scarcity of suitable data, but conceptually the principle is straightforward.

In dealing with the variable "pregnancy order," however, a rather different problem arises. The problem is that it is not possible to allow pregnancy order to vary while holding all other relevant variables constant. For the difference between, say, a third pregnancy and a second pregnancy is not only that the third pregnancy contains an element of "thirdness" (that is, the characteristic of an event happening for the third time) while the second pregnancy contains "secondness." In addition, the third pregnancy inevitably follows two previous pregnancy outcomes while the second pregnancy follows only one. So if "pregnancy order" is allowed to vary, so too must the number of previous pregnancy outcomes.

Of course, if "number of previous outcomes" has no effect on risk then it does not need to be held constant. Suppose therefore, we try to investigate the effect of the variable "number of previous outcomes" on the risk of fetal loss in the current pregnancy. As before, we allow this variable to vary and hold all other relevant variables constant. But one of the relevant variables is known^{1,2} to be previous pregnancy history—that is, the descriptive set of previous outcomes, including the number, type, and order of these outcomes. And if the number of previous outcomes is allowed to vary, so too must the previous pregnancy history. So it seems that the only variable whose effect on risk can be quantified is the conglomerate variable "previous pregnancy history." One can, at least in principle, examine the changes in risk to a current pregnancy when the previous pregnancy history is allowed to vary, while the other risk factors (such as maternal age and social class) are held constant.

Of course, we can only examine risk in those women who initiate a further pregnancy after a given previous pregnancy history. These women will be a subgroup of all women with the given pregnancy history. It is impossible to estimate the hypothetical risk for the remainder of the group (those who did not initiate a further pregnancy), so that even if this hypothetical risk were different from the actual risk in the pregnancies that did occur we could not take account of it. Exactly the same difficulties apply when one examines the effect on risk of maternal age or social class.

The risk rates specific to particular "previous pregnancy histories" (such as those reported by Léridon¹ or Roman *et al*²) may be aggregated in various ways in an attempt to draw general conclusions. If the risk rates are aggregated according to the number of previous outcomes, using a population weighting, one arrives at the familiar cross-sectional data relating risk to pregnancy order. The use of the variable "previous pregnancy history" would enable one to interpret the characteristics of the cross-sectional data, since at each pregnancy order one would know the relative weight accorded to any previous pregnancy history.

Aggregation of the data may be carried out in other ways. Studies that have concentrated on pregnancy order may have obscured the fact that other features of previous pregnancy history may have even more bearing on risk. For instance, it may be that there is an important distinction between a set of previous outcomes containing only live births and a set containing at least one fetal loss. In this case, aggregation should be by type, not number of previous outcomes.

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¹ Léridon H. *Popul Studies* 1976;**30**:319-35.

² Roman E, *et al*. *Early Hum Dev* 1978;**2**:131-8.

SIR,—Mr N Mantel (3 November, p 1147) has commented on our paper entitled "Perinatal mortality by birth order within cohorts

based on sibship size" (22 September, p 693). In fact, we had already discussed our results in terms of the issue which Mr Mantel has raised. Unfortunately, his letter and some subsequent letters to the *BMJ* appear to have ignored some of the relevant data in our paper. In addition to calling attention to these data, we also include here comments which we hope will help to clarify a few of the salient points.

Mr Mantel illustrated his point by supposing that all mothers whose first pregnancy outcome results in a perinatal death will have a second birth. He further conjectured that only half the mothers whose first baby survives the perinatal period will have a second birth. This degree of self-selection for pregnancy will produce a halving in the perinatal mortality rate for second compared with first birth. Thus the first question is whether his hypothesis of a 50% reduction is even approximately true in the Norwegian data for mothers having one or two births.

The answer to this question can be found by consulting table II of our paper. From this table it can be seen that only 44% of mothers with a perinatal death occurring with their first birth went on to have a second birth, based on the entire seven-year observation period, 1967-73. But 41% of mothers having a favourable outcome with their first birth went on to have a second birth. Thus for mothers having one or two births the potential bias due to self-selection for pregnancy does not appear, and yet there is a decrease in perinatal mortality of 45% for second compared to first birth.

More detailed analysis of our data suggests that if we had relied on a three-year rather than a seven-year study period an apparent difference would have emerged in the pattern of reproduction for mothers having one or two births. There is a tendency for mothers quickly to seek to compensate for a previous adverse pregnancy outcome. If the study period is severely truncated, therefore, the results can also be biased.

We have also examined the subgroup of mothers with one or two births whose first birth was recorded in 1967. This subgroup represents those mothers in the study with the longest interval for subsequent observation for reproduction. In this subgroup we found that 80% of the mothers went on to have a second baby regardless of whether their first survived the perinatal period or not. This result is a much better approximation to data expected for mothers with complete sibships during this time in Norway. It is reasonable to infer, therefore, that the large majority of mothers in our study desired at least two children. Consequently the self-selection for pregnancy bias is negligible for sibships of one or two.

For sibships greater than two, increasingly longer study periods are necessary for quantifying the potential bias. We have recently extended our study period to 10 years, 1967-76, in order to examine our earlier results in more detail. Ideally, we ought to have data for completed sibships. Unfortunately, even if such data were available for a population over a long period of time, there is still the problem that perinatal mortality rates can be greatly influenced by time trends. We think the time trend bias is negligible for the original seven-year study period. More current data from 1977 onwards suggest, however, that marked changes in birthweight-specific perinatal mortality rates have occurred recently in Norway (T Bjerkedal, personal communication).

Furthermore, if the average desired number of children in a population is less than three, then some bias due to self-selection for pregnancy will surely influence the results for sibships larger than two, regardless of whether complete sibships are available or not. Currently in Norway the average desired number of children is between two and three. The Norwegian Fertility Survey, 1977, found that the number of children considered ideal was on average 2.57. (T Noack, personal communication). Moreover, it should be noted that in populations which do not show a similar desire to limit the number of children the possible

bias due to self-selection for pregnancy can be expected to disappear.^{1,2}

Despite the limitation for larger sibships in using all mothers in Norway, it is still possible to circumvent the bias by searching for subgroups of mothers who may expect to have three or more children. One possible subgroup of mothers are those having their first baby before age 20. If this subgroup is also restricted to those having their first birth in 1967, then for the 10-year study period we have found perinatal mortality rates of 30.0, 23.7, and 11.9 per 1000 births for mothers with their first, second, and third births respectively. Moreover, those mothers with a perinatal loss in their second delivery were only slightly more likely to go on to a third birth compared with mothers whose second baby survived the perinatal period (52% compared with 41% of mothers).

Finally, we would like to stress that regardless of some of the biases that we and others have pointed out in the longitudinal approach, these same biases are latent, and generally unsuspected, in the conventional cross-sectional analysis. Now that data for longitudinal analyses are becoming available, we think that it is essential to begin to utilise this important information.

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² James WH. *J R Statist Soc Ser C* 1969;**18**:276.

* * * This correspondence is now closed.—Ed, *BMJ*.

Imaging gall-bladder emptying

SIR,—Dr D P Maudgal and others (19 January, p 141) studied gall-bladder emptying using a standardised oral cholecystogram with films taken at 15, 30, and 60 minutes after a liquid test meal containing fat. Their results agree well with our own measurements made using real-time grey-scale ultrasound.¹

In 13 normal subjects, mean percentage gall-bladder emptying after a fatty meal was $24\% \pm 2.5\%$ at 15 minutes and $50\% \pm 2.6\%$ at 30 minutes, compared with $28\% \pm 3.8\%$ and $44.2\% \pm 3.0\%$ respectively in the reported study of Dr Maudgal and his colleagues. Standard deviations of ultrasound measurements were 8.7% and 9.0% at 15 and 30 minutes, compared with 22.0% and 17.5% for oral cholecystogram, suggesting that more precise measurement is possible with ultrasound. In most of our subjects emptying appeared complete at 40 minutes with a $59\% \pm 4.3\%$ reduction in volume; after this some refilling seemed to occur, as was noted with the oral cholecystogram. However, the similarity of their values, despite using cumulative totals, at 30 and 60 minutes ($44.2\% \pm 3.0\%$ and $48.5\% \pm 2.7\%$ respectively) may be deceptive since our data suggest that maximal emptying may have occurred during this 30-minute interval and been missed. The dynamics of emptying can only be surmised from these three-point observations.

The radioisotopic technique used in the complementary study by Dr T C Northfield and others (p 143) is a refinement of the method of Englert and Chiu,² which showed that a fatty meal reduced the steady-state count levels by $84\% \pm 3\%$ in a mean time of 54 (range 23-105) minutes. Thus the isotope method suggests either more complete emptying than is calculated from the oral cholecystogram or, possibly, more flushing of the

contents of the gall bladder without complete contraction. Despite this, Chapple *et al.*³ using a similar method on models and patients with gall stones, found a good correlation between gall-bladder volume on the oral cholestogram and isotope counts but suggested that isotopic methods underestimated emptying owing to the background count.

Radioisotopic techniques have the advantage over oral cystograms that frequent counts can be made, giving a better idea of the kinetics of emptying; but radioactive substances still have to be injected. Dynamic imaging of the isotope-filled gall bladder is possible but is extremely slow compared with ultrasound. Using real-time ultrasound it is possible to scan virtually continuously, if this is wanted, so that a smoother emptying curve can be constructed. In our studies measurements were made every two minutes. Not only can the reduction in volume be measured but the pattern of movement and contraction of the gall bladder during emptying can be seen and recorded.

Since the presentation of our preliminary data we have carried out further studies (which are to be published), using a static B-mode grey-scale ultrasound scanner, which—though it does not show movement—has better definition, so that in skilled hands measurement can be more precise. These studies, also using two-minute intervals, show excellent agreement with the real-time data.

Though the various stimuli to contraction of the gall bladder are well documented⁴⁻⁶ and pressures at the sphincter of Oddi and within the duct system have been measured,⁷ there is virtually no method of obtaining information on the dynamics of emptying which is not restricted by either being invasive or using ionising radiation. The situation has been well summarised in the statement that "methods of studying gall bladder motility are on the whole unsatisfactory."⁸

Despite the recent claim⁹ that cholelscintigraphy "represents a considerable improvement over the techniques previously used," we would suggest that modern grey-scale ultrasound, using either static or real-time imaging, is a major advance in observing and measuring the gall bladder accurately, safely, and acceptably. Hazard-free continuous imaging is possible and it is hoped that with ultrasound the precise dynamics of gall-bladder emptying will now be clarified.

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¹ Ornstein MH, Palframan A, Meire H. *Gut* 1978;19: A971.

² Englert E, Chiu VSW. *Gastroenterology* 1966;50: 506-18.

³ Chapple MJ, Nolan DJ, Low-Beer TS, Davies ER. *Br J Radiol* 1975;48:19-22.

⁴ Banfield WJ. *Gastroenterology* 1975;69:770-7.

⁵ Schoenfield LJ. *Diseases of the gall bladder and biliary system*. New York: Wiley, 1977:53-5.

⁶ Scott GW. In: Circus W, Smith AN, eds. *Scientific foundations of gastroenterology*. London: Heinemann, 1980:547-64.

⁷ Lynn JA. In: Wright R, Alberti KGM, Karran S, Millward-Sadler GH, eds. *Liver and biliary disease*. London: W B Saunders, 1979:228-32.

⁸ Sherlock S. *Diseases of the liver and biliary system*, 5th ed. Oxford: Blackwell, 1975: 704.

⁹ Spellman SJ, Shaffer EA, Rosenthal L. *Gastroenterology* 1979;77:115-20.

Gall stones and ischaemic heart disease

SIR,—Mr A Faichney and Mr L A Donaldson (9 February, p 402) suggest that data from Glasgow do not conform with our finding of a negative correlation between the necropsy prevalence of gall stones and ischaemic heart

disease mortality in nine British towns (1 December, p 1389). From our paper they estimate that the necropsy prevalences of gall stones among men and women under 60 years of age are similar to those that they have recorded in Glasgow (3.4% among men and 10% among women). On the other hand they point out that ischaemic heart disease mortality in Scotland is known to be higher than in England.

In fact, the prevalences under age 60 for all of our nine towns combined are 6.8% among men and 11.6% among women. Among men the highest prevalence is 11.9% in Chester and seven of the towns have prevalences higher than Glasgow. Among women the highest prevalence is 20.7% in Ipswich and five of the towns have prevalences higher than Glasgow. Although this comparison between Glasgow and the nine towns is not age standardised it does not argue against a negative correlation between gall stones and ischaemic heart disease.

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Ethical conflicts in long-term care of the aged

SIR,—The psychological analysis made by Dr Astrid Norberg and her colleagues (9 February, p 377) while assessing the patient-care worker relationships in long-stay wards is absolutely fascinating (9 February, p 377). We feel, however, that this article contains several disturbing statements.

Firstly, the authors repeatedly mention that death from water deficiency is a painless and peaceful process. Although the accuracy of this statement cannot be verified, it seems to be contrary to orthodox teaching and experience: dehydration is an unpleasant condition and there is no reason to believe that elderly patients do not feel thirst. Moreover, dehydration itself is associated with apathy, depression, dysphagia, and oesophageal obstruction¹; a vicious circle is therefore easily triggered off. It is also well established that dehydration is associated with nausea, lightheadedness, a drop in blood pressure, and an increase in heart rate; the renal perfusion may also drop, leading to oliguria and uraemia,² and cerebral hypoperfusion may lead to impaired mental functions and confusion. Surely death from dehydration cannot be as painless and peaceful a process as Dr Norberg and her colleagues claim.

Secondly, whereas we would agree with the authors that intravenous infusions may not be recommended in dying patients who can no longer be spoon-fed, we fail to understand why a nasogastric tube cannot be inserted and adequate nutrition (at least so far as hydration is concerned) maintained. It must be emphasised that at no time are we told that these are dying patients; in fact the group of patients studied are those who "could no longer be spoon-fed." It is not uncommon for elderly patients to go through phases where spoon-feeding becomes difficult and even not possible; but if adequate hydration is maintained and adequate treatment instituted many patients improve. To deprive these patients of fluids and allow them to die of dehydration is not too remote, we feel, from euthanasia.

Finally, we are bewildered by the selection criteria our Swedish colleagues use to admit patients for terminal geriatric care: confusion and urinary incontinence. We, in fact, consider these two conditions as indications for admission to an acute geriatric ward, and it is surprising in how many instances a treatable condition can be detected and controlled and patients discharged

back to the community mentally alert and continent. Is it possible that the authors mean dementia—which is an irreversible deterioration of mental functions—rather than confusion, which is an acute usually reversible mental impairment? But then their statement that "patients may lie for several years with a gastric tube" would be difficult to understand as the average life expectancy of patients with dementia is less than two years³; in fact, it is reported that 60% of those patients hospitalised for dementia die within six months.³ In our area the major reason for admission to a long-stay or continuing care ward is when elderly patients are so disabled, either physically or mentally or both, that they need constant nursing care, and even then only after they have undertaken a full rehabilitation programme lasting usually not less than three months. The average length of stay in our continuing care wards is almost one year and it is very rare for survival for 10 years to be recorded, owing to the severity of the disease process by the time they reach our continuing care wards.

We are glad that this paper has been included under the heading "For Debate" as otherwise we fear it might have encouraged marked nihilistic attitudes towards the elderly population, and a tendency to leave older confused and incontinent patients to die of dehydration without properly investigating and treating reversible causes for this state such as metabolic abnormalities, chest infection, and even drug abuse.

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² Schwartz WB. In: Beeson PB, McDermott W, eds. *Cecil-Loeb: Textbook of medicine*. 13th ed. Philadelphia: W B Saunders, 1971:1619.

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Not a career for academic high fliers?

SIR,—Your correspondent Professor Sam Shuster (2 February, p 335) is surely wrong about our demands for quality in students. Every trade or profession, be it of dustman, politician, skilled craftsman, or scientist, must always aim at improvement and perfection within its own vision. Otherwise our society becomes sterile. If medicine is attracting students of such calibre that we have a disproportionate share of the high fliers we should feel proud, not embarrassed. The fault surely lies in those vocations which fail to meet the aspirations of youth.

We shall not choose good doctors either by selecting only those of lower academic calibre—although the suggestion does, in fact, indicate the source of many of our present educational problems and of many social and personal misfits in society today. We have allowed school teachers to choose the subjects and standards by which the whole career of the individual is controlled. Teaching is a specialised art, but ability in it does not make a teacher an ideal arbiter of what is needed for a different career, any more than a brilliant scientific research worker is the best person to teach science.

The fault here does not lie wholly with the school teachers. It lies with ourselves. We have not yet decided what we need in a doctor or how to develop that potential when the student enters a medical school. Hence also our present stultifying need to add more and more post-graduate requirements before a doctor can