placental carcinogenic effects of stilboestrol. We are at present engaged in an investigation, using similar methodology, of possible aetiological factors in carcinoma of the endometrium in young women.

The above are, of course, only a small sample of the many studies which could have been cited. We hope, however, that they will remind your readers that cancer registration is worth while even if it is feasible to collect only the simplest of information.

LEO KINLEN

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MARTIN VESSEY

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[1] Alderson, M R, and Nayak, R, *British Journal of Preventive and Social Medicine*, 1961, **25**, 168.
[2] Vessey, M P, Johnson, B, and Donnelly, J, *British Journal of Preventive and Social Medicine*, 1974, **28**, 104.
[3] Lee, W R, Alderson, M R, and Downes, J E, *British Journal of Industrial Medicine*, 1972, **29**, 188.
[4] Doll, R, *Journal of the Irish Medical Association*, 1973, **66**, 117.

Social policy and the NHS

SIR,—In his review of my book *Sociology and Social Policy* (13 December, p 634) Mr Rudolf Klein suggests that I am "intolerant" of the failings of the Labour Government of 1964-70. He says I failed "to cite the conclusion of the most convincing analysis made of its social policy record (by Michael Stewart in Wilfred Beckerman's *The Labour Government's Economic Record* 1964-70) that there was in fact a 'measurable improvement' in the direction of equality in the distribution of income." He goes on to add that though I may disagree with Stewart's conclusion "the point at least needs arguing."

Mr Klein is incorrect about a question of fact. First, the papers covering the period 1964-70 which are included in my book were written and published before Stewart's paper appeared. Second, the point *has* been argued at length in the *Listener*.[1] I argued that Mr Stewart's conclusion rested primarily on one table which he had produced and which was technically incorrect. Though we engaged in a long correspondence subsequently in the *Listener* and argued about many other matters, Mr Stewart did not take issue with that central contention.

Evidence published recently further suggests that, in a number of different senses, there was no "measurable improvement" in the direction of equality in the distribution of income during 1964-70. In a detailed review published in 1974 Nicholson[2] concluded that "the degree of inequality of final income remained remarkably constant throughout the whole period 1961-71." He worked out a weighted average of the Gini coefficient for six main types of family for different definitions of income. The coefficient for income after all taxes and benefits was 24.7 for 1964-5 and 24.7 for 1970. Again, the first report of the Royal Commission on the Distribution of Income and Wealth gives the information shown in the accompanying table.[3]

I would invite Mr Klein not only to examine such data for evidence of marked *trends*, but also to ponder the fundamental implications of the ratio between top and bottom 10% of approximately 10:1. For the period 1964-70, no less than for other periods of history, it is the scrupulous weighing of evidence rather than comfortable contemporary myth or the desire to believe we live in a reasonable and fair society that must govern the conclusions which historians and social scientists draw.

PETER TOWNSEND

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[1] Townsend, P, *Listener*, 1972, **87**, 558.
[2] Nicholson, J L, in *Poverty, Inequality and Class Structure*, ed D Wedderburn. London, Cambridge University Press, 1974.
[3] Royal Commission on the Distribution of Income and Wealth, *Report No 1, Initial Report on the Standing Reference*, tables G.13 and G.15. Cmnd 6171. London, HMSO, 1975.

***Professor Townsend sent a copy of this letter to Mr Klein, whose reply is printed below.—ED, *BMJ*.

SIR,—Professor Townsend has missed the point in his reply to my review of his collection of essays. The reason why I raised the question of his views about the performance of the 1964-70 Labour Government was precisely because I knew that his interpretation had been challenged and that he had engaged in a controversy with other academics. Even if he is satisfied that his interpretation was right—and that his critics were wrong—it seems to me odd to publish a collection of essays in 1975 which allows ex cathedra statements made before 1970 to stand without any reference to subsequent intellectual debates. As some of the essays in the collection date from 1973 it would have been easy enough to include material referring to the 1972 arguments between Professor Townsend and Michael Stewart. This might have alerted the innocent reader to the fact that interpreting the statistics of income distribution is extraordinarily difficult and that it is dangerous to be over-confidently dogmatic about what is actually happening.

These difficulties are indeed illustrated by the figures quoted in Professor Townsend's letter. He asks me to ponder on the fact that the top tenth household share of total income is almost 10 times greater than that of the bottom tenth. In fact, I find it impossible to draw any conclusion whatsoever about whether our society is "reasonable and fair" (to use Professor Townsend's words) from figures such as these. For, as he knows perfectly well, the top and bottom tenths of households are not directly comparable without considerable qualification. In the top tenth are concentrated large households with multiple earners.[1] In the bottom tenth are concentrated single-person households, usually retired, with no earners. If one recalculates the Family Expenditure Survey data on a per capita basis,[2] then a much-changed picture emerges: the difference between the top and bottom tenths shrinks from 10:1 to just under 4:1. Equally, in looking at trends across time and the impact of government policies, it is crucial to remember that the proportion both of single-person households, generally old, and of wives going out to work—both factors likely to *increase* inequality in household incomes between the top and bottom groups—is growing; it could therefore be that statistics which suggest no change in the distribution actually mask the effectiveness of public policy in preventing a drift to greater inequality.

Happily Professor Townsend and I agreed about the need to weigh the evidence and to avoid myth-making. Where we differ, perhaps, is that I think it is at least as important to avoid creating new myths about our society—based on an over-simple view of a complex social reality—as to dispel traditional complacency.

RUDOLF KLEIN

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[1] Klein, R E, *British Medical Journal*, 1975, **4**, 63.
[2] Barnes, J, personal communication.

Percentage shares of net and final income received by quantile groups[3]

Quantile group	Net income			Final income		
	1964	1965	1970	1964	1965	1970
Top 10%	24·5	23·4	23·4	23·5	23·3	23·5
11-20	14·7	14·6	15·3	15·2	15·2	15·5
21-30	12·4	12·4	12·7	12·8	12'8	12·9
31-40	11·1	11·2	11·0	11·1	11·1	11·2
41-50	9·4	10·3	9·6	9·7	9·8	9·5
51-60	7·8	7·8	8·4	8·5	8·5	8·2
61-70	7·3	7·2	7·3	7·2	7·1	7·0
71-80	6·2	6·3	5·8	5·8	5·7	5·6
81-90	4·2	4·3	4·1	4·2	4·3	4·1
91-100	2·3	2·5	2·6	1·9	2·3	2·4
Total	100	100	100	100	100	100
Gini coefficient	33·1	31·8	31·9	32·8	32·2	32·8

HLA antigens in haemochromatosis

SIR,—In a French (Rennes) series of 20 cases of haemochromatosis Simon *et al*[1] found a significant excess ($P<0.001$) of patients with the antigen HLA-A3 but no excess with HLA-B7. On the other hand, in an Irish (Galway) series of six cases of haemochromatosis and one of haemosiderosis Dr Jacqueline M Walters and her colleagues (29 November, p 520) have found a significant excess ($P<0.001$) of patients with both A3 *and* B7.

In view of these findings it was decided to HLA-type the seven patients with haemochromatosis undergoing periodic therapeutic venesection at the Aberdeen and North-east Scotland Blood Transfusion Centre, the HLA-A antigens tested for being 1, 2, 3, 9, 11, 28, and 29 and the HLA-B antigens being 5, 7, 8, 12, 13, 14, 18, 27, W15, W22, W35, and W40. In this series, as in the Irish one, there is a significant excess ($P<0.002$, using Bodmer's[2] correction) of patients with both A3 and B7. The expected number of such cases (0·95) is based on the number of subjects with both A3 and B7 in a series of 186 blood donors and blood transfusion centre and hospital staff and on exclusion of one of the patients as being a son of one of the others.

Galway series			Aberdeen series		
Age (years)	HLA-A	HLA-B	Age (years)	HLA-A	HLA-B
44	2, 3	27, 40	41	1, 9	8, 12
49	1, 3	7, 8	53	2, 3	7
34	11, 3	7, 12	53	3	7, 12
53	1, 3	7, 14	53	3	7, 14
56	1, 3	7, 15	61	3	7, 40
56	3	7	58†	3	7
68*	3	7	28†	3	7

*Haemosiderosis. †Father and son.

The results from the Irish and Scottish series are shown in the table. (The French results cannot be similarly presented.) Striking though they are in themselves, they involve such small numbers that many further series will be nedeed to settle the issue one way or other, especially as most of these too will, unavoidably, be small and as there was no excess of B7 as such in the French series.

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1 Simon, M, *et al*, *Nouvelle Presse Medicale*, 1975, **19**, 1432.
2 Bodmer, W F, *National Cancer Institute Monographs*, 1973, **36**, 127.

Medical aspects of North Sea oil

SIR,—With reference to the report by a working party set up by the Scottish Council of the BMA[1] may I draw your attention to one or two erroneous statements?

It is stated in the report (p 10) that "Although a diver can breathe compressed air down to a depth of 50 m he will require a mixture of oxygen and helium for a greater depth. At more than 50 m the nitrogen in the air becomes narcotic and must be replaced by another inert gas." This is technically incorrect as compressed air can be breathed down to 90 m (300 feet) although at this depth narcosis makes it impractical to use. The reason for the 50-m limit on air is the Offshore Installations (Diving Operations) Regulations 1974, which impose this limit (article 14). While it is desirable to have this limit, this depth is exceeded sometimes on units not covered by the regulations and I myself have often dived to 76 m on air in the Royal Navy.

Of greater importance is the definition on p 11 of "bounce" diving. To state that the time limit at 150 m is "no longer than 10 minutes" is completely erroneous, as our company has tables in use which allow 30 minutes at 500 feet (152 m) and 600 feet (182 m). One of the other leading companies has tables that allow 120 minutes at 550 feet (167 m) and 60 minutes at 600 feet. Decompression for these divers is of course considerably longer than the five hours quoted, but they certainly do not fall into the "saturation" classification.

Also the decompression time quoted for the "saturation diving" definition is unrealistic —"after such saturation for a week at a level of 200 m, the diver will require a fortnight for decompression—a severe restriction." One of the *slowest* decompression profiles for saturation is the US Navy schedule. Even on this procedure decompression from 200 m (656 feet) will take only 186 hours (7 days 18 hours), which is vastly different from 14 days. We also have faster profiles which would allow safe decompression from this depth in either 100 hours 30 minutes ($4\frac{1}{2}$ days) or 80 hours ($3\frac{1}{4}$ days). The shorter of these procedures is generally used only in emergencies due to pulmonary oxygen toxicity problems, but to quote 14 days is way out. The prospect of this type of decompression expectation could well deter prospective members of hyperbaric medical/surgery teams.

Finally, "the belief that the surgical team should go to the diver," while emphasised by medical people at a recent conference in Aberdeen, is a view which we in the diving fraternity, in certain circumstances, would question.

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1 British Medical Association Scottish Council, *Report of the Working Party on the Medical Implications of Oil Related Industry*. Edinburgh, BMA, 1975.

Tertiary syphilis and acute vertebral collapse

SIR,—In a recent leading article (20 December, p 669) you cite Ghosh and Holt[1] as recently describing vertebral collapse in association with tabes dorsalis. The detailed case report as published by them was, on inspection, extremely familiar—so familiar, in fact, that I am in no doubt that it had originally been published some five months earlier[2] by two of the clinicians involved in the management of this patient's acute cauda equina compression.

While duplication of case reporting must inevitably occur as the number of journals available proliferates, it is perhaps unfortunate that you were possibly unaware of the initial report of this unusual, but treatable, cause of vertebral collapse and paraplegia. It is also necessary to point out that while the histology of the area of the collapsed lumbar vertebra showed many features of chronic inflammation, the pathologist was unable to state that this was definitely a gumma, although we did suggest that on the evidence this was by far the most likely diagnosis.

I do feel that it is not unreasonable to expect that you should consider at least all United Kingdom publications (which might be relevant to the chosen subject) before writing what is, after all, meant to be an authoritative review for those in the profession less familiar with that particular subject than you should be.

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1 Ghosh, A K, and Holt, S, *British Journal of Venereal Diseases*, 1975, **51**, 349.
2 Griffiths, R W, and Rose, M J, *Journal of Neurology, Neurosurgery and Psychiatry*, 1975, **38**, 558.

Role of community hospitals

SIR,—Dr K S Cliff (25 October, p 239) asks what is the role of the community hospital. The answer surely is that it is to meet certain restricted hospital needs on a local basis, to bridge the geographical and social gap between the larger and more sophisticated district general hospitals.

There is some agreement that such a hospital can successfully provide: (1) inpatient facilities for medical cases within the scope of a general practitioner staff; (2) preconvalescent care for local people discharged early from the DGH; (3) geriatric and terminal care for those who live in the vicinity; (4) specialist outpatient facilities, *x*-ray and a pathology collecting service; and (5) an "on-call" minor trauma service. In addition, the service to the local population can be greatly enhanced if a health or medical centre, with all the facilities that are associated with general practice, is physically part of the same complex. Such a hospital can often be staffed by nurses who are not willing to travel a long way to a more distant DGH and also by other less highly trained local people.

The only point upon which there seems to be a divergence of opinion is the question whether such a hospital should support general surgery. Most surgeons stress the waste of their time spent travelling, the duplication of expensive theatre equipment, and so forth. This is certainly an area that deserves independent cost/benefit study. So far as visiting is concerned I believe that the visiting of geriatric and terminal care patients assumes greater importance than in the case of acute and usually short-term surgical patients. It is also this class of patient to whom the "local" character of the hospital is most beneficial.

E O EVANS

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***Dr Evans sent a copy of this letter to Dr Cliff, whose reply is printed below.—ED. *BMJ*.

SIR,—I am in broad agreement with Dr Evans's comments, though one must express some concern that the community hospital should not be turned into an expensive convalescent and geriatric hospital. There is good evidence to show that some peripheral hospitals which could become community hospitals find difficulty in recruitment of staff because of the slow turnover and throughput through the beds due to their restricted use as a preconvalescent and geriatric unit.

While some surgeons may see the community hospital and the performance of surgical operations in it as a "waste of time," my paper did indicate what might happen to a district general hospital should that facility be withdrawn. As yet I have not seen the evidence in respect of the costings relative to surgery in a district general hospital and a peripheral hospital, nor the cost benefit that accrues to the general public as a whole in not having to travel anything up to 20 miles to receive a minor surgical procedure which could be adequately carried out in a community hospital. At this stage we are, of course, in the process of examining the role of the hospital practitioner grade in the NHS as a whole, and it would seem that surgical services are an area in which the hospital practitioner grade could play a fundamental role in the community hospital concept, where the population is large enough to warrant this.

While agreeing with Dr Evans in respect of items 1-5 in his letter, I think one must be careful in respect of statements regarding visiting geriatric patients in a local community, for evidence now suggests that because of the mobility of the population as a whole the elderly patient may have no surviving relatives at all in the community in which she