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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 To deny an apuberal teenager the opportunity to choose low dose androgen treatment until he is in his 16th year would be unusual by present standards. Given the likely ensuing timescale, his doctor might as well refer him straight to an endocrinologist instead of a paediatrician.

A testicular volume of 4 ml is well within the range found in boys with irreversible hypogonadotrophic hypogonadism and therefore by no means necessarily indicates that puberty is beginning. Many boys with hypogonadotrophic hypogonadism start puberty but fail to progress beyond the early stages.¹ Moreover, a history of cryptorchidism (especially if bilateral) or anosmia should prompt an even earlier referral.¹ Neither does a family history of pubertal delay necessarily support a diagnosis of constitutional delay in growth and puberty, given the high prevalence of constitutional delay in growth and puberty among first degree relatives of patients with hypogonadotrophic hypogonadism.

A recurring theme in the personal stories posted on the www.KallmannS.org website by men with irreversible hypogonadotrophic hypogonadism is of just how difficult it was for them as teenagers to screw up the courage to go to see their family doctor about a lack of secondary sexual characteristics. On being told “not to worry, because it’s only pubertal delay,” many felt (or were made to feel) so crushed and foolish that they then put off seeing a doctor until many years later.

Richard Quinton consultant endocrinologist Royal Victoria Infirmary, Newcastle on Tyne NE3 2NJ richard.qinton@nuth.nhs.uk

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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 Stulmeneijer 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 has ended.

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.¹ 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 neuroscience University of Glasgow, Institute of Neurological Sciences, Glasgow G51 4TF ac54p@udcf.gla.ac.uk

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increasing the world in the United States and around the world. In the context of these increases, we are surprised that methods of care that might prevent caesarean delivery have not been pursued more aggressively.

Caesarean delivery is strongly correlated to the age of the mother, parity, and increasing gestational age within the term period of pregnancy.1 If caesarean delivery is an adverse outcome worthy of prevention, if risk factors for caesarean delivery can be identified, and if a latent period exists between the identification of risk and the development of situations requiring caesarean delivery then perhaps a preventive approach—encouraging patients with risk factors to enter labour before their risk can become disease—could lower caesarean delivery rates safely.

Our working group recently described the use of risk driven, prostaglandin assisted induction of labour, and this intervention was associated with a rate of caesarean delivery of only 4%15 While Declercq et al think that research should be done to elucidate whether the risks of primary caesarean delivery in cases of no indicated risk will be offset by associated benefits, we hope that an equal amount of time and effort will be spent on developing and testing methods that might safely prevent, or lower, rates of caesarean delivery performed for this and the other more traditional indications.

James M Nicholson assistant professor james.nicholson@uphs.upenn.edu
Linda C Kellar first year fellow
Peter F Cronholm clinic director
Department of Family Practice and Community Medicine, 2 Gates, Hospital of the University of Pennsylvania, 3400 Spruce Street, Philadelphia, PA 19104, USA

Competing interests: None declared.


Early epidurals increase caesarean rate, meta-analysis shows

Editor—The study reported by Mayor in her news item uses the term “neuromuscular analgesia” and claims that early epidurals do not increase the rate of caesarean deliveries.1 This is confusing as the study was not of epidurals versus patient-controlled intrathecal analgesia during labour. Anesthesiology 1997;87:487-94.

In the study by Prins et al2 the epidurals were not given before the first request for analgesia. Women received intrathecal fentanyl, and in the narcotic arm, intrathecal morphine. If the clinical features that are present in many cases of neuroleptic malignant syndrome (any more than it can cause anti-cholinergic effects) are present, then the drug can be stopped.

The trial was about two separate and independent investigations. The one investigating the use of risk driven, prostaglandin assisted induction of labour, and the other more traditional indications.

Clomipramine and neuroleptic malignant syndrome

Letters

Editor—Chaudhuri’s concerns relate to a possible placebo effect and the way we treat our patients. We consider it unlikely that the positive effects of our treatment are attributable mainly to non-specific treatment factors, since the results were maintained at eight months. Moreover, others have shown that cognitive behaviour therapy was more effective than attention control conditions shortly after treatment and at long term follow-up.1,4

At the start of therapy all patients experienced severe limitations in performing activities appropriate to age. A characteristic belief of patients with chronic fatigue syndrome, especially in cases of passive patients, is that fatigue is made worse by exercise. This cognition, although functional in the first phase of the condition, is dysfunctional in the longer term and maintains activity avoidance and symptoms. By challenging these and other activity related cognitions, activity regulation is possible. Thus, patients are taught to regulate and increase their physical and other activities in a systematic and safe way. Our results show that this is possible. The distinction between physically passive and relatively active patients, based on actometry, is helpful to select the correct approach to help the patient.

As in other chronic conditions, we do not have one treatment that leads to recovery in all patients. We believe that our study clearly shows that many of the participants benefited from cognitive behaviour therapy and were able to function as normal adolescents again.

Competing interests: None declared.


Authors’ reply

Editor—Chaudhuri’s concerns relate to a possible placebo effect and the way we treat our patients. We consider it unlikely that the positive effects of our treatment are attributable mainly to non-specific treatment factors, since the results were maintained at eight months follow up even in the absence of contacts with the therapist (unpublished data). Moreover, others have shown that cognitive behaviour therapy was more effective than attention control conditions shortly after treatment and at long term follow-up.1,4

At the start of therapy all patients experienced severe limitations in performing activities appropriate to age. A characteristic belief of patients with chronic fatigue syndrome, especially in cases of passive patients, is that fatigue is made worse by exercise. This cognition, although functional in the first phase of the condition, is dysfunctional in the longer term and maintains activity avoidance and symptoms. By challenging these and other activity related cognitions, activity regulation is possible. Thus, patients are taught to regulate and increase their physical and other activities in a systematic and safe way. Our results show that this is possible. The distinction between physically passive and relatively active patients, based on actometry, is helpful to select the correct approach to help the patient.

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Competing interests: None declared.

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. 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. Conversely in serotonin syndrome there is hyperkinesia, hyperreflexia, and clonus.

Descriptions of adverse reactions to psychotrophic 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.

Authors’ reply

Ede et al—Clomipramine is not a neuroleptic and cannot be considered an “antidepressant.” However, as mentioned in our article, this drug has an appreciable blocking effect at dopamine receptor sites, the traditional domain of the neuroleptic drug. This is a weak effect, but it is more potent than several other antidepressant agents. This action is recognised in the current edition of the BNF, which says that neuroleptic malignant syndrome may, very rarely, arise in the course of antidepressant treatment.

In addition to 50 worldwide reports received regarding clomipramine and neuroleptic malignant syndrome or suspected neuroleptic malignant syndrome, in addition to four reports received by the Committee on Safety of Medicines and two published case reports.

We agree that we should have made clear that this patient’s muscle rigidity was of the lead pipe variety, although some widely accepted diagnostic criteria require only severe muscle rigidity. The diagnostic criteria that we tabulated were based on Levinson and Sternbach and referenced in our article.

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.

Eddie Faddoul and Graham Golledge

Competing interests: None declared.

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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 special 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 by Devereaux et al., is the norm in psychotherapy research when comparing two 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.


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 dismayed to see seasoned colleagues who were slow to change.

I then saw many new drugs get pulled from the market (rofecoxib is not the first non-steroidal anti-inflammatory drug to be withdrawn) and various medical fads come and go. Evidence based medicine appropriately shed light on the poor evidence available to support most things that physicians do.

Armed with this keener analytical approach, I came to realise that most claims of benefit are greatly exaggerated compared with absolute incremental changes and that most patients are not like trial subjects. Although an intervention might benefit the patient who sits before me, the immediate common sense logic of its hypothesis and conclusions make me advise to proceed down this route with caution. Being a doctor is already a difficult job. Being advised that all your thoughtful patient experience has actually made you a worse doctor is demotivating. Perhaps experienced doctors and patients would have a different set of criteria about what good care is?

Graeme M Mackenzie general practitioner
Whitehaven CA28 7RG
graeme.mackenzie@gp-as92041.nhs.uk
Competing interests: None declared.


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. Accepted practice is often developed for a "one issue patient." Reality means multiissue patients, who themselves have limited ability to follow all the investigation and "treatment" recommended by the single issue academic establishment. Many indications for treatment are immediately met with contraindications. Experience allows general practitioners to cut back on too much excess investigation and treatment while still striving to meet the guidelines.

Protecting the patient from the iatrogenic harm of excess health care used to be a core skill of the general practitioner. Is this being taken away from us as well?

A system that fails to value the soft end points and often efficient and effective care that experience brings will have to restructure to meet the demand and that inevitably will lead to a hugely expanded system with resource implications. I am not disputing the findings of the paper reported by Spurgeon, that the standard of care may drop with years spent in practice, but the immediate common sense logic of its hypothesis and conclusions make me advise to proceed down this route with caution. Being a doctor is already a difficult job. Being advised that all your thoughtful patient experience has actually made you a worse doctor is demotivating. Perhaps experienced doctors and patients would have a different set of criteria about what good care is?

Graeme M Mackenzie general practitioner
Whitehaven CA28 7RG
graeme.mackenzie@gp-as92041.nhs.uk
Competing interests: None declared.


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