social worker for the child. Who looks after the family? A guardian ad litem may be appointed (with his or her own costly legal representation), but the guardian's role is specifically to ensure the welfare of the children. There is a desperate need for a social worker for the family, to provide unbiased advice, explain parents' rights, and support the family at complex conferences.

Current practice undermines not only the family but also the key social workers, whose therapeutic relationships and opinions are not valued. Overspent, uncapped budgets are used to obtain assessments and therapy from outside social services. Practice varies enormously between teams in the same authority. Foster care is overflowing with children, and its use should be audited.

Professionals reading of the heartbreaking cases in which a child has died realise that social services have addressed the causes thoroughly: they now use this system for all disclosures. Referrals to social services are not neutral and may not remain confidential; they may be very damaging. I would support any doctor who thinks carefully before making a routine referral to social services.

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- 1 Child protection: medical responsibilities. *BMJ* 1996;313: 671-3. [With commentaries by F N Bamford and I Heath.] (14 September.)
- 2 Gibbons J, Conroy S, Bell C. *Operating the child protection system*. London: HMSO, 1995.
- 3 Bullock R, Little M, Millham S, Mount K. *Messages from research*. London: HMSO, 1995.
- 4 Reader P, Duncan S, Gray M. *Beyond blame*. Oxford: Routledge, 1993.

Child's needs should be central

EDITOR,—I am concerned about some of the issues raised in the case discussed in the ethical debate about child protection.¹ Given the nature of the case, it is entirely appropriate that social workers should have investigated it, as it was indeed an abuse of parental responsibility towards a child. I am not surprised at the child claiming that she must have slept through anything that happened. I have seen a teenage girl who denied the possibility of being pregnant despite having had no period for three months and having recurrent bouts of morning sickness, a positive result on pregnancy testing, and a viable first trimester fetus visible on ultrasonography. I suspect that the response of the child in the case debated reflects the human psyche's ability to deny unpleasant facts and should not be taken at face value.

The author tries to take an overview of his or her relationship with the entire family, with the result that the child's needs have become more peripheral than they should be. As medical professionals, we can manage only certain aspects of such complicated cases, and I believe that we are doing a disservice to the child if we do not involve other professionals more capable of continuing the investigations. It is by no means always the case that incest is disclosed at the time of a teenage girl's pregnancy being discovered. I would caution against the belief that "careful advice coming from a trusted doctor" is the input that a sex abuser needs.

Iona Heath's remark in her commentary on the case—that the outcome of these interventions rarely seems adequate—reflects a natural desire for a solution to what is an almost irreconcilable situation. My experience in paediatrics suggests that it is important not to ignore evidence that contradicts an apparently plausible explanation of such situations. I think that we do a disservice to young people, who need a voice

and an advocate on their behalf. All too many victims of familial sexual abuse suffer in the long term; they are likely to leave home, which puts them at risk from other dangers, such as prostitution, drugs, and violence. We want to prevent the abused person from becoming a potential abuser. We protect the perpetrator at great risk to the victim.

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- 1 Child protection: medical responsibilities. *BMJ* 1996;313: 671-3. [With commentaries by F N Bamford and I Heath.] (14 September.)

Girl should have been offered a chance of safety

EDITOR,—I read the recent ethical debate on child protection with increasing uneasiness.¹ Although the priority of the child's welfare and the danger of professionals "going it alone" are pointed out, other important issues are not addressed.

Did it not seem worrying to the general practitioner that the girl behaved as she did when the allegations were made? Phrases such as "she offered no explanation" indicate to me a lack of insight into her experiences. Children subjected to sexual abuse, especially over a long period, may be unable to confirm that it occurred, even when there is irrefutable medical evidence. In this case, fetal DNA from samples obtained after termination of her pregnancy matched that of her stepfather. Her denial probably arose out of fear of her abuser. It would take a long time for such a child to trust any adult to keep her safe. In this case she was right not to "offer an explanation": hopefully, that provided her with some protection when the judge decided that this family should remain intact.

The general practitioner talks of the "so called abuser." Impregnating a 14 year old girl would count as sexual abuse to most people; having been sexually abused as a child is an explanation but not an excuse. It seems that the abuser was not asked to take any responsibility for his actions but was allowed to remain at home. What message did this give him for the future?

Did the general practitioner see no way of supporting the mother in protecting her children?

If anything, this case report is an example of how child abusers can seduce and split professionals, no matter how experienced they think they are. No one says that the system we have now is perfect, but it is, as it must be, child centred. Not all residential care units or foster homes are abusive. I wonder if this girl, if given a chance of safety, would have been able to offer an explanation to satisfy her general practitioner.

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- 1 Child protection: medical responsibilities. *BMJ* 1996;313: 671-3. [With commentaries by F N Bamford and I Heath.] (14 September.)

Prodigy, a computer assisted prescribing scheme

Interim data show that it is worth taking the scheme further

EDITOR,—I wish to address the criticisms of the computer assisted prescribing scheme Prodigy that Iain E Buchan and colleagues raise.¹ Prodigy is an innovative project that seeks to involve

practising general practitioners in evaluating and refining a product intended for use by general practitioners. The primary role of the Sowerby Unit for Primary Care Informatics at the University of Newcastle is to evaluate rigorously and objectively the results of the various phases of testing and to highlight their implication for improving the prototypes. I believe that the collaborative methodology that has been used is new to the NHS and has merit (I N Purves *et al*, annual conference of primary health care specialist group of British Computer Society, 1996). The Prodigy team has received, sought out, and welcomed criticism from health service researchers and people working in health informatics around the world.

An interim report on Prodigy was written to inform the community in a politically charged atmosphere.² The report is clearly a snapshot of currently available information, with conclusions based on a number of evaluations (I N Purves *et al*, annual conference of primary health care specialist group of British Computer Society, 1996).² Quotes by Buchan and colleagues originate from the conclusions of the report but were not immediately linked to the data in the way that their letter suggests. Buchan and colleagues juxtapose the quoted conclusion of "confirming [the] desirability" of Prodigy to the responses to the question "How much would you want to continue with Prodigy?"¹ but the text that was juxtaposed in the report was "94% ... consider Prodigy to be a concept worth developing (16% being extremely happy, and 78% endorsing its development as long as either 'some improvement' or 'significant improvement' is made)."² This is not confirming desirability but supporting a concept. Similarly, the interim report does not claim that Prodigy's "effectiveness" is confirmed¹ because of "relative reduction in the rise of expenditure of 1.1%." In fact it states, "At this early stage we have insufficient data ... to make any statistical inferences."² Finally, Prodigy has been "thrust" on no one: all 137 sites using it volunteered.

Prodigy is a combination of active and passive systems. It is not ready to be rolled out: the data in the interim report suggest only that the concept is acceptable and worth taking further. Prodigy is an innovative yet practical step in clinical computer systems, and one has to start somewhere. Reliable and statistically robust conclusions will be possible once the study is completed; only then will we be in a position to judge the merit or otherwise of Prodigy.

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- 1 Buchan IE, Hanka R, Pencheon D, Bundred P. Introduction of the computer assisted prescribing scheme Prodigy was premature. *BMJ* 1996;313:1083. (26 October.)
- 2 Purves IN. *Prodigy interim report*. Newcastle upon Tyne: Sowerby Unit for Primary Care Informatics, University of Newcastle, 1996.

Decisions will be taken after receipt of final report next autumn

EDITOR,—I wish to provide some background to the computer assisted prescribing scheme Prodigy, which Iain E Buchan and colleagues criticise.¹ Prodigy is a three year research and development project whose main aim is to test the likely acceptability of decision support for prescribing in general practice. The evaluation aspects of the report are the responsibility of Dr Ian Purves and his team at Newcastle upon Tyne. No decisions will be taken on whether to proceed to any form of national implementation until the NHS Executive has received the final evaluation report, due next autumn.

Under normal circumstances we would leave it to Purves to decide when and how to publish the results of the evaluation. In the light of the intense interest in the project among doctors and industry, however, we took the rather unusual step of inviting Purves to prepare an interim report that we could put in the public domain.² The interim report is just that: an appreciation of work in progress, not a definitive statement of results. The conclusions in the report ("our very tentative assessment ...") are guarded, and the strongest claims are made not for the Prodigy system itself but for only the general concept of patient specific decision support.

Those who wish to see formal tests of statistical significance will have to wait patiently, as we are doing, for the final report. In the meantime, we continue to have every confidence in the integrity, statistical or otherwise, of Purves and his team.

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1 Buchan IE, Hanka R, Pencheon D, Bundred P. Introduction of the computer assisted prescribing scheme Prodigy was premature. *BMJ* 1996;313:1083. (26 October.)

2 Purves IN. *Prodigy interim report*. Newcastle upon Tyne: Sowerby Unit for Primary Care Informatics, University of Newcastle, 1996.

Spinal cord injuries have fallen in rugby union players in New South Wales

EDITOR,—In 1995 Noakes and Jakoet stated, "we still do not know the true incidence of spinal cord injury or cervical injury in any rugby playing country."¹ An 11 year retrospective study has determined the incidence among participants of rugby union and rugby league in the state of New South Wales (NSW), Australia.

The data presented here are based on claims paid by the NSW government sporting injuries insurance scheme.² This scheme notes the frequency of spinal cord injury with neurological deficit occurring in senior and junior registered participants (all players and a small number of referees, touch and goal judges, ballboys, and time keepers). The insurance scheme was the insurer for rugby league in NSW during 1984-94 inclusive and for rugby union during 1984-9 inclusive. From 1990 rugby union was covered by a private insurer, which supplied the remaining data. The data do not include injuries

incurred during games conducted by schools or non-registered organisations. All claims recorded here were for spinal cord injury with resultant quadriplegia or paraparesis.

The incidence of spinal cord injury in rugby league in the 11 years 1984-94 in NSW was 0.18/10 000 registered participants per year (table 1). The figure for rugby union was 0.53/10 000; although this suggests a greater risk in rugby union compared with rugby league, the difference did not achieve significance. Rugby union differs from rugby league in having four extra men in the scrum, and whereas in rugby league a tackled man is allowed to get up and "play the ball," in rugby union a spontaneous ruck of up to 16 men forms over the downed man. Nine of the 14 spinal cord injuries that occurred in rugby union were related to the scrum, while five occurred in players involved in rucks and mauls. Of the 16 spinal cord injuries that occurred in rugby league, four were related to the scrum, eight occurred during tackles, and four were a result of other contact with an opponent.

The force of engagement in the scrum has been implicated as a cause of spinal cord injury.³ Cases of spinal cord injury in rugby union fell significantly during 1984-94 ($r^2 = 0.36$, $P < 0.05$), and this may reflect changes in the rules relating to a "phased sequence of scrum engagement" and the collapse of scrums.⁴ However, there is no reason for complacency, because the current season has already seen one case of quadriplegia in rugby league and two cases in rugby union.

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1 Noakes T, Jakoet I. Spinal cord injuries in rugby union players. *BMJ* 1995;310:1345-6.

2 New South Wales Government Sporting Injuries Insurance Scheme. *Annual reports, sporting injuries committee, 1984-95*. Sydney: NSWGSIS, 1984-95.

3 Taylor TKF, Coolican MRJ. Spinal cord injuries in Australian footballers, 1960-1985. *Med J Aust* 1987;147:112-8.

4 International Rugby Football Board. *Australian Rugby Football Union handbook 1995. Laws of the game of rugby football*. Sydney: Australian Rugby Football Union, 1995:131 (law 20(2)).

Table 1—Number of cases of spinal cord injury incurred in rugby league and rugby union in New South Wales, 1984-94

	Rugby league		Rugby union	
	Cases	Participants	Cases	Participants
1984	0	84 529	1	23 357
1985	2	78 739	2	23 590
1986	1	80 219	3	24 600
1987	2	81 515	4	22 020
1988	2	79 966	2	23 510
1989	1	79 974	1	23 510
1990	1	78 627	0	22 964
1991	2	80 762	0	25 150
1992	4	83 656	0	26 584
1993	0	81 923	0	23 921*
1994	1	84 810	1	23 921*
Average No of participants/year		81 338		23 921
Average No of cases/year		1.45		1.27
Incidence/year		0.18/10 000		0.53/10 000

*Average for 1984-92, because actual numbers of juniors and seniors who participated in 1993 and 1994 were not available. Odds ratio for rugby union compared with rugby league: 2.98 (95% confidence interval 0.28 to 32.27).

Relation between treatment benefit and underlying risk in meta-analysis

Standard of "label invariance" should not be abandoned

EDITOR,—Stephen J Sharp and colleagues' discussion of the dangers of attempting to compare treatment benefit with underlying risk in meta-analyses of placebo controlled trials of a presumed active treatment is excellent in two respects.¹ Firstly, the authors clearly show a point that I have made—that when the difference in mean observed outcome between groups is related to the mean outcome in the placebo group then a spurious correlation is induced.² Secondly, they make the important point that it is more relevant to establish the relation between baseline characteristics of the patient and the treatment effect: after all, the prescribing doctor can assess the patient when he or she presents but does not know what the placebo outcome would be.

I am not encouraged by the authors' discussion of the second of the three methods they consider (comparing difference with average). They seem to be in danger of making the sort of conceptual mistake that they condemn. For example, they show, rightly, that if the true placebo results are constant across all trials but if the true active results vary then there will be a correlation between difference and mean. When they say, however, that this correlation is misleading because "in truth there is no relation with underlying risk" they are assigning to placebo a unique role, which they are not prepared to assign to treatment, and hence are abandoning (as may be seen from the model in the appendix) the standard of "label invariance" implicit in many approaches to analysing clinical trials.

An alternative interpretation of the correlation can be given if the treatment group is taken as indexing the risk. It then reflects the fact that those who have the truly better outcome stand to lose more if switched to placebo. If the authors reject this as being too contrived an explanation then they are implicitly admitting that other methods are needed to deal not only with trials comparing two active treatments but also with withdrawal trials (as, say, in epilepsy) and with diseases (such as asthma) in which trials are rarely run in patients who are not currently being treated. In fact, they will be smuggling into their conclusions assumptions that go beyond the pure logic of clinical trials.

For me the moral is: if you want to study differential effects of treatment then make sure that your predictive factors are observable antecedents of the outcome.

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1 Sharp SJ, Thompson SG, Altman D. The relation between treatment benefit and underlying risk in meta-analysis. *BMJ* 1996;313:735-8. (21 September.)

2 Senn SJ. Importance of trends in the interpretation of an overall odds ratio in the meta-analysis of clinical trials. *Stat Med* 1994;13:293-6.

Authors' reply

EDITOR,—We agree with Stephen Senn that analyses based on measurable patient characteristics rather than on underlying risk are preferable from both clinical and statistical viewpoints. Such analyses, however, require data on individual patients, which are not usually available to meta-analysts at present, while it may be possible to assess risk by using data from