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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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 quadriparesis.

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: NSWGSIIS, 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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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