the Italian cases a faulty water supply has been implicated.¹ However, viral contamination of implicated batches, confirmed by polymerase chain reaction or animal infectivity studies, has not been established. Nor has hepatitis A virus infection by associated red cell donations been reported. Clinical hepatitis A virus infection, ascribable to factor VIII concentrate, has not been found in the other countries represented at the meeting and specific assays have revealed only nine seroconversions in more than 400 patients with haemophilia A (ascribed to community factors) in the past five years. Furthermore, the age related prevalence of hepatitis A virus antibody, in more than 1200 haemophilic patients, was similar to or less than that in the general population. Most reports related to factor VIII products prepared by using solvent detergent treatment combined with ion exchange chromatography, although similar data are available for pasteurised products⁵ and products monoclonally purified with solvent detergents.

Transmission of hepatitis A virus by factor VIII, if it occurs at all, is a rare event, as is true of transmission by single donor blood products.⁶ This is supported by the lack of raised concentrations of alanine aminotransferase, a marker of hepatitis, in numerous trials of current factor VIII products in previously untreated patients, apart from one case in Italy.⁷

Data were also presented for the absence of hepatitis A virus nucleic acid from 77 batches of factor VIII treated with solvent detergent. In addition, evidence was given that hepatitis A virus is cleared by a combination of antibody neutralisation, differential precipitation, and freeze drying during the preparation of factor VIII.

Although solvent detergent treatment is ineffective in inactivating non-enveloped viruses,⁸ current data suggest that hepatitis A virus transmission by factor VIII concentrates is not a major problem. Most of those present in Edinburgh concluded that, although research into additional viral inactivation or clearance procedures should be pursued vigorously, caution should be exercised in adopting such approaches until they have been fully evaluated for safety and efficacy.

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- 1 Mannucci PM. Outbreak of hepatitis A among Italian patients with haemophilia. *Lancet* 1992;339:819.
- 2 Gerritzen A, Schneeweis KE, Brackmann HH, Oldenburg J, Hanfland P, Gerlich WH, *et al.* Acute hepatitis A in haemophiliacs. *Lancet* 1992;340:1231-2.
- 3 Peerlinck K, Vermeylen J. Acute hepatitis A in patients with haemophilia A. *Lancet* 1993;341:179.
- 4 Temperley JJ, Cotter KP, Walsh TJ, Power J, Hillary IB. Clotting factors and hepatitis A. *Lancet* 1992;340:1466.
- 5 Kreuz W, Klamann D, Auerswald G, Auberger K, Gürtler L, Rabenaw H, *et al.* Absence of hepatitis A after treatment with pasteurised factor VIII concentrate in children with haemophilia A and von Willebrand disease. *Lancet* 1993;341:446.
- 6 Lee KK, Vargo LW, Le CT, Fernando L. Transfusion acquired hepatitis A outbreak from fresh frozen plasma in a neonatal intensive care unit. *Pediatr Infect Dis J* 1992;11:122-3.
- 7 Mariani G, Di Paolantonio T, Baklaia R, Mannucci PM. Prospective hepatitis C evaluation of a high purity solvent detergent treated factor VIII concentrate. *Blood* 1991; 78(suppl 1):55a.
- 8 Mannucci PM. Clinical evaluation of viral safety of coagulation factor VIII and IX concentrates. *Vox Sanguinis* 1993;64: 197-203.

Should susceptible homosexual men be offered immunisation?

EDITOR,—From evidence of an increasing incidence of rectal gonorrhoea and HIV-1 infection in homosexual men B G Evans and colleagues have concluded that unsafe sexual behaviour has increased after a period of decline.¹ Hepatitis A virus infection is also a significant risk to homosexual men. The virus is spread by the faeco-oral route and may be transmitted by oral-anal contact.² Several reports have documented an increase in

Numbers (percentages) of cases of hepatitis A virus infection, 1989-92

	Men		Women	Children	Total
	Homosexual	Heterosexual			
1989	4 (9)	15 (33)	13 (29)	13 (29)	45 (100)
1990	10 (18)	15 (27)	11 (20)	19 (35)	55 (100)
1991	23 (48)	11 (22)	7 (15)	7 (15)	48 (100)
1992	28 (48)	15 (30)	9 (17)	6 (13)	58 (100)
Total	65 (32)	56 (27)	40 (19)	45 (22)	206 (100)

hepatitis A infections in homosexual men in the United Kingdom,³ the United States, Canada, and Australia.⁴

The table shows the cases of hepatitis A virus infection presenting to our laboratory, which serves a large genitourinary medicine clinic as well as inner city general practitioners. Infection was confirmed by the presence of IgM specific to hepatitis A (Abbott MEIA IgM capture test). All patients had a clinical illness consistent with hepatitis.

The rise in hepatitis A virus infection seen in homosexual men presenting to the clinic over the past four years may be due to the increased use of oral-anal contact in the hope of reducing HIV transmission⁵ or to a general increase in promiscuity in this group. It is important, therefore, that advice given on homosexual practices to avoid HIV infection should also take hepatitis A virus infection into account because of the different routes of transmission of the viruses.

A vaccine inactivated by formaldehyde (Havrix, SmithKline Beecham) prepared from the HM175 strain of hepatitis A virus has recently been licensed for use in the United Kingdom, but its routine use in homosexual men is not recommended at present. The Department of Health guidelines state that there are currently no data to support routine immunisation in this group.⁵ However, with accumulating evidence of increasing hepatitis A virus infection in homosexual men and the increased morbidity associated with hepatitis A virus infection in adults compared with children, serious consideration must now be given to offering hepatitis A virus vaccine to homosexual men. Screening to identify susceptible individuals should be done before vaccination because of the cost of the vaccine (£27.20 for a primary course).

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- 1 Evans BG, Catchpole MA, Heptonstall J, Mortimer JY, McCarrigle CA, Nicholl AG, *et al.* Sexually transmitted diseases and HIV-1 infection among homosexual men in England and Wales. *BMJ* 1993;306:426-8. (13 February.)
- 2 Corey L, Holmes KK. Sexual transmission of hepatitis A in homosexual men. *N Engl J Med* 1980;302:435-8.
- 3 Kani J, Nandwani R, Gilson RJC, Johnson AM, Maguire HC, Tedder RS. Hepatitis A virus infection among homosexual men. *BMJ* 1991;302:1399.
- 4 Hepatitis A among homosexual men. *MMWR* 1992;41:161-4.
- 5 Department of Health. *Immunisation against infectious disease*. London: HMSO, 1992.

The Hawksley random zero sphygmomanometer

Comparison with mercury instrument is illogical

EDITOR,—The provocative article by Rónán M Conroy and colleagues¹ has created remarkable irritation in the cardiovascular research community.² I would like to draw attention to an aspect that has been neglected—namely, the validity of testing the random zero against the standard mercury sphygmomanometer.

Random zero sphygmomanometers were introduced in 1970 by Wright and Doré because researchers wanted to improve the quality of blood pressure measurement with the standard mercury device. Contrary to the frequently stated misconceptions that the random zero device reduces last digit preference (which it was not intended to do),^{3,4} “the main purpose of the instrument was to eliminate bias from preconceived ideas and to make blind duplicate readings possible.”⁵ Whatever the direction of observer bias—whether resulting in overestimation or underestimation of true values—the readings obtained with a random zero sphygmomanometer are intended to be different from those taken with the standard mercury sphygmomanometer. It is therefore illogical to use the standard mercury device as the reference against which the Hawksley instrument is tested. If the Hawksley random zero instrument is doing what it is supposed to do—that is, improving the validity of replicate measurements—then it is expected to produce lower systolic and diastolic pressures than the standard mercury sphygmomanometer. Thus the observation of such a difference must not be taken as an argument against the use of a random zero sphygmomanometer.

According to the overwhelming available evidence,^{2,3} however, this difference is much smaller than that found by Conroy and colleagues. The authors did not even attempt to present reasonable explanations for their amazing quantitative observations. Thus I am not surprised that John Garrow and Carolyn Summerbell, in what seems to have been a reproducible video assessment, were unable to replicate the findings.² I believe that Conroy and colleagues' article has caused confusion by challenging established medical practice on unsteady scientific grounds.

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- 1 Conroy RM, O'Brien E, O'Malley K, Atkins N. Measurement error in the Hawksley random zero sphygmomanometer: what damage has been done and what can we learn? *BMJ* 1993;306: 1319-22. (15 May.)
- 2 Correspondence. The Hawksley random zero sphygmomanometer. *BMJ* 1993;307:123-5. (10 July.)
- 3 Kronmal RA, Rutan GH, Manolio TA, Borhani NO. Properties of the random zero sphygmomanometer. *Hypertension* 1993;21:632-7.
- 4 Lawson M, Fredericks S, Johnston A. The Hawksley random zero sphygmomanometer: unbiased assessment of blood pressure? *J Hum Hypertens* 1993;7:97.
- 5 Wright BM. “Failure” of the random zero sphygmomanometer in general practice. *BMJ* 1985;291:137.

Authors' reply

EDITOR,—We are gratified that our paper has generated critical debate.¹ We are asked where the source of error lies. It might lie with the observers, the device, the patient, or a combination of these.

Were our observers misusing the Hawksley device? We hardly think so. Not only were they trained to meet the stringent requirements of the British Hypertension Society's protocol but they were also carefully instructed in the proper use of the device. We had anticipated the criticism of observer prejudice, and the observers were not told the purpose of the study.

The device itself has several potential sources for error. For example, the valve at the rear may not completely seal off the reservoir when it is closed, and, during deflation, movement of mercury around the valve may be observed, resulting in air being trapped below the valve possibly because of pressure from the bladder. As in standard sphygmomanometers, the mercury does not always return to zero when the bladder is deflated; this is obvious to users of the standard sphygmomanometer but is hidden with the Hawksley device. We now have the mark II version, which is presumably more accurate than its predecessor. Might the manufacturers tell us the reasoning that motivated any technical modifications?

It has been pointed out that underestimation by the Hawksley sphygmomanometer was much greater in our study than in others. We have performed regression analysis on our data,² which shows that the error increases with rising blood pressure, this being significant for both systolic ($p=0.022$) and diastolic pressures ($p=0.044$). This led us to examine the blood pressures of the subjects in the studies tabulated by D Churchill and colleagues.³ We found that the mean blood pressure of our patients (150/90 mmHg) was higher than that in other studies (134/82 mmHg,⁴ 116/80 mmHg,⁵ 117/72 mmHg,⁶ and 117/75 mmHg⁷). This therefore at least partly accounts for the discrepancy in the magnitude of error between studies.

Hans Werner Hense, who finds our call for debate a "remarkable irritation," has none the less aired an interesting view—namely, that the Hawksley sphygmomanometer, by virtue of its design, "is expected to produce lower systolic and diastolic pressures than the standard mercury sphygmomanometer." This may well be so, but it does not diminish the argument that we cannot have one measurement for research and another for clinical practice unless a correction factor can be applied to adjust one for the other.

John Garrow and Carolyn Summerbell were unable to demonstrate the inaccuracy we and others have shown when a video camera was substituted for the human observer.³ This is interesting, but before commenting we would need more data.

John A Dinning admonishes us for having extolled the Hawksley sphygmomanometer in 1981.³ We admit having accepted the Hawksley instrument uncritically, and we stand accountable along with many others for doing so. This raises the broader issue of who should be responsible for determining the accuracy of a device—the user or the manufacturer? However much the scientific community may quibble over the magnitude of error, there is now a substantial body of evidence showing that the Hawksley sphygmomanometer not only underestimates blood pressure^{7,8} but may influence the pressures recorded in unpredictable ways.⁹ It is now mandatory that the Hawksley mark II instrument be submitted to independent validation (as we urged nearly two years go) before it is marketed for use in research into hypertension.

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2 O'Brien ET, Mee F, Atkins N, O'Malley K. Inaccuracy of the Hawksley random zero sphygmomanometer. *Lancet* 1990;336:1465-8.

3 Correspondence. The Hawksley random zero sphygmomanometer. *BMJ* 1993;307:123-5. (10 July.)

4 Evans JG, Prior IAM. Experience with the random-zero sphygmomanometer. *British Journal of Preventive and Social Medicine* 1970;24:10-5.

5 Labarthe DR, Hawkins CM, Remington RD. Evaluation of performance of selected devices for measuring blood pressure. *Am J Cardiol* 1973;32:546-53.

6 De Gaudemaris R, Folsom AR, Prineas RJ, Luepker RV. The random-zero versus the standard mercury sphygmomanometer: a systematic blood pressure difference. *Am J Epidemiol* 1985;121:282-90.

7 Parker D, Liu K, Dyer AR, Giunetti D, Liao Y, Stamler J. A comparison of the random-zero and standard sphygmomanometers. *Hypertension* 1988;11:269-72.

8 Kronmal RA, Rutan GH, Manolio TA, Borhani NO. Properties of the random zero sphygmomanometer. *Hypertension* 1993;21:632-7.

No harm done

EDITOR,—Rónán M Conroy and colleagues are wrong to state in their critique of the random zero sphygmomanometer and its users that "results of the entire [World Health Organisation MONICA] project will have to be corrected for measurement bias" and "data will have to be adjusted before results from the Hawksley instrument can be pooled with those from countries using mercury instruments."¹

The WHO's MONICA (MONitoring Cardiovascular disease) project analyses the relation between trends in coronary heart disease mortality, non-fatal myocardial infarction, coronary risk factors, and acute coronary care over 10 years within defined communities. End points of the study are therefore not the levels of anything, but changes over time within each centre. There is no intention to pool results. Standardisation is primarily longitudinal within each population, so some differences in methods between populations are permitted.² This use of the random zero machine has been identified in 13 out of 44 populations whose initial risk factor findings have been published.³

Blood pressure measurement is notoriously susceptible to multiple sources of measurement bias. It is unrealistic to assert that these can be corrected retrospectively. Most medical staff are taught badly on the simple mercury sphygmomanometer. In the early 1980s, when the MONICA study was planned, the random zero machine was the widely used standard and usefully involved retraining of observers. It was more likely to be available over 10 years than any single electronic device then known and promised to provide reasonable consistency over time. Use was recommended but not obligatory.

Even if the differences reported are consistent (and the discrepancy between systolic and diastolic readings is unexplained), the authors are quite wrong in implying that MONICA investigators should be throwing their random machines away. That would be the worst possible advice. The answer to their article's subheading, "What damage has been done?" for MONICA is "Virtually none, provided investigators continue to use the blood pressure device they used first"—or, to quote an American saying, "Dance with the partner you came in with."

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1 Conroy RM, O'Brien E, O'Malley K, Atkins N. Measurement error in the Hawksley random zero sphygmomanometer: what damage has been done and what can we learn? *BMJ* 1993;306:1319-22. (15 May.)

2 Tunstall-Pedoe H for WHO MONICA Project. The World Health Organization MONICA Project (monitoring trends and determinants in cardiovascular disease): a major international collaboration. *J Clin Epidemiol* 1988;41:105-14.

3 Pajak A, Kuulasmaa K, Tuomilehto J, Ruokokoski E for WHO MONICA Project. Geographical variation in the major risk factors of coronary heart disease in men and women 35-64 years. *World Health Stat Q* 1988;41:115-40.

Preventing neurocysticercosis

EDITOR,—W Y Nassar and W J K Cumming are incorrect in stating that people may acquire neurocysticercosis by eating partially cooked

pork.¹ Doing this would result in tapeworm infestation. Neurocysticercosis can be acquired only by ingestion of the eggs shed from the tapeworm into human faeces. Poor hygiene of the food handler or food contaminated with faecal material is the mode of infection. Thus people can develop the disease without ever eating pork. This was clearly illustrated in an outbreak of neurocysticercosis in an orthodox Jewish community: the source was traced to a domestic employee with active infection with *Taenia solium*, which she had probably acquired in her native Mexico.² Preventing neurocysticercosis principally depends on improved basic sanitation and hygiene.

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1 Nassar WY, Cumming WJK. Minerva. *BMJ* 1993;307:142. (10 July.)

2 Schantz P, Moore A, Munoz J. Neurocysticercosis in an orthodox Jewish community in New York City. *N Engl J Med* 1992;327:692-5.

Ultrasonography and handedness

Don't confuse direction with degree

EDITOR,—Kjell Salvesen and colleagues claim, on the basis of a randomised controlled trial, that ultrasound examination in utero is associated with subsequent "non-right handedness."¹ The term non-right handedness has generated much confusion in the literature on handedness since it is very different from its seemingly commonsense equivalent of left handedness. When assessed by questionnaire, handedness shows a symmetric bivariate mixture distribution,² within which it is necessary to distinguish between direction of handedness (right *v* left) and degree of handedness (strong *v* weak handedness, be it in right or left handers). Non-right handedness is confusing because it lumps together strong left handers, weak left handers, and weak right handers, with the latter group typically being larger than the other two groups. The problem is then that it is not clear if an association is with direction of handedness or degree of handedness. Furthermore, the problem cannot be resolved simply by the use of non-parametric statistics but actually requires a more complex parametric approach.

In Salvesen and colleagues' study most of the effect shown seems to be due to differences in degree of handedness. Effects are non-significant for items such as drawing, writing, and use of scissors, for which "mixed" responses are very rare, and for those items which show significant effects the difference is principally in the proportion of mixed responses rather than left responses (items 3, 12, 16, and 17).

The implications of an effect on degree of handedness are different from those on an effect on direction of handedness; in particular, the Satz model of pathological left handedness³ applies only to direction of handedness. Degree of handedness has been relatively little studied, but it is known to increase with age in children.⁴ It is therefore important for the authors to confirm that differences in age at assessment cannot explain the differences between the screened and control groups.

Recent work in humans and animals suggests that differences in degree of lateralisation may reflect developmental instability resulting either from a lack of heterosis, or from fluctuating asymmetry due to "developmental noise" originating in a range of sources.⁵ If the effect of ultrasound on degree of handedness can be repli-