotherapy research when comparing two or more different psychotherapies. A similar debate on the interpretation of such trials occurred in the psychotherapy literature.1 Research in surgery and psychotherapy share other similarities beyond having to account for practitioner expertise. There is the issue of blindness—hard to achieve for both patient and doctor in these disciplines—as well as the "why test it, it’s obvious it makes a difference" argument. Both disciplines could learn from each other about the design and analysis of clinical research.

Simon Hatcher senior lecturer in psychiatry Department of Psychological Medicine, Faculty of Medical and Health Sciences, University of Auckland, Private Bag 92019, Auckland 1, New Zealand shatcher@auckland.ac.nz

Competing interests: None declared.


Erik T Walbeehm special registrar plastic surgery Rotterdam, 3022 BC, Netherlands erikwalbeehm@mac.com

Competing interests: None declared.


Old docs and new tricks

Seasoned doctors may be better than young doctors at some things

Editor—Spurgeon reports that the doctors’ standards of care drop with years in practice.1 When I began clinical practice in the late 1980s I thought that one key to being a “good doc” was keeping up with the latest drugs and technologies. I was disappointed with a psychologist.


Efficiency is important

Editor—in British general practice, where everyday demand exceeds capacity, the efficient general practitioner is king. That is one thing that experience should bring. If all general practitioners followed every guideline the system would collapse, and although a few patients would have exemplary care, many would have no care at all as they would just not get seen because they would find the wait intolerable. Perhaps this is what happens now in secondary care, where care delivered is often very good but access is less and less.

Erik T Walbeehm

Competing interests: None declared.


Target SHOs and registrars for communication skills training

Editor—Kidd et al argue that undergraduates need to learn clinical and communication skills side by side.1 Every doctor needs communication skills, from pathologists to surgeons and physicians. In the Netherlands undergraduates spend considerable time on how to communicate with patients. But I think that this is targeting the wrong group, and time is taken away from essential preclinical and clinical studies. Students learn to talk to simulated patients, with, for example, a Dukes’s D colon cancer, at a time in their studies when they don’t know what cancer is, what a Dukes’s D colon cancer means, and what the impact is on a patient. Students receive this kind of training in their second year and have forgotten about it when they become senior house officers.

Senior house officers (SHOs) and registrars should be doing the training. We do the damage in our "bad news" talks. We know what we are talking about, and we do it daily. We should be the ones filmed on camera and evaluated. This should be done with clinicians with experience, together with "communicators," and not only by people who studied communication skills who have never had to tell anybody that they have cancer and are dying, or let a family know that a patient has died. The impact of this is greater than most people can imagine, and I think poor communication on those subjects often reflects the inability of the doctor to deal with his or her own feelings.

The same goes for communicating with colleagues. In my hospital, emergency departments are now filmed in major trauma cases, and the people on the floor are actually the ones being filmed. Evaluation is then done by the surgeons, together with a psychologist.

Communication skills are essential, but not at the expense of medical students’ pre-clinical and clinical curriculum. The target groups should therefore be senior house officers and specialist registrars.

Erik T Walbeehm

Competing interests: None declared.