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RESEARCH METHODS & REPORTING

Consort 2010 statement: extension to cluster randomised trials

The Consolidated Standards of Reporting Trials (CONSORT) statement was developed to improve the reporting of randomised controlled trials. It was initially published in 1996 and focused on the reporting of parallel group randomised controlled trials. The statement was revised in 2001, with a further update in 2010. A separate CONSORT statement for the reporting of abstracts was published in 2008. In earlier papers we considered the implications of the 2001 version of the CONSORT statement for the reporting of cluster randomised trial. In this paper we provide updated and extended guidance, based on the 2010 version of the CONSORT statement and the 2008 CONSORT statement for the reporting of abstracts.

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Many journals now require that reports of trials conform to the guidelines in the Consolidated Standards of Reporting Trials (CONSORT) statement, first published in 1996,[1] revised in 2001,[2] and revised most recently in 2010.[3] The statement includes a checklist of items that should be included in the trial report. These items are evidence based whenever possible and are regularly reviewed.[4] The statement also recommends including a flow diagram to show the progression of participants from group assignment through to the final analysis. An explanation and elaboration of the rationale for the checklist items is provided in an accompanying article.[4]

The standard CONSORT statement focuses on reporting parallel group randomised controlled trials in which individual participants are randomly assigned to study groups. However, in some situations it is preferable to randomly assign groups of people (such as communities, families, or medical practices) rather than individuals. Reasons include the threat of "contamination" (the unintentional spill-over of intervention effects from one treatment group to another) of some interventions if individual randomisation is used.[5 6] Also, in certain settings, randomisation by group may be the only feasible method of conducting a trial.[7] Trials with this design are variously known as field trials, community based trials, group randomised trials, place based trials, or (as in this paper) cluster randomised trials.[8] Although we would recommend the standard use of the term "cluster randomised trial" we recognise that those searching electronically for cluster trials may need to expand their search strategy to ensure that cluster trials using the terms "community" or "group" randomised trials are included.

In earlier papers we considered the implications of the CONSORT statement for the reporting of cluster randomised trials.[9 10] Here we present updated guidance, based on the 2010 revision of the CONSORT statement,[3] and the 2008 CONSORT extension for the reporting of abstracts.[11 12]

Scope of this paper

Cluster randomised trials are characterised by their multilevel nature; most often cluster trials involve two levels—the cluster and their individual members, such as general practice and patient—although trials of more than two levels, such as hospital-ward-patient, do exist. In this paper we focus on two level cluster trials for simplicity and refer to the groups that are randomised as "clusters" (these could be families, wards, etc) and we refer to the individual members of the clusters as "participants" (as they are usually individual people) unless there is ambiguity in a particular context. On occasion, however, a single person may be a cluster, with their teeth or eyes or limbs or multiple lesions as the members of the cluster. Measurements of these teeth, eyes, etc, within one individual will be correlated and so should not be treated as independent observations. A particular context for such trials is split mouth trials in dentistry.[13] However, those studies have additional considerations relating to the randomisation and the comparisons being within individuals. We do not consider them in detail in this paper.

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In some situations another form of clustering can be observed in individually randomised trials—for example, several patients receiving care from the same therapist or surgeon.[14] This type of clustering is also not the focus of this paper—it is discussed in the CONSORT extension for non-pharmacological treatments.[15] Nor are we interested in trials with one cluster per intervention. Trials with one cluster per arm should be avoided as they cannot give a valid analysis, as the intervention effect is completely confounded with the cluster effect.[16] It has been recommended that the minimum number of clusters per arm to ensure a valid analysis should be at least four.[17] Sometimes trials are inappropriately referred to as using cluster randomisation—for example, "To avoid patient interference a cluster randomisation was performed to alternate months."[18] This study was in fact not truly randomised nor was it a cluster trial. However, there is a particular design within the scope of cluster randomised trials, the cluster randomised crossover trial, where each participating cluster receives both intervention and control treatments consecutively, in separate periods, but the order of treatment is randomised.[19] Similarly, many stepped wedge designs, which randomise in terms of the period for receipt of the intervention, may be seen as a type of cluster crossover trial if the unit of randomisation is a cluster.[20 21]

We note that cluster randomised trials have no connection to cluster analysis; an exploratory multivariate statistical technique used to define clusters of similar people. We also note that the statistical issues raised by cluster randomised trials are different to those raised by cluster sampling, in which natural clusters such as geographical areas are identified and some clusters are chosen to be studied (for example, as in Mucklebecker 2006),[22] preferably at random.

In summary, our focus is on trials that are cluster randomised by design and have two or more clusters per intervention arm.

Updating the CONSORT statement for cluster randomised trials

The updated CONSORT 2010 statement includes a 25 item checklist. The wording was simplified and clarified and the specificity of certain elements of the statement made more explicit, for example by breaking some items into sub-items. Methodological advances reported in the literature since the 2001 statement were also reviewed and taken into account where appropriate.

To ensure that the extension for cluster trials reflected the wording of the updated CONSORT statement, and to integrate any important advances in the methodology for cluster trials since 2004, we decided to update the cluster extension in the summer of 2010.

The updating process

To identify papers relevant to the methodology for cluster randomised trials published between 2004 and 2010, we undertook an electronic search of Medline, Embase, and the Cochrane Methodology Register and a full text search of *Statistics in Medicine*, *Clinical Trials*, *Contemporary Clinical Trials*, and *BMC Medical Research Methodology*. The search yielded 1198 abstracts. One researcher (MKC) initially assessed the abstracts for relevance and classified 155 as potentially relevant to the update of the CONSORT extension. Each author took primary responsibility for aggregating and synthesising evidence most relevant to the reporting of a trial in particular areas—for example, analysis, intracluster correlation coefficients. We also reviewed all correspondence that had been received after the publication of the 2004 extension for cluster trials. We decided to reformat the checklist for cluster trials in line with the style currently promoted by the CONSORT Group, as used, for example, in the extensions for non-pharmacological interventions[15] and pragmatic trials.[23] In this updated style, additions to the main CONSORT checklist items are presented in a separate column rather than being integrated directly into the text of the checklist.

The researchers met face to face and by teleconference on several occasions to discuss updating the checklist and revision of the text, with additional discussion in conference calls and by email. A draft revised paper was distributed to the larger CONSORT Group for feedback. After consideration of their comments a final version of the extension was prepared and approved by the CONSORT executive.

Box 1 presents the noteworthy changes from the 2004 cluster extension paper. As for previous CONSORT checklists, we have included only those items deemed fundamental to the reporting of a cluster randomised controlled trial—that is, providing a minimum standard for the reporting of cluster trials. Moreover, a few items may be crucial to a trial but not included, such as approval by an institutional ethical review board, because funding bodies strictly enforce ethical review and medical journals usually address reporting ethical review in their instructions for authors. It is also not the purpose of this paper to provide a best practice guide on the design, conduct, and analysis of cluster randomised trials—these issues have been outlined by several other authors.

Advances in methodology since 2004

The 2004 extension paper[10] outlined the principal implications of adopting a cluster randomised trial (summarised in box 2). These included the need to account for the non-independence of participants within clusters and the need to be explicit about the level of inference at which the trial interventions and trial outcomes are targeted. Since 2004 there have been several methodological advances in the specialty. Detailed overviews of these methodological developments are presented elsewhere.[17 24-27]

Advances in reporting requirements since 2004

In 2008 the CONSORT Group also produced a separate reporting checklist for abstracts of reports of randomised controlled trials,[11 12] which presented a minimum list of essential items that should be reported within a trial abstract. The motivation for the extension for the reporting of abstracts was multi-fold but it was clear that readers of journals often base their assessment of a trial on the information presented in the abstract and as such quality reporting was particularly important within this aspect of the trial report. Therefore as part of the update process for the reporting of cluster trials we also reviewed the CONSORT extension for abstracts and highlighted the key areas where cluster trial specific reporting requirements would apply.

Quality of reporting of cluster trials

Early surveys of published cluster trials found that the conduct and reporting of the trials were often poor.[5 27-33] One study, however, found clear signs of improvement in the methods and reporting of cluster trials published in the *BMJ* from 1983 to 2003.[34]

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Box 1: Noteworthy changes from CONSORT 2004 extension for cluster randomised trials

- Separate presentation of the standard CONSORT checklist items and extension specific to cluster trials (table 1⇓)
- Provision of updated examples of good reporting practice
- Provision of an augmented checklist for abstracts of cluster randomised controlled trials
- Expansion of item 7a (sample size) to include the possibility of unequal cluster sizes
- Discussion of CONSORT 2010 item 7b (interim analysis guidelines) included in the context of cluster randomised controlled trials
- Item 10 (generation of random allocation sequence for participants) replaced by items 10a, 10b, and 10c

Box 2: Methodological considerations in cluster randomised trials

Design

- Observations on participants in the same cluster tend to be correlated (non-independent), so the effective sample size is less than that suggested by the actual number of individual participants. The reduction in effective sample size depends on average cluster size and the degree of correlation within clusters, ρ, also known as the intracluster (or intraclass) correlation coefficient (ICC).
- The intracluster correlation coefficient is the proportion of the total variance of the outcome that can be explained by the variation between clusters. A related coefficient of variation, *k*, between clusters has also been described.[90] Although no simple relation exists between *k* and ρ for continuous outcomes, another study described the relation for binary outcomes.[91] For both types of variable, when *k*=0, ρ also=0. Unlike *k*, the intracluster correlation coefficient cannot be defined for time to event data.
- Adjusting for the intracluster correlation is necessary for a valid analysis, but it reduces statistical power. For the same statistical power the overall sample size needs to be larger in a cluster randomised trial than in an individually randomised trial.
- If m is the cluster size (assumed to be the same for all clusters), then the inflation factor, or "design effect," associated with cluster randomisation is 1+(m−1)ρ. Although typically ρ is small (often <0.05) and it is often not known when a trial is planned (and only estimated with error after a trial is completed), its impact on the inflation factor can be considerable if the clusters are large. In general, the power is increased more easily by increasing the number of clusters rather than the cluster size.
- Cluster randomised trials may use a simple, completely randomised design, a matched cluster design, or a stratified design. If an appropriate matching factor is used, the matched design gains power relative to the completely randomised design. However, such matching variables may be difficult to identify, especially if the number of clusters to be matched is large. In addition, calculating the intracluster correlation coefficient from a matched design is generally problematic because the variation between clusters cannot be disentangled from the effects of the intervention or interventions and the matching factors.[8] The use of stratified designs is therefore generally preferable.[92]

Conduct

- The conduct of cluster randomised controlled trials differs in some ways from that of trials that randomise individuals. In particular, random allocation is done at the cluster level. Clusters are usually randomised all at once (or in batches) rather than one at a time, as in most individually randomised trials. This feature of the cluster randomised trial facilitates the use of a matched design.
- Sometimes consent can be sought both at cluster level and at individual participant level before randomisation. Commonly, however, prior consent to randomisation by individual cluster participants is not feasible.[93] In such circumstances, once clusters have been randomly assigned, participants in the clusters can no longer be asked for their consent to be randomised to receive either of the interventions, only for consent to receive the intervention to which their group has been assigned, and for consent to follow-up. This introduces the possibility of post-randomisation selection bias[5 94] as well as ethical concerns,[95-97] which have been explored in qualitative studies.[98] These concerns are analogous to those arising from randomised consent designs[99-101] for individually randomised trials.
- The concept of what blinding means in the context of a cluster randomised controlled trial is also complex—some patients may know which intervention they are allocated to but not that they are in a trial.

Analysis

- Cluster randomised controlled trials also present special requirements for analysis.[102] If the inference is intended at the participant level, they should not be analysed as if the trial was individually randomised as, if ρ is greater than zero, this gives spurious precision.
- The data could be analysed as if each cluster was a single individual, but this approach ignores the information collected on participants within a cluster and hence may not use the full richness of the dataset. Advances in software for the analysis of cluster randomised controlled trials have been reviewed.[24]
- Bayesian approaches to the analysis of cluster randomised trials have also been developed.[103-105]
- For in depth discussion of survival analysis that links to *k*, see Hayes and Moulton.[17]

Interpretation

- The interpretation of the results from cluster randomised trials may be more complicated than individually randomised trials as the conclusions may relate to the clusters, to the participants in those clusters, or to both.

Recent reviews have shown that deficiencies in reports of cluster trials remain common.[35-42] For example, the unit of analysis error (where results were analysed without accounting for the clustering) was seen in 19/40 (48%) of medical care trials,[35] and among 75 reports of cluster trials in cancer prevention and control a third (34%) "failed to report any analyses that were judged to be appropriate."[37] Among 50 reports of cluster trials of screening interventions, 32 (64%) reported using a method of analysis that took account of clustering, but in several the method used was not stated explicitly.[40] Those authors found that reporting was much better in high influence journals.

Three recent reviews have considered reporting in relation to the CONSORT extension for cluster trials. The first of these reviews examined the reports of 106 cluster randomised trials in children, published from 2004 to 2010.[41] Issues specific to cluster trials were poorly reported. The rationale for using a cluster design was given in 32% of the articles; how clustering was accounted for in sample size calculation and in analysis were reported in 59% and 65% of trials; and 55% of flow diagrams (which were included in 80% of the articles) omitted some information on the numbers of participants or clusters. Overall, 37% of the articles reported an intracluster correlation coefficient. The second review examined 300 randomly sampled cluster randomised trials published during 2000-08.[42] Of those presenting sample size calculations, 60% accounted for clustering in the design, whereas 70% accounted for clustering in analysis. Only 18% of the articles reported an intracluster correlation coefficient. Both of these studies saw only a slight improvement in reporting over time, but the third review suggested the opposite.[40]

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In summary, there is some evidence for improved reporting of cluster randomised trials but the quality of reporting remains well below an acceptable level.

Extension of CONSORT 2010 to cluster trials

Table 1 presents the revised checklist for the reporting of a cluster randomised controlled trial (updated in line with CONSORT 2010). Some items are extended to cover the reporting requirements relating to the cluster design, and item 10 is replaced by items 10a, 10b, and 10c, acknowledging the added complexity imposed on the randomisation and recruitment by the cluster design. Items requiring an extension from the CONSORT 2010 statement, and those items particularly relevant to the reporting of cluster randomised trials, are explained, with illustrative examples. As all our examples have been taken from previously published papers, it is inevitable that several do not display all the desirable elements of good reporting. Where this is the case, or where there might be ambiguity, we have attempted to identify which specific aspects of reporting are addressed.

We reviewed the checklist for abstracts and provide an augmented checklist for reporting of abstracts within cluster randomised controlled trials in table 2⇓.

Title and abstract

Item 1a

Standard CONSORT item: identification as a randomised trial in the title

Extension for cluster trials: identification as a cluster randomised trial in the title

Example

Cluster randomised trial of a targeted multifactorial intervention to prevent falls among older people in hospital[43]

Explanation

The primary reason for identifying the design in the title is to ensure appropriate indexing of the study as a cluster randomised trial in Medline. This indexing ensures ease of identification of these studies for inclusion in systematic reviews. "Community" randomised and "group" randomised are also widely used descriptions for such trials (for example, the trial report by Cavalcante et al)[44], especially when entire communities are randomised. A recent review of cluster randomised trials showed that identification of cluster randomised trials remains problematic, with only 48% of cluster randomised controlled trials published in 78 journals between 2000 and 2007 being clearly identified as cluster randomised in titles or abstracts.[45] Identification of the trial as a cluster randomised trial also ensures that readers will not be misled by apparently large sample sizes.

Item 1b

Standard CONSORT item: structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)[11 12]

Extension for cluster trials: outlined in table 2

Example: presented in figure 1⇓

Explanation

In 2008 a CONSORT extension on reporting abstracts was published.[11 12] We have provided an augmented checklist for this aspect of trial reporting in this update (table 2). As the extension on reporting abstracts was published only recently, and the 2004 extension to cluster trials did not specifically deal with abstracts, we were not able to find examples of good reporting tackling all the items required. We have therefore developed an abstract based on enhancing a published abstract (fig 1). Although this increases the word count by over a third, many journals now follow PubMed in no longer setting a limit.

In addition to the items recommended for all trials it is important that abstracts for cluster randomised controlled trials give the number of clusters per arm (and numbers of participants) and the level of inference (that is, the level at which hypotheses and conclusions are to be made) for the primary outcome, to allow appropriate interpretation.

Introduction

Background and objectives

Item 2a

Standard CONSORT item: scientific background and explanation of rationale

Extension for cluster trials: rationale for using cluster design

Example 1

Our intention was to enhance the application of evidence by the whole labour ward team so, to minimise contamination, the unit of randomisation and analysis was the obstetric unit[46]

Example 2

A cluster randomization was chosen for practical reasons and to prevent contamination by preference of patient or physician (selection bias)[47]

Explanation

Under the principles of the Helsinki declaration it is unethical to expose people unnecessarily to the risks of research.[48] Because a cluster randomised design increases the complexity of the research and usually requires more participants than an individually randomised design (to ensure equivalent statistical power), it is particularly important that the rationale for adopting a cluster design is outlined in the introduction.[49] In a recent review of cluster randomised trials, the reviewers[50] found that a third of published cluster trials could have used individual randomisation instead. As such, it is important that all alternatives are considered and that if a cluster trial is adopted it is clear why it was judged to be the most appropriate and robust design to adopt.[51]

Item 2b

Standard CONSORT item: specific objectives or hypotheses

Extension for cluster trials: whether objectives pertain to cluster level, individual participant level, or both

Example

The main aim of the present study is to use the rigour of a RCT [randomised controlled trial] in an evaluation comparing the effects of different approaches to knowledge transfer on policy and practice in the care of preterm babies in another setting . . . The main outcomes were at the level of unit policy and practice [neonatal units were the clusters][52]

Explanation

Descriptions of specific objectives and hypotheses need to make it clear whether they pertain to the individual participant level, the cluster level, or both. When objectives and hypotheses are targeted at the cluster level, analysis and interpretation of results at the cluster level might be appropriate. When the objectives

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and hypotheses are targeted at the individual participant level, statistical techniques at the individual level are usually required.

Methods

The main difference when reporting a cluster trial, as opposed to an individually randomised trial, is that there are two levels of inference rather than one: the cluster level and the individual participant level.[53] Thus, to allow readers to interpret the results appropriately, it is important to indicate explicitly the level at which the interventions were targeted, hypotheses generated, randomisation done, and outcomes measured.

Trial design

Item 3a

Standard CONSORT item: description of trial design (such as parallel, factorial) including allocation ratio

Extension for cluster trials: definition of cluster and description of how design features apply to clusters

Example 1 (definition of clusters)

Clusters were health centers with more than 400 residents aged 65.0-67.9 y in low-middle socioeconomic status municipalities (average population 127,000 individuals) in the Santiago Metropolitan area[54]

Example 2 (description of how the design applied to clusters)

This study was a pragmatic, cluster randomised, factorial, controlled trial. A 2×2 factorial design was used to assess the effect of each intervention and to explore the effect of the interventions combined . . . The four allocated groups were general practitioners' [the cluster] use of C reactive protein testing (1), training in enhanced communication skills (2), the interventions combined (3), and usual care (4)[55]

Example 3 (definition of how the design applied to clusters)

Hospitals [the clusters] were matched by country, type of hospital (public, private or social security), and baseline caesarean section rate (15-20%, 21-35%, or >35%), and the paired units were randomly assigned to intervention or control[56]

Explanation

The cluster is the unit of randomisation and it should be appropriately defined so that it can be adequately taken into account in the analysis. Whether the cluster randomised design is parallel, matched pair, or other, and whether the treatments have a factorial structure, has implications for the appropriate analysis of the outcome data and thus should be reported. Random assignment generally ensures that any baseline differences in group characteristics are the result of chance rather than of some systematic bias.[57] In individually randomised trials the sample size can be sufficiently large to ensure balanced baseline characteristics across groups. In cluster randomised designs, if the number of clusters is large, simple randomisation may often be sufficient.[8] This is not usually the case for these designs, however, because the number of clusters to be randomised is often small. Even if cluster specific characteristics are balanced (that is, characteristics of the randomly allocated clusters), researchers have little control over the participants within each cluster (this is the case whether the number of clusters is large or small).[8] As a result, some form of constraint (matching or stratification) is often imposed on randomisation in a cluster randomised design in an attempt to increase precision and minimise imbalance across treatment groups (see also item 8b). Any constraint imposed on the cluster randomised trial affects the sample size and the analysis and thus should be reported.

Item 3b

Standard CONSORT item: important changes to methods after trial commencement (such as eligibility criteria), with reasons

Example

Concerns that lower than expected pneumonia incidence rates would affect the trial's ability to detect an effect of nutritional supplementation on pneumonia incidence resulted in a protocol amendment 4 months into the study. A further 12 health centers were approached to join the study, eight of which were subsequently randomly assigned … to either the nutritional supplement intervention alone arm (n=4) or the control arm (n=4)[54]

Explanation

In cluster randomised controlled trials, as in any trial, important changes may have occurred to the study methods after the original protocol was written. Knowing about these changes and the reasons for them can aid interpretation, especially if there may be suspicions of bias. Changes that affect the number of clusters will be particularly important in cluster randomised controlled trials.

Participants

Item 4a

Standard CONSORT item: eligibility criteria for participants

Extension for cluster trials: eligibility criteria for clusters

Example

The study comprised 41 practices in Wessex . . . Inclusion criteria were ≥4 medical partners; list size >7000; a diabetes register with >1% of practice population; and a diabetes service registered with the health authority . . . Nurses reported all new cases of diabetes to the trial office. Willing patients aged 30-70 were included in the trial. Patients were excluded if they were private patients, housebound, mentally ill, had severe learning difficulties, or were subsequently found to have been diagnosed previously with, or not to have, diabetes, or were found to have type 1 diabetes[58]

Explanation

In cluster randomised trials two sets of eligibility criteria are considered—the eligibility of the clusters (for example, the general practices) to be included in the trial and the eligibility of individual participants (for example, the eligible patients within each general practice) to be included in clusters. As such, both sets of eligibility criteria need to be reported.

Interventions

Item 5

Standard CONSORT item: the interventions for each group with sufficient details to allow replication, including how and when they were actually administered

Extension for cluster trials: whether interventions pertain to cluster level, individual participant level, or both

Example 1 (intervention at individual participant level)

In the intervention group [consisting of seven sectors or clusters] the windows of all 241 houses [units of observation within sectors] (with a total of 1336 inhabitants) were covered with loosely hanging polyester curtains impregnated with the pyrethroid insecticide . . . In the 222 houses in six

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of the control sectors [clusters] the windows were covered with non-impregnated curtains and in one randomly selected control sector [cluster] with 106 houses no curtains were provided[59]

Example 2 (intervention at cluster and individual participant level)

See example in box 3.[60]

Example 3 (intervention at cluster level)

The purpose of this study is to evaluate the effect of randomizing GP practices to prescribing feedback using academic detailing (postal bulletin plus an educational outreach visit) compared to postal bulletin alone on prescribing of cardiovascular preventive therapies in patients with CVD or diabetes . . . The postal bulletins contained individualized GP prescribing feedback and educational information based on the 2003 European guidelines on CVD prevention. . . . The feedback was displayed using graphs and included the actual number of GP-registered patients not receiving recommended therapy[61]

Explanation

It is important to describe whether the intervention was targeted at the cluster level or the individual participant level. The level of inference is not necessarily the same as the level at which the intervention is applied, although inference at the cluster level is usually appropriate for interventions applied at the cluster level.

Outcomes

Item 6a

Standard CONSORT item: completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed

Extension for cluster trials: whether outcome measures pertain to cluster level, individual participant level, or both

Example 1 (outcome at level of cluster)

The purpose of this study is to evaluate the effect of randomizing GP practices to prescribing feedback using academic detailing (postal bulletin plus an educational outreach visit) compared to postal bulletin alone on prescribing [at the cluster level] of cardiovascular preventive therapies in patients with CVD or diabetes[61]

Example 2 (outcome at level of individual participant rather than cluster)

We evaluated the effect of a computer based clinical decision support system and cardiovascular risk chart [both targeted at physicians—at the cluster level] on patient centred outcomes of absolute cardiovascular risk and blood pressure[62]

Example 3 (outcomes at cluster level and individual participant level)

Primary outcomes were chosen for their ability to assess effectiveness and quality of service and included number of visits, referrals from RHC [rural health centres] for antenatal, intrapartum or postpartum problems [cluster level outcomes], place of delivery and low birthweight infant (<2500 g) [individual level outcomes]. The secondary outcomes were antenatal diagnosis of hypertension and twin pregnancy, perinatal mortality, operative delivery, preterm delivery (<37 weeks) [individual level outcomes] and the proportion of visits at which fundal height measurement was recorded and plotted on the antenatal record in the new model [cluster level outcome][63]

Explanation

Whether an intervention is evaluated at the cluster level or individual participant level has implications for the appropriate analysis of the outcome data. It is therefore important that the trial report is explicit about the level at which outcomes are measured.

Sample size

Item 7a

Standard CONSORT item: how sample size was determined

Extension for cluster trials: method of calculation, number of clusters, cluster size(s) (and whether equal or unequal cluster sizes are assumed), a coefficient of intracluster correlation (ICC or *k*), and an indication of its uncertainty

Example 1

We calculated sample size with a method that takes into account the intracluster correlation coefficient, the number of events, the expected effect, and the power of the study. We assumed an intracluster correlation of ρ=0.2, a minimum of 25 patients for each practice, and a worst case control rate of 50%. Under these assumptions we anticipated a power of 87% to detect a difference of 15% in rates between the two groups with α=0.05 with 60 practices for each intervention group[64]

Example 2

The calculation of the sample size . . . was based on 0.6% detection of CIN grade 2+ using the conventional Pap test and an expected 33% increase using liquid-based cytology with an α of 0.05 and β of 0.20, an intraclass correlation coefficient of 0.05, an average cluster size of 250, and a standard deviation of 200. This resulted in a coefficient of variation [k] of 0.8 and design effect of 1.59. By multiplication of the design effect by sample size without cluster effect, a sample size of 44 947 women in each group was obtained[47]

Explanation

A principal difference between the planning of a cluster randomised trial and that of an individually randomised trial is the calculation of the sample size. To retain equivalent power to an individually randomised trial, the number of individuals in a cluster randomised trial needs to be increased. When clusters are of similar size, the key determinants of the increase required (which can be substantial) are the intracluster correlation coefficient and the average cluster size (see box 2 for formula). Sample size calculations when cluster sizes are unequal are discussed elsewhere.[65] Reports of cluster randomised trials should state the assumptions used when calculating the number of clusters and the cluster sample size.

The power of the trial can also be decreased by imbalances in cluster sizes at recruitment and by differences in the intracluster correlation coefficient across clusters.[66] Differences in expected recruitment across clusters should be reported, since sample size calculations usually assume equal cluster sizes.

Item 7b

Standard CONSORT item: when applicable, explanation of any interim analyses and stopping guidelines

Example

These calculations conservatively included a 10% design effect, which was expected to be negligible in view of the small cluster size and rare outcome. The data monitoring and ethics committee did conditional power calculations in

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Box 3: Example of precise details of intervention at cluster and individual level from Murphy et al[60]

Tailored practice care

- An action plan for each practice was agreed with the practice and regularly reviewed by the study research nurse and practice
- The study nurse maintained regular contact with the practices
- The practice received a two page study newsletter every four months

Academic detailing

- An academic general practitioner (one per centre) made one 90 minute educational outreach visit to each intervention practice to promote drug prescribing guidelines for secondary prevention through interactive case based scenarios
- A study research nurse (one per centre) delivered another 90 minute session on behaviour change, which was intended to facilitate reflection on change to patient lifestyle and, through role play, new techniques to be used by the practice

Tailored patient care

- At the first intervention consultation, the patient and practitioner together identified areas of management that could be improved and the patient was invited to prioritise one particular aspect of his or her lifestyle for change
- Possible ways of achieving targets reflecting optimal management were identified and action plans individualised so that small, realistic goals for change were agreed
- A booklet containing information on all the key risk factors for coronary heart disease was used by practitioners in discussions on initial target setting and then given to the patients

Regular consultations

Patients were invited for an appointment with the general practitioner or nurse every four months; targets and goals for optimal secondary prevention were reviewed at each visit

2003 and recommended that the trial be continued until October 2008[67]

Explanation

As in many individually randomised trials, there may be a need for an independent body, such as a data monitoring committee,[68 69] to monitor accumulating data to protect patients in the trial and future patients. The main principles of interim analysis remain the same for cluster randomisation as for individual randomisation,[70] and the interim analysis would probably use the same statistical methods that were planned for the final analysis of a cluster randomised trial. Few cluster randomised trials seem to have explicit stopping boundaries, however, and cluster randomised controlled trials may need some additional considerations. For instance, if more information becomes available about the assumptions on which the original power calculations were based, or if clusters are recruited over a period of time, an interim analysis after, for example, 25% of the person time has been completed, may not provide as much as 25% of the information due to the expected variation between clusters[17]; a particular amount of person time is more informative if it comes from more, rather than fewer, clusters.[71]

Randomisation

Sequence generation

Item 8b

Standard CONSORT item: type of randomisation; details of any restriction (such as blocking and block size)

Extension for cluster trials: details of stratification or matching if used

Example 1 (stratification)

Neonatal units were stratified by designation of level of care within the managed Clinical Neonatal Networks (n=25) and by level of care delivered (level I, II or III), and then ordered alphabetically by name of hospital and imported into statistical computer software Stata V.9 at the London School of Hygiene and Tropical Medicine. The programme generated a series of blocks of varying size (two, four or six) for each stratum and allocated units to control or active intervention randomly within each block[52]

Example 2 (stratification)

The allocation schedule for random assignment of care models to clinics was computer generated, including stratification by study site and clinic characteristics, at a central location (WHO, Geneva, Switzerland) by the Statistical Unit of the UNDP/UNFPA/WHO/World Bank Special Programme of Research, Development, and Research Training in Human Reproduction[72]

Example 3 (matching)

We . . . paired the 14 [urban sectors of Trujillo, Venezuela] according to the incidence of cutaneous leishmaniasis in the 12 months before the baseline household survey. For each of the seven pairs we randomly allocated one sector (using computer created random numbers) . . . to the intervention group and the other to the control group[59]

Explanation

The way in which the randomisation sequence is generated depends on the design: if the number of clusters is large, a completely (cluster) randomised design may be a convenient option and then simple randomisation might be sufficient.[8] Otherwise, restricted randomisation may be useful. For a stratified design (for example, stratification by centre), the sequence is generated independently within each stratum. In addition, to control the probability of obtaining an allocation sequence with an undesirable sample size imbalance in the intervention groups, the randomisation within strata can be restricted. Common techniques for restricted randomisation include permuted blocks and the random allocation rule (that is, a single permuted block).[73] The random allocation rule is useful when all clusters are available at the time of generating the sequence, which is often the case. Using (more than one) random permuted blocks is more common for individually randomised designs, in which participants arrive sequentially. For the specific form of stratification of two clusters per stratum (a matched paired design), the sequence should allocate the two interventions at random to the clusters within pairs.

Allocation concealment mechanism

Item 9

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Standard CONSORT item: mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal sequence until interventions were assigned

Extension for cluster trials: specification that allocation was based on clusters rather than individuals and whether allocation concealment (if any) was at cluster level, individual participant level, or both

Example (allocation was based on clusters and concealed at cluster level)

The treatment allocation for each site was kept in Geneva until the site had completed the basic introductory training of study personnel (both in standard-model and control-model clinics [clusters]). When local investigators were ready to implement the training workshops for the staff if the clinic were assigned the new model, the study statistician sent the treatment allocation by facsimile directly to the principal investigator of the selected site[72]

Explanation

In individually randomised trials, adequate concealment of the treatment allocation is crucial for minimising potential bias. If the person recruiting participants has foreknowledge of the allocation, bias can result.[74] In a cluster randomised trial, allocation of treatment is predetermined for each member of the cluster. Hence the potential for selection bias (selective inclusion of patients into the trial) within clusters is particularly high.[5 6] It is therefore important that authors outline any strategies that were implemented to minimise the possibility of selection bias—for example, whether clusters were identified and recruited before randomisation, whether allocation was concealed from the person or people who provided access to the cluster or provided permission for the cluster to be included in the trial (for example, the cluster "guardian" or "gatekeeper")[75], whether all patients within a cluster were included or, if not, whether recruitment of patients was by a person masked to the cluster allocation. Allocation concealment for participants might be achieved by selecting participants before randomising clusters.[5]

Implementation

Item 10

Standard CONSORT item: who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions

Extension to cluster trials: replaced by items 10a, 10b, and 10c

Item 10a

Extension for cluster trials: who generated the random allocation sequence, who enrolled clusters, and who assigned clusters to interventions

Example 1 (enrolment of clusters and method of assignment)

Rural primary schools with 150 children or more, and over 15 children per class were eligible for inclusion. . . .Meetings were held in participating schools to explain the nature and purpose of the trial to parents or legal guardians, and written informed consent was obtained. Schoolchildren with parental consent and no history of adverse reaction to sulfa-based drugs were eligible for recruitment. . . . We stratified schools into three groups according to school examination performance in previous years . . . Ten schools were randomly selected from each school-performance stratum, and within each stratum schools were randomly allocated to one of six coded drug groups by use of block randomisation .. . according to a computer-generated random number list by an investigator . . . blind to drug group[76]

Example 2 (who generated sequence and who assigned clusters to interventions)

We conducted the trial in a contiguous area encompassing most of Ward 29 and all of Ward 30 in Eastern Kolkata, a legally registered urban slum with a population of about 60,000 residents. Before vaccination, a census of the population enumerated all households and persons in the study area and characterized the socioeconomic status, water source, and hygiene status of each household. Each household and person in the census were assigned a unique study identification number. The census, together with geographic mapping, was used to define 80 contiguous geographic clusters that served as the units of randomization. . . . The clusters were stratified according to ward and the number of residents who were 18 years of age or younger (<200 vs. ≥200 persons) and the number of residents who were older than 18 years (<500 vs. ≥500 persons), resulting in eight strata. For each stratum, a statistician who was unaware of the study-group assignments used a table of random numbers to assign half the 80 clusters to each vaccine[77]

Explanation

As outlined in the original CONSORT explanatory paper[78] it is important that all the steps of the random allocation process from generation to implementation are adequately described. Within cluster randomised trials the randomisation process has the added complexity of having two levels involved in the allocation process—the inclusion and allocation of clusters and the inclusion of cluster members. As such, the implementation processes adopted for each step need to be outlined separately.

Item 10b

Extension for cluster trials: mechanism by which individual participants were included in clusters for the purposes of the trial (such as complete enumeration, random sampling)

Example 1

The family practices associated with the 2 study sites served as the units of randomization. . . . Family practices associated with the clinical study sites were randomly assigned to the liquid-based cytology or the conventional Pap test group. All women screened at 1 of the participating family practices were included in the study[47]

Example 2

All cluster residents were eligible to receive a study vaccine if they were 24 months of age or older, had no reported fever or had an axillary temperature of no more than 37.5°C at the time of administration, and were not pregnant or lactating. All subjects or their guardians provided written informed consent[77]

Example 3

Twenty seven practices agreed to participate in the trial: 10 were therefore randomly allocated to each of the two intervention arms and seven to the usual care arm. To ensure sufficient numbers of patients with adequate follow up data, 30 patients were randomly sampled from each practice[62]

Explanation

A previous study[5] showed that selection bias can arise in cluster randomised controlled trials, especially at the point when participants are selected from within clusters. This bias can be reduced by complete enumeration and inclusion of all

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participants identified as eligible or, if a sample from the cluster is required, by having a third party make the selection or blinding the person identifying participants until after assessment of eligibility.

Item 10c

Extension for cluster trial: from whom consent was sought (representatives of cluster, or individual participants, or both), and whether consent was sought before or after randomisation

Example 1 (who provided consent)

Because outcome data were routinely collected at hospitals and no personal identifiers were transmitted, all the institutional review boards waived the requirement for individual consent. Responsible authorities from all the hospitals provided written consent, and birth attendants also provided written consent[56]

Example 2 (consent process and when consent sought)

There were 12 antenatal-care clinics in each of three study sites and 17 in the fourth site that were eligible for the study and where the health authorities agreed to let the clinics be included in the trial . . . [cluster-level consent]. The [individual level] informed consent procedure was based on the single-consent design proposed by Zelen. Thus, informed consent was requested only from women attending the antenatal clinics assigned to the new model; women who refused were cared for according to the standard practice in their clinic. However, such women were counted in the intention-to-treat analysis as being assigned to the new-model group. Women attending the standard-model clinics received the protocols recommended in each country, in the best format offered in these clinics . . . [72]

Explanation

Many reports of cluster trials do not indicate at which level consent was sought or its timing in relation to randomisation, which can lead to bias.[79] For example, if consent is sought at the cluster level post-randomisation but from the active arm only, there may be differential attrition of whole clusters. Even if consent is sought from all clusters, attrition bias can arise if consent to treatment or provision of data is sought from individual cluster members in the active arm only (see also item 5 and box 2). This reinforces the need to be able to report flow of patients within clusters over the course of the trial (see item 13).

Specific details of how interventions were administered should be described, because there are implications for consent and for adherence. When the intervention is applied at the cluster level (such as mass media advertising), it may not be feasible to obtain consent pre-randomisation from individual cluster members—that is, consent to randomise may have to be sought at cluster level only. Likewise, consent to intervention may have to be sought at the cluster level as individual cluster members may not be able to decline the intervention. Consent will often be sought at both the cluster level (for example, from the "gatekeeper") and from individual cluster members to contribute data. The specific issues around informed consent in cluster trials have been considered elsewhere.[80] (See also item 10b.)

Blinding

Item 11a

Standard CONSORT item: if done, who was blinded after assignment to interventions (for example, participants, care providers, those assessing outcomes) and how

Example

To prevent selective assessment bias, study personnel—gynecologists, pathologists, cytotechnologists, and others—involved in the follow-up and review of histology and cytology were blinded to the cytology screening system used. . . . A panel of 4 experienced pathologists who were blinded to the cytological system, the original cytological and histological findings, and all follow-up data reviewed the histology[47]

Explanation

In individually randomised trials the importance of blinding is well recognised, as a lack of blinding can lead to the inflation of study effects.[74 78] For individually randomised trials it is therefore recommended that participants and outcome assessors should be blinded wherever possible to the intervention received. In cluster trials, however, the concept of blinding is more complicated. Often the delivery of interventions in cluster trials involves a range of different people (for example, trainers training health professionals in the use of guidelines; health professionals implementing the guidelines for individual patients; outcome assessors who could be trainers; health professionals, patients, or a completely different group) at the different cluster levels and it is often unclear from trial reports who was and was not blinded within the trial.[36 42] It is further recognised that many cluster trials are pragmatic by nature, as they are often designed to evaluate changes in service provision or interventions to change practitioners' or patients' behaviour in real world settings. In such circumstances it is widely acknowledged that blinding of the intervention is often not possible (although blinding of those assessing outcome may still be possible). If this is the case, however, it is important that it is explicitly acknowledged in the trial report—the CONSORT statement for pragmatic trials recommends that if blinding was not done, or was not possible, an explanation as to why should be included in the text.[23]

Statistical methods

Item 12a

Standard CONSORT item: statistical methods used to compare groups for primary and secondary outcomes

Extension for cluster trials: how clustering was taken into account

Example 1

Because we randomised obstetric units . . . we analysed rates of marker clinical practices by obstetric units[46]

Example 2

The primary outcome was expressed as the mean rate difference between groups (with 95% CI). This value was measured as the difference between matched hospitals (intervention hospital minus control hospital) in caesarean section rate change (caesarean section rate in the intervention period minus caesarean section rate in the baseline period) . . . A one-sample two sided t test was used to assess whether the mean rate difference between groups was statistically different from zero[56]

Example 3

We used cluster specific methods because practices rather than patients were randomised, and we expected that variance in how patients were managed would be partly explained by the practice. Because some patients had more than one consultation, we added a third level to the analysis to account for the likelihood that variance in what was done at each

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consultation would be partly explained by the patient . . . [81] [This article also included a box to explain the three level logistical hierarchical model used.]

Explanation

Identification of the level of inference allows readers to evaluate the methods of analysis. For example, if the inference was targeted at the cluster level (at general practitioners rather than patients, for example) and outcomes were aggregated at the cluster level, sophisticated cluster adjusted analyses are not needed (as in examples 1 and 2). However, if the cluster sizes are unequal, weighting may be recommended because the variance of the cluster summary statistics is not constant. If the inference was targeted at the individual patient level, the analysis would need to adjust for potential clustering in the data.

Results

Participant flow

Item 13a

Standard CONSORT item: for each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for primary outcome

Extension for cluster trials: for each group, the numbers of clusters that were randomly assigned, received intended treatment, and were analysed for primary outcome

Item 13b

Standard CONSORT item: for each group, losses and exclusions after randomisation, together with reasons

Extension for cluster trials: for each group, losses and exclusions for both clusters and individual participants

Examples: presented in figures 2⇓ and 3⇓

Explanation

The flow diagram is a key element of the CONSORT statement and has been widely adopted. For cluster trials it is important to understand the flow of clusters as well as the flow of participants (fig 2). The potential for differential adherence and follow up is exacerbated in the cluster randomised design because there are two levels at which drop-outs can occur—whole clusters or individual participants in a cluster. It is therefore important to describe the flow of both clusters and participants when reporting a cluster randomised trial; this information is essential to interpret the study appropriately.

Although we recommend a flow diagram for communicating the flow of clusters and participants throughout the study (fig 2), the exact form and content should vary in relation to the specific features of a trial. We previously presented three options for modifying the CONSORT flow diagram for presenting clustered data—presenting the flow of data based only on clusters, only on individual participants, or on both.[9] Further experience suggests that the type of diagram should depend on the type of analysis because different approaches to analysis require information at different levels of the clustered design.

For example, if the analysis is aggregated at the level of the cluster, the flow diagram should relate to the data at cluster level. To allow meaningful interpretation, the diagram also needs to include a measure of the cluster size (and an indication of how variable cluster sizes are). If, however, the analysis is multilevel or hierarchical, the flow diagram should present data flow for both clusters and individual participants.

Baseline data

Item 15

Standard CONSORT item: a table showing baseline demographic and clinical characteristics for each group

Extension for cluster trials: baseline characteristics for cluster and individual participant levels as applicable for each group

Example: presented in table 3⇓

Explanation

Random assignment by individual ensures that any differences in group characteristics at baseline are the result of chance rather than some systematic bias.[74] For cluster randomised trials, however, the risk of chance imbalance is greater as clusters rather than individuals are randomised and the number of clusters is often small (although imbalances can occur even when the number of clusters is not limited).[66] It is therefore important to present summary baseline information for both clusters and individuals, most simply as tables of summary data (table 3).

Numbers analysed

Item 16

Standard CONSORT item: for each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups

Extension for cluster trials: for each group, number of clusters included in each analysis

Example: presented in figure 4⇓

Explanation

The number of participants who contribute to the analysis of a trial is essential to interpreting the results. However, not all participants may contribute to the analysis of each outcome. In a cluster trial this fact is compounded by the possibility that not all clusters may contribute to a particular analysis. Because the sample size calculation and hence the power of the study is calculated on the assumption that all participants and (especially) all clusters will provide information, the number of participants and clusters contributing to a particular analysis should be reported so that any potential drop in statistical power can be assessed (fig 4).

Outcomes and estimation

Item 17a

Standard CONSORT item: for each primary and secondary outcome, results for each group, and estimated effect size and its precision (such as 95% confidence interval)

Extension for cluster trials: results at cluster or individual participant level as applicable and a coefficient of intracluster correlation (ICC or *k*) for each primary outcome

Example: presented in table 4⇓

Explanation

When reporting the results of a cluster randomised trial, point estimates with confidence intervals should be reported for primary outcomes. Given the impact of the extent of the intracluster correlation on the power of the study, the intracluster correlation coefficient or *k* statistic for each primary outcome being analysed should also be provided to assess the magnitude of the clustering for each outcome (however, if a cluster level analysis is being undertaken, the concept of the intracluster correlation is less relevant as each cluster provides a single data

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point). In some situations, especially if it is believed the intervention will significantly affect the intracluster correlation coefficient, publishing the intracluster correlation coefficient from both the control and the intervention arm may be useful. This information, together with the cluster size or the design effect, allows readers to assess the appropriateness of the original sample size calculations. Showing both adjusted and unadjusted estimates for clustering would provide another indication of the extent of the clustering. Several authors have published observed study intracluster correlation coefficients to help with the planning of future cluster trials.[82-86]

Discussion

Generalisability

Item 21

Standard CONSORT item: generalisability (external validity, applicability) of trial findings

Extension for cluster trials: generalisability to clusters or individual participants (as relevant)

Example (at the cluster level)

Although our trial was completed successfully from both a methodological and practical point of view, our results may not be generalisable. The 21 participating practices tended to be large, with good nursing support, and may have been particularly committed to improving their quality of care . . . Furthermore, the observed intervention effect would probably have been greater if the trial had not taken place in the context of a health authority audit initiative relating to patients with coronary heart disease, backed by a financial incentive[87]

Explanation

In the discussion section of any trial report, the external validity of the results should be considered. External validity is more complicated for cluster randomised trials because the results may be generalisable to the clusters, to the participants in those clusters, or to both, and thus the level at which external validity is addressed should be identified.

Comment

Reports of randomised controlled trials should include key information on the methods and findings to allow readers to accurately interpret the results. This information is particularly important for meta-analysts attempting to extract data from such reports. The CONSORT 2010 statement provides the latest recommendations from the CONSORT Group on essential items to be included in the report of a randomised controlled trial. In this paper we introduce and explain corresponding updates in an extension of the CONSORT checklist specific to reporting cluster randomised trials.

Use of the CONSORT statement for the reporting of two group parallel trials is associated with improved reporting quality.[88] We believe that the routine use of this proposed extension to the CONSORT statement will eventually result in similar improvements to cluster trials.

When reporting a cluster randomised trial, authors should address all 25 items on the CONSORT checklist using this document in conjunction with the main CONSORT guidelines.[3] Depending on the type of trial done, authors may also find it useful to consult the CONSORT extensions for non-pharmacological treatments[15] and non-inferiority trials.[89]

The CONSORT statement can help researchers designing trials in the future and can guide peer reviewers and editors in their evaluation of manuscripts. Many journals recommend adherence to the CONSORT recommendations in their instructions to authors. We encourage them to direct authors to this and to other extensions of CONSORT for specific trial designs. The most up to date versions of all CONSORT recommendations can be found at www.consort-statement.org.

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Summary points

Reports of randomised trials should include key information on their methods and findings

Custer randomised trials have additional reporting considerations; we previously provided guidance on these in 2004

This paper provides updated guidance on the reporting of cluster randomised trials based on the 2010 revision of the CONSORT statement

New guidance is provided on the reporting of abstracts of cluster randomised trials

Routine use of this guidance should lead to improved quality of reporting

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Tables

Table 1| CONSORT 2010 checklist of information to include when reporting a cluster randomised trial

Section/topic and item No	Standard checklist item	Extension for cluster designs	Page No*
Title and abstract			
1a	Identification as a randomised trial in the title	Identification as a cluster randomised trial in the title	
1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)[11 12]	See table 2	
Introduction			
Background and objectives:			
2a	Scientific background and explanation of rationale	Rationale for using a cluster design	
2b	Specific objectives or hypotheses	Whether objectives pertain to the cluster level, the individual participant level, or both	
Methods			
Trial design:			
3a	Description of trial design (such as parallel, factorial) including allocation ratio	Definition of cluster and description of how the design features apply to the clusters	
3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons		
Participants:			
4a	Eligibility criteria for participants	Eligibility criteria for clusters	
4b	Settings and locations where the data were collected		
Interventions:			
5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	Whether interventions pertain to the cluster level, the individual participant level, or both	
Outcomes:			
6a	Completely defined prespecified primary and secondary outcome measures, including how and when they were assessed	Whether outcome measures pertain to the cluster level, the individual participant level, or both	
6b	Any changes to trial outcomes after the trial commenced, with reasons		
Sample size:			
7a	How sample size was determined	Method of calculation, number of clusters(s) (and whether equal or unequal cluster sizes are assumed), cluster size, a coefficient of intracluster correlation (ICC or *k*), and an indication of its uncertainty	
7b	When applicable, explanation of any interim analyses and stopping guidelines		
Randomisation			
Sequence generation:			
8a	Method used to generate the random allocation sequence		
8b	Type of randomisation; details of any restriction (such as blocking and block size)	Details of stratification or matching if used	
Allocation concealment mechanism:			
9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	Specification that allocation was based on clusters rather than individuals and whether allocation concealment (if any) was at the cluster level, the individual participant level, or both	
Implementation:			
10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	Replaced by 10a, 10b, and 10c	
10a		Who generated the random allocation sequence, who enrolled clusters, and who assigned clusters to interventions	

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Table 1 (continued)

Section/topic and item No	Standard checklist item	Extension for cluster designs	Page No*
10b		Mechanism by which individual participants were included in clusters for the purposes of the trial (such as complete enumeration, random sampling)	
10c		From whom consent was sought (representatives of the cluster, or individual cluster members, or both) and whether consent was sought before or after randomisation	
Blinding:			
11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those assessing outcomes) and how		
11b	If relevant, description of the similarity of interventions		
Statistical methods:			
12a	Statistical methods used to compare groups for primary and secondary outcomes	How clustering was taken into account	
12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses		
Results			
Participant flow (a diagram is strongly recommended):			
13a	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	For each group, the numbers of clusters that were randomly assigned, received intended treatment, and were analysed for the primary outcome	
13b	For each group, losses and exclusions after randomisation, together with reasons	For each group, losses and exclusions for both clusters and individual cluster members	
Recruitment:			
14a	Dates defining the periods of recruitment and follow-up		
14b	Why the trial ended or was stopped		
Baseline data:			
15	A table showing baseline demographic and clinical characteristics for each group	Baseline characteristics for the individual and cluster levels as applicable for each group	
Numbers analysed:			
16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	For each group, number of clusters included in each analysis	
Outcomes and estimation:			
17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)	Results at the individual or cluster level as applicable and a coefficient of intracluster correlation (ICC or *k*) for each primary outcome	
17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended		
Ancillary analyses:			
18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing prespecified from exploratory		
Harms:			
19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms106)		
Discussion			
Limitations:			
20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses		
Generalisability:			
21	Generalisability (external validity, applicability) of the trial findings	Generalisability to clusters and/or individual participants (as relevant)	

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Table 1 (continued)

Section/topic and item No	Standard checklist item	Extension for cluster designs	Page No*
Interpretation:			
22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence		
Other information			
Registration:			
23	Registration number and name of trial registry		
Protocol:			
24	Where the full trial protocol can be accessed, if available		
Funding:			
25	Sources of funding and other support (such as supply of drugs), role of funders		

*Page numbers optional depending on journal requirements.

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Table 2| Extension of CONSORT for abstracts11 12 to reports of cluster randomised trials

Item	Standard checklist item	Extension for cluster trials
Title	Identification of study as randomised	Identification of study as cluster randomised
Trial design	Description of the trial design (for example, parallel, cluster, non-inferiority)	
Methods:		
Participants	Eligibility criteria for participants and the settings where the data were collected	Eligibility criteria for clusters
Interventions	Interventions intended for each group	
Objective	Specific objective or hypothesis	Whether objective or hypothesis pertains to the cluster level, the individual participant level, or both
Outcome	Clearly defined primary outcome for this report	Whether the primary outcome pertains to the cluster level, the individual participant level or both
Randomisation	How participants were allocated to interventions	How clusters were allocated to interventions
Blinding (masking)	Whether or not participants, care givers, and those assessing the outcomes were blinded to group assignment	
Results:		
Numbers randomised	Number of participants randomised to each group	Number of clusters randomised to each group
Recruitment	Trial status*	
Numbers analysed	Number of participants analysed in each group	Number of clusters analysed in each group
Outcome	For the primary outcome, a result for each group and the estimated effect size and its precision	Results at the cluster or individual level as applicable for each primary outcome
Harms	Important adverse events or side effects	
Conclusions	General interpretation of the results	
Trial registration	Registration number and name of trial register	
Funding	Source of funding	

*Relevant to conference abstracts.

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Table 3| **Example of baseline information for each group given at individual and cluster levels (adapted from Sur et al).77 Values at individual level are numbers (percentages) and at cluster level are means (standard deviations) unless stated otherwise**

Variables	Vi vaccine (n=18 869)	Hepatitis A vaccine (n=18 804)
Individual level		
Mean (SD) age (years)	28.5 (18.0)	27.9 (17.8)
Male	9876 (52)	9920 (53)
Hindu	12 335 (65)	10 825 (58)
Mean (SD) No of members of household	7.1(3.9)	7.0(3.7)
Head of household able to read and write	13 980 (74)	13 099 (70)
Monthly household per capita expenditure above median of 500 rupees	7795 (41)	7636 (41)
At least one luxury item owned in household	4131 (22)	3918 (21)
Tube-well or faucet as source of drinking water in household	2711 (14)	1824 (10)
Flush toilet in household	905 (5)	577 (3)
Access to specific place for waste disposal in household	18 547 (98)	18 429 (98)
Household farther from treatment centre than median distance	8900 (47)	9935 (53)
Cluster level		
Age groups (No/cluster):		
2-18 years	256 (118)	273 (115)
>18 years	503 (84)	500 (93)
All residents per cluster (years)	29.0 (5.0)	28.5 (4.6)
Households (No/cluster)	142 (27)	146 (36)
Vaccinated participants (No/cluster)	472 (103)	470 (104)
Cases of typhoid fever during year before study (No/1000 cluster residents)	1.54 (1.40)	1.38 (1.38)
Population density (No of residents/100m^2/cluster)	18.4 (17.8)	22.2 (20.1)
Percent vaccine coverage of people ≥2 year of age	61 (11)	60 (12)

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Table 4| Example of study including data on numbers analysed by cluster and intracluster correlation coefficients (adapted from Feder et al)107

Variable	No (%) in intervention group	No (%) in control group	Intracluster correlation coefficient	Adjusted odds ratio (95% CI)	Adjusted χ^2 statistic	P value
No of practices	25	27	—	—	—	—
No of patients	172	156	—	—	—	—
Advice given:						
Cholesterol	54/81 (67)	32/83 (39)	0.013	4.0 (1.9 to 8.2)	12.2	<0.001
Weight	74/169 (44)	32/154 (21)	0.098	3.0 (1.5 to 35.8)	10.5	<0.01
Diet	46/169 (27)	22/154 (14)	0.053	2.4 (1.2 to 4.7)	6.2	<0.05

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Figures

Original abstract (Acolet et al)[52]

ABSTRACT (original)

Background Research findings are not rapidly or fully implemented into policies and practice in care.

Objectives To assess whether an "active" strategy was more likely to lead to changes in policy and practice in preterm baby care than traditional information dissemination.

Design Cluster randomised trial.

Participants 180 neonatal units (87 active, 93 control) in England; clinicians from active arm units; babies born <27 weeks' gestation.

Control arm Dissemination of research report; slides; information about newborn care position statement.

Active arm As above plus offer to become "regional champion" (attend two workshops, support clinicians to implement research evidence regionally), or attend one workshop, promote implementation of research evidence locally.

Main outcome measures Timing of surfactant administration; admission temperature; staffing of resuscitation team present at birth.

Results 48/87 lead clinicians in the active arm attended one or both workshops. There was no evidence of difference in post-intervention policies between trial arms. Practice outcomes based on babies in the active (169) and control arms (186), in 45 and 49 neonatal units respectively, showed active arm babies were more likely to have been given surfactant on labour ward (RR=1.30; 95% CI 0.99 to1.70); P=0.06); to have a higher temperature on admission to neonatal intensive care unit (mean difference=0.29°C; 95% CI 0.22 to 0.55; P=0.03); and to have had the baby's trunk delivered into a plastic bag (RR=1.27; 95% CI 1.01 to 1.60; P=0.04) than the control group. The effect on having an "ideal" resuscitation team at birth was in the same direction of benefit for the active arm (RR=1.18; 95% CI 0.97 to 1.43; P=0.09). The costs of the intervention were modest.

Conclusions This is the first trial to evaluate methods for transferring information from neonatal research into local policies and practice in England. An active approach to research dissemination is both feasible and cost-effective.

Trial registration Current controlled trials ISRCTN89683698

Enhanced abstract (with changes outlined in red)

Background Research findings are not rapidly or fully implemented into policies and practice in care.

Objectives To assess whether an "active" strategy was more likely to lead to changes in unit-level policies, and in practice for preterm babies than traditional information dissemination.

Design Cluster randomised trial.

Participants 180 neonatal units (87 active, 93 control) in England; clinicians from active arm units; babies born <27 weeks' gestation.

Control arm Dissemination of research report; slides; information about newborn care position statement.

Active arm As above plus offer to become "regional champion" (attend two workshops, support clinicians to implement research evidence regionally), or attend one workshop, promote implementation of research evidence locally.

Randomisation Neonatal units were stratified by level of care and randomly allocated to control or active intervention within blocks of varying size; all eligible babies within these units were assessed for practice outcomes.

Main outcome measures Timing of surfactant administration at cluster (policy) level; admission temperature; staffing of resuscitation team present at birth (at policy and individual practice level). Clinicians in active arm not blind to allocation but blind in control arm; outcomes assessed by assessors independent of the trial.

Results 48/87 lead clinicians in the active arm attended one or both workshops. There was no evidence of difference in post-intervention policies between trial arms based on 62 and 69 clusters (RR for policy specifying which paediatric staff should be present 0.92 (0.84 to 0.99); for delivery in plastic bag or wrapping 0.98 (95% CI 0.90 to 1.07; P=0.71)). Practice outcomes based on babies in the active (169) and control arms (186), in 45 and 49 neonatal units respectively, showed active arm babies were more likely to have been given surfactant on labour ward (RR=1.30; 95% CI 0.99 to1.70); P=0.06); to have a higher temperature on admission to neonatal intensive care unit (mean difference=0.29°C; 95% CI 0.22 to 0.55; P=0.03); and to have had the baby's trunk delivered into a plastic bag (RR=1.27; 95% CI 1.01 to 1.60; P=0.04) than the control group. The effect on having an 'ideal' resuscitation team at birth was in the same direction of benefit for the active arm (RR=1.18; 95% CI 0.97 to 1.43; P=0.09). There were no important adverse events. The costs of the intervention were modest.

Conclusions This is the first trial to evaluate methods for transferring information from neonatal research into local policies and practice in England. An active approach to research dissemination by neonatal units is both feasible and cost effective.

Trial registration Current controlled trials ISRCTN89683698

Funding Bliss Innovation in Care Programme, and CEMACH

Fig 1 Example of abstract for report of cluster randomised trial

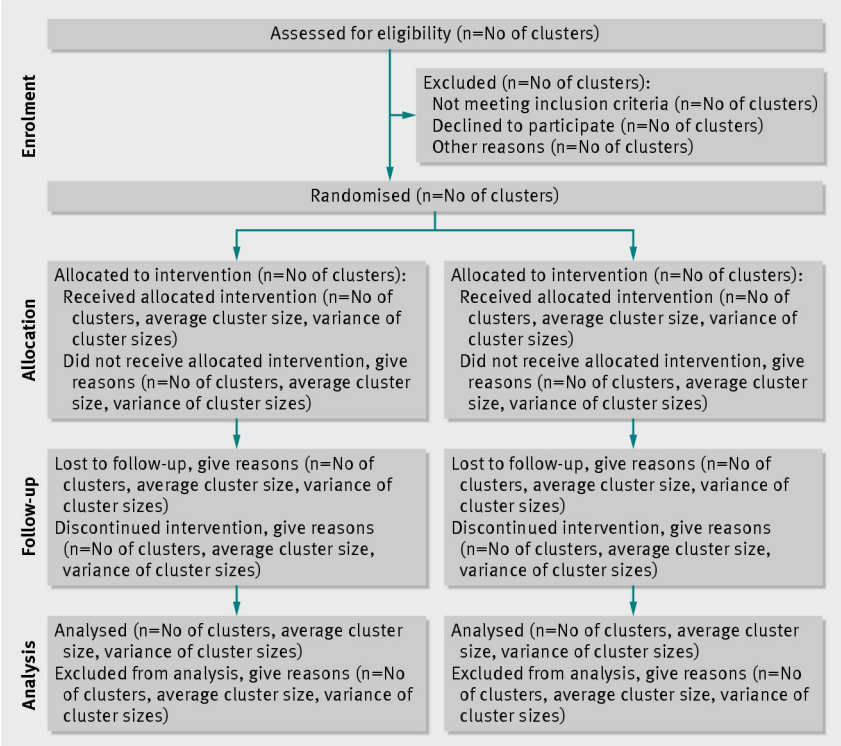

Fig 2 Recommended format for flow diagram of progress of clusters and individuals through phases of randomised trial

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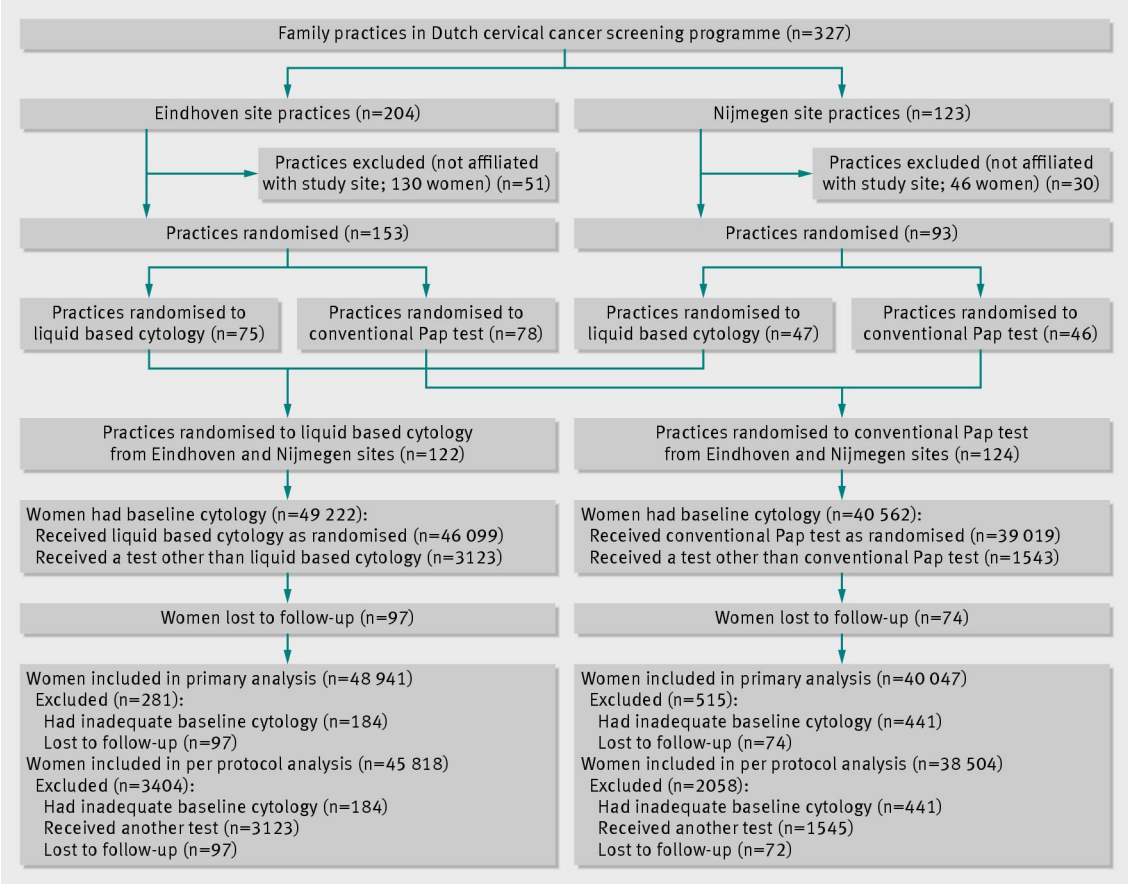

Fig 3 Example of flow diagram for cluster trial (adapted from Siebers et al[47])

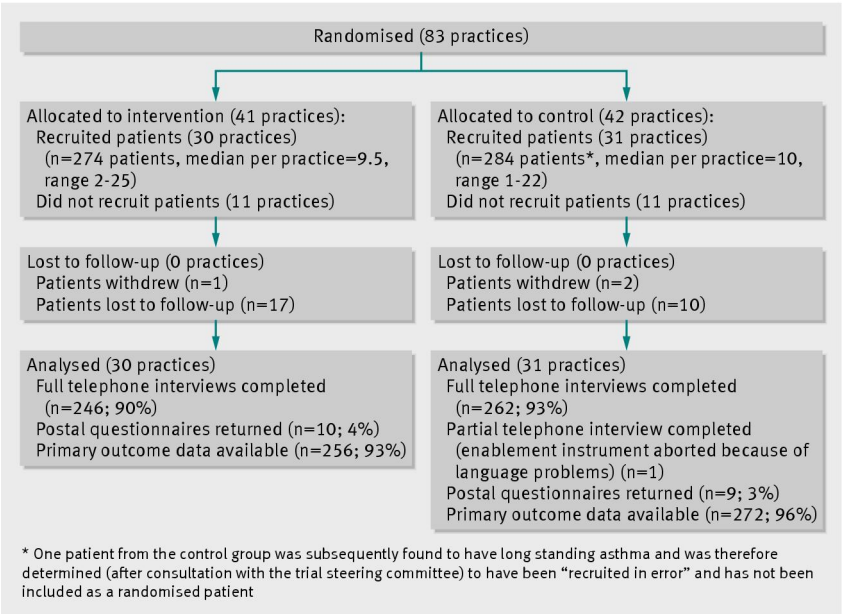

Fig 4 Example of flow of clusters from recruitment to analysis (reproduced from Francis et al)[108]

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