Quality of care, risk management, and technology in obstetrics to reduce hospital-based maternal mortality in Senegal and Mali (QUARITE): a cluster-randomised trial

*Alexandre Dumont, Pierre Fournier, Michal Abrahamowicz, Mamadou Traoré, Slim Haddad, William D Fraser, for the QUARITE research group**

Summary

Background Maternal mortality is higher in west Africa than in most industrialised countries, so the development and validation of effective interventions is essential. We did a trial to assess the effect of a multifaceted intervention to promote maternity death reviews and onsite training in emergency obstetric care in referral hospitals with high maternal mortality rates in Senegal and Mali.

Methods We did a pragmatic cluster-randomised controlled trial, with hospitals as the units of randomisation and patients as the unit of analysis. 46 public first-level and second-level referral hospitals with more than 800 deliveries a year were enrolled, stratified by country and hospital type, and randomly assigned to either the intervention group (n=23) or the control group with no external intervention (n=23). All women who delivered in each of the participating facilities during the baseline and post-intervention periods were included. The intervention, implemented over a period of 2 years at the hospital level, consisted of an initial interactive workshop and quarterly educational clinically-oriented and evidence-based outreach visits focused on maternal death reviews and best practices implementation. The primary outcome was reduction of risk of hospital-based mortality. Analysis was by intention-to-treat and relied on the generalised estimating equations extension of the logistic regression model to account for clustering of women within hospitals. This study is registered with ClinicalTrials.gov, number ISRCTN46950658.

Findings 191 167 patients who delivered in the participating hospitals were analysed (95 931 in the intervention groups and 95 236 in the control groups). Overall, mortality reduction in intervention hospitals was significantly higher than in control hospitals (odds ratio [OR] 0·85, 95% CI 0·73–0·98, p=0·0299), but this effect was limited to capital and district hospitals, which mainly acted as first-level referral hospitals in this trial. There was no effect in second-level referral (regional) hospitals outside the capitals (OR 1·02, 95% CI 0·79–1·31, p=0·89). No hospitals were lost to follow-up. Concrete actions were implemented comprehensively to improve quality of care in intervention hospitals.

Interpretation Regular visits by a trained external facilitator and onsite training can provide health-care professionals with the knowledge and confidence to make quality improvement suggestions during audit sessions. Maternal death reviews, combined with best practices implementation, are effective in reducing hospital-based mortality in first-level referral hospitals. Further studies are needed to determine whether the benefits of the intervention are generalisable to second-level referral hospitals.

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*Members listed at end of paper

Research Institute for Development, Université Paris Descartes, Sorbonne Paris Cité, UMR 216, Paris, France (A Dumont MD); **Research Centre of CHU Sainte-Justine, University of Montreal, Canada** (A Dumont, Prof W D Fraser MD); **Research Centre of CHUM (CRCHUM), University of Montreal, Montreal, Canada** (Prof P Fournier MD, Prof S Haddad MD); **Department of Epidemiology and Biostatistics, McGill University, Montreal, Canada** (Prof M Abrahamowicz PhD); **URFOSAME, Referral health center of the Commune V, Bamako, Mali** (Prof M Traoré MD); **and Department of Obstetrics and Gynaecology, University of Montreal, Montreal, Canada** (Prof W D Fraser)

Correspondence to:
Dr Alexandre Dumont, UMR 216, IRD-Université Paris Descartes, 4 Avenue de l'Observatoire, 75006, Paris, France.
alexandre.dumont@ird.fr

Introduction

Maternal mortality remains high in sub-Saharan Africa,[1] particularly in health facilities that provide emergency obstetric care. In many places across west Africa, more than 1% of women die giving birth in hospitals.[2] Significant inroads in reducing maternal mortality cannot be made without substantially increasing access to emergency obstetric care services.[3] However, service availability and quality of care in these referral hospitals are varied. Updating the skills of many professionals who do not currently have the competencies required to provide emergency obstetric care is urgently needed.[4]

An overview of interventions aimed at improving the performance of health professionals in low-income countries suggests that: simple dissemination of written guidelines is often ineffective; educational outreach visits and audit with feedback are generally effective; and multifaceted interventions might be more effective than single interventions.[5] Facility-based maternal death reviews seem particularly suitable to audits that aim to improve emergency obstetric care in referral hospitals in low-income countries.[6,7] Although the results of some observational studies are promising,[8–12] the current literature provides no rigorous evidence regarding the effectiveness of maternal death reviews in improving maternal outcomes, either alone or in combination with other interventions, nor concerning their nationwide implementation.

The primary objective of the QUARITE (quality of care, risk management, and technology in obstetrics) trial was to assess whether a multifaceted intervention to promote maternal death reviews and training for emergency obstetric care in referral hospitals would reduce hospital-

based mortality. The secondary objectives were to improve perinatal health, resource availability, and medical practices.

Methods

Setting and participants

For the **protocol** see http://www.thelancet.com/protocol-reviews/08PRT-6935

The trial was undertaken in Senegal and Mali from Sept 1, 2007, to Oct 30, 2011. The public health system, which is almost the only provider of modern health-care services in both countries, is based on primary health-care facilities or community health centres, district hospitals, regional hospitals, and national or teaching hospitals. Whereas these hospitals offer comprehensive emergency obstetric care, community health centres provide only basic obstetric services, including assisted deliveries. When an emergency complication arises in the community health centre, the patient is referred to a district or regional hospital. Mild complications are managed at the first level of care—the district hospital. Patients needing more specialised health-care services are referred to second-level care—regional or national hospitals. In the capital cities of both countries (Dakar in Senegal and Bamako in Mali), both first and second referral hospitals are available. Disparities are apparent in the resource allocation and geographical accessibility between hospitals in Dakar and Bamako and hospitals outside the capitals.[13] The trial consisted of a 1-year pre-intervention or baseline period (year 1), a 2-year intervention period (years 2 and 3), and a 1-year post-intervention period (year 4), when the primary outcome was assessed. First-level and second-level public referral hospitals with more than 800 deliveries a year that had a functional operating room and had not done maternal death reviews previously were eligible to participate. Centres were included on the basis of formal, informed consent on the part of the hospital director and the person in charge of maternity services. All women who had delivered in each of the participating facilities during the study period were included. Women who delivered at home or in another centre with postnatal transfer were excluded. The study setting and methods were published in detail at the trial's inception.[13] This trial has been approved by the ethics committee of Sainte-Justine Hospital in Montreal, Canada, which manages the operating funds, and by the national ethics committees in Senegal and in Mali.

Study design

We used a stratified cluster-randomised parallel-group trial design. The hospital was the unit of randomisation to avoid contamination between practitioners in the same service, since the intervention directly targeted teams of professionals. Hospitals were stratified by country and hospital type (hospitals in the capital, regional hospitals, and district hospitals outside the capital). To ensure balance in size (number of deliveries per year) between hospitals assigned to the two groups, within each stratum blocked randomisation was used, with each block including two hospitals of similar size. Investigators were informed of the allocation status of the individual hospitals only after the collection of baseline data was completed and immediately before the first workshop, as per protocol.

After a 1-year pre-intervention data collection phase, each hospital was randomly assigned, in August 2008, to either an intervention group, in which the intervention was implemented, or a control group. All participating hospitals were randomised simultaneously, after their list was provided, which eliminated any risk of allocation bias.

The formula for calculating the required number of patients is that used for a cluster-randomised controlled trial design.[13] The calculation was based on an overall maternal mortality rate of 1·5% in the pre-intervention phase and an expected reduction of 30% in maternal mortality in the hospitals of the intervention group, compared with the control group. To account for clustering of the outcomes within hospitals, we used the intraclass correlation coefficient (ICC) estimated in the pilot study in Senegal.[14] The calculation showed that a total of 38 205 patients and 46 hospitals allowed us to achieve a power of 82% to detect a 30% reduction in hospital-based maternal mortality between groups (OR 0·70) with two-sided significance test at α=0·05 and with ICC=0·001 (ACluster-design 2005, version 2.0, World Health Organization).

Intervention

The multifaceted intervention in the experimental group was implemented at the hospital level and targeted health-care professionals. The sequence of activities during the 2 years was directed toward developing local leadership and empowering obstetric teams. No financial incentive was provided. First, one doctor and one midwife who were responsible for maternity services from each hospital in the intervention arm took part in a 6-day training workshop provided by certified instructors in September, 2008, in Senegal and in October, 2008, in Mali. Using the ALARM (Advances in Labour and Risk Management) international course,[15] the session consisted of 3 days of training in best practices in emergency obstetric care, 1 day of training in maternal death reviews, 1 day of awareness training related to economic, sociocultural, and ethical barriers (including sexual and reproductive rights), and 1 day of training in adult education methods. At the end of the session, a normative evaluation was done. These trainees then attended two recertification sessions (once a year) to verify their knowledge, update them on the clinical content and process of maternal death audits, discuss their roles, share their experiences, and confirm their capacity to provide leadership in their clinical settings. Just after the initial training, a multidisciplinary audit committee including physicians, midwives, nurses, and

administrators was created in each participating site and trained in the process of undertaking maternal death reviews. The audit cycle and onsite training were then launched in each intervention site with the support of external facilitators (certified instructors) during their quarterly educational outreach visits, in accordance with the approach proposed by WHO.[7] The topics were selected by the audit committee depending on the principal causes of maternal mortality in a given hospital, as identified during the reviews. If needed, local trainers who took part in the initial training workshop developed new clinical guidelines or updated existing guidelines according to best practices for emergency obstetric care.

The hospitals randomised to the control group did not receive any intervention from the research team. Administrators of these hospitals were informed that the 6-day training workshop would be provided at the end of the trial.

Outcomes and blinding

The primary outcome was hospital-based maternal death, measured as the vital status of the mother at hospital discharge. A system of data collection, independent of the intervention process, was set up in all participating hospitals. This system was based on the WHO global survey on maternal and perinatal health.[16] All deliveries that took place in participating centres were registered by local data collectors (appropriately trained nurses or midwives). They completed a standard form for each eligible patient that included information on maternal characteristics, prenatal care, labour and delivery, diagnosed complications, and vital status of both mother and child at hospital discharge. This information was extracted from the hospital registers and from available medical records whose quality and archiving procedures were regularly monitored by the country-level study coordinators. Special attention was paid to ascertain all maternal deaths.[13] These data were obtained on an ongoing basis throughout the study and transferred to the national coordinating centre for double data entry using Epi Info 2000 software, version 3.5. The electronic records containing the clinical data were cleaned on a quarterly basis, then transmitted to the trial's main coordinating centre in Montreal for quality control and stored in a secure location.[13] An independent data security and monitoring board did two planned blinded interim analyses at the end of the first and second years of intervention[13] and, on the basis of their results, recommended continuation of the trial.

The data collection and the implementation of the intervention were undertaken by different and independent organisations in each country. The organisations were not blinded with respect to randomisation but they were not involved in the assessment of the outcome. Until the end of the study, access to the clinical database was restricted to the data manager in Montreal, Canada.

We also assessed the effects of the intervention on three types of secondary outcomes: resource availability in each hospital, medical practice for emergency obstetric care, and perinatal mortality. Availability of resources required to provide high quality emergency obstetric care, as proposed by WHO in the African context, was quantified by the hospital complexity index.[16] We assessed the separate effects of the intervention on the total complexity index score and on each of its eight subscores, corresponding to the availability of specific resources. For each hospital, the index was calculated separately for the baseline and year 4, on the basis of a systematic, standardised inventory of available resources. Perinatal deaths were assessed for all singleton pregnancies and were defined as either stillbirths, early neonatal deaths that occurred within the first 24 h after the birth, or those that occurred later before hospital discharge. Medical care was assessed through the following essential obstetric interventions, considered effective in reducing maternal and perinatal mortality: assisted delivery (forceps and vacuum extraction), caesarean section, transfusion and hysterectomy, or transfer to another, more specialised health facility.

We undertook a survey in participating hospitals in both control and intervention groups during the post-intervention period regarding maternal death reviews and continuous education practices. We collected detailed information on specific activities implemented during the intervention period in each participating hospital using in-depth interviews with health services managers.

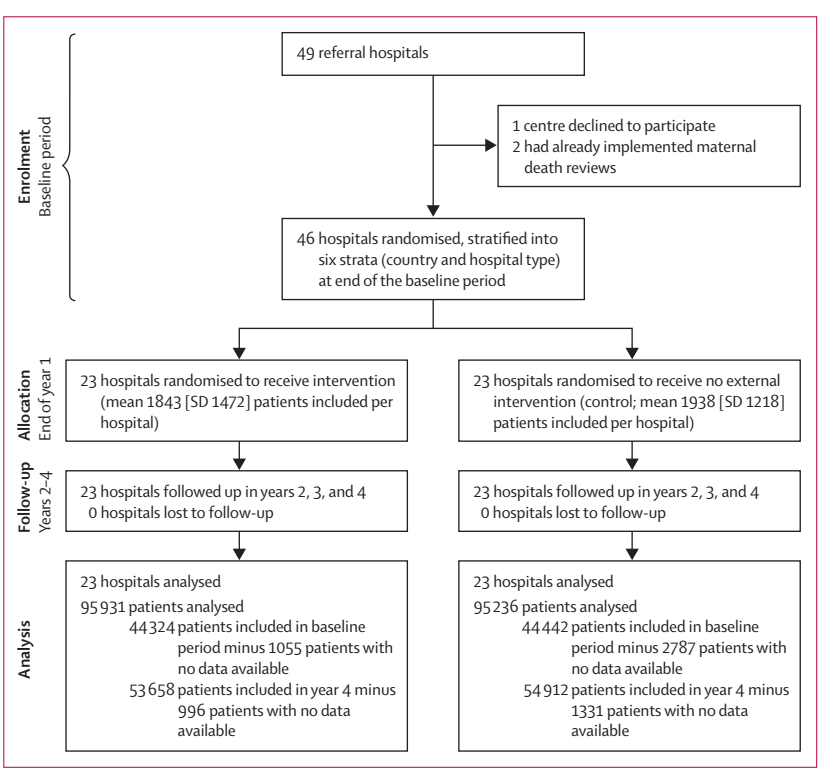

Figure 1: **Study flow diagram**

Analysis

The intervention effect on the primary outcome was estimated as the difference between the allocation groups in the change of individual mothers' risk of hospital-based mortality from the baseline (year 1) to the post-intervention (year 4) periods. Primary intention-to-treat analyses used hospital-based death of individual mothers as the binary individual-level outcome and relied on the generalised estimating equations (GEE) extension of the logistic regression model, with exchangeable covariance structure, to account for clustering of women within hospitals.[17] Using the difference-in-differences approach,[18] additional reduction of the risk that a mother in the intervention group would die before being discharged from hospital, relative to the reduction in the control group, was estimated by the odds ratio with 95% CIs for the interaction between indicators of trial group (intervention *vs* control) and time (year 4 *vs* 1) from the GEE model. The GEE model-based two-sided Wald test of this interaction, at $\alpha=0\cdot05$, was used to test the significance of the intervention effect. The GEE model adjusted for the two stratification variables: hospital type and country, as well as for variables selected a priori as potential risk factors for hospital-based mortality, including both (a) baseline (year 1) characteristics of hospitals (availability of adult intensive care unit, blood bank, anaesthetist, and gynaecologist-obstetrician) and (b) characteristics of individual women (residence, age, parity, previous caesarean delivery, any pathology during pregnancy, prenatal visit attendance, multiple pregnancy, referral from another health facility, antepartum or post-partum haemorrhage, pre-eclampsia or eclampsia, prolonged or obstructed labour, uterine rupture, and puerperal infection or sepsis). Because of the small number of hospitals in each of the six strata, as well as important differences in both the resources present in particular hospitals and in the characteristics of the women who delivered in the different hospitals, such adjustments were important to minimise potential confounding bias. Interactions with each of the two stratification variables were tested at two-tailed $\alpha=0\cdot05$ to assess whether the intervention effect varied by hospital type or country. If the interaction was significant, the intervention effects were reported separately for each stratum of the respective variable.

All individual-level secondary binary outcomes, related to either medical practice or perinatal deaths, were analysed using the same methods as for the primary outcome. The only difference was that, for some outcomes, the multivariable GEE logistic models, which accounted for clustering of outcomes within the hospitals, did not converge. This occurred whenever a given outcome was not observed at all in any of the mothers or infants, in some among the 46 participating hospitals. For those outcomes, we reported the results of a corresponding conventional multivariable logistic regression, fitted at the preliminary stage of GEE analyses. The odds ratios (OR) estimated through the conventional logistic regression models are known to be similar to the corresponding GEE-based ORs, but the standard errors are systematically underestimated when the logistic model, which ignores clustering, is fitted to clustered data.[17] Therefore, to account for the spuriously low p values, for all outcomes analysed with logistic regression, we used a conservative cutoff of $p<0\cdot001$ as a criterion for significance.

To assess the effect of the intervention on the resource availability, quantified by the hospital complexity index score, we adapted the difference-in-differences approach, described above for the primary outcome, to the analyses of a quantitative hospital-level outcome. Specifically, for the total complexity score and for each of its eight subscores, we estimated the multivariable mixed linear model, with 46 hospitals as the units of the analysis. Exchangeable covariance structure was assumed to account for the correlation of the two complexity scores, for years 1 (baseline) and 4 (post-intervention), within the same hospital. The multivariable mixed linear model adjusted the effects of the year, the randomisation group and their interaction for the two stratification variables

	Year 1		Year 4	
	Intervention (n=23)	Control (n=23)	Intervention (n=23)	Control (n=23)
Mean number of qualified personnel per hospital (SD)	22·5 (22·9)	26·7 (27·7)	20·9 (12·9)	21·6 (12·7)
Doctors	3·0 (1·7)	3·2 (1·7)	3·3 (2·1)	4·3 (3·1)
Midwives	9·7 (8·9)	9·4 (9·4)	6·1 (6·3)	6·4 (5·7)
Nurses with midwifery training	7·2 (14·7)	10·8 (18·1)	7·9 (6·4)	8·3 (5·3)
Nurses with anaesthesiology training	2·7 (1·6)	3·3 (3·6)	2·7 (2·3)	2·6 (2·4)
Country, n (%)				
Hospitals in Mali	11 (47·8%)	11 (47·8%)	11 (47·8%)	11 (47·8%)
Hospitals in Senegal	12 (52·2%)	12 (52·2%)	12 (52·2%)	12 (52·2%)
Type of hospital, n (%)				
Hospital in the capital	6 (26·1%)	6 (26·1%)	6 (26·1%)	6 (26·1%)
Regional hospital outside the capital	7 (30·4%)	7 (30·4%)	7 (30·4%)	7 (30·4%)
District hospital outside the capital	10 (43·5%)	10 (43·5%)	10 (43·5%)	10 (43·5%)
Level of care,* n (%)				
First-level referral hospital	15 (65·2%)	16 (69·6%)	15 (65·2%)	16 (69·6%)
Second-level referral hospital	8 (34·8%)	9 (39·1%)	8 (34·8%)	9 (39·1%)
Resource availability per hospital, n (%)				
Obstetrician-gynaecologist on staff	17 (73·9%)	15 (65·2%)	17 (73·9%)	18 (78·3%)
Physician specialised in anaesthesia on staff	12 (52·2%)	10 (43·5%)	11 (47·8%)	10 (43·5%)
Staff member specialised in anaesthesia (nurse or physician) available 24 h a day	7 (30·4%)	11 (47·8%)	10 (43·5%)	8 (34·8%)
Blood bank	14 (60·9%)	13 (56·5%)	17 (73·9%)	16 (69·6%)
Adult intensive care unit	8 (34·8%)	9 (39·1%)	9 (39·1%)	8 (34·8%)

Data are mean (SD) or number of hospitals (%). *Mild complications are managed in the district hospital, which constitutes the first level of care. If the patient needs more specialised health-care services, they are referred to the regional or national hospital—the second level of care.

***Table 1:* Characteristics of hospitals by group allocation during the baseline period (year 1) and the post-intervention period (year 4)**

(country and hospital type). Statistical significance of the intervention effect was assessed by testing the year-by-group interaction, which indicated if the mean change in the complexity scores (year 4–year 1) over time differed between the two trial arms.

To include all eligible women in the intention-to-treat analyses, missing data for individual characteristics were imputed based on their distributions in the study population. In sensitivity analysis, women who died before labour were excluded because they usually sought care only after developing severe complications at home. We hypothesised that the multifaceted intervention could not have changed the outcomes for these high-risk women. All analyses were done with SAS version 9.2 statistical software. This study is registered with Current Controlled Trials, number ISRCTN46950658.

Role of the funding source

The trial was funded by the Canadian Institutes of Health Research. The sponsor of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data in the study and had final responsibility for the decision to submit for publication.

Results

The trial flow chart is presented in figure 1. Of the 49 eligible hospitals, 46 agreed to participate and were included in the trial. No hospital was lost to follow-up.

We identified 1067 qualified health-care professionals (doctors, midwives, and nurses) in the maternity units at trial entry (536 in Senegal and 531 in Mali). Staff turnover was low in participating hospitals (overall 3% per year). In both countries, obstetric care was provided by doctors, midwives, and registered nurses with midwifery training. Staff specialised in anaesthesia (nurse or physician) participated in the management of complicated cases. During the baseline period, the mean number of qualified personnel per hospital was 22·5 (SD 22·9) in intervention and 26·7 (SD 27·7) in control hospitals, and it did not change markedly during the post-intervention period (table 1). The mean number of deliveries annually was 1843 (SD 1472) and 1936 (SD 1218) for the intervention and control hospitals, respectively. In all, 197 336 patients were enrolled during the baseline and post-intervention periods (97 982 in intervention and 99 354 in control arms). Of these, clinical records were not retrieved for 6169 (3%) patients, and no information except the date of delivery was available for 2051 of 97 982 (2·1%) in the intervention arms and 4118 of 99 354 (4·1%) in the control arms. These women were excluded from the analyses. Since we identified all the female deaths that occurred in the participating hospitals using the various registries available (admissions, hospitalisations, operating rooms, and morgue),[13] we assumed none of the patients with no data available died during their hospital stay. Finally, 191 167 patients who delivered in the participating hospitals were analysed (95 931 in the intervention arms and 95 236 in the control arms).

The infrastructural capabilities of the participating hospitals were associated with the level of care. The personnel were more qualified (ie, specialist gynaecologist-obstetricians, physicians specialised in anaesthesia) and the health services were more specialised (ie, intensive care unit, blood bank, microbiology laboratories) in second-level than in first-level referral hospitals. Of the 12 hospitals in the capital cities (Bamako and Dakar), three were second-level referral teaching hospitals and nine were first-level referral hospitals. The 14 regional hospitals outside the capital cities were all

	Year 1		Year 4	
	Intervention (n=43 269)	Control (n=41 655)	Intervention (n=52 662)	Control (n=53 581)
Residence				
In the city of the hospital	36 974 (85·4%)	35 782 (85·9%)	45 560 (86·5%)	47 272 (88·2%)
Outside the city but in the same region	4008 (9·3%)	5125 (12·3%)	5957 (11·3%)	5386 (10·1%)
Outside the region	2242 (5·2%)	509 (1·2%)	1115 (2·1%)	854 (1·6%)
Age ≥35 years	4356 (10·1%)	4195 (10·1%)	5325 (10·1%)	5210 (9·7%)
Nulliparous	28 435 (65·7%)	26 939 (64·7%)	35 908 (68·2%)	36 397 (67·9%)
Previous caesarean delivery	3112 (7·2%)	2782 (6·7%)	5196 (9·9%)	4920 (9·2%)
Any pathology before pregnancy*	406 (0·9%)	324 (0·8%)	516 (1·0%)	648 (1·2%)
No prenatal visit	4221 (9·8%)	4535 (10·9%)	4780 (9·1%)	5250 (9·8%)
Any pathology during current pregnancy†	3976 (9·2%)	3401 (8·2%)	3897 (7·4%)	4834 (9·0%)
Multiple pregnancy	1768 (4·1%)	1555 (3·7%)	2139 (4·1%)	2135 (4·0%)
Referred from another health facility	11 644 (26·9%)	9384 (22·5%)	15 382 (29·2%)	13 097 (24·4%)
Forceps/vacuum extraction	921 (2·1%)	854 (2·1%)	1019 (1·9%)	1068 (2·0%)
Elective caesarean delivery	920 (2·1%)	954 (2·3%)	1711 (3·2%)	1786 (3·3%)
Emergency antepartum caesarean delivery	1627 (3·8%)	966 (2·3%)	2256 (4·3%)	1110 (2·1%)
Emergency intrapartum caesarean delivery	6483 (15·0%)	5268 (12·6%)	8963 (17·0%)	8931 (16·7%)
Any obstetric complication	10 585 (24·5%)	9533 (22·9%)	13 599 (25·9%)	14 136 (26·4%)
Antepartum or post-partum haemorrhage	2743 (6·3%)	2477 (6·0%)	3165 (6·0%)	2869 (5·4%)
Pre-eclampsia/eclampsia	1758 (4·1%)	1233 (3·0%)	2385 (4·5%)	2172 (4·0%)
Prolonged/obstructed labour	7173 (16·6%)	6649 (16·0%)	9725 (18·5%)	10 937 (20·4%)
Uterine rupture	321 (0·7%)	248 (0·6%)	348 (0·7%)	267 (0·5%)
Puerperal infection/sepsis	242 (0·6%)	428 (1·0%)	354 (0·7%)	216 (1·4%)
Blood transfusion	1220 (2·8%)	1074 (2·6%)	1644 (3·1%)	1310 (2·4%)
Hysterectomy	127 (0·3%)	114 (0·3%)	152 (0·3%)	115 (0·2%)
Transportation to another hospital	72 (0·2%)	83 (0·2%)	53 (0·1%)	35 (0·1%)

Data are number of patients (%). *Pathology diagnosed before pregnancy: HIV, chronic respiratory conditions, cardiac or renal diseases, sickle cell trait, or chronic hypertension. †Pathology diagnosed during current pregnancy: pyelonephritis or urinary infection, malaria, severe maternal anaemia (<70 g/L), gestational diabetes, pregnancy-induced hypertension, vaginal bleeding near full-term, or chorioamnionitis.

Table 2: **Characteristics of patients by group allocation during the baseline period (year 1) and the post-intervention period (year 4)**

	Intervention group			Control group			Effect of the intervention		
	Baseline rate	Rate in year 4	Rate change*	Baseline rate	Rate in year 4	Rate change*	Difference in rate change (95% CI)	Adjusted OR† (95% CI)	p
Hospitals in the capital	2·5 (52/20543)	1·4 (39/27615)	−1·1	2·8 (46/16704)	3·2 (73/23032)	0·4	−1·4 (−2·9 to −0·2)	0·86 (0·74 to 0·99)	0·0374
Regional hospitals	18·1 (241/13305)	15·5 (204/13135)	−2·6	13·3 (211/15906)	10·2 (189/18499)	−3·0	0·4 (−3·4 to 4·3)	1·02 (0·79 to 1·31)	0·8911
District hospitals	16·1 (152/9421)	9·5 (113/11912)	−6·6	8·8 (80/9045)	9·9 (119/12050)	1·0	−7·6 (−11·7 to −3·6)	0·65 (0·55 to 0·77)	<0·0001
Total	10·3 (445/43269)	6·8 (356/52662)	−3·5	8·1 (337/41655)	7·1 (381/53581)	−1·0	−2·5 (−4·2 to −0·9)	0·85 (0·73 to 0·98)	0·0299

Data are number of maternal deaths per 1000 patients (crude hospital-based maternal mortality rates) by group allocation and period. *Post-intervention period – pre-intervention rate. †Additional reduction of the risk that a mother in the intervention group would die before being discharged from hospital, relative to the reduction in the control group, adjusted by country, hospital characteristics (availability of adult intensive care unit, blood bank, anaesthetist, and gynaecologist-obstetrician), and patient characteristics (age, parity, previous caesarean delivery, any pathology during pregnancy, prenatal visit attendance, multiple pregnancy, referral from another health facility, antepartum or postpartum haemorrhage, pre-eclampsia/eclampsia, prolonged/obstructed labour, uterine rupture, puerperal infection/sepsis). Clustering was taken into account using generalised estimating equations models and interchangeable structure of the residual covariance matrix.

Table 3: **Primary outcome**

	Intervention group		Control group	
	Year 1	Year 4	Year 1	Year 4
Uterine rupture	32 (7·2%)	24 (6·7%)	25 (7·4%)	24 (6·3%)
Haemorrhage	144 (32·4%)	122 (34·3%)	111 (32·9%)	128 (33·5%)
Pre-eclampsia/eclampsia	101 (22·7%)	63 (17·7%)	78 (23·1%)	85 (22·3%)
Obstructed labour	5 (1·1%)	2 (0·6%)	4 (1·2%)	3 (0·8%)
Puerperal infection/sepsis	41 (9·2%)	21 (5·9%)	15 (4·5%)	26 (6·8%)
Other direct causes*	25 (5·6%)	29 (8·1%)	34 (10·1%)	19 (5·0%)
Other indirect causes†	95 (21·3%)	95 (26·7%)	68 (20·2%)	94 (24·6%)
Unclassified	2 (0·4%)	0	2 (0·6%)	1 (0·3%)
Total	445 (100%)	356 (100%)	337 (100%)	381 (100%)

Data are number of maternal deaths (%). *Excluding uterine rupture, antepartum or post-partum haemorrhage, pre-eclampsia, obstructed labour, and puerperal infection, the complications during surgery or anaesthesia, suspected amniotic fluid embolism, and thromboembolism (not confirmed by autopsy but diagnosis based on clinical judgment) were the most common direct causes of maternal death. †Anaemia, malaria, HIV/AIDS, and cardiovascular disease were the most common indirect causes of maternal death.

Table 4: **Causes of hospital-based maternal mortality by group allocation during the baseline (year 1) and post-intervention period (year 4)**

See Online for appendix

second-level referral hospitals, and the 20 district hospitals were all first-level referral hospitals (tables 1 and 2). Essential resources for emergency obstetric care did not change markedly during the study period, except for the number of blood banks, which increased in both allocation groups but remained somewhat more frequent in the intervention hospitals (table 1). The availability of staff specialising in anaesthesia differed between the two groups. The two groups also differed in some patient characteristics in the baseline year: the proportions of women who lived outside the area of the hospital, had an obstetric complication, or had an emergency caesarean delivery (antepartum or intrapartum) were higher in intervention than in control hospitals (table 2). These differences could partly explain why women in the intervention arm had higher pre-intervention crude hospital-based mortality (table 3). There were no missing data for hospital characteristics, whereas for patient characteristics the proportion of missing data varied from 0% for parity to a maximum of 1% for age (1910 of 191 167 patients).

Crude hospital-based maternal mortality in the baseline period was higher in regional hospitals than in capital and district hospitals in both allocation groups (table 3). During the study period, the secular trends of crude maternal mortality rates in regional hospitals were similar in the intervention and control groups. By contrast, in both capital and district hospitals, crude mortality decreased markedly in the intervention group and increased slightly in the control group (appendix p x). There was a steady decline in the intervention hospitals in the capital cities. For the district hospitals outside the capital cities, the benefit was demonstrated later (year 4) following the education programme. In multivariable GEE analyses that accounted for clustering and were adjusted for hospital and patient characteristics, the post-pre-intervention reduction of hospital-based maternal mortality in intervention hospitals was significantly greater than the reduction in control hospitals (adjusted OR 0·85; 95% CI 0·73–0·98, p=0·0299). However, there was a statistically significant interaction with hospital type (p=0.0107). Analyses that accounted for this interaction indicated that the benefits of the intervention were limited to capital hospitals (adjusted OR 0·86; 95% CI 0·74–0·99, p=0·0374) and district hospitals (0·65; 0·55–0·77, p<0·0001), with no significant effect for regional hospitals (1·02; 0·79–1·31, p=0·89). Excluding maternal deaths before labour from the analyses did not substantially change the results (0·84; 0·73–0·97, p=0·0229 for the effect pooled across hospital types). The intervention effects did not vary significantly across the two countries (OR 1·11 for the interaction with the country; 95% CI 0·83–1·48, p=0·47).

Antepartum or post-partum haemorrhage, pre-eclampsia or eclampsia, and indirect causes (anaemia, malaria, HIV/AIDS, and cardiovascular disease) were the leading causes of hospital-based maternal deaths in both groups (table 4). In the post-intervention period, we noted a marked decrease in the number of deaths related to haemorrhage, pre-eclampsia or eclampsia, and puerperal infection in the intervention group.

Tables 5 and 6 summarise the results for secondary outcomes related to resource availability, medical practice,

and perinatal mortality. In the case of a significant interaction with the country or the hospital type, separate intervention effects are reported, respectively, for Mali and Senegal or for capital, regional, and district hospitals. Firstly, the intervention resulted in a significant increase in the probability of transfusions (overall effect was marginally significant, adjusted OR 1·44, 95% CI 0·99–2·11, p=0·06), especially in regional hospitals (2·32; 1·52–3·55, p<0·0001). Furthermore, the probability of emergency antepartum caesarean deliveries increased significantly (1·33; 1·19–1·50, p<0·0001). The main indications for antepartum caesarean delivery were pre-eclampsia and eclampsia (27% of all antepartum caesareans). By contrast, the overall frequency of intrapartum caesarean deliveries decreased significantly (adjusted OR 0·87, 95% CI 0·82–0·92, p<0·0001), but the effects varied significantly by country and hospital type. Specifically, the study intervention had no effect on the probability of intrapartum caesarean deliveries in the capital hospitals, whereas for regional and district hospitals it decreased in probability in Senegal but increased in Mali (table 6). The main indications for intrapartum caesarean deliveries were prolonged or obstructed labour, suspected cephalopelvic disproportion (37%) and fetal distress (16%). The overall frequency of assisted deliveries (forceps or vacuum) increased in Senegal (adjusted OR 3·10, 95% CI 1·85–5·20, p<0·0001), with no effect in Mali (0·51, 0·16–1·59, p=0·25). Intervention had no effect on the overall resource availability, as the increases over time in the total score for the hospital complexity index were similar for the two trial arms (table 5). However, the improvements over time in the mean scores indicating the provision of clinical protocols and training were significantly greater in the intervention than in the control group (table 5). The study intervention resulted in a marginally significant decrease in neonatal mortality before 24 h (adjusted OR 0·74; 95% CI 0·61–0·90, p=0·0023 in logistic model) and no significant effects on stillbirths (1·05; 0·91–1·22, p=0·48). Finally, the effects on neonatal mortality after the first day of life varied considerably depending on hospital type, with a significant decrease in capital hospitals (0·24; 0·13–0·45, p<0·0001), no effect in district hospitals (0·81; 0·41–1·62, p=0·56), and marginally significant increase in regional hospitals (2·36, 1·36–4·09, p=0·0022 in logistic model).

Regular educational outreach visits by a certified instructor to promote maternal death reviews occurred in all intervention hospitals as required by the protocol, and no control hospital received this type of visit by external facilitators during the study period (appendix). National support from opinion leaders varied somewhat between Senegal and Mali. A professor from the Department of Obstetrics and Gynaecology of the University of Bamako (MT) took part in all visits in Mali. In Senegal, an obstetrician-gynaecologist from the National Reproductive Health Office visited only half of the intervention hospitals during the first supervision, and did not take part in the other visits. The proportions of intervention hospitals that planned regular meetings for maternal death reviews, trained staff personnel in emergency obstetric care, and updated or developed new clinical guidelines were high (95·6%, 100%, and 74%, respectively), but these activities were also implemented in some control hospitals (appendix). This was particularly true for regional hospitals in the control group, where four of the seven maternity units carried out maternal death reviews and continuous staff education in emergency obstetric care with the support of the government or international organisations and new clinical guidelines for emergency obstetric care were introduced in five of the seven control hospitals. In the hospitals in the intervention arm, the audit meetings were held every two or three months

	Intervention group			Control group			Difference between mean changes (95% CI)	p
	Baseline	Year 4	Mean change	Baseline	Year 4	Mean change		
Basic services	6·78 (0·52)	6·87 (0·34)	0·09	6·74 (0·62)	6·74 (0·92)	0	0·09 (–0·29 to 0·47)	0·6454
General medical services	13·87 (3·51)	15·09 (4·75)	1·22	13·52 (4·79)	15·35 (4·17)	1·83	–0·61 (–2·49 to 1·27)	0·5179
Laboratory tests	8·96 (1·62)	8·74 (1·32)	–0·22	8·78 (1·35)	9·22 (1·35)	0·44	–0·65 (–1·54 to 0·23)	0·1446
Anaesthesia resources	4·43 (0·73)	4·61 (1·75)	0·18	4·22 (1·57)	4·39 (1·59)	0·17	0·01 (–0·98 to 0·98)	1
Emergency obstetric care	10·26 (1·84)	10·96 (1·66)	0·70	9·30 (2·30)	10·04 (2·23)	0·74	–0·04 (–0·79 to 0·70)	0·907
Intrapartum care	8·22 (1·44)	9·30 (1·61)	1·08	8·70 (1·87)	8·91 (1·50)	0·21	0·87 (–0·26 to 1·99)	0·1265
Human resources	8·43 (3·41)	9·57 (3·40)	1·14	8·48 (3·64)	9·35 (3·69)	0·87	0·26 (–1·30 to 1·83)	0·7386
Protocol and training	5·48 (1·90)	7·30 (1·15)	1·82	7·04 (0·98)	7·00(2·37)	–0·04	1·87 (0·70 to 3·03)	0·0023*
Total (all categories)	66·43 (8·25)	72·43 (10·52)	6·00	66·78 (12·50)	71·00 (10·51)	4·22	1·78 (–1·95 to 5·51)	0·3539

Data are mean (SD, complexity index) by group allocation and period, unless otherwise stated. *The range of service available in each facility were assessed with an adapted version of the complexity index developed for the WHO Global Survey project in African countries.[16] This index consists of eight categories reflecting the: standard of building/basic services, maternal intrapartum care and human resources; availability of general medical care, anaesthesiology, emergency obstetric services; and provision of screening tests and academic resources and clinical protocols. The original hospital complexity index was used by WHO in Latin America and Asia and adapted for use in Africa. We implemented minor adaptations to reflect the context of Mali and Senegal. The maximum total score (all of the eight categories) in one hospital is 100.

Table 5: **Secondary outcomes—hospital complexity index**

	Intervention group			Control group			Adjusted OR (95% CI)	p
	Baseline	Year 4	Mean change	Baseline	Year 4	Mean change		
Blood transfusion (G)	2·82% (1220/43269)	3·12% (1644/52662)	0·30%	2·58% (1074/41655)	2·44% (1310/53581)	–0·13%	1·44 (0·99 to 2·11)	0·0597*
Hospitals in capital	1·59% (327/20543)	0·97% (267/27615)	–0·62%	1·19% (199/16704)	1·15% (266/23032)	–0·04%	0·64 (0·41 to 1·00)	0·0502*
Regional hospitals	4·75% (632/13305)	6·94% (912/13135)	2·19%	4·74% (754/15906)	4·26% (788/18499)	–0·48%	2·32 (1·52 to 3·55)	<0·0001†
District hospitals	2·77% (261/9421)	3·90% (465/11912)	1·13%	1·34% (121/9045)	2·12% (256/12050)	0·79%	0·97 (0·62 to 1·53)	0·9022
Blood transfusion in patients with haemorrhage (L)	44·48% (1220/2743)	51·94% (1644/3165)	7·47%	43·36% (1074/2477)	45·66% (1310/2869)	2·30%	1·30 (1·10 to 1·52)	0·0017*
Hysterectomy (G)	0·29% (127/43269)	0·29% (152/52662)	0%	0·27% (114/41655)	0·21% (115/53581)	–0·06%	1·39 (0·90 to 2·14)	0·1415
Hysterectomy in patients with haemorrhage (L)	4·63% (127/2743)	4·80% (152/3165)	0·17%	4·60% (114/2477)	4·01% (115/2869)	–0·59%	1·34 (0·89 to 2·01)	0·1644
Transportation to another hospital (L)	0·17% (72/43269)	0·10% (53/52662)	–0·07%	0·20% (83/41655)	0·07% (35/53581)	–0·13%	1·81 (1·06 to 3·10)	0·0307
Transportation in patients with complication (L)	0·68% (72/10585)	0·39% (53/13599)	–0·29%	0·20% (83/41655)	0·07% (35/53581)	–0·13%	1·81 (1·06 to 3·10)	0·0307
Elective caesarean delivery (G)	2·13% (920/43269)	3·25% (1711/52662)	1·12%	2·29% (954/41655)	3·33% (1786/53581)	1·04%	0·92 (0·68 to 1·26)	0·6261
Senegal	2·44% (571/23413)	3·06% (757/24742)	0·62%	3·03% (675/22274)	4·14% (970/23448)	1·11%	0·71 (0·44 to 1·14)	0·1578
Mali	1·76% (349/19856)	3·42% (954/27920)	1·66%	1·44% (279/19381)	2·71% (816/30133)	1·27%	1·22 (0·94 to 1·58)	0·1276
Emergency antepartum caesarean delivery (L)	3·76% (1627/43269)	4·28% (2256/52662)	0·52%	2·32% (966/41655)	2·07% (1110/53581)	–0·25%	1·33 (1·19 to 1·50)	<0·0001†
Emergency intrapartum caesarean delivery (L)	15·92% (6483/40722)	18·41% (8963/48695)	2·49%	13·26% (5268/39735)	17·62% (8931/50685)	4·36%	0·87 (0·82 to 0·92)	<0·0001†
Senegal								
Hospitals in capital	10·78% (745/6914)	17·60% (1280/7273)	6·82%	9·72% (649/6677)	13·21% (884/6690)	3·49%	0·87 (0·75 to 1·02)	0·0936
Regional hospitals	21·02% (2072/9855)	19·15% (1818/9493)	–1·87%	15·04% (1600/10635)	25·03% (2670/10667)	9·99%	0·45 (0·40 to 0·50)	<0·0001†
District hospitals	6·65% (306/4599)	9·40% (506/5385)	2·75%	2·17% (75/3456)	4·50% (195/4332)	2·33%	0·59 (0·43 to 0·81)	0·0011*
Mali								
Hospitals in capital	14·40% (1778/12350)	14·80% (2718/18365)	0·40%	14·06% (1278/9087)	16·87% (2535/15029)	2·81%	0·93 (0·84 to 1·04)	0·2125
Regional hospitals	23·58% (586/2485)	41·77% (1013/2425)	18·19%	13·96% (610/4370)	16·75% (1090/6507)	2·79%	2·23 (1·87 to 2·67)	<0·0001†
District hospitals	22·04% (996/4519)	28·24% (1628/5764)	6·20%	19·17% (1056/5510)	20·87% (1557/7460)	1·70%	1·25 (1·08 to 1·44)	0·0021*
Forceps/vacuum extraction (G)	2·26% (921/40722)	2·09% (1019/48695)	–0·17%	2·15% (854/39735)	2·11% (1068/50685)	–0·04%	0·95 (0·43 to 2·11)	0·904
Senegal	1·25% (279/22368)	2·24% (496/22151)	0·99%	1·76% (366/20768)	0·65% (142/21689)	–1·11%	3·10 (1·85 to 5·20)	<0·0001†
Mali	3·32% (642/19354)	1·97% (523/26544)	–0·35%	2·57% (488/18967)	3·19% (926/28996)	0·62%	0·51 (0·16 - 1·59)	0·2460
Perinatal outcomes, % (n)‡								
Stillbirth (G)	9·39% (3883/41368)	8·40% (4238/50426)	–0·99%	8·60% (3441/39992)	8·32% (4270/51324)	–0·28%	1·05 (0·91 to 1·22)	0·4828
Neonatal mortality before 24 h (L)	1·16% (434/37485)	0·97% (446/46188)	–0·19%	0·90% (332/36551)	1·07% (505/47054)	0·17%	0·74 (0·61 to 0·90)	0·0023*
Neonatal mortality after 24 h and before hospital discharge (L)	0·62% (232/37485)	0·40% (185/46188)	–0·22 %	0·27% (99/36551)	0·21% (99/47054)	–0·06%	0·88 (0·63 to 1·25)	0·4826
Hospitals in capital	0·44% (83/18894)	0·13% (33/25576)	–0·31%	0·20% (31/15499)	0·18% (38/21140)	–0·02%	0·24 (0·13 to 0·45)	<0·0001†
Regional hospitals	1·00% (108/10757)	1·00% (107/10737)	0%	0·34% (45/13229)	0·18% (28/15600)	–0·16%	2·36 (1·36 to 4·09)	0·0022*
District hospitals	0·52% (41/7834)	0·46% (45/9875)	–0·06%	0·29% (23/7823)	0·32% (33/10314)	0·03%	0·81 (0·41 to 1·62)	0·5564

Data are % (n/N; crude hospital–based rates) by group allocation and period, unless otherwise stated. For medical practice and perinatal outcomes, (G) or (L) indicates which of the two alternative types of multivariable models were used to estimate the adjusted effect of the intervention on the specific outcome: (G) the results of the generalised estimating equations (GEE) model with exchangeable structure of the residual covariance matrix to take into account the clustering, are presented whenever the GEE model converged. (L) The logistic model was fitted when the GEE model did not converge. For both types of models, the intervention effect is shown as the adjusted odds ratio (OR; 95% CI). Adjusted ORs present the additional change in the odds for a given procedure (for practice outcomes) or for the risk of perinatal death, relative to the concurrent change (from year 1 to year 4) in the control group, adjusted for country, hospital type and hospital characteristics, patient characteristics, and birthweight (for perinatal outcomes). Overall effect is reported always; the subgroup–specific effects are reported only if there is a significant interaction with the corresponding stratification variable (ie, country and/or hospital type). *p value between 0·001 and 0·003 (a marginally significant effect). †In GEE models, p<0·05 is significant, whereas p between 0·05 and 0·06 is considered a marginally significant effect. †In logistic models, because of underestimated variance, only p<0·001 is considered significant. ‡Perinatal outcomes were assessed for singletons only, excluding multiple pregnancies from the analyses. Adjusted OR=additional change in the odds ratio for a given procedure (for practice outcomes) or for the risk of perinatal death, relative to the concurrent change (from year 1 to year 4) in the control group, adjusted for country, hospital type and hospital characteristics, patient characteristics, and birthweight (for perinatal outcomes).

Table 6: **Secondary outcomes—medical practice**

depending on the number of cases to be audited. Between six and 23 people per hospital (median 13 [IQR 9–19]) attended these meetings. Actions recommended by the audit committees to prevent maternal deaths varied considerably between centres and were context-specific. However, making organisational changes to improve

service availability and monitoring patients were the actions implemented most frequently during the intervention period. We observed no unintended effect of maternal death reviews, such as litigation or threats to personnel, in any of the hospitals. Additionally, the local trainers at each site organised in their own hospitals between four and eight training sessions in best practices during the intervention period, with the support of external facilitators. Between eight and 38 people per hospital (median 15 [IQR 10–21]) attended these sessions. The most recurrent topics were pre-eclamspia and management of post-partum haemorrhage.

Discussion

Hospital-based maternal mortality was reduced by 15% in Mali and Senegal in the year after a multifaceted intervention that was implemented over a 2-year period to promote maternal death reviews and onsite training in emergency obstetric care. This effect was limited to capital and district hospitals.

This complex intervention was based on a combination of three potentially effective approaches for improving performance among health-care professionals: developing opinion leaders;[19] undertaking educational, clinically-oriented, and evidence-based outreach visits focused on emergency obstetric care;[20,21] and conducting clinical audits (maternal death reviews).[22] Indeed, the significant reduction of maternal mortality in the capital and district hospitals reflects the combined effect of all the above components of the intervention. The lack of effect on stillbirth is not surprising because the intervention did not specifically target prenatal care, which is considered effective in reducing fetal intrauterine deaths.[23] By contrast, the decrease in neonatal mortality before 24 h could be explained by the overall quality improvement in intrapartum care and changes in the mode of delivery.[23,24] On the other hand, the effect of the intervention on neonatal mortality after 24 h varied considerably depending on the type of hospital. The likely reason is that other, possibly unmeasured neonatal and institutional variables, might have affected neonatal outcomes after the first day of life. The sequence of activities during the 2-year intervention period was directed toward developing local leadership and empowering obstetric teams. Indeed, 22 of the 23 intervention hospitals planned regular meetings for maternal death reviews and all intervention hospitals provided regular onsite training on emergency obstetric care. Quarterly visits by a trained external facilitator and onsite training facilitated maternal death reviews by providing health care professionals with the knowledge and confidence to make quality improvement suggestions during the audit sessions. The intervention resulted in medical practice changes (increase in the probability of transfusions and antepartum caesareans for hypertensive complications), which are considered effective in reducing maternal mortality.[3] Indeed, the intervention was particularly effective in reducing maternal deaths from haemorrhage and pre-eclampsia or eclampsia. The decrease in intrapartum caesarean deliveries could also have contributed to the improvement of maternal outcomes, because this mode of delivery is associated with increased risks in mothers in Mali and Senegal.[24] Although local leadership and ownership, as well as the nature and focus of recommendations drawn up during maternal death reviews, varied considerably by hospital, concrete interventions were implemented comprehensively to improve the quality of care in all intervention hospitals. Organisational changes to improve 24 h service availability and patient monitoring contributed most to improving emergency obstetric care and, as a consequence, to improving maternal outcomes. We also noted no unintended effect of the reviews, such as litigation or threats to personnel, in any of the hospitals.

This effect of the multifaceted intervention was limited to capital and district hospitals, which mainly acted as first-level referral hospitals in this trial. The poor effect in second-level referral (regional) hospitals outside the capitals could be due to potential contamination bias. Indeed, during the period of this trial, international (bilateral cooperation) and governmental organisations implemented maternal death reviews and onsite training in four of the seven regional hospitals in the control group (appendix).

Comparing the effect of this intervention with other results in similar contexts is difficult because of the lack of strong evidence (panel). Most papers on the effectiveness of training in emergency obstetric care describe positive reactions, increased knowledge and skills, and improved behaviour after training, but fail to show improved maternal outcomes.[22] Observational or quasi-experimental studies in sub-Saharan countries on the effect of critical incident audits and feedback (including maternal death) suggested a decrease in hospital-based maternal mortality ranging from 27% to 80%, but failed to control for concurrent reduction in mortality, reflecting secular trends.[8–12,25]

This study is one of the largest trials on hospital-based maternal mortality in sub-Saharan Africa. The participant hospitals were representative of the existing health system in both countries, taking into account the various contexts and levels of care. The large database (around 100 000 patients per year) and the system of data control we implemented in all participating hospitals allowed us to obtain reliable information on maternal outcomes.[13] Thus, this trial provides strong evidence to promote maternal death reviews and onsite training in referral hospitals in low-resource settings.

Potential limitations of this trial should be taken into account in interpreting our results. First, data are restricted to hospital-based maternal deaths; therefore, maternal mortality in the population cannot be inferred. We have no data for trends in deaths outside of hospital,

Panel: Research in context

Systematic review

We searched PubMed for cluster randomised controlled trials, before-and-after studies, and studies that used interrupted time series analyses done in low-income or middle-income countries and published in English between Jan 1, 1970, and Dec 31, 2011, with the following MeSH and text searches: ("maternal mortality" AND "maternal death review") OR ("maternal mortality" AND "in-facility death audits" OR ("maternal mortality" AND "confidential enquieries" OR ("maternal mortality" AND "audit and feedback" OR ("maternal mortality" AND "educational outreach visits") OR "emergency obstetric care training" OR "obstetric audit". We found no randomised trials and five before-and-after and interrupted time series studies (Martey 1993,[8] Mburaku 1995,[9] Dumont 2006,[10] Kongnyuy 2008,[12] and van den Akker 2011[25]) that assessed the effects of the implementation of the audit approach on maternal mortality.

Interpretation

In low-income countries, most published papers on the effects of training on emergency obstetric care report such benefits as increased knowledge and skills, and improved behaviour after training, but fail to show improved maternal outcomes. Conversely, before-and-after studies resulting from local initiatives in sub-Saharan countries on the effect of maternal death reviews showed a marked decrease in hospital-based maternal mortality ranging from 27% to 80%, but failed to control for concurrent reduction in mortality, reflecting secular trends. The QUARITE trial showed that a multifaceted intervention based on educational outreach visits and in-facility death audits reduced hospital-based maternal mortality by 15% relative to the simultaneous changes in mortality in the control hospitals. This effect was larger in first-level referral (district) hospitals. This trial confirms that a large-scale implementation of maternal death reviews combined with onsite training is effective to reduce hospital-based maternal mortality in low-income countries.

but the reduction in hospital-based deaths, especially the incremental reduction observed in the first-level hospitals in the intervention group, is unlikely to be explained by an increase in out-of-hospital deaths. Despite our efforts to ascertain all maternal deaths, some eligible women with no data available could have died before discharge. Since the frequency of these women with no data was very low and twice as high in the control group (4·1% *vs* 2·1%), the assumption that none of these women died before discharge could not explain a significant reduction of mortality in the intervention hospitals. Second, the QUARITE trial was a pragmatic cluster-randomised controlled trial in two countries with ongoing national programmes designed to reduce maternal mortality. The most relevant programmes that could potentially have had an effect on hospital-based maternal mortality and were implemented during this research were: the free caesarean section programmes launched in 2005 in Mali and Senegal to reduce the financial barrier for caesarean deliveries (patients did not pay for surgery, supplies, or drugs); and the national maternity referral system launched in 2002 in Mali to improve the accessibility to emergency obstetric care services. The free caesarean section programme was implemented in all referral hospitals in Mali and all referral hospitals in Senegal, except in the capital (Dakar).[26,27] Strengthening the referral system involved all referral hospitals in Mali.[27] For these reasons, these co-interventions were balanced between the hospitals in the two allocation groups. This balance ensured that the incremental reduction of maternal mortality observed in the intervention group, relative to simultaneous changes in mortality in the control group, could not be attributed to concurrent implementation of the aforementioned programmes. Accordingly, such co-interventions should not affect the results regarding the effectiveness of our multifaceted intervention. The activity related to data collection may also have had an impact on quality improvement and maternal outcomes in participating hospitals. However, this data collection effect was a priori similar in the two groups (intervention *vs* control). Finally, the variation in the implementation of the key components of the multifaceted intervention between participating hospitals of the intervention group could have resulted in an underestimation of the real effectiveness of our multifaceted intervention. However, all those hospitals had educational outreach visits and regular onsite training, which are potentially effective interventions to improve the quality of care and, as a consequence, to improve maternal outcomes.[20,21] Thus, we do not believe the heterogeneity among sites of ownership and adoption of the audit approach—ie, maternal death reviews—had an important role in underestimating the real effectiveness of this programme.

Facility-based maternal death reviews combined with educational outreach visits by a trained external facilitator are effective in reducing hospital-based maternal mortality in first-level referral hospitals with high maternal mortality rates. This multifaceted intervention was particularly effective at reducing maternal death from direct obstetric causes (antepartum or postpartum haemorrhage, pre-eclampsia or eclampsia, and puerperal infection). The preventive strategies and treatments for these complications are well known.[3] Our educational programme facilitated the implementation of these best practices. Further studies are needed to determine whether the benefits of this intervention are generalisable to second-level referral hospitals.

Contributors

AD participated in developing the project and was responsible for the scientific aspects of the trial and all its components. PF participated in developing the project and was responsible for its administration. MA participated in developing the project, planned and coordinated the statistical analyses. WDF participated in developing the project and ensured the trial was carried out in accordance with best practices. MT participated in developing the project and was responsible for the intervention implementation. AD wrote the first version of the protocol and, with PF, coordinated its development and approved the final version. AD, PF, MA, MT, and WDF obtained the funding for the project. All authors provided feedback and made revisions to the manuscript.

Conflicts of interest

We declare that we have no conflicts of interest.

QUARITE trial research group

Writing Committee Alexandre Dumont, Pierre Fournier, Michal Abrahamowicz, Mamadou Traoré, Slim Haddad, William Fraser. *Steering Committee* Alexandre Dumont, Pierre Fournier,

Michal Abrahamowicz, William D Fraser, François Beaudoin, Slim Haddad. *Statistical Analyses* Michal Abrahamowicz, Rebecca Burne. *Data Management* Anna Koné, Drissa Sia, Papa Dambé. *Data Security and Monitoring Committee* José Villar (Chair), Christiane Welffens-Ekkra, Allan Donner.

Participating institutions and staff

Society of Obstetricians and Gynaecologists of Canada (Ottawa, Canada): André Lalonde, François Beaudouin, Astrid Buccio, Jean-Richard Dortonne, François Couturier, Gilles Perrault, Pierre Levesques; CRCHUM (Montreal, Canada): Pierre Fournier, Slim Haddad, Anna Kone, Drissa Sia, Caroline Tourigny; Institut de recherche pour le développement (Dakar, Senegal): Alexandre Dumont, Mamadou Yatoudème Ndiaye, Papa Dambé; Centre de Recherche du CHU Sainte-Justine (Montreal, Canada): Sylvie Cossette, Carole Gariépy, François Beaudoin, William D. Fraser; CAREF (Bamako, Mali): Mouhamadou Gueye, Mamadou Kani Konate; HYGEA (Dakar, Sénégal): Idrissa Diop, Amadou Sow. *Participating hospitals in Senegal* Centre Hospitalier Universitaire Abass Ndao: Prosper Bamboky, Keita Soma Diallo; Hôpital Principal: Pierre Dionne, Lô Asta; Centre Hospitalier Régional El Amadou Sahir Louga: Voulimata Fall, Maty Diop Guèye, Fatoumata Dedhiou; Centre Hospitalier Régional de Tambacounda: Maréme Soda Samba, Fatou Cissé Seck; Centre de Santé de Référence de Kédougou: Sène Doudou, Aïssatou Samba; Centre de Santé de Référence de Bakel: Yaya Baldé, Astou Guèye Thiombane; Centre de Santé de Référence de Goudiry: Julien Manga, Aïssata Sy Ndiaye, Absa Sene; Centre de Santé de Référence de Koungel: Mamadou Sarr, Diahaby Aminata Sylla; Centre Hospitalier Régional de Thiès: Fatou Rachel Sarr, Mbaye Khady Sarr; Centre Hospitalier Régional de Diourbel: Malick Gueye, Mbaye Binetou Diatta; Centre Hospitalier Matlaboul Fawzeïni: Mouhamadou M Seck, Niane Coumba Sarr Guéye; Centre Hospitalier Régional de Saint-Louis: Lamine Diouf, Oumou Kalsoum Fall; Centre de Santé de Référence de Richard Toll: El Hadji Lamine Dieye, Fall Ndèye Niang; Centre Hospitalier Départemental de Ourossogui: Charles Fall, Mame Laina Diattara, Astou Traoré; Centre Hospitalier Départemental de NDioum: Sidy Dieye, Ousmane Thiam, Kadiata Seck, Coumba Mbow, Aminata Dime; Centre Hospitalier Régional de Ziguinchor: Guy Boukar Faye, Badiane Aida Gaye; Centre de Santé de Référence de Sédhiou: Kalidou Konté, Fatou Ndoye; Centre Hospitalier Régional de Kolda: Aïdara Seynabou Sylla, Jacqueline F Ngom; Hôpital Général de Grand YOFF: Mariéme Fall, Aïssatou Diouf; Centre Hospitalier Régional El Hadji Ibrahima Niass: Dembo Girassy, Maye Seck Ly; Centre de Santé de Référence Nabil Choucaire: Seynabou Beye, Djipméra Fatou Ndiaye; Centre de Santé de Référence Youssou Mbargane: Dieynaba Ndao Ndiaye, Khady Fall; Centre de Santé de Référence Ndamatou Touba: Tacko Seck Leye; Hôpital de Pikine: Codou Séne, Amsatou Cisse. *Participating hospitals in Mali* Centre de Santé de Référence de Fana: Salif Sidibé, Aïssata Daba Traoré; Centre de Santé de Référence de Dioila: Broulaye Diarra, Korotoumou Doumbia; Centre de Santé de Référence de Markala: Bintou Coulibaly, Bintou Cissé; Hôpital Régional de Kayes: Mahamadou Diassana, Drissa Konaté, Dienfra Diarra; Centre de Santé de Référence de Niono: Moussa Modibo Diarra, Soundiè Fané; Hôpital Régional de Ségou: Haka Kokain, Souma Boiaré; Centre de Santé de Référence de San: Aliou Bagayogo, Diamilatou Diallo; Hôpital régional de Mopti: Famakan Kané, Pierre Coulibaly, Fatoumata Tolo, Fatoumata Dolo; Centre de Santé de Référence de Bougouni: Daouda Goïta, Bintou Sidibé; Centre de Santé de Référence de Koutiala: Moustapha Coulibaly, Sadio Tounkara, Mariam Beledogo; Hôpital régional de Sikasso: Mala Sylla, Cécile Dembélé, Rokia Touré; Centre de Santé de Référence de Kadiolo: Emilien Diarra, Ramata Fofana; Centre de Santé de Référence de Yanfolila: Aliou Coulibaly, Kadiatou Samaké; Centre de Santé de Référence Commune I de Bamako: Modibo Soumaré, Haoua Ba, Salimata Coulibaly; Centre de Santé de Référence Commune II de Bamako: Lassana Sissoko, Penda Fané, Hamsa Maïga; Centre de Santé de Référence Commune IV de Bamako: Moustapha Touré, Fanta Koné; Centre de Santé de Référence Commune V de Bamako: Oumar Soumana Traoré, Assitan Dembélé; Centre de Santé de Référence Commune VI de Bamako: Aminata Cissé, Feue Aminata Yattara, Katahit Zeneba; Centre Hospitalier Universitaire Point G de Bamako: Maïga Bouraïma,Kadiatou Traoré; Centre de Santé de Référence de Koulikoro: Hamadou Coulibaly, Oumou Konaté; Centre de Santé de Référence de Nioro du Sahel: Youssouf Coulibaly, Maïmouna Kassibo, Binta Kontao; Centre de Santé de Référence de Macina: Moussa Kanté, Diakité Diallo, Aminata Koné.

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