

been developing such systems for the management of a variety of medical conditions,<sup>2</sup> of which a system for the diagnosis, physical examination, and treatment of hypercholesterolaemia is currently undergoing field trials with general practitioners in the west midlands. The system is supplied for the use of a Psion Organiser and incorporates regional knowledge in a particular subject as appropriate for use in primary care or by junior hospital medical staff. The medical knowledge supplied facilitates patient management up to a level considered appropriate for the general practitioner or non-specialist physician and contains locally determined referral criteria for specialist attention. Preliminary results suggest that such a format is acceptable to both general practitioners and practice nurses and that programmable pocket computers are an appropriate vehicle for the devolution of medical expertise to non-specialists or non-medical staff such as practice nurses. Inappropriate referrals to hospital clinics may also be reduced by providing clear guidelines to referring general practitioners with this medium.

We also envisage using such systems for providing useful advice on foreign travel prophylaxis, poisoning, laboratory sampling requirements, protocols for standard medical tests, etc.

The potential for devolution of medical expertise is just beginning to be realised. Pocket sized computers can retain and instantaneously recall a large amount of medically relevant data and, unlike a text book, can be readily updated, are portable, and have a variety of other functions.

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1 Wyatt J. Construction of clinical scoring systems. *Br Med J* 1990;300:538-9. (24 February.)

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## Streptokinase in acute aortic dissection

STR.—The report by Mr J Butler and colleagues emphasises the risk of leaving major medical management decisions to inexperienced junior medical staff.<sup>1</sup> In each of the cases described the house officer was apparently the first medical practitioner to see patients admitted as emergencies. This is an illogical method for assessing patients. The administration of potentially life saving but also potentially life threatening treatments surely requires a higher level of skill and experience than should be expected from a house officer. It is noteworthy that in all of the patients described in this report aortic dissection should have been considered on the basis of the chest radiograph, which was available to the admitting house officer. There is insufficient data in the report by Dr Nicholas P Curzen and colleagues<sup>2</sup> to judge whether the same circumstances occurred in their patients, although in one case the chest radiograph was suggestive of aortic dissection.

In the study by Mr Butler and colleagues it is not surprising that the diagnosis of aortic dissection was not considered by the admitting house officer, both in view of the limited experience in cardiovascular diseases that most medical students receive during their training and also the limited exposure to training in radiology in most undergraduate medical training.

The problems concerned in distinguishing acute aortic dissection from other causes of severe acute chest pain should not cause the uses of thrombolytic agents to be reduced. Rather it is

an important reason for changing the way in which patients admitted with acute conditions are assessed. Surely it is no longer satisfactory to delegate assessment of acutely ill patients to the most junior members of the medical team; this should now be the responsibility of a more senior (that is, registrar grade) admitting physician. This will improve the accuracy of the initial management of these patients and will help to prevent the potential catastrophes described by Mr Butler and colleagues. It may also help to prevent an epidemic of unnecessary requests for investigation for possible aortic dissection before doctors administer thrombolytic treatment.

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2 Curzen NP, Clarke B, Gray HH. Intravenous thrombolysis for suspected myocardial infarction: a cautionary note. *Br Med J* 1990;300:513. (24 February.)

## Medical problems of adults who were sexually abused in childhood

STR.—Assessing the possible effects of adversity during childhood on health in adulthood requires studies that either include appropriate control subjects for comparison or eliminate the effects of confounding variables statistically. In their study on the medical problems of adults who were sexually abused in childhood Dr R P Arnold and colleagues used neither of these techniques.<sup>1</sup> This leads to most basic problems in interpreting their results. For example, we would surely expect the total number of medical specialties contacted by any person (whether sexually abused in childhood or not) to increase with age. The data presented (tables I and III) show exactly this expected correlation between age and the number of specialties contacted for the seven abused women (Spearman's test,  $\rho=0.88$ ;  $z=2.16$ ;  $p<0.05$ ). This renders the interpretation of the total specialties contacted extremely hazardous without comparison with appropriate matched controls.

Dr Arnold and colleagues suggest that childhood sexual abuse may be an aetiological factor in the development of physical symptoms in adulthood. Their data support this hypothesis weakly. The duration of abuse correlates positively with the number of non-psychiatric admissions (derived from table II) ( $\rho=0.81$ ;  $z=1.98$ ;  $p<0.05$ ). The interpretation of this result, however, is unclear as the study confounds sexual abuse with severe family disturbances, owing to biased sampling. The authors themselves point out that they ascertained that most of the patients "had unambiguous histories of severe family problems in childhood," so that the group was "not representative of all cases of childhood sexual abuse." Clearly, comparison with carefully selected control subjects (for example, people exposed as children to comparable durations of severe marital discord or violence but not to sexual abuse) is needed before we can attribute any excess ill health in adulthood specifically to the sexual abuse component of family disturbance during childhood.

The null hypothesis that sexual abuse during childhood does not alter demand for medical services in adulthood cannot be rejected without much more careful work. The topic of childhood sexual abuse is so sensitive that studies to assess its possible long term effects should meet the highest scientific standards if their results are to inform rather than inflame public debate.

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1 Arnold RP, Rogers D, Cook DAG. Medical problems of adults who were sexually abused in childhood. *Br Med J* 1990;300:705-8. (17 March.)

2 Meddis R. *Statistics using ranks: a unified approach*. Oxford: Basil Blackwell, 1984.

STR.—In their paper Dr R P Arnold and colleagues draw attention to the possible long term physical effects of childhood sexual abuse.<sup>1</sup> Research on this subject is less extensive than that on the psychological effects, in fact it is paltry: to our knowledge there have been only three studies in non-clinical populations.<sup>2,4</sup> There are problems, however, in drawing any conclusions from the type of model employed in the paper by Dr Arnold and colleagues. It is debatable whether adult psychiatric patients are a useful sample to study as the prevalence of childhood sexual abuse in such patients is consistently higher than expected.<sup>5</sup> In addition, psychiatric patients have a greater incidence of physical illness than non-psychiatric patients and may well be referred more and have a higher rate of interventions as a group.<sup>6,7</sup> Indeed, any samples comprising patients with clinical complaints are problematical—some surveys performed at normal outpatient gynaecological clinics, for example, suggest that the prevalence of childhood sexual abuse in such patients may be extremely high, perhaps in the region of 65%.<sup>8</sup>

The authors' method of finding patients was also interesting; they seem to have been found by chance and not elicited by a structured interview or questionnaire, as advocated by some authorities.<sup>9</sup> Therefore, they may well not have been representative of patients who were sexually abused in childhood—for example, one patient had made 60 attempts at suicide.

One particularly important point is the statement, which is often cited, that victims of childhood sexual abuse seem to suffer abdominal pain, headache, and backache in adulthood. There is no evidence of this from studies in non-clinical samples, and though Bachmann *et al*<sup>10</sup> state this in their review, the original sources do not say this.<sup>11</sup> Of course, there have been studies in patients with chronic pelvic pain that have found a high prevalence of childhood sexual abuse,<sup>12,14</sup> but this is not the same thing at all. This is quite apart from the issue of whether the high prevalence of childhood sexual abuse found in these populations is any greater than the prevalence in the general population, in which estimates in women have varied from 6% to 62%<sup>15</sup>: in this light findings of prevalences of 35%<sup>14</sup> or 64%<sup>13</sup> have far less clear implications. It suits many of us to believe that there is a clear somatic link between childhood sexual abuse and later physical symptoms, but the evidence simply does not yet exist.

The study by Dr Arnold and colleagues is based on a small, highly selected population in which case definition is unclear, and there are no control data. To apply the results from these seven psychiatric patients to the general population and suggest that they provide evidence of useful predictive factors of relevance to clinical practice is misleading. Childhood sexual abuse is undoubtedly important; it is also topical. It may well be a marker for the much wider issue of childhood neglect, and a broader perspective needs to be adopted to enable us to begin to tackle the difficult issues (medical, social, political, and economic to name but four) of what to do about this. Nor is it yet clear what the best methods of treatment in adults are should childhood sexual abuse be uncovered. Is it, therefore, enough to say that the general practitioner has a key part to play in deflecting multiple referrals when it is far less clear what the general practitioner (and the physician, gynaecologist, surgeon, and psychiatrist) should do? The time for descriptive, poorly focused studies is past and there is an urgent need to embark on proper community surveys of childhood sexual abuse, its incidence, and effects as exemplified by some<sup>15,16</sup> before any