the Townsend score based on the 1991 census as a measure of deprivation, and the number of people with limiting longstanding illness was taken from the 1991 census. We compared the relation found with the relation between deprivation and mortality in 0-64 year olds for the years 1982-91.

Whereas linear regression suggested that only 21% of the variation among wards in standardised mortality ratios in 0-64 year olds could be explained by deprivation, it suggested that deprivation explained 66% of the variation among wards in standardised limiting longstanding illness in 0-64 year olds. Also, the difference in standardised longstanding illness between the most and least deprived fourths of wards was larger than the difference in standardised mortality (table).

Relation between deprivation and standardised mortality ratio and standardised longstanding illness ratio in 0-64 year olds

	Standardised mortality ratio	Standardised longstanding illness ratio
Most deprived fourth	108	128
Second fourth	86	92
Third fourth	87	85
Least deprived fourth	83	80

The relation between deprivation and longstanding illness in Worcester and District Health Authority seems to be much stronger than that between deprivation and premature mortality. If this is true in other areas it would be sensible to use standardised longstanding illness ratios instead of standardised mortality ratios in the formula for resource allocation.

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Gradient between north and south remains

EDITOR,—It is surprising that Margaret Eames and colleagues occasionally mentioned, but did not incorporate in their abstract or their section on the "implications for the *Health of the Nation*," the overall geographical finding that premature mortality increases steeply from the south to the north of England.¹ The differences in regional mortality have been fairly constant for at least the past 30 years^{2,3} and the search for reasons for and solutions to the north-south divide is surely one of the greatest challenges to medical and public health research. The omission was particularly disappointing in view of the new finding of the paper that variations in deprivation account for some, but not all, regional variations in premature mortality (and hence the north-south gradient). The paper implies the need to target not only improvements in health care but also improvements in social and material circumstances in the north more than the south, but these implications were omitted.

Two additional points arise from the paper. Firstly, the authors seem to have attributed to individual people an overall average characteristic of an area (the ecological fallacy). Thus the authors state: "Although, intuitively, increasing affluence may not be expected to have a continual effect on mortality, this was not seen in the dataset." However, the measures of affluence (or, rather, its inverse, deprivation) are composed of percentages—for example, the percentage unemployment—in a given area and therefore a continuous relation would be expected. The authors thus seem to imagine, wrongly, that in electoral wards (which are highly heterogeneous with respect to deprivation

of individual people) the deprivation measures used in some sense describe the deprivation of all the residents.

Secondly, the attempt to show that the prevalence of smoking explains north-south variation in mortality from diseases related to smoking was inadequate and should have been omitted. If two regions have values at opposite extremes and the 12 others show little variation this cannot support, even crudely, a hypothesis about general north-south variation.

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Supervised discharge of mentally ill people

EDITOR,—We endorse Robert Buglass's comments¹ concerning recommendations for the care of mentally ill people in the community.² In particular, we share his concern that, in its present form, supervised discharge may not be an advance on current guardianship arrangements.

We recently conducted a postal survey to determine the attitudes of consultant general psychiatrists working with adults in the Oxford region towards the proposed new legal powers (supervised discharge, extended leave of absence, and guardianship). Part of the questionnaire was adapted from a survey into attitudes towards community supervision orders.³ After five weeks there had been a 71% (37/52) response rate.

Supervised discharge—Thirty four of the 37 respondents stated that they would be prepared to use the new legislation regarding supervised discharge, though 19 of them had reservations. Respondents were offered eight possible reasons that might make them reluctant to use supervised discharge. Three aspects caused concern: supervised discharge would generate an increased workload, for which resources are not available (25/37); there is no sanction on the patient if he or she does not comply (24/37); and supervised discharge would be used instead of properly resourced community care (20/37). Several respondents commented that the legislation would not be appropriate for patients lacking insight (for example, those who did not see the need to take regular medication to keep them well).

Extended leave of absence—Thirty one respondents were prepared to use an extension of leave of absence from six months to one year, but 12 respondents did not think that this would be a useful option. Reservations were expressed about the coercive nature of the legislation and the possibility that it might be abused. Several respondents commented that if it was necessary to keep a bed available for the patient on leave the proposal would be impractical. Again respondents stated that this option could not replace adequately resourced community care programmes.

Guardianship—The reasons for the unpopularity of current guardianship arrangements are unclear. We offered seven possible reasons for not using it. Three presented problems for practitioners: there is no way of enforcing provisions if the patient is unwilling (33/37); guardianship arrangements do not ensure that patients receive the care they need (25/37); and it is difficult to gain access to the patient if he or she is unwilling (25/37).

We acknowledge the difficulties that the Department of Health faces in drafting new legislation; the differing views held both between and within interested parties must be taken into account. In addition, treatment of patients in the community without their consent may contravene the European Convention on Human Rights and has therefore been rejected as an option.

In conclusion, supervised discharge increases the responsibility on mental health workers to ensure assertive follow up of seriously mentally ill people who find it hard to accept the treatment they need. This may help identify patients earlier in a relapse. There is a risk, however, that the public and other groups will assume that supervised discharge offers new powers to keep people well in the community. In fact there are no new powers, and these patients still have to deteriorate and then be readmitted compulsorily before they can receive the treatment they need.

We thank the consultants for completing the questionnaires.

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Bed sharing and smoking in the sudden infant death syndrome

EDITOR,—R Scragg and colleagues report that if a Maori mother smokes and shares her infant's bed the infant's risk of dying of the sudden infant death syndrome is increased.¹ Their suggestion that bed sharing increases the risk of the syndrome even in the absence of maternal smoking, however, needs to be considered with caution. In the authors' previous report the relative risk associated with bed sharing in Maoris was 1.4, and this was after allowance for potentially confounding variables, including maternal smoking.²

Studies in several countries have reported ethnic differences in mortality from the sudden infant death syndrome, and the lower incidence of the syndrome among Asians compared with other groups in Britain has recently been recognised.³ Our survey of 374 mothers (172 Asian, 202 white) attending our antenatal clinic in west Birmingham showed pronounced differences in the infant care practices of the two groups despite similarities in social class and housing.⁴ We found that 162 (94%) Asian infants slept in their parents' bedroom in the first year compared with 123 (61%) white infants ($p < 0.0001$), and three times as many Asian infants as white infants slept in the parental bed (62 (36%) v 22 (11%)).⁴ A random sample of 156 mothers (72 Asian, 84 white) from this population showed that 30 (36%) white mothers smoked compared with two (2.8%) Asian mothers.

Thus white mothers are more likely to smoke but much less likely to share their infant's bed. Nevertheless, white infants have twice the mortality from the sudden infant death syndrome of Asian infants.³ Our observations therefore do not support Scragg and colleagues' hypothesis that bed sharing (even in the absence of maternal smoking) increases the risk of the syndrome. Moreover, Scragg and colleagues failed to find a positive relation between paternal smoking and the risk of the sudden infant death syndrome. Gantley *et al* showed, however, that Bangladeshi men in Britain smoke much more than other Asian men, yet their infants have a similar (equally low) mortality

from the syndrome.⁵ Further work is needed on the physiological and behavioural aspects of the parent-infant interaction that occurs among groups in whom bed sharing is common before this practice is implicated in the pathogenesis of the sudden infant death syndrome.

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Helping women with premenstrual syndrome

EDITOR,—P M S O'Brien's review of the premenstrual syndrome is misleading in its discussion of late luteal phase dysphoric disorder.¹ O'Brien is incorrect in stating that this diagnosis "refers only to the mood disturbance of premenstrual syndrome"; the diagnostic criteria for the disorder listed in the *Diagnostic and Statistical Manual of Mental Disorders*, third edition, revised include physical and behavioural symptoms as well,² giving a category essentially identical with what O'Brien terms "primary premenstrual syndrome."

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Concurrent infection with hepatitis B and C viruses

EDITOR,—Larry Mimms and colleagues report an interaction between hepatitis B and C virus in patients who received both viruses through contaminated blood transfusions: coinfection with hepatitis C virus reduced the multiplication of hepatitis B virus.¹ Our experience with patients coinfecting with these viruses is different.²

We tested serum samples from a cohort of 60 haemophilic subjects for markers of hepatitis C virus infection by second generation enzyme linked immunosorbent assay (ELISA) and recombinant immunoblot assay and for presence of hepatitis C virus RNA by reverse transcriptase polymerase chain reaction. Forty two of the serum samples tested were positive for antibodies to hepatitis C virus by ELISA, of which 35 were positive and seven were indeterminate (c22 band only) by recombinant immunoblot assay. RNA from hepatitis C virus was detected by the polymerase chain reaction in 33 of these 42 samples, including four of those that were indeterminate by recombinant immunoblot assay. Five of the

cohort were known to be chronic carriers of hepatitis B virus: all were positive for antibodies to hepatitis C virus but none was positive for hepatitis C virus RNA. Only four of the 37 patients who were not carriers of hepatitis B virus were negative for hepatitis C virus RNA. This difference was significant ($P < 0.001$, Fisher's exact test).

Our data suggest that replication of hepatitis B virus inhibits replication of hepatitis C virus in coinfecting chronic carriers of hepatitis B virus. This raises the possibility that inhibiting hepatitis B virus replication by treating such patients with interferon may result in reactivation of hepatitis C virus infection once the treatment is stopped. In conclusion, the virological outcome of coinfection with hepatitis B virus and hepatitis C virus may not be as straightforward as Mimms and colleagues suggest. It may depend on which virus infects a patient first as well as the relative doses of infection.

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Clusters of anophthalmia

No link with benomyl in Italy . . .

EDITOR,—Since the *Observer* reported a possible link between apparent clusters of anophthalmia and microphthalmia in England and Wales and the pesticide benomyl,¹ several articles have commented on possible strategies to manage public and media concern, the methodology to identify clusters, the difficulty in assessing disease clusters with unreliable data, and the weakness of retrospective studies.^{2,3} Analysis of cases routinely reported to the national congenital malformation surveillance scheme (CMSS) in England and Wales and to the European congenital malformations registries (EUROCAT) suggested that pesticides based on benomyl are unlikely to be major causes of anophthalmia and microphthalmia.^{4,5}

In Italy the National Committee of the Congenital Malformations Registries also made a descriptive analysis of cases of anophthalmia and microphthalmia collected during 1986-90 and presented the results to the Ministry of Health. After exclusion of malformations associated with chromosomal and other genetic syndromes, 33 cases of anophthalmia and 78 cases of microphthalmia were reported in 940615 births, giving prevalences of 0.35 and 0.83 per 10000 births respectively; results similar to those of the European registries.⁴ The prevalence of the two malformations, separately and together, was evaluated for 18 regions: interregional distribution was not significantly heterogeneous ($\chi^2 = 18.61$, $df = 17$, $P = 0.3$), and in no region was the prevalence significantly different from the mean prevalence.

A correlation analysis by region was then made between the prevalence of the malformations and use of benomyl based pesticides (derived from reports of the National Statistical Institute (ISTAT)). There was no significant correlation by region, nor when the regions were grouped by four levels of pesticide consumption. An analysis by parental occupation was performed on a subset of the data (768005 births) with babies with preauricular tags acting as controls. Of the 95 parents of babies with anophthalmia and microphthalmia, 4.2% (two fathers and two mothers) had agricultural occupations compared with 4.7%

(48/1032) of the control parents (odds ratio=0.9 (95% confidence interval 0.23 to 2.55)).

On the basis of these results, the association between use of benomyl and anophthalmia and microphthalmia seems unlikely.

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. . . or in Norway

EDITOR,—The relation between parental exposure to the fungicide benomyl and anophthalmia in children has received attention.¹ We have investigated the hypothesis that a maternal occupational activity that could result in exposure to benomyl during the first trimester is a risk factor for anophthalmia or microphthalmia. A Norwegian multiregister study offered the opportunity to explore this association.

A national cohort of farming families has been established from all five agricultural or horticultural censuses during 1969-89. All 157360 personal farm holders born later than 1924 have been identified, and information concerning the activity at the farm at the time of the censuses has been gathered for the individual holding. By means of their national personal identification number, the spouse and children of the holders were identified in the central person register. The linking procedures have identified 248696 farmers and spouses born during 1925-74 and 323359 children born during 1952-91. Linkage with the medical birth registry of Norway gave information on the perinatal outcome of 192416 pregnancies during 1967-91.

Benomyl was introduced in Norway in 1971. It is mainly used as a fungicide in greenhouses and orchards; it is also used to a limited extent during the spring in fields of vegetables and, since the mid-1970s, in the late autumn in grain fields. Its use has been limited in Norway: the maximum sale was 1682 kg active compound in 1980. We considered farm activities after 1970 that might result in exposure to benomyl at a relevant time: work in greenhouses throughout the year; in orchards for children conceived in April-June; with vegetables grown in fields for children conceived in January-April; or cultivating grain for children conceived in August-November. Eleven per cent ($n = 21843$) of the entire cohort of infants born in 1967-91 had potentially been exposed to benomyl according to these criteria. Four of the 192416 children had been diagnosed as having anophthalmia or microphthalmia (*International Classification of Diseases*, eighth revision, codes 744.0-744.1), yielding a crude prevalence of 0.21/10000 new-