Induction of labour versus standard care to prevent shoulder dystocia in fetuses suspected to be large for gestational age in the UK (the Big Baby trial): a multicentre, open-label, randomised controlled trial

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Summary

Background The benefits and harms of early induction of labour to reduce shoulder dystocia in fetuses suspected to be large for gestational age (LGA) are uncertain. We aimed to investigate whether early induction of labour is associated with a reduced risk of shoulder dystocia compared with standard care.

Methods In this open-label, randomised controlled phase 3 trial, women aged ≥18 years with a suspected LGA fetus (estimated fetal weight >90th customised percentile) as identified by ultrasound scan between 35 weeks and 0 days (35^{+0} weeks) of gestation and 38^{+0} weeks' gestation, recruited from 106 hospitals across England, Scotland, and Wales in the UK, were randomly assigned (1:1) by web app to standard care or induction of labour between 38^{+0} weeks' gestation and 38^{+4} weeks' gestation using minimisation, balancing site, estimated fetal weight percentile (≤95th EFW percentile or >95th EFW percentile), and maternal age (≤35 years or >35 years). Key exclusion criteria included drug-treated diabetes, gestational diabetes, and elective caesarean section or induction already planned or indicated for any reason. Our primary outcome was incidence of shoulder dystocia, assessed by a masked independent expert adjudication panel who reviewed participants' delivery notes. Induction of labour was anticipated to result in birth 10·5 days earlier with a 300 g lower birthweight on average than standard care. We did an intention-to-treat (ITT) analysis in all participants for whom we had primary outcome data, and a per-protocol analysis in participants in the induction group who went into labour or were induced at 38^{+0} to 38^{+4} weeks' gestation versus participants in the standard care group who had not started labour, been induced, or had an elective caesarean section before 38^{+4} weeks' gestation. This study was registered with ISRCTN (18229892) and is no longer recruiting.

Findings Between June 8, 2018, and Oct 25, 2022, 2893 women were randomly assigned to induction of labour (n=1447) or standard care (n=1446); the trial was terminated before the target of 4000 participants was reached on advice of the data monitoring committee following the lower-than-expected incidence of shoulder dystocia in the standard care group. Two participants in the induction group and seven in the standard care group had missing data for the primary outcome and were excluded from the ITT analysis. In the ITT analysis, 33 (2·3%) of 1445 babies in the induction group versus 44 (3·1%) of 1439 in the standard care group had shoulder dystocia (risk ratio [RR] 0·75 [95% CI 0·51–1·09]; p=0·14) with a mean difference of –6·0 days' (95% CI –6·3 to –5·6) gestation and –163·6 g (–190·0 to –137·1) birthweight between trial groups. 355 (24·6%) of 1446 mothers in the standard care group were induced, delivered, or went into labour at or before 38^{+4} weeks' gestation. In the per-protocol analysis, 27 (2·3%) of 1180 babies in the induction group versus 40 (3·7%) of 1074 in the standard care group had shoulder dystocia (RR 0·62 [0·41–0·92]; p=0·019), and there was a mean difference of –8·1 days' (–8·4 to –7·9) gestation and –213·3 g (–242·0 to –184·6) birthweight between trial groups. One neonatal death occurred from perinatal asphyxia after shoulder dystocia in the standard care group, and one neonatal death occurred following sepsis and congenital pneumonia in the induction group. 88 (6·1%) of 1447 mothers in the induction group had an adverse event versus 108 (7·5%) of 1446 in the standard care group (RR 0·81 [0·62 to 1·06]; p=0·13). Similar numbers of serious adverse events were reported in both groups.

Interpretation No significant difference in incidence of shoulder dystocia was found between trial groups in the ITT analysis, probably due to the high proportion of earlier-than-expected deliveries in the standard care group reducing the intended between-group differences in gestational age and birthweight. However, in the per-protocol analysis, compared with all deliveries after 38^{+4} weeks' gestation, induction of labour between 38^{+0} weeks' gestation and 38^{+4} weeks' gestation did show a significant reduction in shoulder dystocia. This study provides pregnant women with suspected LGA fetuses and their clinicians important information about choices and decision making for timing and mode of birth.

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See Comment page 1718

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Introduction

Shoulder dystocia is defined as a vaginal cephalic birth that requires additional obstetric manoeuvres to deliver the fetus after the head has delivered and gentle traction has failed.[1,2] Potential complications of shoulder dystocia include maternal haemorrhage and third-degree and fourth-degree perineal tears. For the baby, complications include fractures, brachial plexus injury, hypoxic ischaemic encephalopathy, and neonatal death.[1] Shoulder dystocia can also result in substantial psychological trauma to the mother, family, and clinical teams involved.[3] Harm associated with shoulder dystocia is a common reason for litigation in obstetrics.[4]

Appropriate management of shoulder dystocia includes clinical awareness, staff training, and appropriate protocols and emergency drills.[5] Preventive measures start with antenatal awareness of risk factors, including obesity, diabetes, and fetal growth and size. Macrosomia (variably defined as fetal weight of more than 4·0 kg or more than 4·5 kg) and the fetus being large for gestational age (LGA; >90th percentile) are associated with an increased risk of shoulder dystocia.[2,6]

Detection of LGA is an important aspect of antenatal care that is usually done alongside surveillance for small size for gestational age and fetal growth restriction. Standard screening in the UK is by serial assessment and plotting of fundal height and, if indicated by size or trajectory of the growth curve, referral for ultrasound biometry and estimation of fetal weight. In pregnancies at increased risk of growth disorders (eg, in women with diabetes) serial assessment by ultrasound is recommended.[7]

Earlier delivery of an LGA fetus should reduce the baby's birthweight, mitigating the risk of shoulder dystocia. A 2016 Cochrane review of trials of induction of labour found a reduced risk of shoulder dystocia associated with induced deliveries compared with expectant management (risk ratio [RR] 0·60 [95% CI 0·37–0·98]).[8] A 2017 systematic review, however, found that this reduction did not reach statistical significance (RR 0·57 [95% CI 0·30–1·08]).[9] Both reviews included data from the same four trials and a total of 1190 participants, and their conclusions were largely driven by results of the largest trial, by Boulvain and colleagues (with 817 participants), which included babies with a birthweight higher than the 95th estimated fetal weight (EFW) percentile and found that induction from 37 weeks' gestation reduced the incidence of severe shoulder dystocia compared with expectant care.[10] Because of emerging evidence of potential long-term consequences of early-term deliveries,[11] and the continued view in contemporary guidelines[2] that induction of labour does not prevent shoulder dystocia in

Research in context

Evidence before this study

Fetuses that are large for gestational age (LGA) have an increased risk of perinatal complications, and earlier delivery by induction of labour might reduce this risk. We searched MEDLINE for systematic reviews from Jan 1, 2000 to May 31, 2017 with the search terms "induction of labour", "macrosomia", "large for gestational age", and "shoulder dystocia". Our search identified two systematic reviews (2016 and 2017) of four trials with a total of 1190 participants. These reviews found a reduced risk of shoulder dystocia, fracture, and brachial plexus injury following early induction of labour compared with expectant care; however, only one review concluded that these differences were statistically significant. The conclusions of both reviews were largely driven by the results of a single trial, which had a protocol of induction of labour starting from 37 weeks' gestation.

Added value of this study

To our knowledge, this is the largest randomised controlled trial of induction of labour to prevent shoulder dystocia, and included more than twice as many pregnancies as the combined total of all trials included in the two selected systematic reviews. The study assessed a protocol of induction from 38 weeks' gestation compared with delivery after 39 weeks' gestation and found that early delivery of a baby suspected to be LGA can reduce the risk of shoulder dystocia and can have no effect on secondary neonatal outcomes. Contrary to previous evidence, induction was found to decrease the need for caesarean sections and did not increase third-degree and fourth-degree perineal tears. The study provides important information for clinical management options, and empowers pregnant women to choose the time and mode of delivery of their suspected LGA baby.

Implications of all the available evidence

On the basis of our per-protocol analysis, we found that induction of labour in pregnancies with LGA fetuses can reduce shoulder dystocia at 38 weeks' gestation as well as from the previously reported 37 weeks' gestation. Contrary to previous studies, our large trial found that induction of labour did not increase maternal trauma in terms of third-degree and fourth-degree tears, and reduced postpartum haemorrhage and emergency caesarean sections. We also found no change in adverse neonatal outcomes with induction of labour. Therefore, the timing and mode of delivery in LGA pregnancies can account for maternal choice and include planned caesarean section to eliminate the risk of shoulder dystocia, or induction of labour to reduce risk of shoulder dystocia, without increasing the risk of adverse maternal outcomes.

non-diabetic mothers with a suspected macrosomic fetus, we felt that this clinical challenge required further investigation. We therefore conducted a randomised controlled trial to investigate the potential benefits and harms of induction of labour from 38 weeks' gestation to reduce the risk of shoulder dystocia and provide data to help pregnant women with suspected large babies (and their clinicians) make better informed choices.

Within this publication, we use terms such as pregnant women. However, it is important to acknowledge that it is not only people who identify as women for whom the results of this trial are of interest and relevant. The authors maintain that reporting of studies and delivery of care must be appropriate, inclusive, and sensitive to the needs of individuals whose gender identity does not align with the sex they were assigned at birth.

Methods

Study design

The Big Baby trial was a prospective, phase 3, open-label, parallel-group, multicentre, randomised controlled trial of induction of labour versus standard care for pregnant women with suspected LGA fetuses. Recruitment was from 106 National Health Service (NHS) hospitals across England, Scotland, and Wales in the UK. The trial protocol was approved by the South West Exeter Research Ethics Committee (18/SW/0039) and has been published[12] and since updated.[13] The statistical analysis plan can also be accessed online.[14] This study is registered with the ISRCTN registry, number 18229892.

Participants

Potentially eligible pregnant women were approached after a fetus was suspected to be LGA (>90th percentile) following ultrasound scan between 35 weeks and 0 days (35^{+0} weeks) of gestation and 38^{+0} weeks' gestation. The EFW was derived from ultrasound measurements of the head, abdomen, and femur using Hadlock's formula.[15] The EFW percentile was derived from Gestation Related Optimal Weight (GROW) software version 1.5,[16] customised for maternal height, weight, parity, and ethnic origin.[17] Customised GROW charts and percentiles for fetal and neonatal weight assessment to identify LGA babies have a stronger association with predicting adverse outcomes than non-customised, population-average standards.[18–21] GROW charts are used in most NHS maternity units in the UK as part of the Growth Assessment Protocol programme;[22,23] these maternity units constituted the pool of hospitals that participated in the trial.

We included women aged 18 years or older who were pregnant with a fetus with a weight higher than the 90th customised GROW fetal weight percentile on ultrasound scan at 35^{+0} weeks' gestation to 38^{+0} weeks' gestation and cephalic presentation. Scans were not done routinely but on clinical indication or following large or accelerating fundal height measurements. Our exclusion criteria were multifetal pregnancy, non-cephalic presentation, receiving drug treatment (with insulin or oral hypoglycaemics) for diabetes or gestational diabetes, induction being contraindicated, elective caesarean section or induction already planned or indicated for any reason, planned home birth, being a prisoner, a current diagnosis of a psychiatric disorder that required treatment with antipsychotic medication, previous stillbirth, inability to give informed consent, or the fetus having a known serious abnormality. Women with drug-treated diabetes were excluded because of the recommendation by the National Institute of Health and Care Excellence (NICE) to routinely offer induction between 37 weeks' gestation and 38^{+6} weeks' gestation.[7]

Individuals eligible for random assignment had to have obstetrician agreement to participate in the trial. They were offered written information and invited to have a detailed discussion with trial-site midwives and obstetricians about the trial. Potential participants were given sufficient time to consider and gave informed consent either face to face or, during the COVID-19 pandemic, remotely in writing. Eligible women were not randomly assigned if the research team was not available to offer participation in the trial.

A concern at the time of trial design was that individuals who were otherwise eligible would, after being informed of the risks of shoulder dystocia and induction of labour, choose an elective caesarean section or decline random assignment because of a preference for either induction of labour or expectant care. Those who declined random assignment were therefore invited to join a parallel cohort study to provide comparative data on outcomes.

Before random assignment, we collected maternal health-related quality-of-life data using the 5-level EQ-5D as well as routine demographic data, including age, self-reported ethnicity data according to GROW categories (appendix p 11), parity, height, weight, and smoking status.

See Online for appendix

Randomisation and masking

By means of a bespoke online web tool developed by the Warwick Clinical Trials Unit and made accessible to all recruiting sites, participants were randomly assigned 1:1 to either booking induction of labour between 38^{+0} weeks' gestation and 38^{+4} weeks' gestation or standard care. A backup telephone service was available in the event that research teams were not able to access the online tool. Women were enrolled by site research midwives and obstetricians and randomly assigned using minimisation, balancing site, EFW percentile (≤95th EFW percentile or >95th EFW percentile), and maternal age at recruitment (≤35 years or >35 years). The randomisation sequence that assigned grouping was generated by Warwick Clinical Trial Unit statisticians. After randomisation, research midwives and statisticians had no involvement in the rest of the trial.

To ensure allocation concealment, random assignment only took place once all baseline data had been collected. Participants and their clinical teams were not masked to treatment allocation. Once enrolment into the online tool occurred, the tool allocated a group and participants were immediately informed of their allocation in person.

Procedures

The intervention was the offer of induction of labour between 38⁺⁰ weeks' gestation and 38⁺⁴ weeks' (266–270 days) gestation. The method of induction followed the standard practice of the participating hospital. Standard care was defined as the hospital's usual care. In the standard care group, to help create a sufficient gap in gestational age between trial groups, induction of labour before 39⁺⁴ weeks' gestation was discouraged unless clinically indicated. On the basis of a previous analysis of regional health service data (appendix p 38) we aimed to have a mean difference of at least 1·5 weeks (10·5 days) of gestation between trial groups that resulted in a 300 g mean difference in birthweight. Inevitably, clinical decisions, participant, or spontaneous labour meant women in either group of the trial could be induced or deliver outside the planned delivery windows. We collected data on additional infant and maternal outcomes by postal questionnaire, supplemented by telephone interviews if required, 2 months and 6 months after delivery (appendix p 7).

Outcomes

Our primary outcome was incidence of shoulder dystocia, defined as a vaginal cephalic birth requiring additional obstetric manoeuvres to deliver the fetus after the head has delivered and gentle traction has failed.[2] Copies of the delivery notes of all randomly assigned participants were assessed centrally by an independent expert adjudication panel consisting of a midwife, senior obstetrician, junior obstetrician, and neonatologist. At least two members of the panel reviewed each participant's delivery notes. In cases of discrepancy, all members of the panel were consulted. The panel was masked to the trial allocation, and all records were redacted to ensure that no site details, participant details, or details of antenatal care were provided.

Secondary outcomes included gestational age and birthweight (to show between-group differentiation), duration of hospital admission, and other neonatal and maternal peripartum outcomes from routinely recorded data; the full list of secondary outcomes is presented in the appendix (p 7). A health economic evaluation and more exploratory subgroup analyses will be reported separately.[24]

In previous trials, the incidence of individual harm consequential to shoulder dystocia was small. Even a very large trial would be unlikely to show statistically significant differences between study groups. We therefore prespecified three composite harm outcomes: intrapartum birth injury (ie, fractures of clavicle or long bones of upper extremity or brachial plexus injury in the baby), prematurity-associated problems (ie, the need for phototherapy or respiratory support), and maternal intrapartum complications (third-degree or fourth-degree perineal tear, cervical tear, or primary postpartum haemorrhage [defined as blood loss of ≥500 mL, with blood loss of ≥1000 mL designated major postpartum haemorrhage]).[25]

We compared by trial group the number of adverse events and serious adverse events. Common terminology criteria for adverse events and System Organ Classes were used to categorise both adverse events and serious adverse avents. Safety data for mothers and babies were analysed separately.

Statistical analysis

To calculate our target sample size in the absence of information on the incidence of shoulder dystocia in the UK, we took the incidence in the trial by Boulvain and colleagues (3·9%)[9] done in France, Belgium, and Switzerland and rounded it up to 4%. In that trial, the intervention resulted in a two-thirds reduction of clinically significant shoulder dystocia (RR 0·32 [95% CI 0·15–0·71]), suggesting that a 50% reduction (from 4% to 2%) would be a plausible target. To show such a reduction at a significance of 5% with 90% power required 3252 participants. However, because of uncertainty about the baseline incidence of shoulder dystocia in our population, we increased the sample size estimate to 4000 and asked the independent data monitoring committee to monitor data throughout the trial to be able to terminate recruitment when data were sufficient to answer the research question.

Comparative analyses were done under an intention-to-treat framework. For the primary analysis and estimating odds ratios (ORs), a generalised linear model with family binomial and link logit, adjusted for site, EFW percentile, and maternal age at recruitment was used, with the addition of variance–covariance clustering to allow for correlated errors within the sites. We estimated RRs by using the log-binomial generalised linear model (link log). We report RRs for ease of interpretation, apart from a few cases where convergence was not reached and ORs were estimated using the Firth model.[26] For the primary outcome, we also did a preplanned and prespecified per-protocol analysis (appendix p 39), which compared participants randomly assigned to early induction of labour and induced at 38⁺⁰ weeks' gestation to 38⁺⁴ weeks' gestation with participants in the standard care group who had not started labour by 38⁺⁴ weeks' gestation and who had not been induced or delivered by elective caesarean section before 38⁺⁴ weeks' gestation. The primary analysis population consisted of all randomly assigned participants; however, unless the proportion of missing data was greater than 10%, participants who had missing

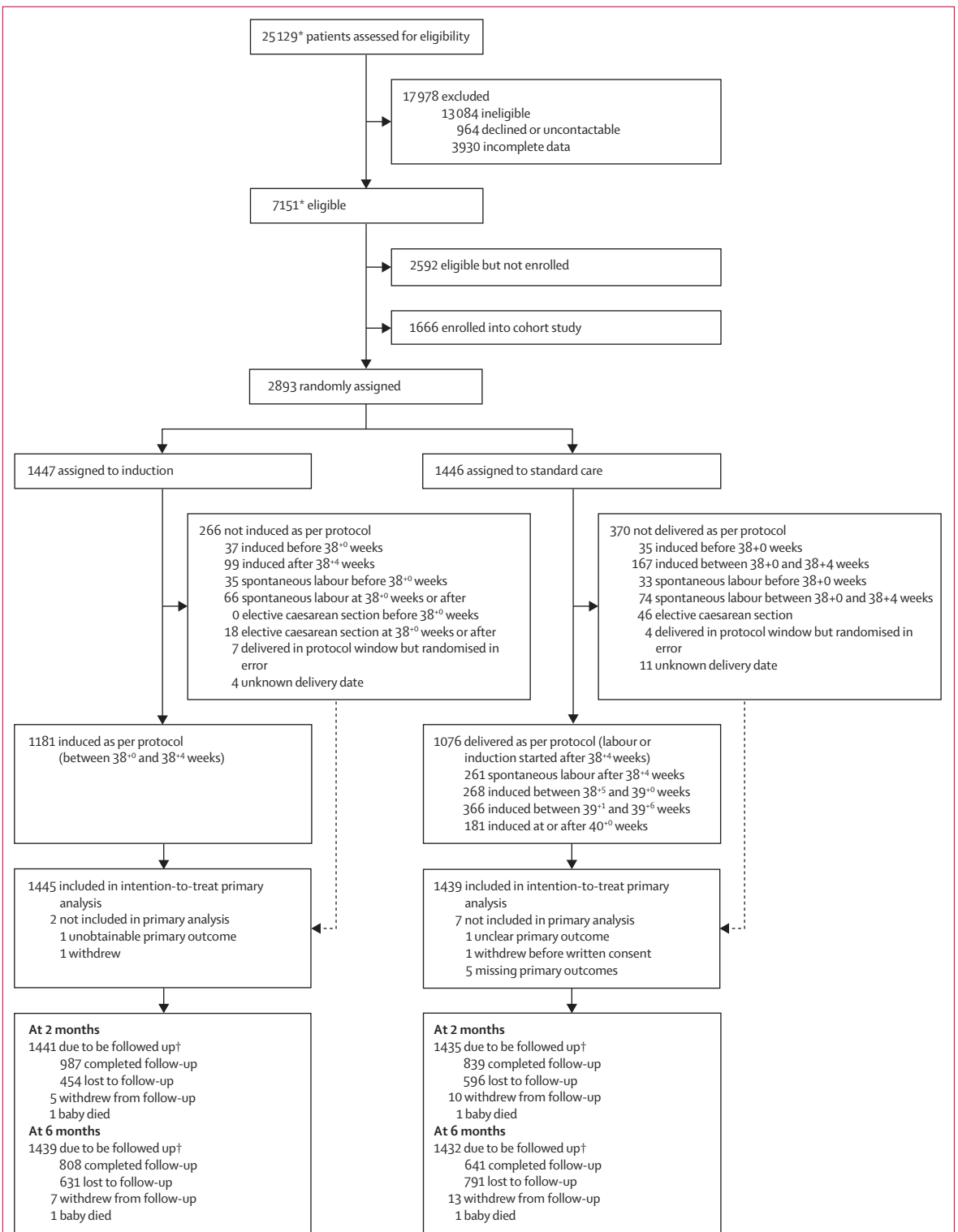

Figure: **CONSORT diagram**
Weeks refers to weeks of gestation. *Including three not included in screening logs. †Including 8-week follow-up period after due date, after which participant was considered lost to follow-up if no response had been received.

data for the primary outcome were excluded from the analysis population without imputation. Preplanned subgroup analyses were conducted for participant BMI in early pregnancy (<25 kg/m² or ≥25 kg/m²) and EFW percentile (≤95th or >95th). All secondary analyses were adjusted for site, EFW percentile, and participant age. All participants with available safety data were included in safety analyses. Regression diagnostics were checked, and model fit was assessed for the primary and secondary analyses (appendix p 34). Analyses followed a statistical analysis plan[14] agreed and signed by the data monitoring committee before database hard lock and final analysis. Stata (version 18) was used for data validation and analyses.

Role of the funding source

The funder of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report.

Results

Between June 8, 2018, and Oct 25, 2022, we identified 25 129 potentially eligible pregnant women from 106 NHS hospitals across England, Scotland, and Wales (appendix p 37). Of these, 17 978 were excluded (figure). Of the 7151 eligible individuals, 2893 were randomly assigned (1447 to the intervention group and 1446 to the control group) and 1666 chose an elective caesarean section, induction of labour, or standard care, and joined the cohort study. The data monitoring committee did not advise any adjustment to sample size following the planned key event analysis in February, 2020, based on data from the first 1000 participants. In October, 2022, recruitment was stopped on advice of the data monitoring committee after their review of the data showed that, with the current incidence of shoulder dystocia in the standard care group, recruitment of 12 884 women would be required to achieve 90% power, whereas, after the COVID-19 pandemic, the recruitment rate was only approximately 10 women per week (appendix p 40).

Of the 1447 women in the induction group, 1181 (81·6%) were induced between 38^{+0} weeks' gestation and 38^{+4} weeks' gestation, 37 (2·6%) were induced before 38^{+0} weeks' gestation, 99 (6·8%) were induced after 38^{+4} weeks' gestation, 35 (2·4%) went into spontaneous labour before 38^{+0} weeks' gestation, 66 (4·6%) went into spontaneous labour at 38^{+0} weeks' gestation or after, 18 (1·2%) had an elective caesarean section, seven (0·5%) were randomly assigned in error, and four (0·3%) had an unknown delivery date. Of the 1446 participants in the standard care group, 1076 delivered as per protocol (ie, labour or induction started after 38^{+4} weeks' gestation), 355 (24·6%) were induced or delivered at or before 38^{+4} weeks' gestation or had an elective caesarean section; four (0·3%) were randomly assigned in error, and 11 (0·8%) had an unknown delivery date (figure). 20 (1·4%) of 1447 participants in the induction group and 17 (1·2%) of 1446 in the standard care group withdrew from the trial after random assignment (appendix p 8). The mean age at recruitment was 28·8 years (SD 5·3), with 1639 (56·7%) of 2893 participants primiparous and 2419 (83·6%) British European (table 1). We obtained primary outcome data from 1445 (99·9%) of 1447 pregnancies in the induction group and 1439 (99·5%) of 1446 pregnancies in the standard care group (figure). Nine (0·3%) of 2893 randomly assigned women had missing primary outcome data (two in the induction group and seven in the standard care group), and the data were considered to be missing at random; due to the small proportion of participants with missing data, no sensitivity analyses imputing missing data were done.

Analysis by intention-to-treat found the incidence of shoulder dystocia to be 2·3% (33 of 1445) in the induction group and 3·1% (44 of 1439) in the standard care group, a non-significant reduction of 25% (RR 0·75 [95% CI 0·51–1·09]; table 2). The per-protocol analysis included 2254 (77·9%) of 2893 participants (1180 [81·5%] of 1447 in the induction group and 1074 [74·3%] of 1446 in the standard care group; figure). The incidence of shoulder dystocia was 2·3% (27 of 1180) in the induction group and 3·7% (40 of 1047) in the standard care group, a statistically significant reduction of 38% (RR 0·62 [95% CI 0·41–0·92]).

For the intention-to-treat analysis, the mean gestation at delivery was 38^{+4} weeks or 270 days (SD 3·0) for the intervention group and 39^{+3} weeks or 276 days (SD 5·6) in the standard care group (mean difference –6·0 days [95% CI –6·3 to –5·6]; table 2). Mean birthweights were 3693 g (SD 349·8) in the induction group and 3857 g (SD 375·2) in the standard care group (mean difference –163·6 g [–190·0 to –137·1]; table 2). The birthweight percentile was similar in both groups, with 610 (42%) of 1447 babies in the induction group and 576 (40%) of 1446 babies in the standard care group with a birthweight higher than the 90th percentile. In the induction group, 260 (18·0%) of 1447 babies weighed more than 4000 g, and in the standard care group, 469 (32·4%) of 1446 babies weighed more than 4000 g. In the per-protocol analysis, the mean differences were –8·1 days (95% CI –8·4 to –7·9) for gestation and –213·3 g (95% CI –242·0 to –184·6) for birthweight (table 2), and 195 (16·5%) of 1180 babies in the induction group and 384 (35·8%) of 1074 babies in the standard care group had birthweights of more than 4000 g.

The mean total duration of hospital stay (from admission to discharge) was longer in the induction group than the standard care group by 0·40 days (95% CI 0·24–0·55). This difference was driven by an increased duration of hospital stay before delivery (0·50 days [0·40–0·60]; table 3). Compared with the standard care group, participants in the induction group were less likely to have a pre-labour caesarean section (relative RR 0·38 [0·24–0·59]) or emergency caesarean section

	Induction (n=1447)	Standard care (n=1446)
Age at recruitment, years*		
Mean (SD)	28·8 (5·3)	28·8 (5·4)
≤35 years	1286 (88·9%)	1286 (88·9%)
>35 years	161 (11·1%)	160 (11·1%)
Previous pregnancies of ≥24 weeks' gestation†		
0	808 (55·8%)	831 (57·5%)
1	386 (26·7%)	344 (23·8%)
2	149 (10·3%)	177 (12·2%)
3+	104 (7·2%)	92 (6·4%)
Missing	0 (0·0%)	2 (0·1%)
Ethnicity‡		
British European	1228 (84·9%)	1191 (82·4%)
East European	32 (2·2%)	46 (3·2%)
West African	19 (1·3%)	16 (1·1%)
Indian	39 (2·7%)	30 (2·1%)
Pakistani	30 (2·1%)	51 (3·5%)
Declined	2 (0·1%)	2 (0·1%)
Other	97 (6·7%)	108 (7·5%)
Missing	0	2 (0·1%)
BMI at early pregnancy visit, kg/m²		
Underweight (<18·5)	32 (2·2%)	40 (2·8%)
Healthy (≥18·5 to <25)	518 (35·8%)	510 (35·3%)
Overweight (≥25 to <30)	404 (27·9%)	398 (27·5%)
Obese (≥30)	493 (34·1%)	496 (34·3%)
Missing	0	2 (0·1%)
Diet-controlled gestational diabetes§		
Yes	60 (4·1%)	72 (5·0%)
No	1387 (95·9%)	1372 (94·9%)
Missing	0	2 (0·1%)
Smoker at early pregnancy visit§		
Yes	134 (9·3%)	154 (10·7%)
No	1313 (90·7%)	1290 (89·2%)
Missing	0	2 (0·1%)
Received corticosteroid for fetal lung maturation during pregnancy§		
Yes	45 (3·1%)	25 (1·7%)
No	1401 (96·8%)	1413 (97·7%)
Missing	1 (0·1%)	8 (0·6%)

Data are n (%) or mean (SD). Age, ethnicity, and BMI data were collected at recruitment. Gestational diabetes, smoking status, and corticosteroid use data were collected at baseline visit. *Stratification variable. †For information on previous pregnancies see appendix (p 9). ‡Ethnicity categories containing <1% of participants were grouped together in the Other category; for full details see appendix pp 10–12. §Collected at baseline visit.

Table 1: **Demographic and baseline characteristics**

	Induction	Standard care	Mean difference (95% CI) or adjusted analysis*
Intention-to-treat analysis			
n	1447	1446	..
Gestation at delivery, days	270 (3·0; n=1446)	276 (5·6; n=1440)	−6·0 (−6·3 to −5·6)
Birthweight, g	3693 (349·8; n=1446)	3857 (375·2; n=1440)	−163·6 (−190·0 to −137·1)
Birthweight >90th percentile	610 (42·2%)	576 (39·8%)	..
Birthweight >4000 g	260 (18·0%)	469 (32·4%)	..
Shoulder dystocia†	33/1445 (2·3%)	44/1439 (3·1%)	0·75 (0·51 to 1·09); p=0·14*
Per-protocol analysis			
n	1180	1074	..
Gestation at delivery, days	270 (2·0)	278 (4·1)	−8·1 (−8·4 to −7·9)
Birthweight, g	3686 (337·0)	3899 (357·5)	−213·3 (−242·0 to −184·6)
Birthweight >90th percentile	487 (41·3%)	402 (37·4%)	..
Birthweight >4000 g	195 (16·5%)	384 (35·8%)	..
Shoulder dystocia	27 (2·3%)	40 (3·7%)	0·62 (0·41 to 0·92); p=0·019*

Data are n (%) or mean (SD). Statistical analysis based on complete data. All caesarean sections were counted as no shoulder dystocia. *Relative risk (95% CI) and p value from generalised linear model, with family binomial and link log, adjusted by site, estimated fetal weight percentile (≤95th or >95th) and maternal age (≤35 years or >35 years), with clustering adjustments for site. †Primary analysis.

Table 2: **Gestational age, birthweight, and incidence of shoulder dystocia by study group, according to intention-to-treat and per-protocol analyses**

(relative RR 0·79 [0·66–0·94]). Participants in the induction group were also less likely than participants in the standard care group to require delivery by forceps (relative RR 0·78 [0·62–0·99]; table 3).

Women in the induction group had fewer primary postpartum haemorrhages (ie, a loss of ≥500 mL blood) than participants in the standard care group (648 [44·8%] of 1447 *vs* 709 [49·0%] of 1446; RR 0·91 [95% CI 0·84–0·98]; table 3). More participants had a second-degree perineal tear in the induction group than in the standard care group (405 [28·0%] of 1447 *vs* 354 [24·5%] of 1446; RR 1·14 [1·01–1·29]), but the induction group had fewer episiotomies than the standard care group (286 [19·8%] of 1447 *vs* 327 [22·6%] of 1446; RR 0·87 [0·76–1·00]). No significant differences were found between the induction and standard care groups in third-degree perineal tears (33 [2·3%] of 1447 *vs* 32 [2·2%] of 1446; RR 1·03 [0·64–1·66]), fourth-degree perineal tears (4 [0·3%] of 1447 *vs* 5 [0·3%] of 1446; RR 0·79 [0·21–2·94]), cervical tears, retained placenta, sepsis or pyrexia in labour, or hospital readmission rates within 30 days. No maternal deaths occurred in the study, although two neonatal deaths occurred. Our composite outcome of maternal intrapartum complications (third-degree or fourth-degree perineal tear, cervical injury, or primary postpartum haemorrhage, or a combination of these complications) showed a reduction in the induction group (663 [45·8%] of 1447 participants) compared with the standard care group (727 (50·3%) of 1446; RR 0·91 [0·84–0·98]; table 3).

No statistically significant differences were found in neonatal secondary outcomes between study groups, including admission to the neonatal unit after delivery (RR 1·11 [95% CI 0·90–1·38]) and neonatal hospital readmissions within 30 days of postnatal inpatient discharge (RR 1·18 [0·97–1·44]; table 4).

	Induction (n=1447)	Standard care (n=1446)	Adjusted estimate (95% CI)	p value*
Delivery timings				
Time between delivery of head and delivery of body, min†				
Mean (SD)	1·09 (0·94; n=996)	1·21 (1·02; n=923)	−0·13 (−0·21 to −0·04)	0·0047
Missing	40/1036 (3·9%)	46/969 (4·7%)	..	..
Time from commencement of active second stage of labour until fetal expulsion, min†				
Mean (SD)	46·2 (49·1; n=939)	50·9 (51·2; n=859)	−4·81 (−9·43 to −0·20)	0·041
Missing	2/1036 (0·2%)	6/969 (0·6%)	..	..
NA or unknown due to timing of arrival at hospital	95/1036 (9·2%)	104/969 (10·7%)	..	..
Time in labour ward, h				
Mean (SD)	21·2 (17·2; n=1407)	19·0 (16·6; n=1419)	2·19 (0·94 to 3·43)	0·0006
Missing	40 (3%)	27 (2%)	..	..
Duration of hospital stay before delivery, days‡				
Mean (SD)	1·97 (1·45; n=1444)	1·46 (1·41; n=1436)	0·50 (0·40 to 0·60)	<0·0001
Missing	2 (0·1%)	7 (0·5%)	..	..
NA (not born in hospital)	1 (0·1%)	3 (0·2%)	..	..
Duration of hospital stay after delivery, days‡				
Mean (SD)	1·46 (1·46; n=1446)	1·56 (1·47; n=1440)	−0·11 (−0·21 to 0·001)	0·052
Missing	1 (0·1%)	6 (0·4%)	..	..
Total duration in hospital from admission to discharge, days				
Mean (SD)	3·42 (2·13; n=1445)	3·02 (2·14; n=1436)	0·40 (0·24 to 0·55)	<0·0001
NA (not born in hospital)	1 (0·1%)	3 (0·2%)	..	..
Missing	1 (0·1%)	7 (0·5%)	..	..
Delivery characteristics				
Labour type onset				
Spontaneous	89 (6·2%)	358 (24·8%)	RRR 0·19 (0·15 to 0·24)	<0·0001§
Induced	1327 (91·7%)	1021 (70·6%)	1 (ref)	..
No labour (caesarean section)	30 (2·1%)	61 (4·2%)	RRR 0·38 (0·24 to 0·59)	<0·0001§
Missing	1 (0·1%)	6 (0·4%)	..	..
Mode of delivery				
Spontaneous vaginal delivery	799 (55·2%)	704 (48·7%)	1 (ref)	..
Vaginal delivery, ventouse	57 (3·9%)	61 (4·2%)	RRR 0·82 (0·57 to 1·20)	0·31§
Vaginal delivery, forceps	157 (10·9%)	177 (12·2%)	RRR 0·78 (0·62 to 0·99)	0·042§
Vaginal delivery, rotational forceps	22 (1·5%)	21 (1·5%)	RRR 0·92 (0·50 to 1·69)	0·79§
Elective caesarean section	39 (2·7%)	61 (4·2%)	RRR 0·56 (0·37 to 0·85)	0·0064§
Emergency caesarean section	372 (25·7%)	416 (28·8%)	RRR 0·79 (0·66 to 0·94)	0·0066§
Missing	1 (0·1%)	6 (0·4%)	..	..
Presentation at birth				
Cephalic	1439 (99·5%)	1428 (98·8%)	1 (ref)	..
Breech	3 (0·2%)	5 (0·4%)	RRR 0·60 (0·14 to 2·48)	0·48§
Transverse lie	3 (0·2%)	7 (0·5%)	RRR 0·43 (0·11 to 1·69)	0·23§
Missing	2 (0·1%)	6 (0·4%)	..	..
Maternal outcomes				
Primary postpartum haemorrhage (≥500 mL blood loss)				
Yes	648 (44·8%)	709 (49·0%)	RR 0·91 (0·84 to 0·98)	0·016
Missing	1 (0·1%)	6 (0·4%)	..	..
Major primary postpartum haemorrhage (≥1000 mL blood loss)				
Yes	203 (14·0%)	220 (15·2%)	RR 0·91 (0·77 to 1·09)	0·32
Missing	1 (0·1%)	6 (0·4%)	..	..
Episiotomy				
Yes	286 (19·8%)	327 (22·6%)	RR 0·87 (0·76 to 1·00)	0·054
Missing	1 (0·1%)	6 (0·4%)	..	..

(Table 3 continues on next page)

	Induction (n=1447)	Standard care (n=1446)	Adjusted estimate (95% CI)	p value*
(Continued from previous page)				
Perineal injury degree¶				
First	161 (11·1%)	148 (10·2%)	RR 1·08 (0·88 to 1·34)	0·47
Second	405 (28·0%)	354 (24·5%)	RR 1·14 (1·01 to 1·29)	0·037
Third	33 (2·3%)	32 (2·2%)	RR 1·03 (0·64 to 1·66)	0·91
Fourth	4 (0·3%)	5 (0·4%)	RR 0·79 (0·21 to 2·94)	0·73
Unknown	2 (0·4%)	3 (0·6%)	..	..
Cervical laceration				
Yes	13 (0·9%)	13 (0·9%)	RR 0·99 (0·46 to 2·14)	0·99
Missing	1 (0·1%)	6 (0·4%)	..	..
Retained placenta requiring manual removal				
Yes	39 (2·7%)	37 (2·6%)	RR 1·05 (0·67 to 1·64)	0·83
Missing	1 (0·1%)	6 (0·4%)	..	..
Maternal death				
Yes	0	0	NA	NA
Missing	1 (0·1%)	6 (0·4%)	..	..
Sepsis in labour or within 24 h postpartum				
Yes	84 (5·8%)	95 (6·6%)	RR 0·88 (0·66 to 1·17)	0·38
Missing	1 (0·1%)	7 (0·5%)	..	..
Fever >38°C in labour or within 24 h postpartum				
Yes	93 (6·4%)	106 (7·3%)	RR 0·87 (0·67 to 1·14)	0·31
Missing	1 (0·1%)	7 (0·5%)	..	..
Maternal readmissions				
Hospital readmission within 30 days of discharge				
Yes	80 (5·5%)	100 (6·9%)	RR 0·80 (0·60 to 1·06)	0·12
Missing	1 (0·1%)	8 (0·6%)	..	..
Composite outcome				
Third-degree or fourth-degree perineal tear, cervical laceration or tear, primary postpartum haemorrhage, or a combination of these complications				
Yes	663 (45·8%)	727 (50·3%)	RR 0·91 (0·84 to 0·98)	0·013
Missing	3 (0·2%)	6 (0·4%)	..	..
Adverse events				
Any maternal adverse events up to the point of discharge from hospital following delivery				
Yes	88 (6·1%)	108 (7·5%)	RR 0·81 (0·62 to 1·06)	0·13
Missing	1 (0·1%)	8 (0·6%)	..	..

Data are n (%) unless otherwise stated. RR=risk ratio. RRR=relative risk ratio. NA=not applicable. *Unless otherwise stated, for continuous outcomes: linear regression adjusted for site, estimated fetal weight percentile (≤95th or >95th), and maternal age (≤35 years or >35 years), with standard care as the reference group. For categorical outcomes: generalised linear model, with family binomial and link log adjusted for site, estimated fetal weight percentile (≤95th or >95th), and maternal age (≤35 years or >35 years), with standard care as the reference group. Statistical analysis based on complete data. †Vaginal deliveries only. ‡The end of the third stage of labour was used as the delivery timepoint. §Multinomial logistic regression, adjusted for site, maternal age, and estimated fetal weight percentile. ¶Some participants reported multiple degrees of tears.

Table 3: **Hospital stay, mode of delivery, and maternal outcomes**

One neonatal death occurred in the standard care group from perinatal asphyxia after shoulder dystocia, and one neonatal death occurred in the induction group following sepsis and congenital pneumonia. The induction group had two cases of hypoxic ischaemic encephalopathy, neither of which were associated with shoulder dystocia; both babies received therapeutic hypothermia and had no concerns reported at 6-month follow-up. No humeral or clavicular fractures occurred in either group. Four (0·3%) cases of brachial plexus injury occurred among the 1447 deliveries in the induction group; three were transient and recovered by 8 weeks post-delivery (two of which were associated with shoulder dystocia, and one of which was a serious injury that required nerve surgery but was not associated with shoulder dystocia). Two (0·1%) cases of brachial plexus injury occurred among 1446 deliveries in the standard care group; one was transient and associated with shoulder dystocia and the other was not associated with shoulder dystocia, was treated with physiotherapy, and recovered by 6 months post-delivery. The difference in incidence of brachial plexus injuries was not statistically

	Induction (n=1447)	Standard care (n=1446)	Adjusted estimate (95% CI)	p value*
Neonatal outcomes				
Stillbirth				
Yes	0	0	NA	NA
Missing	1 (0·1%)	6 (0·4%)	..	..
Neonatal death				
Yes	1 (0·1%)	1 (0·1%)	OR 1·00 (0·10 to 9·59)	>0·99†
Missing	1 (0·1%)	6 (0·4%)	..	..
Apgar score at 5 min				
Score 7–10 (good)	1417 (97·9%)	1421 (98·3%)	RR 1·62 (0·87 to 3·01)	0·13
Score 0–6 (poor)	26 (1·8%)	16 (1·1%)	..	..
Missing	4 (0·3%)	9 (0·6%)	..	..
Humeral fracture				
Yes	0	0	NA	NA
Missing	1 (0·1%)	6 (0·4%)	..	..
Clavicular fracture				
Yes	0	0	NA	NA
Missing	1 (0·1%)	6 (0·4%)	..	..
Brachial plexus palsy				
Yes‡	4 (0·3%)	2 (0·1%)	RR 1·95 (0·36 to 10·65)	0·44
Missing	1 (0·1%)	6 (0·4%)	..	..
Admission to neonatal unit or additional care received§				
Yes	155 (10·7%)	139 (9·6%)	RR 1·11 (0·90 to 1·38)	0·34
Missing	1 (0·1%)	6 (0·4%)	..	..
Duration of stay at neonatal unit, days¶				
Mean (SD)	3·2 (4·0; n=155)	2·9 (2·3; n=139)	0·37 (–0·39 to 1·12)	0·34
Missing	0/155	0/139	..	..
Hypoxic ischaemic encephalopathy				
Yes	2 (0·1%)	0	OR 4·96 (0·24 to 103·17)	0·30†
Missing	1 (0·1%)	7 (0·5%)	..	..
Use of phototherapy				
Yes	44 (3·0%)	28 (1·9%)	RR 1·57 (0·98 to 2·50)	0·061
Missing	1 (0·1%)	7 (0·5%)	..	..
Supplemental oxygen				
Yes	46 (3·2%)	51 (3·5%)	RR 0·90 (0·61 to 1·33)	0·60
Missing	1 (0·1%)	6 (0·4%)	..	..
Mechanical ventilation				
Yes	8 (0·6%)	3 (0·2%)	RR 2·65 (0·70 to 10·05)	0·15
Missing	1 (0·1%)	6 (0·4%)	..	..
Non-invasive respiratory support				
Yes	30 (2·1%)	30 (2·1%)	RR 1·00 (0·60 to 1·64)	0·99
Missing	1 (0·1%)	6 (0·4%)	..	..
Extracorporeal membrane oxygenation				
Yes	0	0	NA	NA
Missing	1 (0·1%)	6 (0·4%)	..	..
Nitric oxide therapy				
Yes	2 (0·1%)	0	OR 4·95 (0·24 to 103·07)	0·30†
Missing	1 (0·1%)	6 (0·4%)	..	..
Hypoglycaemia				
Yes	50 (3·5%)	43 (3·0%)	RR 1·16 (0·77 to 1·73)	0·48
Missing	1 (0·1%)	8 (0·6%)	..	..

(Table 4 continues on next page)

significant (RR 1·95 [95% CI 0·36–10·65]). Neither case of permanent brachial plexus injury was associated with shoulder dystocia. The proportion of babies with the composite outcome of intrapartum birth injury (a fracture, brachial plexus injury, or both) did not significantly differ between the two study groups. Our composite outcome of prematurity-associated problems (one or both of use of phototherapy or respiratory support) occurred in 90 (6·2%) of 1447 deliveries in the induction group and 77 (5·3%) of 1446 deliveries in the standard care group (RR 1·16 [0·87–1·56]; table 4). Linear regression model assumptions and fit were evaluated for the continuous outcomes (appendix p 29).

A similar number of adverse and serious adverse events were reported in both groups (table 3, appendix pp 14–21). Of the mothers, 52 (3·6%) of 1447 in the induction group and 78 (5·4%) of 1446 in the standard care group reported at least one serious adverse event (appendix pp 14–16). Of the babies, 141 (9·7%) of 1447 in the induction group and 122 (8·4%) of 1446 in the standard care group had at least one serious adverse event reported (appendix pp 18–20). Comparison of maternal and neonatal outcomes at 2 months and 6 months showed no differences (appendix pp 22–28).

The subgroup analysis for maternal BMI (appendix p 13) showed an interaction effect ($p_{interaction}$=0·041), with the induction group having a statistically significant reduction in the incidence of shoulder dystocia compared with the standard care group in the group with BMI of less than 25 kg/m² only. In both BMI groups, participants in the induction group delivered babies with lower birthweights than the standard care group. However, the mean difference in birthweights between the induction group and standard care group was higher for participants with a BMI of less than 25 kg/m² (194 g [SE 21·0]) than in those with a BMI of 25 kg/m² or greater (145 g [17·8]). These subgroup results need to be interpreted with caution because of factors such as caesarean section rates and accuracy of ultrasound potentially being different in the two BMI groups. The interaction effect in the subgroup analysis for EFW (≤95th percentile *vs* >95th percentile) was not statistically significant ($p_{interaction}$=0·85).

We recruited 1666 women into the parallel cohort study, detailed results of which are in the appendix (p 31). Of these, 274 (16·4%) requested a planned caesarean section and 1392 (83·6%) did not request a planned caesarean section. Participants in the cohort study were on average older than those recruited to the trial, with a mean age of 31 years (SD 5·1). Results were generally similar between participants in the randomised controlled trial and those not requesting caesarean section in the cohort study; however, participants who requested a caesarean section had babies with a higher mean birthweight (4041·5 g [SD 409·3]) and a greater proportion had a baby that was LGA (ie, >90th percentile; 179 [65·3%] of 274) than participants recruited into the trial (1186 [41·0%] of 2893; appendix p 32).

Discussion

Analysis according to random assignment, including the 25% of pregnancies in the standard care group that were also delivered early, showed no statistically significant difference in incidence of shoulder dystocia between the two study groups, whereas exclusion of the early deliveries resulted in a statistically significant reduction in shoulder dystocia in the induction group compared with the standard care group. No statistically significant differences were found in secondary neonatal outcomes, although participants in the induction group had fewer caesarean sections and postpartum haemorrhages but longer hospital stays before delivery (table 3). Contrary to an evidence review by NICE[27] on induction of labour for suspected fetal macrosomia, no increase was found in third-degree or fourth-degree perineal tears in the induction group of our study.

The mean gestation time and birthweight difference between trial groups (6·0 days and 163·6 g) was substantially smaller than our pretrial aim (10·5 days and 300 g), mostly because of the large number of earlier-than-expected deliveries in the standard care group, which probably contributed to the incidence of shoulder dystocia in the standard care group (3·1%) being lower than our pretrial assumption (4%). According to the preplanned, per-protocol analysis, excluding the participants in the standard care group who had delivered or been induced by 38^{+4} weeks' gestation resulted in a larger between-group difference (8·1 days and 213·3 g) and a significantly higher incidence of shoulder dystocia in the standard care group (3·7%) than in the induction group (2·3%; table 2). Therefore, our per-protocol analysis supports the idea that a woman with a suspected LGA baby who opts for induction between 38^{+0} and 38^{+4} weeks' gestation can expect to have a lower risk of shoulder dystocia at delivery than if she waits for labour to start spontaneously.

These findings suggest a relationship between the difference in size for gestational age between study groups and the degree of reduction in shoulder dystocia risk. Boulvain and colleagues[10] reported a larger reduction in shoulder dystocia rate than was found in our trial (RR 0·32 [95% CI 0·12–0·85]). In their study, most randomisations started before 38 weeks' gestation, and participants were induced within 3 days of random assignment, which resulted in greater between-group differences in gestational age (10·5 days) and birthweight (287 g) than in our study. Our study started inductions only from 38 weeks' gestation to reduce risk of childhood sequelae following early-term delivery.[11,28] Earlier deliveries in Boulvain and colleagues' trial might have been the reason for their between-group difference in neonates requiring phototherapy,[9] whereas in our study, the difference was not significant (table 4).

Babies in Boulvain and colleagues' trial were heavier at birth than in our trial, with an average birthweight of 4118 g in their control group versus 3899 g in our standard care group, excluding early deliveries (table 2).[10] Their inclusion criterion was EFW of higher than the 95th percentile according to Hadlock[29] (4189 g at 39 weeks' gestation), which is more than 200 g higher than the average 95th-percentile 39-week weight in the UK (3951 g)[30] and the equivalent 91st to 97th percentile average in France (3970 g),[31] where a large proportion of their participants originated. Although shoulder dystocia is strongly associated with fetal size, 48% of cases occur with infants who weigh less than 4000 g.[2,32] Our study casts a wider net, with customised GROW percentiles identifying 9·6% of pregnancies as LGA (>90th percentile) in the UK population,[30] whereas using the Hadlock[29] 95th percentile as the cutoff would designate only 2·6% as LGA. Customisation also takes maternal characteristics such as height, weight, and parity into account, which helps to identify babies that exceed their typical weight range, personalised for each pregnancy. Customised charts therefore detect many additional LGA fetuses at increased risk of adverse outcome that are not detected by various one-size-fits-all, population-based percentile (LGA) or weight (macrosomia) standards.[18–21] Customised fetal weight percentiles also perform better, according to diagnostic ORs, at predicting LGA (>90th percentile) birthweight, compared with the Hadlock (>90th percentile) fetal weight standard predicting macrosomia (>4000 g).[33]

	Induction (n=1447)	Standard care (n=1446)	Adjusted estimate (95% CI)	p value*
(Continued from previous page)				
Neonatal readmissions				
Hospital readmission within 30 days of postnatal inpatient discharge				
Yes	190 (13·1%)	160 (11·1%)	RR 1·18 (0·97 to 1·44)	0·092
Unknown‖	24 (1·7%)	20 (1·4%)	..	..
Missing	3 (0·2%)	9 (0·6%)	..	..
Composite outcomes				
Intrapartum birth injury—fractures, brachial plexus injury, or both injuries				
Yes	4 (0·3%)	2 (0·1%)	RR 1·95 (0·36 to 10·65)	0·44
Missing	1 (0·1%)	6 (0·4%)	..	..
Prematurity associated problems—use of phototherapy, respiratory support, or both				
Yes	90 (6·2%)	77 (5·3%)	RR 1·16 (0·87 to 1·56)	0·32
Missing	1 (0·1%)	7 (0·5%)	..	..

Data are n (%) unless otherwise stated. OR=odds ratio. RR=risk ratio. NA=not applicable. *Unless otherwise stated, for continuous outcomes: linear regression adjusted for site, estimated fetal weight percentile (≤95th or >95th), and maternal age (≤35 years or >35 years), with standard care as reference group. For categorical outcomes: generalised linear model, with family binomial and link log adjusted for site, estimated fetal weight percentile (≤95th or >95th), and maternal age (≤35 years or >35 years), with standard care as reference group. Statistical analysis based on complete data. †Penalised logistic regression (Firth algorithm) used due to small frequencies. ‡Of these infants, three in the induction group and one in the standard care group had transient brachial plexus palsy, and one in the induction group and one in the standard care group required treatment for more than 8 weeks, neither of which were associated with shoulder dystocia. §Counted if admitted to intensive care, high-dependency care, special care, or transitional care. ¶Only calculated if they received additional care. Both same and different hospital transfers were included. If babies were admitted into multiple hospitals, the durations of both stays have been combined for the admissions that were either intensive care, high-dependency care, special care, or transitional care. ||Sites specifically returned forms saying unknown, whereas missing refers to cases where no information was given.

Table 4: **Neonatal outcomes**

Because of such differences between standards, and the fact that the LGA-suspected pregnancies were selected by the clinically in-use GROW standard only, we were unable to do a preplanned comparison that would fairly assess performance by other standards.

Our trial was not powered to look at short-term or long-term adverse effects on the infants. Sadly, one fetal death occurred from shoulder dystocia in the standard care group and one fetal death occurred from sepsis after a long labour in the induction group. Although two cases of hypoxic ischaemic encephalopathy occurred after induction, no cases of fracture, hypoxic ischaemic encephalopathy, or permanent brachial plexus injury occurred after shoulder dystocia. Improved multidisciplinary training on UK labour wards in the decade or so before commencement of the trial might have reduced risks for such complications.[5]

A strength of the trial was the ambitious recruitment target; although this target was not met, the study was still able to recruit a large number of participants, partly due to the participation of many trial sites that already had a fetal growth surveillance programme in place with referral pathways for ultrasound assessment, albeit with a focus on detecting small babies. The study was able to recruit pregnant women with a fetus suspected to be LGA for random assignment despite, or because of, providing detailed information about the risks and benefits of the available options, including induction of labour at 38 weeks' gestation, standard care, and elective caesarean section. The protocol allowed prospective participants time to reflect on the information, and opportunity to discuss any concerns before giving consent to join the trial.

The COVID-19 pandemic affected the recruitment rate, which reached its highest point in the second year of the study but then reduced to zero during the pandemic shutdown in April and May, 2020 (appendix p 40). Subsequently, recruitment recovered only partly, most likely due to changes in the clinical and research priorities of many hospitals. The early termination of the trial on advice of the data monitoring committee was, however, not due to the reduced post-COVID recruitment rates, but the low incidence of shoulder dystocia in the standard care group, which related to the many early deliveries in this group resulting in smaller-than-intended differences between trial groups in gestational age and birthweight.

Early interventions in the standard care group could have been due to a combination of factors, such as clinical concerns; awareness of the previously published results by Boulvain and colleagues[10] and subsequent Cochrane review,[8] which suggested benefits of early induction in reducing shoulder dystocia; and information provided as part of the consenting process, which highlighted and reminded participants and their clinicians of the potential complications associated with delivering LGA babies. Furthermore, the trial took place during various NHS investigations into avoidable adverse outcomes in maternity care, which put pressure on frontline staff to err towards intervention when in doubt.

The implication of the early closure of the trial is that 90% power was not achieved, and the resulting effect on interpretation of the results is that the intention-to-treat findings lack precision. However, statistical power was sufficient for continuous outcomes such as birthweight and categorical outcomes such as caesarean sections. The preplanned, per-protocol analysis allowed an assessment of the efficacy of the intervention comparing induction between 38^{+0} and 38^{+4} weeks' gestation with delivery after 38^{+4} weeks' gestation, which showed that the incidence of shoulder dystocia in suspected LGA fetuses can be reduced significantly without having to induce labour earlier than 38 weeks' gestation.

Despite its discontinuation, this study is still, to our knowledge, the largest randomised controlled trial of shoulder dystocia prevention, with more than twice as many pregnancies (n=2893) than in all previous trials combined in the most recent Cochrane meta-analysis (n=1190).[7] The size of the study allows a nuanced assessment of risks and benefits of various options for management of pregnancies with suspected LGA babies.

Another strength of the study was that an independent multiprofessional panel, masked to the trial allocation, reviewed all delivery notes for the presence or absence of shoulder dystocia, against a definition set out at the beginning of the trial and based on national guidelines.[2]

A limitation was the poor predictive value of scan-estimated fetal weight for LGA at birth, which became evident through the ability to use the same GROW standard antenatally and postnatally. We recruited individuals whose EFW was higher than the 90th percentile at 35^{+0} to 38^{+0} weeks' gestation, but at delivery, only 40% of participants in the standard care group and 42% in the induction group had a baby with a birthweight higher than the 90th percentile. The high false positive rate is consistent with evidence from other studies,[33,34] but could also be an overestimate if the largest babies were being delivered earliest after their scan. However, risk of shoulder dystocia is also directly related to high EFW at scan, regardless of birthweight.[35] Preliminary evidence suggests that assessment of fetal growth velocity can make an important contribution to antenatal identification of shoulder dystocia risk in babies who are not LGA.[36] More work is required to understand the apparent systematic overestimation of LGA fetal weight late in the third trimester.

A welcome limitation (because the numbers are too small to analyse for differences) was that severe neonatal outcomes were uncommon in either group of the study, such as hypoxic ischaemic encephalopathy, brachial plexus injury, or death, which could be related to an overall positive attitude towards patient safety-associated guidelines in the units that volunteered for the trial, as

well as training in prevention and management of shoulder dystocia.[5]

For clinical practice, the findings provide support for the concept that early delivery reduces the risk of shoulder dystocia for a baby suspected to be LGA. Clinicians can be reassured that, compared with expectant management, a decision for earlier delivery is a viable option. The study also shows that earlier delivery does not need to be before 38 weeks' gestation to obtain this benefit.

The findings of the study, as well as its limitations, provide information that can be communicated to pregnant women with a suspected LGA fetus, to assist them in making choices about mode and timing of their delivery. First, they should be made aware that ultrasound-assessed fetal weight is an estimate only, with substantial margins of error. Second, the potential short-term and long-term risks and benefits should be discussed with regard to the different delivery pathways. Third, our study supports previous reports that, compared with delivery at 39 weeks' gestation or later, earlier delivery can reduce the risk of shoulder dystocia.[8,10] However, this benefit can be reached without needing to induce before 38 weeks' gestation, and without affecting neonatal outcome including need for phototherapy. We also showed that induction of labour, although associated with an extra half day in hospital pre-delivery, can lead to a reduction in operative deliveries. These findings provide essential information for maternal choices regarding the timing and mode of delivery, including expectant care, induction of labour, or elective caesarean section.

Contributors

JG and SQ led the application for funding, and led the project together with MU with input from DB, SD, JF, AG, RL, SP, and A-MS. Development of the trial protocol and its oversight throughout the trial was provided by JG, SQ, DB, SD, JF, AG, RL, SP, and A-MS with input from EB, KB, GB, HE, JF, LJE, HM, SN, and SW. Warwick Clinical Trials Unit coordinated the trial with supervision from JG, SQ, MU, and DB. KB, GB, and RL did the statistical analysis, with KB and GB directly accessing and verifying the underlying data reported in the manuscript. JG and SQ prepared the initial draft manuscript. All authors had full access to the data in the study and reviewed, edited, and approved the manuscript's submission for publication.

Declaration of interests

JG, EB, and HE work for the Perinatal Institute, a not-for-profit social enterprise which has developed the GROW customised growth chart and percentile calculator used in this study. DB has received grant funding from the UK National Institute for Health and Care Research (NIHR) and the UK Medical Research Council (MRC). She is the Editor in Chief of *Midwifery* journal and receives an honorarium for this role. She is also Chair of Trustees (unpaid) for the Mothers with Anal Sphincter Injuries in Childbirth Foundation. A-MS has received grants from the NIHR as chief investigator or co-investigator on multiple research projects. She is a member of the UK National Screening Committee and a member of the Resuscitation Council UK ReSPECT wider stakeholder group. MU is chief investigator or co-investigator on multiple previous and current research grants from the NIHR, and is a co-investigator on grants funded by the Australian National Health and Medical Research Council and Norwegian Medical Research Council. He was an NIHR Senior Investigator until March 2021. He is a director and shareholder of Clinvivo, which provides electronic data collection for health services research. He receives some salary support from University Hospitals Coventry and Warwickshire. He is a co-investigator on two current and one completed NIHR-funded studies that have, or have had, additional support from Stryker. He has accepted travel expenses from professional bodies for presenting at academic meetings. SQ is Chair of the Trial Steering Committee for the Mifepristone Outpatient Labour Induction study. HM is a member of the NIHR Health Technology Assessment General Funding Commissioning Committee. SP receives support as a UK NIHR Senior Investigator (NF-SI-0616-10103) and from the NIHR Applied Research Collaboration Oxford and Thames Valley. LJE, KB, GB, SD, JF, AG, RL, SN, and SW declare no competing interests.

Data sharing

The trial protocol, statistical analysis plan, and participant documents are available online (www.warwick.ac.uk/bigbaby). Subject to approval by the chief investigators, and in the presence of a data sharing agreement, anonymised trial data and data dictionary can be made available for future ethically approved research. All requests should be directed to the Warwick Clinical Trials Unit Data Sharing Committee (wctudataaccess@warwick.ac.uk) in the first instance.

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