

Letters to the Editor

P6 acupressure reduces morning sickness

I am concerned about a few points in the article by Dundee *et al.* (August 1988 *JRSM*, p 456). I am delighted to see serious efforts made to test controversial therapies, but I have difficulty justifying the conclusions drawn by the authors. There are three confounding variables in the study that all tend to increase the apparent effect of the 'P6' treatment. The 'P6' group was about a week further along in gestation than the others, thus lessening the tendency to nausea. There is 'widespread publicity' already that wrist manipulation is a treatment for nausea, yet the control treatment site was far away at the elbow. The authors also readily admit that their 50% return rate adds bias to their results. It was only by adversely weighting their results to adjust for the latter that the authors found their significant results, because that lessened the apparent effect of the control point treatment. Despite this, though, they report a $P < 0.04$ which indicates that the control point is still significantly effective! And it is not fair to say that if P6 pressure is better than the untreated control, and the dummy point is no different than the control, that one has demonstrated anything about the relationship between the two treatment groups. Making multiple comparisons like this is not kosher. They claim to have done a placebo controlled blinded study, yet end up drawing conclusions based solely on the performance of their untreated, unblinded controls.

This article describes a wonderful demonstration of the placebo effect; and if indeed there is any effect of the P6 pressure therapy it is certainly smaller than this placebo effect. I am concerned that the enthusiastic title and abstract of this article may be misinterpreted by the casual reader and by non-scientists, who may mistakenly think that acupressure had proven effectiveness in this case. I cannot find support for that in this paper.

I hope that more studies along these lines can be done to further define the utility of these traditional therapies.

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The author replies below:

I share Dr Root's concern of the difficulty of carrying out and interpreting scientific studies with acupuncture. Regrettably in this study, and in a subsequent one on reduction of sickness after cancer chemotherapy¹, we were not able to achieve the degree of excellence in scientific clinical investigation which was the hallmark of our studies in postoperative sickness. In these investigations, involving more than 500 patients²⁻⁵ we clearly demonstrated the protective effect of P6 acupuncture/acupressure in preventing opioid-induced sickness. We also demonstrated that the 'dummy' point used in the obstetric study was without antiemetic activity. A thorough search of the literature has not revealed any other proven 'dummy' point.

The slight difference in gestation time in one group was a purely chance occurrence.

Reference to widespread publicity is puzzling. Until our first major publication on traditional Chinese acupuncture (*Br Med J*, 6 September 1986) there was no publicity on the use of wrist bands or wrist pressure for antiemesis in Britain. By this time, more than 90% of the data in our paper had been obtained. Any women who had heard of this would most likely have told us.

We adjusted our findings to make up for the 20% difference in returns between the P6 group and the other two (not for the alleged 50% overall returns - assuming that these 20% had the same frequency of sickness as the controls. Could we have been fairer? We are not aware of the 'kosher' statistical test referred to, we simply compared A with B, B with C and A with C, using a 3x2 contingency test.

Undoubtedly there is some placebo effect from any acupuncture. However taking all three aspects of our antiemetic studies together, there are probably as many difficulties in the minds of the readers in accepting their validity as there are in design of studies to eliminate this effect.

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References

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Tissue oxygenation, venous ulcers and fibrin cuffs

The paper by Mani *et al.* (June 1989 *JRSM*, p 345) raises some interesting points on the interpretation of transcutaneous oxygen (TcPO₂) recordings.

A control TcPO₂ measurement is accepted as necessary in order to make valid interpretations of the below knee TcPO₂¹, and is usually taken from the arm, thigh or precordium. In their study, the control TcPO₂ measurements were taken from the ulcerated limb below the knee. These legs had been shown to be abnormal by Doppler ultrasound, and the control reading was (by definition of venous incompetence) taken from an abnormal region, even though the skin appeared normal and without signs of lipodermatosclerosis. This therefore makes the comparison of ulcer edge to control skin TcPO₂ invalid.

There is a gradient in the TcPO₂ down the limb, from knee to foot of 5-10 mmHg^{1,2}. This is not taken into account and no control measurements were taken below the knee in the normal group for comparison with the ulcer group.