

manner supportive of the 'right to protect public health and, in particular, to promote access to medicines for all'.<sup>3</sup>The US-Australia agreement does not mention equity of access or the quality use of medicines.

The details of the agreement will probably depend on the Medicines Working Group, which will be established 'to promote discussion and mutual understanding of the issues'. It is unknown if these discussions will be secret, but the only members of the Medicines Working Group will be officials from federal government agencies.

If the official line is that there will be no changes to the PBS, then why were pharmaceuticals included in the agreement? The USA has a legislative requirement for negotiations 'to achieve the elimination of government measures such as price controls and reference pricing which deny full market access for United

States products'.<sup>4</sup> Is the US-Australia agreement an exception to this rule? If it is not, inclusion of pharmaceuticals in the agreement could eventually prove to be a costly mistake with potentially adverse consequences for public health.

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## Letters

Letters, which may not necessarily be published in full, should be restricted to not more than 250 words. When relevant, comment on the letter is sought from the author. Due to production schedules, it is normally not possible to publish letters received in response to material appearing in a particular issue earlier than the second or third subsequent issue.

### Are new drugs as good as they claim to be?

Editor, – It was disappointing to read that there are still people questioning the gastrointestinal safety and cost-effectiveness of the COX-2 inhibitors (*Aust Prescr* 2004; 27:2–3). It is even more disappointing when this opinion is referenced to a single non-systematic, heterogenous review article (that is, evidence level 5), which misrepresents the body of evidence in two important ways.

The review claims that non-steroidal anti-inflammatory drugs (NSAIDs) have minimal benefit against which to compare their adverse events. This is based on a very selective use of analgesic data from the literature (which still showed a significant difference to placebo). An alternative view is that NSAIDs are the mainstay of therapy worldwide for the symptomatic relief of arthritis and occupy the first five top rankings for analgesics on the Oxford pain relief table because of their clinical benefits.<sup>1</sup> This is backed by clinical trials where both COX-2 inhibitors and traditional NSAIDs showed statistically and clinically different efficacy to placebo in arthritis.<sup>2,3,4,5</sup>

The article by Wright also states that there is no evidence for reduced gastrointestinal damage from COX-2 inhibitors. He bases this opinion on a single flawed study (CLASS) that had a statistical power of about 45% (that is, less than a 50% chance of detecting any real differences).<sup>6</sup> He neglects to mention the wealth of other data from adequately powered studies that show a significant difference in safety and tolerability between celecoxib and the non-specific NSAIDs.<sup>7,8,9,10,11,12,13</sup>

If the COX-2 inhibitors did not represent a cost-effective treatment then they would not be listed on the Pharmaceutical Benefits Scheme. The Pharmaceutical Benefits Advisory Committee makes this decision based on evidence, not opinion.

Dr Simon McErlane  
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*Associate Professor J. Lexchin, the author of the editorial, comments:*

Dr McErlane dismisses the results of the CLASS study on celecoxib by claiming that it was underpowered to find significant benefits. CLASS was funded by Pharmacia, the company that marketed celecoxib, and the corresponding author was a Pharmacia employee. Pharmacia is now owned by Pfizer. If there was a problem with the design of CLASS then Dr McErlane should look to his own house.

He criticises the article by Dr Jim Wright for ignoring seven articles showing the gastrointestinal benefits of COX-2 inhibitors. However, one was a poster presentation that was otherwise unpublished and two were published either just before or after Dr Wright's piece and would have been unavailable to him.

Dr McErlane has misread Wright's article. Wright does not say that COX-2 drugs have minimal benefits; what he does say is that the benefits need to be seen in the context of

serious adverse events from these drugs. Serious adverse events include not only gastrointestinal problems but other adverse events. Wright combines all serious adverse events as reported in the CLASS study for celecoxib and for other NSAIDs and shows that there is no statistical difference in serious adverse events between celecoxib and the other NSAIDs. In other words, whatever reduction in gastrointestinal harms celecoxib produced was offset by a higher incidence of other serious adverse events.

Dr McErlane's letter provides a good lesson in why doctors should not rely solely on what companies have to say about their products.

### **Prescribing issues for Aboriginal people**

Editor, – I read with interest the paper 'Prescribing issues for Aboriginal people' (*Aust Prescr* 2003;26:106–9). My research into the practice of remote area nursing shows that there are serious problems in the acquisition and use of drugs in remote Aboriginal settings.

I would like to draw your attention to the initiatives taken in Queensland. Unlike the standard treatment manual referred to in the article, a 'Primary Clinical Care Manual' (3rd ed. 2003) has been developed by the Queensland Nursing Council, Royal Flying Doctor Service and Queensland Health, based on statutory regulations, for use by nurses authorised in isolated practice. Under State legislative provisions of the Health (Drugs and Poisons) Regulation 1996, a process is in place for the formal endorsement of nurses in isolated practice areas and for indigenous health workers with specific protocols clarifying their separate responsibilities in relation to drugs and drug use.

Jennifer Cramer  
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### **Ibuprofen use**

Editor, – Over the past five years the use of ibuprofen to treat fever in children has increased dramatically at the Royal Children's Hospital, Melbourne. This is demonstrated by a seven-fold increase in the purchases of ibuprofen packs/year from 1999 to 2003 (Fig. 1). Paracetamol usage and purchase has remained essentially unchanged over the same period, and there has been no significant change in the number or type of patients seen at our hospital. This continually increasing shift in practice has occurred despite the fact that there has been no change in hospital policy on the use of non-steroidal anti-inflammatory drugs. Furthermore, a monthly audit of ibuprofen use on our general paediatric ward showed that 36 of 38 prescriptions for ibuprofen also included paracetamol.

This change in practice may be a combination of three factors. Number one being aggressive marketing of ibuprofen by the drug company, second the change of ibuprofen syrup from Schedule 4 to Schedule 2 in 1998, and finally an increase in the number of British-trained doctors working in our institution. Ibuprofen is far more commonly used in Britain than Australia.

This therapeutic drift is occurring despite a lack of evidence to support it. Paracetamol has been used far more extensively worldwide than ibuprofen, so much so that the risks associated with the use of paracetamol are well known. The same cannot be said for ibuprofen use in children. Ibuprofen has no demonstrated advantages over paracetamol for the treatment of fever, nor has the combined use of these drugs been shown to be of benefit. In fact the combination may lead to an increased incidence of serious adverse effects and confusion regarding their correct dosing.<sup>1,2,3,4</sup>

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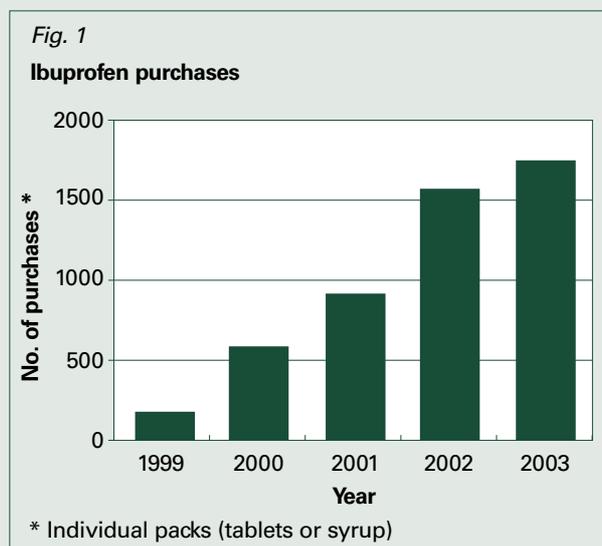
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## Hyponatraemia

Editor, – I think there is an unintentional inaccuracy in the Summary of the article 'Managing drug-induced hyponatraemia in adults' (*Aust Prescr* 2003;26:114–7).

The first sentence of the Summary states that 'drug-induced hyponatraemia occurs in approximately 5% of outpatients...' but the source for this statement seems to be the Introduction which merely states that: 'A Melbourne laboratory found hyponatraemia in 4.8% of 326 923 samples from ambulatory patients ...'.

Obviously the Melbourne sample is not representative of the whole population of ambulatory patients, or outpatients, as implied by the statement in the Summary. It is only a sample of patients who merited a blood sample being sent to the laboratory. Presumably these patients were sick enough for their general practitioner to investigate (we could call them 'sick outpatients'), and there is no account taken of all the ambulatory patients who did not have samples taken ('well outpatients'). The proportion of 'sick outpatients' who have samples sent to a laboratory is very small, surely less than 10% of the whole and probably much less than that. The problem with the statement in the Summary is that it is likely to be cited (especially when it appears in an authoritative publication like *Australian Prescriber*) but quoted out of context and so could mislead. It is certain, surely, that the proportion of outpatients with hyponatraemia is much less than 5%. Frankly, I'd be surprised if it was more than 0.5%.

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*Dr S. Furlanos and Dr P. Greenberg, the authors of the article, comment:*

We thank Mr Baker for drawing our attention to misinterpretation of the first sentence of the Summary.

We hope that other readers, like him, will have read in the Introduction the selection process for the patients referred to in the Summary.

We agree that the first sentence of the Summary should read: 'Hyponatraemia occurs in approximately 5% of ambulatory and 14% of admitted patients referred for blood tests by general practitioners'.

The prevalence of hyponatraemia in other non-admitted patients and in the broader community is also unknown to us.