Does routine ultrasound scanning improve outcome in pregnancy? Meta-analysis of various outcome measures

Heiner C Bucher, Johannes G Schmidt

Abstract

***Objective*—To evaluate the effectiveness of routine ultrasound scanning in pregnancy by a meta-analysis of various outcome measures.**

***Design*—Meta-analysis of randomised controlled trials evaluating the effect of routine ultrasound scanning on perinatal mortality and morbidity. Live birth rate (that is, live births per pregnancy) is included as a measure of pregnancy outcome in addition to the conventional perinatal mortality.**

***Subjects*—15 935 pregnancies (7992 in which routine ultrasound scanning was used and 7943 controls with selective scanning) from four randomised controlled trials.**

***Main outcome measures*—Perinatal mortality, live birth rate, rate of miscarriage, Apgar score <7 at 1 minute, and number of induced labours.**

***Results*—The live birth rate was identical in both screening and control groups (odds ratio=0·99; 95% confidence interval 0·88 to 1·12) although the perinatal mortality was significantly lower in the group who had routine ultrasonography (0·64, 0·43 to 0·97). Differences in perinatal morbidity between the two groups as measured by the proportion of newborn babies with Apgar score <7 at 1 minute were not significant (1·05; 0·93 to 1·19).**

***Conclusion*—Routine ultrasound scanning does not improve the outcome of pregnancy in terms of an increased number of live births or of reduced perinatal morbidity. Routine ultrasound scanning may be effective and useful as a screening for malformation. Its use for this purpose, however, should be made explicit and take into account the risk of false positive diagnosis in addition to ethical issues.**

Introduction

Ultrasound examinations for pregnant women are now routinely performed in many countries. One stage ultrasound screening is recommended by the Royal College of Obstetricians and Gynaecologists in the United Kingdom,[1] a two stage screening has been advocated in Germany,[2] and a three stage screening in France.[3] The United States preventive services task force, however, does not recommend screening.[4] What is the evidence that routine ultrasound scanning may be beneficial for the outcome of pregnancy?

Ultrasonography in pregnancy improves the dating of gestational age,[5] which may lead to a reduction in the number of induced labours in cases in which the gestation is overestimated.[6,7] It is the most accurate means of detecting retardation in fetal growth[8] and multiple gestations,[9] and it is effective in detecting severe malformations.[10] In spite of these achievements, however, the important question is whether routine ultrasonography improves the outcome of pregnancy and whether it shows an overall net benefit. The early detection of fetal growth retardation, for example, may be of some theoretical value by allowing early planned delivery, although there is lack of truly effective therapeutic means to treat growth retardation.[11,12] Evidence from randomised controlled trials, however, suggests that the sonographic identification of fetal growth retardation does not improve the outcome of pregnancy despite increased medical attention.[13,14] The prediction of the date of delivery through ultrasound scanning is certainly more accurate than with the menstrual history.[5] Whether an improved dating of gestational age to prevent "overdue" deliveries will effectively decrease birth complications, however, remains debatable. Complications during spontaneous delivery increase progressively from 37 weeks' gestation.[15] Thus, it may be difficult to define with confidence after which week a delivery is overdue, and it cannot be assumed a priori that the induction of labour, in the absence of other complications, will improve the outcome. Inducing labour in such cases compared with serial monitoring (fetal kicks, volume of amniotic fluid, tests to detect fetal stress) does not seem to change perinatal mortality and morbidity, though the rate of caesarean section may be slightly reduced.[16,17]

Studies of the diagnostic performance of ultrasonography may provide valuable information. Only randomised controlled trials which include measures of the eventual outcome of pregnancy as endpoints, however, are qualified to answer the crucial question of whether routine ultrasonography is truly beneficial or not.

MEASURES OF PREGNANCY OUTCOME

Perinatal mortality is often used to assess the outcome of pregnancy. The detectability of malformations by ultrasonography, however, raises the question of the validity of perinatal mortality as a measure of outcome when routine ultrasound scanning has been used. Fetal malformations are a major contributor to perinatal mortality. In the absence of ultrasonography a malformed fetus unable to survive the extrauterine state is counted as a perinatal death, whereas the same fetus would be considered as an antenatal case of early abortion if detected by ultrasonography. Induced abortions because of malformations detected by ultrasonograph may therefore bias the perinatal mortality because induced abortions decrease the numerator but leave the denominator of perinatal mortality basically unchanged. Therefore, the rate of live births—that is, the number of live births per pregnancy—may be a better measure of the outcome of pregnancy. The purpose of antenatal care is to lead a maximum number of pregnancies to live birth by preventing harm and managing complications during pregnancy and delivery. Whereas the conventional perinatal mortality does not consider possible losses of pregnancies before delivery, the live birth

Department of Internal Medicine, University Polyclinic, Kantonsspital Basel, CH-4031 Basel, Switzerland
Heiner C Bucher, *registrar*

Family Practice and Clinical Epidemiology, CH-8840 Einsiedeln, Switzerland
Johannes G Schmidt, *practice holder*

Correspondence to:
Dr Bucher.

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rate may be considered a measure of the overall pregnancy outcome.

The present study includes an analysis of the conventional perinatal mortality, the live birth rate, and the Apgar score as well as the rate of induced labour in the randomised controlled trials published so far. Some unpublished data were obtained from the authors of these trials. A meta-analysis was performed to maximise statistical power to detect true differences in these measures of pregnancy outcome and because individual studies were too small to be generalisable.[18] This study is therefore different from existing meta-analyses[19] in that it contains the live birth rate as a new, unconventional but important outcome measure.

Methods

SELECTION AND DESCRIPTION OF STUDIES

Randomised controlled trials were identified by a Medline literature search and by a comparison with recent review articles on ultrasound screening in pregnancy.[19 20] A trial was included if the following criteria were met. Firstly, the study had to compare routine ultrasound scanning with selective ultrasound scanning, which means that randomisation had to occur before the first scan. Secondly, perinatal mortality (that is, the number of deliveries and the number of perinatal deaths) and the number of pregnancies had to be reported. Eight randomised controlled trials of ultrasonography in pregnancy were identified. The trial by Bennett *et al* reported perinatal mortality but all pregnant women had an ultrasound scan.[21] The results of this scan were made available in the study group but not in the control group except in case of a clinical problem. Thus, it did not truly compare routine with selective ultrasound and was excluded from our analysis according to the first criterion, as were the studies of Secher *et al*[14] and Neilson *et al*.[22] One trial did not meet the second criterion and because study results were never published in detail was not included.[23] Thus, four studies qualified for the present analysis.[7 24-26]

All trials compared routine with selective ultrasound. The fetal anatomical reference measurement for gestational age was the biparietal diameter in three studies[7 24 25] and the rump length in one study.[26] In all four trials randomisation achieved comparable study and control groups in terms of maternal age, parity, marital and socioeconomic state, and smoking.

Size of study groups, recruiting of subjects, and ultrasonography schedules are summarised in table I. Altogether 15 153 pregnancies were included in the four trials (7992 cases and 7943 controls). Twin pregnancies were included in the analysis and regarded as multiple events in terms of delivery, perinatal mortality, and Apgar score but as one case in terms of pregnancy and live birth rate. The latter may slightly bias the result because it is believed that routine ultrasound scanning may be especially helpful in multiple pregnancies. Counting multiple pregnancies as multiple births for the live birth rate, however, did not change the results (data not shown). We present figures according to one multiple pregnancy=one delivery=one birth because a live birth rate based on the count of multiple births per one randomised pregnancy would make the rate somewhat illogical as it could exceed 100%. An exclusion of twin pregnancies altogether did not change the result. A separate analysis of twins comprised too few cases to allow meaningful statistical analysis (data not shown).

The trials conducted in Helsinki[24] and Trondheim[25] randomised all women on the diagnosis of pregnancy, whereas in the Stockholm[7] and Missouri[26] studies only those pregnant women who had no clinical indication for ultrasound scanning at their first antenatal visit or at 12 weeks' gestation were included. The latter study design may therefore have resulted in a selection of a study group at a comparatively lower risk of complications. Our analysis, however, did not yield any heterogeneity in the results between the studies with these different methods in selection of patients (see table III).

TABLE I—*Randomised controlled trials of routine versus selective ultrasound scanning in pregnancy*

Trial	No of women recruited	Subject selection	Scanning regimen (gestation at scan)	Percentage of pregnancies having ultrasound: Cases (routine ultrasound)	Percentage of pregnancies having ultrasound: Controls (selective ultrasound)
Helsinki[24]	9310	All women at first antenatal visit at maternal health centres	One stage (16-20 weeks)	86·8	77·0
Trondheim[25]	1017	All women at first antenatal visit at general practice	Two stage (19 and 32 weeks)	89·4	10·2
Stockholm[7]	4997	Women without clinical indication for first ultrasound at first visit in three hospitals	One stage (15 weeks)	64·2	31·8
Missouri[26]	915	Women without clinical indication for first ultrasound at first visit at two hospitals	One stage (10-12 (up to 18) weeks)	83·8	23·9

TABLE II—*Randomised controlled trials of routine versus selective ultrasound scanning in pregnancy: development of pregnancies, perinatal deaths, and live births*

Detail	Helsinki[24] Cases	Helsinki[24] Controls	Trondheim[25] Cases	Trondheim[25] Controls	Stockholm[7] Cases	Stockholm[7] Controls	Missouri[26] Cases	Missouri[26] Controls
Women recruited	9310		1017		4997		915	
Lost/not available			8		4			
Randomised	4691	4619	510	499	2482	2511	459	456
Not pregnant	15	2						
Legal abortion	26	21			6	3		
Refused							11	5
Lost to follow up	1	3	1	2			46	38
Miscarriage/intrauterine death	285	284	13	19	96	106	26	24
Abortion for malformation	11						2*	2*
Deliveries†	4389	4347	502	482	2430	2442	376*	394*
Twin babies	72	76	12	8	51‡	40	4	14
Perinatal deaths	20	39	5	5	12	12	2	4
Live births§	4334	4272	491	474	2370	2390	372	383
Low Apgar score (<7 at 1 minute)	286	276	34	23	199*	201*	36	28
Induced labour	594*	569*	32	38	140	218	28	31

*Personal communication.
†Multiple pregnancies=multiple events.
‡24 Twin, 1 triplet.
§Multiple pregnancies=1 event (see methods section).

DATA ANALYSES

The following outcome variables were analysed: live birth rate, perinatal mortality, proportion of babies with Apgar score <7 at 1 minute, and rate of induced labour. For each trial 2×2 contingency tables were constructed by using the number of randomised pregnancies and live births (live birth rate) and the number of perinatal deaths and deliveries (perinatal mortality) in both screening and control groups. Tables were similarly constructed for the Apgar score at 1 minute and the number of induced labours. The live birth rate was analysed on an intention to screen basis (live births per randomised pregnancies). A separate analysis of a live birth rate per pregnancies available for analysis (excluding women who failed to complete the study, had a legal abortion, or who were not pregnant or refused) was not different from the intention to treat analysis (data not shown).

The resulting 2×2 tables were combined according to the Mantel-Haenszel procedure.[27] Summary odds ratio are reported with 95% confidence intervals. The χ^2 test for heterogeneity was calculated for each variable to control the consistency of the effects of ultrasound scanning across the four studies.

Results

Table II shows the results of the four randomised trials with the number lost to follow up and events associated with the loss of pregnancies. The summary estimates of the live birth rate, perinatal mortality, and

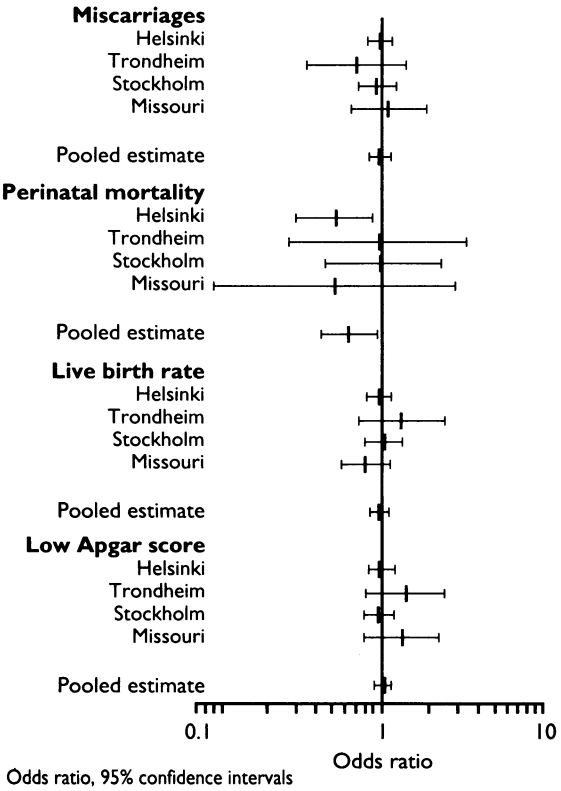
TABLE III—*Meta-analysis of outcome of pregnancy in randomised controlled trials of routine versus selective ultrasound scanning*

Trial	Miscarriages/1000 randomised pregnancies			Perinatal mortality/1000 deliveries			Live birth rate*/1000 randomised pregnancies			Low Apgar score (<7 at 1 minute)/1000 deliveries			Induced labours/1000 randomised pregnancies		
	Cases	Controls	Odds ratio (95% confidence interval)	Cases	Controls	Odds ratio (95% confidence interval)	Cases	Controls	Relative risk (95% confidence interval)†	Cases	Controls	Odds ratio (95% confidence interval)	Cases	Controls	Odds ratio (95% confidence interval)
Helsinki[24]	6·1	6·1	0·99 (0·83 to 1·17)	4·6	9·0	0·51 (0·29 to 0·87)	924	925	0·99 (0·99 to 1·01)	6·6	6·4	1·02 (0·86 to 1·22)	141	138	1·02 (0·90 to 1·15)
Trondheim[25]	2·5	3·8	0·66 (0·32 to 1·35)	10·0	10·5	0·95 (0·27 to 3·31)	963	950	1·01 (0·99 to 1·04)	6·8	4·8	1·46 (0·84 to 2·51)	64	79	0·81 (0·48 to 1·35)
Stockholm[7]	3·9	4·2	0·91 (0·69 to 1·21)	4·9	4·9	1·00 (0·45 to 2·24)	955	952	1·00 (0·99 to 1·02)	8·4‡	8·4‡	1·00 (0·82 to 1·23)	59	91	0·64 (0·51 to 0·81)
Missouri[26]	5·7	5·3	1·08 (0·61 to 1·91)	5·3	10·2	0·52 p=0·36§	811	840	0·97 (0·91 to 1·02)	9·6	7·2	1·37 (0·82 to 2·29)	70	75	0·88 (0·53 to 1·60)
Pooled estimate¶			0·96 (0·84 to 1·10)			0·64 (0·43 to 0·97)			0·99 (0·88 to 1·12)			1·05 (0·93 to 1·19)			0·91 (0·82 to 1·01)
Test for heterogeneity			p>0·50			p>0·50			p=0·45			p=0·46			p=0·005

*Twins and multiple pregnancies=1 pregnancy (see method section).
†Given because odds ratio would give distorted estimate.
‡2364 and 2392 deliveries in study and control group respectively available for analysis.
§Fisher's exact test.
¶Mantel-Haenszel odds ratio.

rate of miscarriage in the four trials are given in table III and the figure. The perinatal mortality showed a significant overall reduction in the screened groups (odds ratio=0·64; 95% confidence interval 0·43 to 0·97), whereas the live birth rate was identical in the two groups (0·99; 0·88 to 1·12).

The significant reduction of perinatal mortality in the screening group (table II) is due mainly to the contribution of the Helsinki trial[24] (though the χ^2 test for heterogeneity showed no significantly different reduction across the four trials (p=0·59). Table II also shows that this reduction in the Helsinki trial was mostly the result of induced abortion of malformed fetuses, that the live birth rate is determined mainly by miscarriages and other events before delivery, and that perinatal deaths alone have a rather negligible impact on the overall number of unsuccessful pregnancies. Evidently the live birth rate is a measure of the overall success of pregnancy, to which complications during delivery contribute only little. Perinatal morbidity as estimated by the proportion of Apgar scores <7 at 1 minute was not different between women with routine ultrasound scanning and selective ultrasound scanning (1·05; 0·93 to 1·19). The number of induced labours was significantly lower in the screened group in one trial.[7] This result, however, is at variance with the observation of no such difference in the other three trials (table III).



Meta-analysis of outcome of pregnancy in randomised controlled trials of routine versus selective ultrasound scanning in pregnancy

Discussion

Authors of over 100 studies that evaluated the use of ultrasonography in obstetrics claimed a benefit of routine scanning.[20] Our meta-analysis of randomised controlled trials shows no evidence that routine ultrasound scanning in pregnancy improves the outcome. Although ultrasonography offers the benefit of a fairly precise dating of gestational age and the early detection of fetal growth retardation and malformations, it does not result in an increase in the number of live births or a decrease in perinatal morbidity as measured by Apgar score. The number of induced labours in the ultrasound group was significantly lower in the Stockholm trial[7] as a result of the more accurate dating of the gestational age but this trial is at variance with the three others. This may suggest that the rate of induced labour is more a function of local obstetric policies than a true benefit of ultrasound screening. We conclude that the combined evidence of randomised controlled trials shows no change in the outcome of pregnancy whether routine ultrasound scanning is performed or not.

The fact that the same trials show a significant overall reduction of the perinatal mortality through routine ultrasound scanning is consistent with a selection bias in the presence of ultrasonography. The significant reduction of the perinatal mortality can therefore be interpreted as a statistical artefact. This is produced by the antenatal loss of fetuses through early termination of pregnancy because of sonographically detected malformations. Malformations are a major contributor to perinatal mortality. In the absence of ultrasonography the delivery of such a malformed fetus may therefore be associated with perinatal death. The loss of fetuses through induced abortions after ultrasound scanning may decrease the number of perinatal deaths without improving the eventual outcome of pregnancy—that is, the number of live births.

LIVE BIRTH RATE AS MEASURE OF PREGNANCY OUTCOME

We propose the live birth rate as a new measure of pregnancy outcome that is unbiased by a possible selective loss of pregnancies before delivery. It seems more meaningful to assess how many pregnancies led to successful delivery of a live baby than to consider only perinatal mortality, which ignores the success of the pregnancy before delivery. Nevertheless, the live birth rate cannot be regarded as an unbiased substitute for the conventional perinatal mortality. The live birth rate is an overall measure of pregnancy outcome which incorporates miscarriages and abortion as well as perinatal deaths and thus all failures of pregnancy combined. It could therefore be biased by an unequal distribution of risk factors for miscarriage such as maternal age, smoking, or parity between study and control group. In all four trials included in this meta-analysis this was not the case.

An even more precise and meaningful measure of pregnancy outcome may be a healthy live birth rate (birth of a healthy baby could be defined as a normal

state of health one week or perhaps one month after birth). We could not, however, include the healthy live birth rate in our analysis because the necessary data were not available.

We conclude that studies and meta-analyses about the effectiveness of routine ultrasound in pregnancy have not sufficiently dealt with the problems inherent in the various outcome measures. We therefore suggest that the live birth rate as an overall estimate of pregnancy outcome should be integrated into the future evaluative research of routine ultrasound scanning in pregnancy.

The use of ultrasonography in the control groups (see table I) may have diluted a possible true effect of ultrasound screening. This seems unlikely, however, given that this statistically powerful meta-analysis could not show an improvement in the live birth rate and that the lack of improvement of the live birth rate was consistent and unrelated to the varying frequency of selective ultrasound scanning in the control groups. As there is no direct evidence that ultrasound scanning may improve the outcome of pregnancy future trials should be conducted with control groups in which ultrasound is performed more selectively. Research efforts should be dedicated to identifying clinical conditions for which selective ultrasonography in pregnancy is truly beneficial. Ultrasonography seems helpful in cases such as unclear vaginal bleeding but the available evidence does not support the notion that the ultrasonographical monitoring of growth retardation is able to improve the outcome.[13 14]

DOES ROUTINE ULTRASOUND SCANNING HAVE ADVERSE EFFECTS?

Ultrasound screening may have additional subtle adverse effects which may cause a loss of pregnancies beyond the increase of induced abortions. The increase in the detection and labelling of complications such as malformations and growth retardation, sometimes falsely due to unavoidable false positive results, may adversely affect the pregnant woman and increase the rate of miscarriage. If, on the contrary, routine ultrasound decreases the number of miscarriages through an early detection of growth retardation for example, this would also affect the live birth rate. The results of our meta-analysis do not support either hypothesis (see table II). This consideration, nevertheless, may further establish the necessity of a measure that reflects the success of the entire pregnancy (that is, live births) and not only the success of delivery (that is, perinatal mortality) in comparing the effects of screening on outcome of pregnancy.

The Helsinki trial was the only trial with a specific intent of screening for malformations.[24] In this trial 2·4 per 1000 pregnant women in the screening group were unnecessarily disturbed by a false diagnosis of a malformed fetus, which turned out to be normal (in comparison, 2·7 per 1000 pregnant women were preserved from delivering a malformed baby). Ultrasonography, like any other test, is not free of errors, and this matters especially in the context of screening. Though routine ultrasound scanning does not improve the outcome of pregnancy, it exposes pregnant women to the risk of false diagnosis of malformations. With growing practical experience this problem may perhaps become smaller,[10] but outside the context of clinical studies overdiagnosis of malformations may well be more important.

The detection of severe malformations through ultrasonography may be sufficient reason to justify its general use. Improved skills of examiners and improved technical quality of ultrasound scanners may make screening even more effective in detecting fetal abnormalities even in a low risk population,[10 28] and in some instances this may avoid late clinical presentation of malformations needing treatment. Whether and to what extent this will decrease infant morbidity and mortality, however, has not been shown.

For many women routine ultrasound scanning may be of considerable benefit by offering the option of an early abortion of a malformed baby and by reducing the concern of giving birth to a malformed baby. This value must be weighed against the risk of false positive diagnosis of malformations. To define the magnitude of such advantages and disadvantages, however, utility analyses may be needed.

Before such analysis is performed, we suggest that routine ultrasound scanning is useful if explicitly declared as a prenatal screening for malformations to which a pregnant woman would have to consent. This would require all efforts to minimise false positive diagnosis of malformations with the possibility of abortion of normal fetuses. If a woman does not consent to screening for malformations, however, routine ultrasound scanning is not indicated.

Clinical implications

- Routine ultrasound scanning in pregnancy is effective in detecting fetal growth retardation, multiple pregnancies, and severe malformations
- This meta-analysis of randomised controlled trials shows that routine ultrasound scanning does not improve the outcome of pregnancy in terms of live birth rate and Apgar score
- Perinatal mortality is reduced because fetuses with severe malformations are aborted in an early stage of pregnancy rather than dying perinatally
- Routine ultrasound scanning in pregnancy is indicated only if explicitly performed to exclude congenital malformations

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Breast feeding and risk of breast cancer in young women

United Kingdom National Case-Control Study Group

Abstract

***Objective*—To investigate whether breast feeding is related to subsequent risk of breast cancer.**

***Design*—Population based case-control study designed primarily to investigate the relation between oral contraceptives and risk of breast cancer; data obtained from questionnaires administered by interviewers, general practitioner notes, and family planning clinic records.**

***Setting*—11 health regions in Britain.**

***Subjects*—Women diagnosed with breast cancer before age 36 living in the defined study areas. One control per case, matched for age, was selected from the list of the case's general practitioner. 755 case-control pairs were interviewed.**

***Main outcome measures*—Duration of breast feeding each liveborn infant; timing of return of menses; hormone use; other risk factors for breast cancer.**

***Results*—Risk of breast cancer fell with increasing duration of breast feeding (relative risk=0·94 per three months' breast feeding; test for trend p=0·026) and with number of babies breast fed (relative risk=0·86; test for trend, p=0·017). Breast feeding each baby for longer than three months conferred no additional benefit. Breast feeding was more strongly negatively associated with risk of breast cancer than duration of postpartum amenorrhoea (χ^2 test for trend, p=0·69). Hormonal suppression of lactation was unrelated to risk of breast cancer (relative risk=0·96 per episode of suppressed lactation; test for trend, p=0·72).**

***Conclusions*—These results suggest that breast feeding protects against the development of breast cancer in young women.**

Introduction

The main results of our large case-control study of breast cancer in young women indicated an increased risk of breast cancer associated with increasing duration of use of combined type oral contraceptive.[1,2] In the first report we noted in passing that breast feeding seemed to be associated with a significantly decreased risk of breast cancer. We report here our detailed findings on the relation between breast feeding and risk of breast cancer in these young women.

Correspondence to:
Professor C E D Chilvers, Department of Public Health Medicine and Epidemiology, University of Nottingham Medical School, Nottingham NG7 2UH.

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Subjects and methods

The study protocol and the statistical methods used have been described in detail.[1] Briefly, all women who had breast cancer diagnosed during 1982-5 and who were resident in any of 11 health regions in the United Kingdom were included, provided that breast cancer was diagnosed before their 36th birthday. For every case, one control was chosen, effectively at random, from the list of that case's general practitioner.[1] The control's date of birth was matched to within six months of the date of birth of the case, and the control had to have been registered with the general practitioner before the date of diagnosis of the case. If a case could not be interviewed no attempt was made to interview her matched control. If the chosen control could not be interviewed a second (or further) control was selected in the same manner as the first. For both cases and controls the study was restricted to white women with no previous malignancy, severe learning disability, or psychiatric condition. The women were seen in their homes by trained interviewers between January 1984 and February 1988. Each case-control pair was interviewed by the same interviewer. A total of 1049 eligible cases were identified and 755 (72%) were interviewed. Of the 755 first controls, 675 (89%) were interviewed; the remaining 80 controls were replaced by second (68) or subsequent (12) choices.

Every control was given a pseudodiagnosis date, the date on which she was exactly the same age as her matching case was at diagnosis. The data analysed were restricted to events before the diagnosis or pseudodiagnosis date. Pregnancy and contraceptive histories were taken by constructing a calendar of events for each month from age 14 to diagnosis or pseudodiagnosis. After the interview data on obstetric and contraceptive history were abstracted from general practitioner notes by trained interviewers, and contraceptive information was also sought from any family planning clinic that the woman recalled attending. The data from all sources were used to construct a lifetime contraceptive calendar. We have not distinguished between brands of combined oral contraceptives or brands of progestogen-only pills. Twenty two women (12 cases, 10 controls) reported having used oral contraceptives but could not say which type; these women were assumed to have taken combined oral contraceptives.

For each recorded pregnancy resulting in a live birth the woman was asked whether she had breast fed the child. If she answered yes she was asked the duration of breast feeding and how long it was until she had her first period after delivery.

We have reported significant differences between cases and controls for several risk factors not related to parity—namely, age at menarche, family history of breast cancer, and a history of biopsy for benign breast disease. These three variables have been adjusted for in all adjusted relative risks in this report. Marital status, age at leaving school, weight, and alcohol consumption one year before diagnosis were similar in cases and

Resource restraints: what do we tell our patients?

EDITOR,—Recently there has been a progressive contraction of the resources made available for health care in New Zealand, a trend that many other countries including the United Kingdom have experienced. These financial constraints have led to increasing instances of treatment rationing or compromise. We wish to inform our British colleagues of a ruling by a New Zealand hospital ethics committee that has been lauded in the New Zealand media as a landmark decision. The Wellington Area Health Board Ethics Committee was asked two questions:

(1) Is it ethically justified in times of resource constraint to compromise the treatment of one group of patients for the benefit of another? The committee's clear advice was that such rationing of treatments (euphemistically termed prioritisation) was appropriate. However, they also advised that if it was perceived that the service was "unduly penalised in the overall structure" then the instigator of the resource constraint (management) should be approached for a review.

(2) In the situation where the optimum treatment for a patient is compromised because of fiscally directed resource constraints, is the clinician ethically required to inform the patient of this and of the possible consequences as they relate to increased side effects and reduced effectiveness of treatment? The committee answered, "The issue of informing patients comes under the principle of veracity or truth telling . . . it is important that the patient be given the truth about the parameters of treatment available . . . and the fact you would like to do more but within constraints it is not possible."

We believe that the committee's replies have important implications for all contemporary clinical practice. We suggest that the practice of clinicians concealing from the patient the consequences of treatment compromise is common and has developed because of a humanitarian concern that such information serves no therapeutic benefit and could probably increase the patient's anxiety. The Wellington Area Health Board Ethics Committee has indicated that this approach is no longer acceptable. We agree, and we feel that this approach is an example of the type of paternalism that public opinion in New Zealand has repeatedly denounced in recent times. We imagine that the same applies in the United Kingdom.

This ethical ruling is a landmark decision that instructs clinicians to be open and honest with their patients concerning issues pertaining to treatment rationing. By citing "reasons of corporate and competitive confidentiality" (the current management phrase in New Zealand) administrators may attempt to gag clinicians faced with resource constraints. The ruling of the ethics committee protects those who speak out from the offensive label of "opportune shroudwavers out for their own gain."

It is a chilling thought that clinicians believing that concealment is humanitarian are in effect colluding with the aims of administrative systems that are developing rationing covertly.

N A SPRY
D S LAMB
P J DADY

Wellington Regional Oncology Unit,
Wellington Hospital,
Private Bag 7902,
Wellington 2,
New Zealand

Priority will be given to letters that are less than 400 words long and are typed with double spacing. All authors should sign the letter. Please enclose a stamped addressed envelope for acknowledgment.

Routine ultrasound scanning in pregnancy

The benefits are clinical . . .

EDITOR,—We agree with Heiner Bucher and Johannes Schmidt that patients who are having an ultrasound scan at 18-20 weeks' gestation should be informed about the purpose of the scan (and give written consent), and that a scan should not be performed in those who do not consent to screening for malformations.[1] The conclusion that routine ultrasonography does not improve the outcome of pregnancy in terms of an increased number of live births or reduced perinatal morbidity creates some confusion.

On the one hand the paper agrees that routine ultrasound scanning in pregnancy is effective in dating the pregnancy and in detecting fetal growth retardation, multiple pregnancies, severe malformation, placenta praevia, and the rest. On the other hand the analysis shows that scanning does not improve the outcome of pregnancy in terms of live birth rate and of perinatal morbidity.[1] Live birth rate or perinatal morbidity is influenced by several other factors occurring during the antenatal and perinatal period. Considerable clinical benefit is gained from dating of gestational age; reduction in the number of induced labours and iatrogenic prematurity; detection and monitoring of fetal growth retardation and multiple gestation; detection of placenta praevia, particularly in the asymptomatic group. Proper management of these clinical conditions has an appreciable effect on reducing maternal morbidity and perinatal mortality and morbidity as well. The value of routine ultrasound scanning at 19 weeks' gestation in a low risk population has been shown previously.[2] We think Bucher and Schmidt in analysing the data have forgotten the clinical application of the findings of ultrasound and concentrated more on the crude outcome levels. If it is done as a one stage procedure then screening for malformation is not the only purpose of doing the ultrasound scan and the other information has an important effect on the subsequent management of the pregnancy.

SUKUMAR BARIK JANE E SPRING
MICHAEL H JONES CAROLE A LUCK

Heatherwood Hospital,
Ascot,
Berkshire SL5 8AA

1 Bucher HC, Schmidt JG. Does routine ultrasound scanning improve outcome in pregnancy? Meta-analysis of various outcome measures. *BMJ* 1993;307:13-7. (3 July.)
2 Luck CA. Value of routine ultrasound scanning at 19 weeks: a four year study of 8849 deliveries. *BMJ* 1992;304:1474-8.

. . . and psychological

EDITOR,—We disagree with the conclusions of the meta-analysis by Heiner Bucher and Johannes Schmidt.[1] In the four main articles analysed the stated objectives were different. In addition, ultrasound scanning was performed at gestational ages varying from as early as 10 weeks[2] to as late as 32 weeks.[3] Since ultrasound scanning at different gestations is often for different indications or objectives it becomes difficult to group these studies together in a meta-analysis. Also, Neilson concludes that routine ultrasound in early pregnancy generally reduces the incidence of induced labours for apparent postmaturity, a conclusion differing from that in this article.[4] Finally, although there may be no significant differences in live birth rates once fetal malformations have been excluded, there are other unmeasurable aspects of routine ultrasound scanning, and one of the most important is the psychological effect on couples (especially on their attitude towards the pregnancy).

In "Can meta-analysis be trusted?" Thompson and Pocock conclude that "meta-analysis is not an exact statistical science that provides definite answers to complex clinical problems," and that quantitative results must be interpreted cautiously.[5] We feel that this is exactly what is wrong with the strongly worded conclusions from this meta-analysis.

JUSTIN C KONJE
RICHARD DE CHAZAL
DAVID J TAYLOR

Department of Obstetrics and Gynaecology,
Leicester Royal Infirmary,
Leicester LE2 7LX

1 Bucher HC, Schmidt JG. Does routine ultrasound scanning improve outcome in pregnancy? Meta-analysis of various outcome measures. *BMJ* 1993;307:13-7. (3 July.)
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Apgar scores are poor predictors of outcome

EDITOR,—I wish to express concern regarding the basis for Heiner Bucher and Johannes Schmidt's conclusion that routine ultrasonography has no impact upon perinatal morbidity.[1] Their sole criterion for assigning morbidity is Apgar score at 1 minute of <7, which is not appropriate. The 1 minute score is used as a practical guide to the necessity or otherwise of neonatal resuscitation. The poor correlation of low 1 minute and 5 minute Apgar scores with acid-base status at birth is well recognised,[2] as is the poor predictive ability of these scores for subsequent neurologic disability.[3,4] Alternative, and more appropriate measures of morbidity include the necessity and duration of admission to a neonatal unit, which also has short term staffing and financial implications, and the presence of postasphyxial encephalopathy, which is superior to the Apgar score in pre-

dicting death or severe handicap after perinatal asphyxia.[5]

PHILIP OWEN

Department of Obstetrics and Gynaecology,
Ninewells Hospital,
Dundee DD1 9SY

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Authors' reply

EDITOR,—We would have liked to include the most meaningful measures of perinatal morbidity in our meta-analysis,[1] and we share the concern of Philip Owen and Justin Konje and his colleagues that the Apgar score alone may be of limited value. Similarly, the rate of induced labours may be of little relevance for the eventual health status of the newborns (Konje and colleagues ignore that Neilson[2] did not include the Helsinki trial[3] in his analysis of the rate of induced labours, which explains the difference from our study). Our study aimed at a critical appraisal of conventional outcome measures in obstetrics and therefore we proposed a "healthy live birth rate" as the most precise and meaningful measure of the eventual pregnancy outcome. The odds ratio for the rate of admission to a neonatal care unit, if calculated for all four trials included in our meta-analysis, is 0·92 (95% confidence interval 0·80 to 1·06), which is not statistically significant. Whether this is a valid measure of perinatal morbidity can also be questioned, but its inclusion confirms our notion that routine ultrasound has not yet been shown to improve perinatal morbidity.

Sukumar Barik and colleagues seem to be trapped in the widespread misinterpretation of surrogate clinical measures and continue to ignore the plea of our paper not to confuse such surrogate measures with the overall outcome variables that matter in the end. Both of us are practising physicians and we are aware of the nature and quality of the allegation that we are ignoring the aspects related to clinical applications. The problem is that the methodology of the study invoked by Barik and colleagues cannot show the alleged value of routine ultrasound scanning.[4]

We do not deny the possibilities of "unmeasurable aspects" of ultrasound scanning. This is exactly why we stipulated the need for utility analyses to define the magnitude and importance of such intangible effects. While Konje and colleagues correctly point out some limitations of meta-analysis, they seem to misunderstand where and for what type of questions these limitations apply. Meta-analysis is used to maximise statistical power to detect true differences (that is, minimising type II errors) and to estimate the possibility of chance as the reason for an observed difference in a single study (that is, minimising type I errors). All trials included in our meta-analysis compared routine with selective ultrasound early in pregnancy, with the exception of one trial that used two stage screening,[5] and we performed meta-analysis because no individual study could show a statistically significant effect.

The careful reader may have understood, therefore, that the questions arising from our paper are not whether meta-analysis can be trusted, but what outcome measures should be used and how conventional measures may be biased and, finally, whether the use of unbiased outcome measures can show an overall benefit of routine ultrasound in pregnancy. So far, there are no data to prove that routine ultrasound scanning can improve the outcome of pregnancy.

H C BUCHER

Department of Internal Medicine,
University Polyclinic,
Kantonsspital Basel,
CH-4031 Basel,
Switzerland

J G SCHMIDT

Family Practice and Institute for Clinical Epidemiology,
CH-8840 Einsiedein,
Switzerland

1 Bucher HC, Schmidt JG. Does routine ultrasound scanning improve outcome in pregnancy? A meta-analysis of various outcome measures. *BMJ* 1993;307:13-7. (3 July.)
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Craniosynostosis

EDITOR,—Recent articles about craniosynostosis in the national press were precipitated by a report of cases in the Selby area of North Yorkshire.[1] We are concerned by the inaccuracy of many of the statements in the media and by the alarm engendered by the reports of supposed clusters of these conditions in certain geographical locations. It would certainly be unusual to see 21 new and unrelated cases of craniosynostosis in a small area over a short time, but to suggest that craniosynostosis in general or, indeed, lambdoid synostosis in particular is "virtually unknown in this country," as was said on the BBC's *Today* programme on 19 July, is both inaccurate and misleading.

About 100 new cases of craniosynostosis (simple and syndromic) present each year to the three supraregional centres in Britain (the Hospital for Sick Children, Great Ormond Street; Oxford; and Birmingham). With regard to posterior (lambdoidal) plagiocephaly—the type that apparently figures significantly in the Yorkshire cases—we have assessed 18 such cases in this unit alone in the past 12 months. This condition may become apparent only during the first two months of life, having not been present at birth. The presentation is usually that of the cosmetic deformity (which is frequently mild), and there is rarely any underlying brain dysfunction. Thus a large number of such children are probably never referred for investigation, which makes estimating the true incidence extremely difficult. Furthermore, in a large proportion of these cases (more than half of those seen at this institution in the past 12 months) there is no radiologically demonstrable sutural synostosis, and in most such cases there is a tendency toward spontaneous correction of the deformity so that surgery is not usually required. In these cases a functional deficiency of the suture rather than actual fusion seems likely. Interestingly, this form of plagiocephaly is often associated with anomalies of the cervical vertebrae, ipsilateral ear, and sternomastoid muscle.

A second report of a supposed cluster of cases of craniosynostosis[2] concerned the Apert syndrome, which is a more complex disorder with serious functional as well as cosmetic implications. Conservative estimates of the frequency of the Apert syndrome are in the region of 1 in 120 000 births,[3] so the occurrence of five cases in a 50 mile radius of Croydon over 15 months is probably little different from the natural prevalence.

We understand that a working party is to be set up to investigate the patients in Yorkshire. The three English supraregional centres already have a joint clinical audit programme; we suggest that it is more appropriate to study the geographical spread of cases in this forum than in the popular press until more accurate details are known.

BARRY JONES
RICHARD HAYWARD
DOMINIC THOMPSON

Craniofacial Centre,
Hospital for Sick Children,
London WC1N 3JH

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Heparin induced thrombosis

May complicate renal replacement treatment

EDITOR,—James B Hunter and colleagues have highlighted heparin induced thrombosis as an important complication of giving heparin to prevent thromboembolism after surgery.[1] Patients receiving renal replacement may also be at risk, and we report a case of heparin associated thrombocytopenia in a patient treated recently in our intensive care unit.

A 62 year old man with a leaking thoracoabdominal aortic aneurysm underwent an emergency operation to repair it, but his recovery was complicated by acute renal failure which necessitated intermittent haemodialysis. He developed cardiac arrhythmias and an anteroseptal infarct and then left ventricular failure. He required inotropic support, monitoring with a pulmonary artery catheter, and continued antiarrhythmic treatment. An echocardiogram showed an ejection fraction of 28% and moderate aortic stenosis. His renal support was changed to continuous venovenous haemodiafiltration.

The haemodiafiltration circuit was anticoagulated with heparin in the usual way, but the filter clotted repeatedly in the first 24 hours. Within 12 hours the patient's platelet count fell from 211×10^9/l to 98×10^9/l, and by 24 hours it was 28×10^9/l. It remained at this level throughout the four days that he received haemodiafiltration, during which time his condition stabilised. His angina was controlled, pulmonary oedema resolved, and renal function began to improve. Renal support was withdrawn, and within 24 hours his platelet count was 58×10^9/l and two days later it was 107×10^9/l.

Heparin induced thrombosis is a rare immunological disorder that occurs classically after six to 12 days of treatment with heparin and has been reported previously in association with continuous arteriovenous haemofiltration.[2] The condition was diagnosed clinically in our case after the sudden fall in platelet count 12 days after the patient's first exposure to heparin followed by the rapid recovery of the count after heparin was stopped. The heparin associated thrombocytopenia was important because of the patient's need for continuous venovenous haemodiafiltration and his damaged coronary circulation. His continuing myocardial ischaemia may have been related to platelet aggregates occluding his coronary circulation—the so called white clot syndrome. This idiosyncratic response to heparin might be relevant if he undergoes cardiac surgery in the future.

Prostacyclin was considered as an alternative to heparin, but because the patient's cardiovascular condition was poor we believed that unpredictable vasodilatation would be undesirable. This case, like the two cases reported by Hunter and col-