

CONSORT 2010 Statement: extension to randomised pilot and feasibility trials

Sandra M Eldridge^{1*}, Claire L Chan¹, Michael J Campbell², Christine M Bond³, Sally Hopewell⁴, Lehana Thabane⁵, Gillian A Lancaster⁶ on behalf of the PAFS consensus group.

- 1 Centre for Primary Care and Public Health, Queen Mary University of London, London, UK
- 2 School of Health and Related Research, University of Sheffield, Sheffield, South Yorkshire, UK
- 3 Centre of Academic Primary Care, University of Aberdeen, Aberdeen, Scotland, UK
- 4 Nuffield Department of Orthopaedics, Rheumatology and Musculoskeletal Sciences, University of Oxford, Oxford, Oxfordshire, UK
- 5 Clinical Epidemiology and Biostatistics, McMaster University, Hamilton, Ontario, Canada
- 6 Department of Mathematics and Statistics, Lancaster University, Lancaster, Lancashire, UK

Corresponding author*

E-mail: s.eldridge@qmul.ac.uk

PAFS consensus group authors (listed alphabetically): Doug Altman¹, Frank Bretz², Marion Campbell³, Erik Cobo⁴, Peter Craig⁵, Peter Davidson⁶, Trish Groves⁷, Freedom Gumedze⁸, Jenny Hewison⁹, Allison Hirst¹⁰, Pat Hoddinott¹¹, Sallie Lamb¹², Tom Lang¹³, Elaine McColl¹⁴, Alicia O’Cathain¹⁵, Daniel R Shanahan¹⁶, Chris Sutton¹⁷, Peter Tugwell¹⁸

- 1 Centre for Statistics in Medicine, Nuffield Department of Orthopaedics, Rheumatology and Musculoskeletal Sciences, University of Oxford, Oxford, UK
- 2 Statistical Methodology and Consulting, Novartis Pharma AG, Basel, Switzerland
- 3 Health Services Research Unit, University of Aberdeen, Polwarth Building, Foresterhill, Aberdeen, UK
- 4 Department of Statistics and Operations Research, UPC, Barcelona-Tech, Spain
- 5 MRC/CSO Social and Public Health Sciences Unit, University of Glasgow, Glasgow, UK
- 6 National Institute for Health Research, Evaluation, Trials, and Studies Coordinating Centre, University of Southampton, Southampton, UK
- 7 BMJ, BMA House, London, UK
- 8 Department of Statistical Sciences, University of Cape Town, Cape Town, South Africa
- 9 Leeds Institute of Health Sciences, University of Leeds, Leeds, UK
- 10 IDEAL Collaboration, Nuffield Department of Surgical Sciences, University of Oxford, Oxford, UK
- 11 Nursing Midwifery and Allied Health Professionals Research Unit, Faculty of Health Sciences and Sport, University of Stirling, Scotland, UK
- 12 Oxford Clinical Trials Research Unit, Nuffield Department of Orthopaedics, Rheumatology and Musculoskeletal Sciences, University of Oxford, Oxford, UK
- 13 Tom Lang Communications and Training International, Kirkland, Washington, USA
- 14 Newcastle Clinical Trials Unit and Institute of Health & Society, Newcastle University, Newcastle Upon Tyne, UK
- 15 School of Health and Related Research, University of Sheffield, Sheffield, UK
- 16 BioMed Central, London, UK
- 17 College of Health and Wellbeing, University of Central Lancashire, Preston, UK
- 18 Department of Medicine, University of Ottawa, Ottawa, Canada

SUMMARY

The CONSORT Statement (CONsolidated Standards for Reporting Trials) is a reporting guideline designed to improve the transparency and quality of the reporting of randomised controlled trials (RCTs). In this article, we present an extension to the CONSORT Statement for randomised pilot and feasibility trials conducted in advance of a future definitive RCT. The checklist applies to any *randomised* study in which a future definitive RCT, or a part of it, is conducted on a smaller scale, regardless of its design (eg cluster, factorial, cross-over) or the terms (eg pilot, feasibility, trial, study) used by authors to describe the study. The extension does not directly apply to internal pilot studies built into the design of a main trial, non-randomised pilot and feasibility studies, or phase II studies but these studies all have some similarities to randomised pilot and feasibility studies and so many of the principles may also apply to these studies.

The development of the extension was motivated by the growing number of studies described as feasibility or pilot studies, and by research that has identified weaknesses in their reporting and conduct. We followed recommended good practice to develop the extension, including performing a Delphi survey, holding a consensus meeting and research team meetings, and piloting the checklist.

Pilot and feasibility randomised studies differ from other randomised trials in their aims and objectives. Consequently, though much of the information to be reported in these trials is similar to that in RCTs assessing effectiveness and efficacy, there are some key differences in the type of information and in the appropriate interpretation of standard CONSORT reporting items. We have retained some of the original CONSORT Statement items, but most have been adapted, some removed, or new items added. The new items cover how participants were identified and consented; if applicable, pre-specified criteria used to judge whether, or how, to proceed with future definitive trial; if relevant, other important unintended consequences; implications for progression from pilot to future definitive trial including any proposed amendments; and ethical approval/research review committee approval confirmed with reference number.

This article includes the 26-item checklist, a separate checklist for the abstract, a template for a CONSORT flow chart for these studies, and explanation of the changes made and examples to support this explanation. We believe that routine use of this proposed extension to the CONSORT Statement will eventually result in improvements in the reporting of pilot trials.

“What this paper adds”

There are a growing number of studies described as pilot or feasibility studies. Research has identified weaknesses in the reporting and conduct of these studies. There are no existing CONSORT guidelines for randomised pilot or feasibility studies.

We present an extension to the CONSORT Statement for randomised pilot and feasibility trials conducted in advance of a future definitive randomised controlled trial, and present the 26-item checklist, a separate checklist for the abstract, a template for a CONSORT flow chart, and explanations and examples for the items. We believe routine use of this proposed extension to the CONSORT Statement will eventually result in improvements in the reporting of pilot trials.

INTRODUCTION

The CONSORT Statement (CONsolidated Standards for Reporting Trials) is a reporting guideline designed to improve the transparency and quality of the reporting of randomised trials. It was first published in 1996, revised in 2001, last updated in 2010[1, 2] and published simultaneously in 10 leading medical journals including the *Lancet*, *JAMA*, *BMJ*, *Annals of Internal Medicine* and *PLoS Medicine*. The CONSORT Statement (www.consort-statement.org) comprises a checklist of the minimum essential items that should be included in reports of randomised trials and a diagram documenting the flow of participants through the trial.

The development of CONSORT guidelines has seen considerable international recognition. The CONSORT Statement has been cited more than 8,000 times and has received support from the World Association of Medical Editors, the Council of Science Editors, the International Committee of Medical Journal Editors, and over 600 journals worldwide. A number of studies have examined the impact of the CONSORT Statement on the reporting quality of published randomised trials and found that adoption of the CONSORT Statement leads to an increase in reporting quality.[3]

In addition to the CONSORT Statement, extensions to the CONSORT checklist for reporting trials with non-inferiority, equivalence, and cluster or pragmatic designs have been published,[4-6] as have extension checklists for reporting harms,[7] different types of interventions (non-pharmacological treatments,[8] and herbal interventions [9]), and patient-reported outcomes.[10] The main CONSORT Statement and all of the current extensions focus on trials for which the research question centres on the effectiveness or efficacy of an intervention. However, some randomised trials, that we are referring to as pilot and feasibility trials, do not have effectiveness or efficacy as their primary focus. Rather, they are designed to support the development of a future definitive trial. By “definitive” in this context we mean an appropriately powered study focussing on efficacy or effectiveness. The need for high standards in conduct and reporting applies just as much to pilot and feasibility trials as it does to trials focusing on effectiveness or efficacy.

SCOPE OF THIS PAPER

In this article, we present an extension to the CONSORT Statement for randomised pilot and feasibility trials conducted in advance of a future definitive randomised controlled trial (RCT) (from here on “future definitive trial”). In keeping with the broad scope of CONSORT, the future definitive trial may be evaluating either the efficacy or the effectiveness of an intervention. The primary aim of the randomised pilot or feasibility trial, however, is to assess feasibility of conducting the future definitive trial.

Note that we make no distinction in this statement between pilot and feasibility randomised trials. Although in practice we recognise that different researchers may have preferences for different terms, the lack of distinction is based on a framework developed by the authors that defines such studies.[11] In that framework, a feasibility study for a future definitive trial asks whether the future definitive trial can be done, should we proceed with it, and if so, how. Pilot studies are a subset of feasibility studies. They ask the same questions about feasibility but have a particular design feature: in a pilot study (that may or may not be randomised) the future definitive trial, or part of it, is conducted on a smaller scale.

For brevity, in this paper we use the term “pilot trial” to refer to any *randomised* study in which a future definitive trial, or a part of it, is conducted on a smaller scale. However these studies may legitimately be referred to using any of the following terms: pilot RCT, randomised pilot trial, pilot trial, pilot study, randomised pilot study, feasibility RCT, randomised feasibility trial, feasibility trial, feasibility study, randomised feasibility study. In fact, we have set no restrictions on the terminology used to describe pilot trials; rather we have specified only that they are randomised, conducted in advance of a future definitive trial, and primarily aim to assess feasibility.

The development of this extension was motivated by the growing number of studies described as feasibility or pilot studies,[12] and by research that has identified weaknesses in the reporting and conduct of these studies.[12-15] We expect that improved reporting quality will lead to more high-quality examples of pilot trials, enabling yet further improvements in the conduct of pilot trials and making it possible for readers to use the results of reported pilot studies in preparing future trials in similar settings and with similar participants. Because the purpose of a pilot trial (to assess feasibility) is different from that of the future definitive trial (to assess effectiveness or efficacy), the focus of the reporting should be different, and that difference is reflected in this extension.

The statement does not apply to internal pilot studies that are built into the design of a main trial, nor to non-randomised pilot and feasibility studies. However, much of what is presented here may apply to, or be adapted to apply to, these types of pilot or feasibility studies or similar types of trial such as “proof of concept” or Phase II trials seen in pharmaceutical drug development.[16, 17]. “Proof of concept” or Phase II trials are small RCTs with the main objective to inform the sponsor whether or not to continue the development of a drug with larger trials. Similar to pilot trials, the focus is on assessing the feasibility of further development rather than assessing effectiveness or efficacy. However, in order to do this, these trials tend to focus on aspects such as safety and potential effectiveness or efficacy. They may use accepted methods devised for phase II trials,[18] to assess the outcome to be used in a future phase III trial (which could be meta-analysed if required),[19] or use, “surrogate outcomes”, that is intermediate measures, often biochemical, which have less direct impact on a patient than, say, cure or death, but which should be associated with these “hard” outcomes. Safety, and potential effectiveness or efficacy are usually less important in pilot trials where the focus is on the development of interventions and their evaluation, and issues related to feasibility may be different. Nevertheless pilot trials do sometimes assess potential effectiveness using surrogate outcomes. For example, oxygenation of the blood as a surrogate measure for improved lung function and survival,[20] or the number of steps walked per day, as a surrogate for clinical measures of heart disease.[21]

The statement is an extension to the standard CONSORT guidelines for reporting RCTs. However, many investigators use qualitative research, alongside other methods, to assess feasibility. The amount of qualitative work conducted at the pilot and feasibility stage, its relationship with any pilot trial, and the way investigators wish to report this work, varies. Stand-alone qualitative studies that are reported separately from the pilot trial, such as Hoddinott et al and Schoultz et al,[22, 23] should follow appropriate reporting guidelines,[24-26] but should provide link references to other pilot work carried out in preparation for the same definitive trial. When qualitative work is reported within the primary report of a pilot trial, it is not always possible to put sufficient detail into the methods section of the report to comply with reporting guidelines for qualitative studies. If this is the case, we recommend an online supplement or appendix to report the methods in detail.[27] O’Cathain, Hoddinott and Schoultz have provided guidelines and examples for conducting qualitative feasibility studies alongside pilot trials.[22, 23, 26, 28, 29]

ADAPTING THE CONSORT STATEMENT FOR PILOT TRIALS

The development of this CONSORT extension for pilot trials is described briefly here and in detail elsewhere (in press). Before developing the checklist for this CONSORT extension for pilot trials, the research team agreed on the definitions of pilot and feasibility studies described in the previous section. This was done by initially considering pilot and feasibility studies to be discrete types of study and therefore needing separate checklists. However preliminary work concluded that pilot and feasibility studies could not be defined in a mutually exclusive way which was compatible with current understanding and the use of these terms amongst the research community. We therefore adopted an overarching definition of feasibility studies with pilot studies being a subset, and developed a single checklist for such studies that used a randomised approach. As described above,

we use the term “pilot trial” to refer to these studies in this paper. The process of agreeing on the definitions of feasibility and pilot studies and the underpinning conceptual framework are reported separately.[11] That work was done in parallel with the development of the checklist which is described in Box 1 and the following paragraphs. We used the principles in Box 2 to guide the work.

Box 1: Stages of adapting the CONSORT Statement for pilot trials

Stage	Activity	Participants	Venue (or virtual meeting)	Date
Stage 1	Drafting of definitions and preliminary adaptation of CONSORT checklist items	Research team	London	December 2012
Stage 2	1st round of modified Delphi process using online administration	Invited experts from research community (trialists, methodologists, statisticians, funders, and journal editors)	Email distribution	July – August 2013
Stage 3	2nd round of modified Delphi	As for round 1	Email distribution	September – October 2013
Stage 4	Review of Delphi results and redrafting checklist	Research team	London	February 2014
Stage 5	Consensus meeting	Invited experts (trialists, methodologists, statisticians, funders, journal editors, and members of the CONSORT executive)	Oxford	October 2014
Stage 6	Review of consensus meeting feedback and drafting final checklist	Research team	Email consultation with consensus participants	December 2014 – December 2015
			Meetings in London	January, June, December 2015
Stage 7	Further review and piloting	Research team	Email consultation with consensus participants	March 2016
			Piloting by independent researchers writing up pilot studies	January – March 2016

In Stage 1, the research team met and worked through each of the existing CONSORT checklist items, agreeing whether it was relevant and should be retained, not relevant and should be excluded, or needed rewording in the context of either a feasibility study or pilot study, thus resulting in two checklists. We then applied the revised checklists to a sample of 30 articles identified from previous work,[13, 15] and our own personal collections.

In Stage 2, a modified Delphi survey was used to seek consensus on the appropriateness of each of the checklist items. Participants (n=93) were asked to rate each item on a scale of 1 to 9 (1=not at all appropriate to 9=completely appropriate). Participants were also given the opportunity to comment on each item, on definitions of pilot and feasibility studies, and on the perceived usefulness of the checklist.[11]

In Stage 3, Delphi participants were asked to review responses from Round 1 of the Delphi survey for items which 70% or more of participants had rated as 8 or 9 and to make additional comments on these items. They were asked to review the remaining items and classify each using one of four options: discard, keep, unsure, or no opinion. They were also asked to add any items they believed

had been missed. In total 93/120 (77.5%) responses were received for Round 1 and 79/93 (84.9%) for Round 2.

In Stage 4, the research team met face-to-face to review the Delphi feedback and to revise the checklist. In Stage 5, the revised checklist was then further reviewed in detail during a 2-day expert consensus meeting. In Stage 6, some checklist items were reworded to ensure clarity of meaning and purpose, and the research team met face-to-face a further three times to agree on the final wording of the checklist, identify examples of good reporting, and to develop the explanation and elaboration section of this paper. A full draft of the paper was then sent to members of the consensus meeting to ensure it fully reflected the discussion of the meeting.

The final checklist is presented in Table 1. As with other CONSORT extensions the original CONSORT item is shown on the left hand side, and the adapted/extension item is shown in the right hand column. In Box 2 there is a list of the methodological considerations and principles that have guided the process.

Table 1: CONSORT checklist of information to include when reporting a pilot trial

Section/topic and item No	Standard checklist item	Extension for pilot trials	Page no where item is reported
Title and abstract			
1a	Identification as a randomised trial in the title	Identification as a pilot or feasibility randomised trial in the title	
1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	Structured summary of pilot trial design, methods, results, and conclusions (for specific guidance see CONSORT abstract extension for pilot trials)	
Introduction			
Background and objectives:			
2a	Scientific background and explanation of rationale	Scientific background and explanation of rationale for future definitive trial, and reasons for randomised pilot trial	
2b	Specific objectives or hypotheses	Specific objectives or research questions for pilot trial	
Methods			
Trial design:			
3a	Description of trial design (such as parallel, factorial) including allocation ratio	Description of pilot trial design (such as parallel, factorial) including allocation ratio	
3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	Important changes to methods after pilot trial commencement (such as eligibility criteria), with reasons	
Participants:			
4a	Eligibility criteria for participants		
4b	Settings and locations where the data were collected		
4c		How participants were identified and consented	
Interventions:			

5 The interventions for each group with sufficient details to allow replication, including how and when they were actually administered

Outcomes:		
6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	Completely defined pre-specified assessments or measurements to address each pilot trial objective specified in 2b, including how and when they were assessed
6b	Any changes to trial outcomes after the trial commenced, with reasons	Any changes to pilot trial assessments or measurements after the pilot trial commenced, with reasons
6c		If applicable, pre-specified criteria used to judge whether, or how, to proceed with future definitive trial
Sample size:		
7a	How sample size was determined	Rationale for numbers in the pilot trial
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)	Type of randomisation(s); details of any restriction (such as blocking and block size)
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	
Implementation:		
10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	
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	
Analytical methods:		
12a	Statistical methods used to compare group for primary and secondary outcomes	Methods used to address each pilot trial objective whether qualitative or quantitative

12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	Not applicable
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 participants who were approached and/or assessed for eligibility , randomly assigned, received intended treatment, and were assessed for each objective
13b	For each group, losses and exclusions after randomisation, together with reasons	
Recruitment:		
14a	Dates defining the periods of recruitment and follow-up	
14b	Why the trial ended or was stopped	Why the pilot trial ended or was stopped
Baseline data:		
15	A table showing baseline demographic and clinical characteristics 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 objective , number of participants (denominator) included in each analysis. If relevant, these analyses should be by randomised group
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)	For each objective , results including expressions of uncertainty (such as 95% confidence interval) for any estimates. If relevant, these results should be by randomised group
17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	Not applicable
Ancillary analyses:		
18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	Results of any other analyses performed that could be used to inform the future definitive trial
Harms:		
19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	
19a		If relevant, other important unintended consequences
Discussion		
Limitations:		

20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	Pilot trial limitations, addressing sources of potential bias and remaining uncertainty about feasibility
Generalisability:		
21	Generalisability (external validity, applicability) of the trial findings	Generalisability (applicability) of pilot trial methods and findings to future definitive trial and other studies
Interpretation:		
22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	Interpretation consistent with pilot trial objectives and findings, balancing potential benefits and harms, and considering other relevant evidence
22a		Implications for progression from pilot to future definitive trial including any proposed amendments
Other information		
Registration:		
23	Registration number and name of trial registry	Registration number for pilot trial and name of trial registry
Protocol:		
24	Where the full trial protocol can be accessed, if available	Where the pilot trial protocol can be accessed, if available
Funding:		
25	Sources of funding and other support (such as supply of drugs), role of funders	
26		Ethical approval/research review committee approval confirmed with reference number

Box 2: Methodological considerations and principles that have guided the development of the CONSORT extension to pilot trials

- The rationale of a pilot trial is to investigate areas of uncertainty about the future definitive trial.
- The primary aims and objectives of a pilot trial are therefore about feasibility, and this should guide the methodology used in the pilot trial.
- Assessments or measurements to address each pilot trial objective should be the focus of data collection and analysis. This may include outcome measures likely to be used in the definitive trial but, equally, it may not.
- Since the aim of a pilot trial is to assess the feasibility of proceeding to the future definitive trial, there needs to be a decision process about how to proceed built into the design of the pilot trial. This may involve formal progression criteria to decide whether to proceed, to proceed with amendments, or not to proceed.
- Methods used to address each pilot trial objective can be qualitative or quantitative. A mixed-methods approach could result in both types of data being reported within the same paper. Equally, a process evaluation or other qualitative study can be done alongside a pilot trial and reported separately in more detail.
- The number of participants in a pilot study should be based on the feasibility objectives and some rationale should be given.
- Formal hypothesis testing for effectiveness (or efficacy) is not recommended. The aim of a pilot trial is not to assess effectiveness (or efficacy) and it will usually be underpowered to do this.

EXTENSION OF CONSORT 2010 TO PILOT TRIALS

Title and abstract

Item 1a

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

Extension for pilot trials: identification as a pilot or feasibility randomised trial in the title

Example 1 (using the words pilot, randomised, and trial)

“Bespoke smoking cessation for people with severe mental ill health (SCIMITAR): a pilot randomised controlled trial”[30]

Example 2 (using the words feasibility, randomised, and trial)

“A cluster randomised feasibility trial evaluating nutritional interventions in the treatment of malnutrition in care home adult residents”[31]

Explanation

The primary focus of these guidelines is randomised pilot and feasibility trials. To ensure that these types of studies can be easily identified from specific search criteria, a title containing the descriptors “pilot” or “feasibility” as well as “randomised” provides a necessary, recognised terminology for selecting randomised pilot and feasibility trials.[13] This would also enable these studies to be easily indexed in electronic databases, for example PubMed.[32] For many studies, although the descriptors may all appear in the title, they may not necessarily occur together, as in: “Feasibility of a randomised trial of a continuing medical education program in shared decision-making on the use of antibiotics for acute respiratory infections in primary care: the DECISION+ pilot trial.”[33].

Furthermore, in some cases authors may use the phrase “randomised pilot study” or “randomised feasibility study,” as in ““Not just another walking program”: Everyday Activity Supports You (EASY) model – a randomized pilot study for a parallel randomized controlled trial.”[21] Such papers could be identified in appropriate searches. However, in general we recommend the descriptors are given together in one phrase, and the word “trial” rather than “study” is used as in “randomised pilot trial” or “randomised feasibility trial”.

Item 1b

Standard CONSORT item: structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts) [34, 35]

Extension for pilot trials: structured summary of pilot trial design, methods, results, and conclusions (for specific guidance see CONSORT abstract extension for pilot trials) (Table 2)

Table 2: CONSORT extension for reporting abstracts of pilot trials

Item	Standard Checklist item	Extension for pilot trials
Title	Identification of study as randomised	Identification of study as randomised pilot trial
Trial design	Description of the trial design (e.g. parallel, cluster, non-inferiority)	Description of pilot trial design (e.g. parallel, cluster)
METHODS		
Participants	Eligibility criteria for participants and the settings where the data were collected	Eligibility criteria for participants and the settings where the pilot trial was conducted
Interventions	Interventions intended for each group	<i>same</i>

Objective	Specific objective or hypothesis	Specific objectives of the pilot trial
Outcome	Clearly defined primary outcome for this report	Pre-specified assessment or measurement to address the pilot trial objective(s) ¹
Randomisation	How participants were allocated to interventions	<i>same</i>
Blinding (masking)	Whether or not participants, care givers, and those assessing the outcomes were blinded to group assignment	<i>same</i>
RESULTS		
Numbers randomised	Number of participants randomised to each group	Number of participants screened and randomised to each group for the pilot trial objective(s) ¹
Recruitment	Trial status ²	<i>same</i>
Numbers analysed	Number of participants analysed in each group	Number of participants analysed in each group for the pilot objective(s) ¹
Outcome	For the primary outcome, a result for each group and the estimated effect size and its precision	Results for the pilot objective(s), including any expressions of uncertainty ¹
Harms	Important adverse events or side-effects	<i>same</i>
Conclusions	General interpretation of the results	General interpretation of the results of pilot trial and their implications for the future definitive trial
Trial registration	Registration number and name of trial register	Registration number for pilot trial and name of trial register
Funding	Source of funding	Source of funding for pilot trial

¹ Space permitting, list all pilot trial objectives and give the results for each. Otherwise, report those which are a priori agreed as the most important to the decision to proceed with the future definitive trial.

² For conference abstracts.

Example

Presented in Figures 1a, 1b, 1c.[21]

Explanation

Abstracts can follow different structures dependent upon a journal's own style. They are typically around 300 words in length. Here we outline what information should be reported in the abstract irrespective of style. This information may also be used for writing conference abstracts. The structure of the abstract does not differ in format from item 1b of the standard CONSORT 2010 guidelines. However, its content focuses on the aims and objectives of the pilot trial and not on the future definitive trial.

It is important that the abstract contains pertinent information on the background, methods, results and conclusions in relation to the feasibility objectives and outcomes, and that it states the study is a "randomised" pilot trial. This will aid researchers in understanding the nature of the paper and again facilitates electronic searching through the inclusion of specific key words as discussed in item 1a. A statement in the abstract that this study is in preparation for a future definitive trial is recommended to place it in context. A description of the areas of uncertainty to be addressed and a statement of the feasibility aims and objectives should be included in the background, and how these objectives have

been addressed in the methods. In the results section, if there are a limited number of pilot trial objectives then all should be listed and results for each objective should be reported. If there are many pilot trial objectives, then agreement should be reached a priori about which are the most important to decide whether to proceed to a future definitive trial, and only these objectives should be reported. An explicit statement relating to whether the future definitive trial is likely to go ahead based on the results of the pilot trial should also form part of the discussion and conclusions.

Introduction

Item 2a

Standard CONSORT item: scientific background and explanation of rationale

Extension for pilot trials: scientific background and explanation of rationale for future definitive trial, and reasons for randomised pilot trial

Example

“Reduced fetal movements (RFM) is a frequently seen problem in maternity care with 6–15% of women reporting attending at least one occasion of RFM to health professionals in the third trimester of pregnancy. RFM, defined by maternal perception of significantly reduced or absent fetal activity, is associated with increased risk of stillbirth and fetal growth restriction (FGR) due to placental dysfunction. Despite this association there is a paucity of evidence to direct clinical management of women presenting with RFM. This has been recently highlighted by guidelines from the Royal College of Obstetrics and Gynaecology (RCOG) and a meta-analysis.... The absence of high-quality evidence has led to wide variation in management strategies for RFM in high-income settings... Although there are randomised controlled trials (RCT) of counting fetal movements by a formal structure (e.g. count to ten) there have been no published RCTs of patient management following presentation with RFM. To undertake an RCT of patient management raises important practical concerns including: maternal anxiety for fetal wellbeing, the need to make a decision regarding participation in a short period of time due to the acute nature of RFM and adherence to protocol. Thus, studies have adopted an approach of changing practice at the unit level in quality-improvement projects or stepwise cluster RCT (AFFIRM, NCT01777022). We performed this study to address whether an RCT of the management of RFM in individual patients was an appropriate trial design, and was feasible with regard to i) maternal recruitment and retention ii) patient acceptability, iii) adherence to protocol. In addition, we wished to confirm the prevalence of poor perinatal outcomes in the study population.”[36]

Explanation

It is important that the scientific background sets the scene and gives the rationale and justification for the future definitive trial and why the pilot trial is needed, because under the principles of the Helsinki declaration it is unethical to expose people unnecessarily to the risks of research.[37] The background and rationale are nicely illustrated in the example. Other related publications, or preliminary work such as systematic reviews, qualitative studies or additional feasibility work, or absence of such work because no one has looked at this topic before, should also be mentioned. The rationale for the randomised pilot trial should be clearly outlined including the areas of uncertainty that need to be addressed before the future definitive trial can take place and why such a trial is needed before proceeding to the future definitive trial. This rationale is usually reported in the final paragraph of the section to provide a justification for the pilot trial.

Item 2b

Standard CONSORT item: specific objectives or hypotheses

Extension for pilot trials: specific objectives or research questions for pilot trial

Example 1 (listing objectives as primary and secondary)

“In this feasibility trial, the research aim was to explore trial design, staff and resident acceptability of the interventions and outcome measures and to provide data to estimate the parameters required to design a definitive RCT....The primary objectives of the trial were as follows:

1. To assess how many care homes accepted the invitation to participate in research.
2. To determine whether the eligibility criteria for care home residents were too open or too restrictive by estimating feasible eligibility and recruitment rate.
3. To assess retention of care homes and residents by estimating 3 and 6-month follow-up rates.
4. To investigate the acceptability of nutritional support interventions to malnourished care home residents in terms of compliance and to care home staff in terms of adherence to the intervention schedule.
5. To assess the acceptability and feasibility (and factors influencing this) of the outcome measures as methods to measure efficacy of the interventions within a definitive trial.

The secondary objectives of the trial were as follows:

1. To investigate the completion of screening tools and questionnaires by care home staff.
2. To determine how many malnourished residents were able to participate in PROMs and to complete the questionnaires.
3. To pilot a Healthcare resource usage (HCRU) questionnaire.
4. To measure key outcome domains (for completion rates, missing data, estimates, variances and 95 % confidence intervals for the difference between the intervention arms) for malnourished care home residents, including physical outcome measures and PROMs.
5. To collect and synthesise data, from which the Intraclass Correlation Coefficient (ICC) and sample size of a definitive cluster RCT (CRCT) could be estimated.”[31]

Example 2 (objectives leading to a mixed methods study)

“The main aim of the study is to assess the feasibility of conducting a definitive trial in terms of recruitment, use and acceptability of the intervention, follow-up at 3 and 6 months, and data collection methods. In addition, the study aims to establish suitable procedures for delivering the intervention and conducting assessments and procedures for ensuring recruitment and retention in the study. Finally, the study aims to discover whether using a structured, individualized approach to lifestyle assessment and referral will improve uptake and participation in lifestyle- and behaviour-change interventions.

The study will also examine, qualitatively, the acceptability of the assessment tool to patients in an acute cardiology setting as well as patients’ experiences of making lifestyle changes in order to develop effective recruitment and retention strategies.

The study will have a number of quantitative objectives:

1. To determine how many patients accept referral to a formal lifestyle programme;
2. To determine how many patients participate in a lifestyle-change intervention or initiate self-managed change;
3. To investigate the uptake of lifestyle intervention in relation to subsequent behaviour change and impact on health-related quality of life, mood and social satisfaction;
4. To estimate feasible eligibility, recruitment and refusal rates, and 3- and 6-months follow-up rates;
5. To measure key outcome domains (that is, for completion rates, missing data, estimates, variances and 95% confidence intervals for the difference between the control and intervention groups) for

patients including clinical indicators and patient-reported measures of social satisfaction; health-related quality of life; and mood;

6. To synthesize data to inform the sample size of a definitive trial;

7. To determine the acceptability (and factors influencing this) of financial incentives as a method to encourage behaviour change, their pricing and factors influencing this.”[38]

Explanation

While many aspects of feasibility may be related to each other, an articulation of specific objectives enables the reader to understand the main areas of uncertainty to be addressed in the pilot trial and provides a working structure for presenting the methods and results in relation to these objectives. In addition, a comprehensive list of objectives enables other researchers to learn from and adopt similar approaches in their own studies.

It may be beneficial to separate the objectives into primary objectives (often those on which decisions about progressing to a future definitive trial may be made) and secondary objectives, as in Example 1, where feasibility objectives are primary and questions related to patient-centred outcomes are treated as secondary. Because it is not always necessary to collect patient-centred outcome data, it is important to give the rationale for collecting such data. For example, the purpose may be to ensure that certain data can be collected, including from specific patient groups (e.g. the elderly as in Example 1), or to ensure that difficult to measure concepts such as lifestyle behaviour change can be assessed appropriately in the future definitive trial (Example 2). It may also be informative to state explicitly which objectives will be answered using quantitative methods and which using qualitative methods as in Example 2.

In Example 2 the list of quantitative objectives are quite informative, but they are taken from the published study protocol. In the published pilot trial the objectives contained far less detail: “*The Healthy Hospital Trial is a single-center, randomized controlled, 2-arm, parallel-group, unblinded feasibility trial that was conducted on 2 cardiology wards at the Leeds Teaching Hospitals Trust. Its primary aim was to explore the feasibility of individualized lifestyle referral assessment, estimate the rate of recruitment, and explore the feasibility of collecting the data and follow-up of participants to inform the sample size of a definitive trial. A secondary aim was to test the concept that an individually tailored assessment improves uptake of lifestyle change compared with usual assessment. The trial protocol has been published elsewhere.*”[39] We recommend putting detailed individual objectives into the pilot trial report itself so that readers can more easily judge the extent to which these have been fulfilled by the study.

Inclusion of an objective to test a hypothesis of effectiveness (or efficacy) is not recommended (see Box 2). However, other kinds of hypotheses may be tested, such as when using an interim or surrogate outcome to address *potential* effectiveness.[40] (See also Scope section). However, a trial should always be adequately powered for any hypothesis test and in a pilot trial it should be clearly stated that the objective is to assess *potential* effectiveness. If tests are carried out without adequate power (as in reality they sometimes are) they should certainly be viewed as secondary and a cautionary caveat included in the discussion.[21]

Methods

Item 3a

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

Extension for pilot trials: description of pilot trial design (such as parallel, factorial) including allocation ratio

Example

“We conducted a parallel-group randomised controlled pilot trial... An unequal randomisation of 2:1 vs 1:1 was chosen to provide experience delivering the hydration intervention to more patients.”[41]

Explanation

The design of any study should be described, be it a definitive trial or a pilot trial. It is not uncommon for pilot trials to adopt ratios other than the usual 1:1 randomisation ratio. 1:1 randomisation provides the greatest power for testing effectiveness in, for example, a future definitive trial. However, a pilot trial commonly involves new, not established, interventions and one of the aims might then be to gain experience in delivering the intervention, in which case it is often better to have as many participants receiving the intervention as is feasible.

Item 3b

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

Extension for pilot trials: important changes to methods after pilot trial commencement (such as eligibility criteria), with reasons

Example

“After randomly assigning 11 patients (5 to standard care), we recognized that patients assigned to standard care were receiving early surgery because, having achieved accelerated medical clearance, they were put on the operating room list. We therefore amended the protocol to randomly assign patients immediately on diagnosis; only those assigned to early surgery received an expedited medical assessment”.[42]

Explanation

Pilot trials are exploratory in nature and so those conducting them should be able to modify the methods if a potential problem becomes apparent. In the case of Buse et al,[42] the original protocol specified that patients had to have medical clearance for a rapid surgery before randomisation, but this led to contamination of the control group since some patients in this group were put on the surgical list for rapid surgery because it had been ascertained that they were suitable for rapid surgery. The revised protocol randomised first and then assessed suitability for accelerated access. Thus the pilot potentially improved the design of the trial that was to follow. It is important to document all changes and give reasons why the changes were made. The example describes changes to the timing of randomisation but there may also be changes to other aspects of the trial such as the treatment regime, eligibility criteria, or outcome variables.

Item 4a

Standard CONSORT item: eligibility criteria for participants

Example

“Thirty-one sequential eligible people with HD [Huntington’s disease] were recruited from the specialist HD clinics in Cardiff, the United Kingdom, and Oxford, the United Kingdom, between March 2011 and November 2011. Inclusion criteria were (1) diagnosis of HD, confirmed by genetic testing and neurological examination, (2) ability to walk independently as primary means of mobility, (3) willing to travel to the exercise center for the intervention, (4) capacity to give informed consent, (5) Unified Huntington’s Disease Rating Scale Total Motor Score (UHDRS-TMS) and Total Functional Capacity (TFC) of at least 5/124 and 5/13, respectively, from last clinic visit, and (6)

maintenance of a stable medical regimen for 4 weeks prior to initiation of study and considered by the recruiting clinician as able to maintain a stable regimen for the course of the study. Participants were not eligible if they (1) had a history of additional prior major neurological condition such as stroke, (2) had an orthopedic condition that limited mobility, (3) demonstrated uncontrolled psychiatric symptoms, (4) were pregnant, (5) demonstrated any contraindication to exercise, or (6) were involved in any interventional trial or within 3 months of completing an interventional trial.”[43]

Explanation

Readers may wish to know how the results of the trial are likely to apply to both the future definitive trial and other future trials with similar participants in similar settings. A variety of participants (patients, doctors, assessors, caregivers, managers etc.) may provide data to address objectives. For example, in a study in nursing homes, residents were interviewed to seek views on acceptability of the intervention, while nurses participated in focus groups to elicit views on randomisation or adherence to treatment protocol.[31] Eligibility criteria should be specified for each set of participants included in a pilot trial. The details provided must be specific enough to identify the clinical and any other populations and the setting from which they were recruited and to confirm that legal issues were complied with, such as having capacity to give informed consent. Details should be sufficient to allow other researchers to interpret, learn from and use the information provided.

Item 4b

Standard CONSORT item: settings and locations where the data were collected

Example

“High-risk adolescents were recruited from three sources: (1) a sample of 205 offspring of BP parents between 12 and 18 years of age enrolled in the NIMH-funded Bipolar Offspring Study at the University of Pittsburgh (BIOS, PI: Birmaher); (2) offspring of adults receiving treatment for BP at Western Psychiatric Institute and Clinic (WPIC); and (3) siblings of youth receiving treatment for BP at the Child and Adolescent Bipolar Services clinic (CABS) at WPIC.”[44]

Explanation

The settings for recruiting patients and collecting data must be specified so that readers can judge the applicability (generalisability) of the findings to other trials as well as the future definitive trial. Authors should also make clear whether any pilot sites have any particular features; for example, organisational features, characteristics that predispose the site to early adoption of new schemes, or specific relationships with the authors, that could affect recruitment, consent, and follow-up. This is because these features may not be replicable in other sites and hence in future trials. As with 4a, details must be sufficient to allow other researchers to interpret, learn from and use the information.

Item 4c

Extension for pilot trials: how participants were identified and consented

Example

“Between May and October 2013, clinical staff at participating gastroenterology outpatient clinics scanned and identified potential participants that met the study inclusion criteria. Then, either study invitation packs were sent to patients with researchers contact details or patients seen consecutively in clinics were approached with the study information. All study information was co-designed with patients from the patient-involvement group. Interested participants then registered their interest with the researcher by telephone or email. This was followed up with a screening visit with the researcher and then informed written consent was obtained.”[45]

Explanation

This is a new item. It is especially important to report details of identification and consent in a pilot trial to allow the feasibility of the recruitment methods to be assessed. The way participants are identified and approached should be described in detail (e.g. by advertisement, or selection from medical records or another dataset) to enable readers to understand the generalisability (applicability) of the results. This may be of particular importance for scaling up for the future definitive trial, as well as being informative for other future trials. In addition, it is important to know of any specific aspects which might not be easy to implement in the future definitive trial. Furthermore, there is sometimes a view that pilot trials do not need to be as rigorous in their processes as other trials so it may be particularly important in these trials to demonstrate rigorous and ethical identification and recruitment processes. If details of the way participants were identified and consented are already published in a protocol, then this should be clearly referenced.

Item 5

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

Example

“Intervention (EXERcise or STRETCHing)

The amount of time required for participating in the exercise activities was the same for the EXER group and the STRETCH group. The only difference was the amount of energy expended during the activity. At the first session, the exercise trainer explained the procedures for the respective intervention (EXER or STRETCH), showed them the equipment available for the exercise or stretch sessions, and the coordinator familiarized the participant with the Actical device. The first two weeks required a minimum of 3 sessions at CI [Cooper Institute] for the trainer to teach them how to use the equipment and complete the exercise or stretch routines. Following the first 2 weeks, participants began doing their exercise program at home or other location (gym, park, etc.), and only had to come to CI once a week for an exercise session. Each EXER/STRETCH session averaged about 30-40 min.

EXERcise Intervention

Supervised exercise sessions at the Cooper Institute (CI) for the participants began by using the treadmills or stationary cycles. The CI trainers also taught patients how to complete home-based exercise sessions (e.g., choice of Wii Sports and Fit, jazzercise, jogging, weight training based on their preferred exercise) that were unsupervised workouts at the patient's home or in the community. The duration of each session generally was the time required to reach 1/3 or 1/4 of the total weekly caloric expenditure. There was a progression to the assigned exercise dose in the first few weeks that got them up to their minimum of 12 kilocalories/kilogram/week (KKW) energy expenditure (e.g., 8 KKW first week, 10 KKW second and 12 KKW by the third week). Participants exercised three times per week.

STRETCH Intervention

The stretch group spent approximately the same amount of time, but at energy expenditures of less than 4 KKW per session. After two weeks of three sessions at CI they moved to once a week at CI and two home-based sessions. A 5-10 minute stretching warm-up period included stretches that exercise the major muscle groups of the body. The series included such traditional “warm-up” stretches as: stretches of the gluts, inner thigh, calves and ankles, Achilles tendon, hamstring stretches, shoulder rolls forward and back, shoulder shrugs, isometrics for the neck hugging knees into the chest, moving forehead to right knee, then to left, then to both, and use of the pelvic tilt. An additional 10-15 minutes consisted of moving on to right and left calf stretches, quad stretches, and then to a series for the arms, hands, fingers, wrist, biceps/triceps, shoulders and back. All of the exercises were designed to be done slowly, emphasizing proper alignment, and rest periods to minimize overall

physical exertion while obtaining general flexibility, and most importantly controlling for contact time with trainers and any social facilitation from participating in such activities. We had a different set of low level/low intensity routines for each of the 12 weeks to minimize boredom with the routines”[46]

Explanation

If the pilot trial is to inform the future definitive trial the authors should report exact details of the treatment given to all study groups, and if one group is treatment as usual this should also be described thoroughly. Details should include who administered the treatment, as well as what it comprised and how often and where it was delivered. The TIDieR guidelines should be followed and the checklist completed.[47] If there are any changes to the details of the treatments given in any group, this must be reported (see item 3b).

Item 6a

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

Extension for pilot trials: completely defined pre-specified assessments or measurements to address each pilot trial objective specified in 2b, including how and when they were assessed

Example

“*Acceptability* and *demand* were assessed in terms of the usage and repeated usage of the intervention by the patients in the trial indicated by logged user statistics. The interventions’ *practicability* was considered as the ability to log in and occurrence of constraints in delivery and was assessed in terms of the percentage of users in adolescents and professionals, its bounce percentage (percentage of login-errors) and other login-problems. The bounce-percentage was logged and participants were asked to report login-errors. *Integration* was assessed in terms of the extent to which our web-based intervention promotes care that was consistent with recognized standards of diabetes care for adolescents including those published by the International Diabetes Federation (IDF) in collaboration with the International Society for Pediatric and Adolescent Diabetes (ISPAD) and the American Diabetes Association (ADA; 3, 33); see also Appendix 1.”[48]

Explanation

In a definitive trial the primary objective is to assess the effect of an intervention or therapy. Investigators are primarily interested in response variables or outcomes that enable them to do that and a clear articulation of pre-specified outcomes is required to guard against bias in the assessment of this effect. In a pilot trial, however, objectives should relate to feasibility (see Box 2 and item 2b) and any measurements or assessments made should enable these objectives to be addressed. To ensure the pilot trial meets its objectives, measures or assessments should be defined to address each separate objective or research question. In the example, objectives were to assess acceptability, demand, practicability and integration. The authors list the measures used for each of these.

Variables that might be considered primary and secondary outcomes for the future definitive trial may be measured in a pilot trial to assess response, completeness or validity. The appropriate measures or assessments would then be response rates, completion rates or measures of validity. Sometimes investigators may wish to measure surrogate outcomes (see example in 7b), variables on the causal pathway of what may eventually be the primary outcome in the future definitive trial, or outcomes at early time points, in order to assess the potential for the intervention to affect likely outcomes in the future definitive trial (see item 2b).

Item 6b

Standard CONSORT item: any changes to trial outcomes after the trial commenced, with reasons

Extension for pilot trials: any changes to pilot trial assessments or measurements after the pilot trial commenced, with reasons

Example 1 (change to assessment time period)

“Our outcome measures examined uptake and cessation because we hoped that our intervention would affect uptake by referring more people and the success rate of those referred by supporting adherence to treatment...The intervention had two distinct phases so, although not planned in the protocol, we examined uptake of services and 4-week quit rates by trial arm, in these two periods.”[49]

Example 2 (change to measurement instrument)

“We defined... initiation of change as participation in a formal program or a self-directed program that was intended to result in change either in diet, physical activity, smoking, or alcohol consumption at any time (binary). In our published protocol, we had proposed 4 categories of change, but we found it difficult to distinguish between “persisted” and “maintained” in the qualitative follow-up interviews; hence, we combined persistence and maintenance of change in 1 category.”[39]

Explanation

An assessment or measure may change during a pilot trial because the change enables investigators to glean more information about the operation of the intervention (as in Example 1) or for reasons of acceptability or practicability (Example 2). In Example 2 it became impractical to use a measurement instrument with four categories when it was identified that researchers could not distinguish between two of the categories. In the interest of full reporting and because of the usefulness of such information to others working in the same field, all such changes should be reported.

Item 6c

Extension for pilot trials: if applicable, pre-specified criteria used to judge whether, or how, to proceed with future definitive trial

Example

“Feasibility (delivery) and acceptability (uptake) of the DECISION+ program were the main outcome measures of this pilot trial. Investigators had established a priori threshold for specific feasibility and acceptability criteria. These were the following: (a) the proportion of contacted FMGs [Family medicine groups] participating in the pilot study would be 50% or greater, (b) the proportion of recruited family physicians participating in all three workshops would be 70% or greater, (c) the mean level of satisfaction from family physicians regarding the workshops would be 65% or greater, and (d) the proportion of missing data in each completed questionnaire would be less than 10%.”[33]

Explanation

This is a new item. The purpose of a pilot trial is to assess the feasibility of proceeding to the next stage in the research process. In order to do this investigators need some criteria on which to base the decision about whether to proceed or not. The next stage in the research process will normally, though not always, be the future definitive trial.

The UK National Institute for Health Research requires that pilot or feasibility studies have clear criteria for deciding whether or not to progress to the next stage: “We expect that when pilot or feasibility studies are proposed by applicants, or specified in commissioning briefs, a clear route of progression criteria to the substantive study will be described. Listing clear progression criteria will apply whether the brief or proposal describes just the preliminary study or both together. Whether

preliminary and main studies are funded together or separately may be decided on practical grounds.”[50]

In many pilot studies, however, such criteria may be best viewed as guidelines rather than strict thresholds that determine progression. In the Example, the authors found that only 24% of the family medicine groups (FMGs) agreed to participate. They state “Not reaching the pre-established criteria does not necessarily indicate unfeasibility of the trial but rather underlines changes to be made to the protocol”.[33] Clearly it is important to discuss whether such changes to protocol are likely to be feasible and this discussion may often benefit from input independent of the trial team, for example from the trial steering committee. This would be a reason for having such a committee in place for a pilot trial. Bugge et al have recently provided further guidance on decision making following a pilot trial.[51]

In addition to the possibility of making changes to the trial protocol, investigators should also be aware that estimates of rates in pilot trials may be subject to considerable uncertainty so that it is best to be cautious about setting definitive thresholds that could be missed simply due to chance variation.[40] In fact it is becoming increasingly common for investigators to use a traffic light system for criteria used to judge feasibility, whereby measures (e.g. recruitment rates) below a lower threshold indicate that the trial is not feasible, above a higher threshold that it is feasible, and between the two that it may be feasible if appropriate changes can be made.

Item 7a

Standard CONSORT item: how sample size was determined

Extension for pilot trials: rationale for numbers in the pilot trial

Example 1 (rationale based on assessment of practicalities and estimating rates)

“..... Since this was a pilot study, a sample size calculation was not performed. The researchers aimed for 120 participants because it was felt this would be a large enough sample to inform them about the practicalities of delivering several self- management courses led by patients with COPD, recruitment, uptake, and attrition.”[52]

Example 2 (rationale based on percentage of number required for future definitive trial)

“As this is a feasibility study a formal sample size calculation is not required, but we estimated the number of participants required as around 10 % of the number required for the Phase 3 trial. The sample size calculation for the Phase 3 trial suggests we need to recruit 1665 participants. Given the participant population, a high level of attrition may be anticipated. We therefore aim to recruit 200 participants to the feasibility trial to inform the design and sample size of the Phase 3 RCT.”[53]

Explanation

The criterion of congruency between the objectives and the sample size holds as true for a pilot trial as for any study. Many pilot trials have key objectives related to estimating rates of acceptance, recruitment, retention or uptake (see item 2b for examples). For these sorts of objectives numbers required in the study should ideally be set to ensure a desired degree of precision around the estimated rate. Additionally, for pilot trials where the key objective focuses on the acceptability/feasibility of introducing the intervention it may be useful to consider how many sites are needed since the acceptability/ feasibility of introduction can sometimes be site dependent. In Example 1, the authors state their reason for choosing their required sample size in relation to estimating rates and to exploring practicalities of implementing the intervention. They could, however, have provided stronger justification for their chosen number, for example likely recruitment or attrition rate and desired precision around these rates, so that the reader (and funder) has more grounds for believing the trial could achieve its objectives beyond a feeling.

Most methodological papers that focus on recommendations about sample size requirements for pilot trials assume that the main aim of such a trial is to estimate a quantitative measure such as the variance (or standard deviation) of an effect size to inform the sample size calculation for a future definitive trial. Methods focus on the precision with which such estimates can be obtained. There are a number of relevant papers.[54-56] Amongst these, a recent paper by Whitehead et al suggests that size of a pilot trial should be related to the size of the future definitive trial.[57] For such a trial designed with 90% power and two-sided 5% significance, they recommend pilot trial sample sizes per treatment arm of 75, 25, 15 and 10 for standardised effect sizes that are extra small (0.1), small (0.2), medium (0.5) or large (0.8), respectively.

Example 2 illustrates another approach that uses a sample that is a certain percentage of the expected future definitive trial size. The authors reference the paper by Cocks and Torgerson which is based on using a sample size under which a one-sided 80% confidence interval for the effect size will exclude the minimum clinically important difference if the null hypothesis is true.[58] This is a similar calculation to that used in estimating sample size needed for efficacy or effectiveness but allows for additional uncertainty in the resulting effect size estimate, thus effectively assessing *potential* effectiveness. If an objective is to assess *potential* effectiveness using a surrogate or interim outcome, investigators will need to use a standard sample size calculation to ensure there is adequate power. However, this type of objective is rare in pilot trials.

Item 7b

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

Example

“The board members were instructed to perform an interim analysis after 60 patients had been enrolled, at which point they could recommend stopping the trial if an overwhelming effect was detected on the basis of the critical significance level ($P \leq 0.02$), as adjusted for the Lan–DeMets alpha-spending function with Pocock boundary”[20]

Explanation

Since pilot trials are small, it is uncommon for them to define criteria for early stopping, but if they do these should be reported. The Example is a pilot trial testing a surrogate outcome. There was considerable uncertainty about the variability of this outcome measure, and so the authors calculated a conservative sample size, but included an interim analysis after recruiting 60 patients, in case their a priori estimates were too large and they had enough information at that stage to inform subsequent trials.

Item 8a

Standard CONSORT item: method used to generate the random allocation sequence

Example

“Participants were randomly allocated to the intervention ‘MBCT group’ or ‘wait-list control group’... Random allocation was computer generated.”[45]

Explanation

Randomisation induces unpredictability in the allocation of each unit of randomisation. This is an important element of ensuring an unbiased treatment effect in RCTs evaluating effectiveness or efficacy because, in the long run, it ensures balance in characteristics between intervention groups. In a pilot trial, the soundness of the randomisation method may not directly influence robustness of the

pilot trial results which are not focused on estimates of effectiveness or efficacy, but a clear description of the process of randomisation is still important for transparent reporting.

In addition, in some pilot trials one of the objectives may be to assess the feasibility of randomisation; it is also important, therefore, that details are reported. If assessing feasibility involves more than one method being used to generate a random allocation sequence, each method should be described adequately.

Item 8b

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

Extension for pilot trials: type of randomisation(s); details of any restriction (such as blocking and block size)

Example 1 (example with blocking)

“Participants were randomised in block sizes of three by computer-generated randomisation to the hydration group or the control group (2:1), stratified by gender.”[41]

Example 2 (two different types of randomisation)

“In addition to random allocation to one of the three treatment arms, we used a 2×2 factorial design to distribute practices and participants across two trial design factors: cluster versus individual allocation and systematic versus opportunistic recruitment (see Figure 1). We randomly assigned 24 practices (8 practices in each of 3 geographical regions (Bristol, Devon and Coventry)) in a 3:1 ratio to cluster (practice) allocation or individual allocation, and in a 1:1 ratio to opportunistic or systematic recruitment. The differential allocation ratio with regard to randomisation method was due to the need to ensure even numbers of practices and participants in each of the three arms across the cluster randomised practices.”[59]

Explanation

The type of randomisation, including whether simple or restricted, should be reported.

For practical reasons simple randomisation is sometimes used in pilot trials even when restricted randomisation is expected to be used in the future definitive trial, and if this is the case this needs to be described.

Restricted randomisation is particularly useful in small trials evaluating the effectiveness of an intervention to ensure balance in certain characteristics between intervention and control groups (see main CONSORT Statement).[2, 60] In pilot trials restricted randomisation may be used to mimic the type of randomisation expected in the future definitive trial or, if it is deemed important, to have balanced groups even if it is not expected to be used in the future definitive trial. In Example 1 stratified randomisation, using blocking, was used.

One of the objectives of a pilot trial may be to assess the feasibility of randomisation; it is therefore possible that different types of randomisation may be tried, as in Example 2.[59]

Item 9

Standard CONSORT item: 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

Example

“Allocation...was implemented using an automated telephone randomization service provided by the Bristol Randomized Trials Collaboration to ensure concealment from clinical staff undertaking recruitment.”[61]

Explanation

Ensuring allocation concealment is a cornerstone of good randomised trial design. This mechanism performs a key function in minimising bias by preventing foreknowledge of treatment assignment which could influence those who enrol participants. In a future definitive trial a single mechanism will be used to conceal allocation. However, in a pilot trial the main purpose of using an allocation concealment mechanism is to establish the feasibility of the mechanism. If there is considerable uncertainty about the mechanism to be used, more than one mechanism may be tried in the pilot trial. We would expect this to be extremely rare but when it does occur the details of each mechanism tried should be fully described.

Item 10

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

Example 1 (who generated the random allocation sequence)

“An independent statistical consultant set up the web-based randomization process to assign eligible participants to intervention or control groups by remote allocation, using permuted blocks of sizes 2 and 4. No one directly involved in the project had access to allocation codes.”[21]

Example 2 (who enrolled participants, who assigned participants to interventions)

“Eligible children and their families were identified by the clinician conducting the assessment. If the child and his or her family were willing to find out more about the study a researcher contacted the family and arranged to visit them at a convenient location (usually at home)...Those willing to take part were randomized to receive either specialist medical care or to specialist medical care plus the Phil Parker Lightning Process (LP). Allocation ... was implemented ... by the Bristol Randomized Trials Collaboration”[61]

Explanation

It is important that the pilot trial confirms that allocation concealment can be implemented in a way that could be replicated in the future definitive trial. This involves knowing who generated the randomisation sequence and who enrolled and assigned participants.

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 1 (blinding of multiple people)

“Patients, families, ICU [intensive care unit] staff, ultrasound technologists, and research personnel were all blinded to drug allocation. The study pharmacist at each center was the only person who was not blinded.”[62]

Example 2 (placebo-controlled)

“A synbiotic formulation (Synbiotic 2000®) containing 4 strains of probiotic bacteria (10^{10} each) plus 4 nondigestible, fermentable dietary fibers (2.5 g each) was provided each day, versus a fiber-only placebo formulation.”[63]

Explanation

In the future definitive trial investigators will want to reduce the chance of a biased result as much as possible. Blinding is seen as one of the most effective ways of doing this, at least in trials where it is feasible to blind (see main CONSORT for details). The main purpose of a pilot trial is to assess the *feasibility* of methods, including those to reduce bias. In some pilot trials it may be useful to report the method of blinding in detail, as in Example 2, to help readers who may wish to replicate the method in future trials.

It is tempting in a pilot trial to try and assess the success of blinding by asking individuals whether they felt they were blinded or not. This was done, for example in Arnold et al.[64] However, this is not recommended because evidence suggests that results of doing this largely reflects the effectiveness of the intervention rather than anything else.[65]

Item 11b

Standard CONSORT item: if relevant, description of the similarity of interventions

Example

“Each study drug infusion was administered using a standard volume-based rate escalation protocol preceded by the administration of 100 mg of hydrocortisone intravenously, 50 mg of diphenhydramine orally or intravenously, and 650 mg of acetaminophen orally to minimize infusion-related reactions and avoid unblinding.”[64]

Explanation

If blinding is done by creating a placebo, it is important in trials assessing the effect of an intervention to detail what features of the placebo were made similar to the active intervention (usually a drug), for example, appearance, taste, smell, method of administration. However, many of the interventions described in pilot trials are not drug interventions. Nevertheless it remains important to describe what was done to try and ensure the intervention and control arms received identical treatment aside from the active ingredient where this is possible. It is equally important to note that for complex interventions it may not be possible or feasible to blind certain individuals to allocation using these types of methods.

Item 12a

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

Extension for pilot trials: methods used to address each pilot trial objective whether qualitative or quantitative

Example 1 (Descriptive and narrative reporting)

“The feasibility outcomes were reported descriptively and narratively. For the clinical endpoints, only descriptive statistics, mean (standard deviation) for continuous outcomes and raw count (%) for categorical outcomes, were reported.”[66]

Example 2 (Confidence intervals)

“For the primary outcomes, the feasibility criteria were the recruitment rate and duration, retention rate, safety, adverse events, compliance, acceptability of the interventions and fatigue...The recruitment rate, consisting of the eligibility and consent rate, was calculated with 95 % CI.... Medians (range) were reported for ordinal data (fatigue), mean (95 % confidence interval (CI)) were reported for continuous data (walking speed and walking distance) and raw count (number, %) was

reported for nominal data. Due to the nature of this feasibility study, it was decided not to conduct any efficacy statistical tests on the walking and fatigue data”[67]

Explanation

A range of methods can be used to address the objectives in a pilot trial. These need not be statistical. Providing information about the methods used ensures that findings can be verified based on the description of the analyses used. The primary focus is on methods for addressing feasibility objectives. These methods are often based on descriptive statistics such as means and percentages but may also be narrative descriptions (Example 1). Typically, any estimates of effect, using participant outcomes as they are likely to be measured in the future definitive trial, would be reported as estimates with 95% confidence intervals without P-values—since, as already noted, pilot trials are not powered for testing hypotheses about effectiveness.

Item 12b

Standard CONSORT item: methods for additional analyses, such as subgroup analyses and adjusted analyses

Extension for pilot trials: not applicable

Explanation

In a definitive trial, analyses of a difference in treatment effect for subgroups or analysis of outcomes adjusted for baseline imbalance may provide useful information. However, such analyses in a pilot trial are not applicable because the primary focus is not on determining treatment effects or differences in effects between subgroups. Rather, the focus is on assessing feasibility or piloting procedures to inform the design of the future definitive trial.

Results

Item 13a

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

Extension for pilot trials: for each group, the numbers of participants who were approached and/or assessed for eligibility, randomly assigned, received intended treatment, and were assessed for each objective

Example

Presented in Figures 2 and 3.[2, 68]

Explanation

As for other trials we recommend a diagram for communicating the flow of participants in a pilot trial. A flow diagram is a key element of the CONSORT Statement and has been widely adopted.[69] A review of RCTs published in five leading general and internal medicine journals found that reporting was considerably more thorough in articles that included a diagram of the flow of participants through a trial, as recommended by CONSORT.[69] A complete CONSORT flow diagram also reduces the time for readers to find the essential information to assess the reliability of a trial. It is also likely to improve the availability of some information which otherwise might not be reported.

Information required to complete a CONSORT flow diagram includes the number of participants evaluated for potential enrolment into the trial, the numbers of participants who were randomly

assigned to each intervention group, received treatment as allocated, completed treatment as allocated, and were analysed for the primary outcome, with numbers and reasons for exclusions at each step.[2, 60]

For pilot trials it may also be important to know the number of participants who were approached (or screened) before being assessed for eligibility for potential enrolment into the trial. This ensures that readers can assess external validity and how representative the trial participants are likely to be compared to all eligible participants.[70] Additionally, for pilot trials it is important to know how many participants were approached before being evaluated for potential enrolment in the trial and how easy it was to recruit them, in order to assess the potential for enrolment for the future definitive trial and other future trials. In some cases where these elements are a major focus of a pilot trial more information may be needed in the flow diagram (Figure 2).

For pilot trials it is appropriate to report the number of participants assessed for each pilot trial objective, rather than the number analysed for the primary outcome (as would be the case for the future definitive trial). If there are a limited number of pilot trial objectives then all should be listed and results for each objective should be reported in the flow diagram. If there are multiple pilot trial objectives, then agreement should be reached a priori about which are the most important to decide whether to proceed to a future definitive trial, and only these objectives should be reported in the flow diagram. A template for a CONSORT flow diagram for pilot trials, including presentation of results for different objectives is shown in Figure 3. The exact form and content may, however, vary in relation to the specific features of the trial. Authors should ensure that their flow diagram matches the key objectives as far as possible.

Item 13b

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

Example

“All 16 patients randomised to the Symptoms Clinic attended the first appointment and 11 completed either three or four appointments. Of the remainder, two were clearly improving at the time they were seen and agreed to early discharge; two found further attendance difficult after a second appointment and one declined any further contact after the first appointment. Several patients randomised to usual care expressed some disappointment at the time of their allocation, although follow-up response rates were comparable between the two groups.”[71]

Explanation

For some randomised trials the flow of participants through each phase of the trial can be relatively straightforward to describe, particularly if there were no losses to follow-up or exclusions. However, in more complex trials, it may be difficult for readers to identify whether and why some participants did not receive the treatment as allocated, were lost to follow-up, or were excluded.[72] In a definitive trial this information is crucial for interpreting generalisability as participants who are excluded after allocation are unlikely to be representative of all participants in the study.[73] In a pilot trial, this information could be used to judge potential generalisability of the future definitive trial but also to assess the acceptability of an intervention to participants and to aid planning of the future definitive trial and other trials in similar settings.

Item 14a

Standard CONSORT item: dates defining the periods of recruitment and follow-up

Example

“Patient enrolment started in August 2003 and was completed in October 2005.”[74]

Explanation

It is important to report dates for all studies for transparency. An added rationale for pilot trials is that factors such as disease definitions, treatment options and reimbursement plans that could affect the future definitive trial may have changed between the date that the pilot trial is conducted and the date the future definitive trial starts. The availability of different treatments outside the trial can also change and may make a difference to people’s willingness to be randomised. Thus recruitment to a pilot trial could be easier, or more difficult, than recruitment to the future definitive trial. In addition, knowing the length of time over which the study took place may be important for planning the future definitive, and other, trials.

Item 14b

Standard CONSORT item: why the trial ended or was stopped

Extension for pilot trials: why the pilot trial ended or was stopped

Example 1 (stopped without reaching intended recruitment but provided sufficient data)

“Use of a patch containing heat-labile toxin from *Escherichia coli* against travellers’ diarrhoea: a phase II, randomised, double-blind, placebo-controlled field trial...Enterotoxigenic *Escherichia coli* (ETEC) is a major cause of travellers’ diarrhoea...We designed this phase II, double-blind, randomised placebo-controlled study to investigate the epidemiology of natural infection with ETEC in placebo recipients with a planned enrolment of 300 individuals, at a placebo-to-LT patch ratio of 2:1...the sample size was intended to establish an estimated rate of natural infection with ETEC to aid in the determination of sample size for future studies, and was not anticipated to provide power to detect significant differences between placebo and LT recipients. The study was halted when enrolment reached 201, because the planned interval for conduct had been exceeded, and it was thought that a placebo group greater than 100, although less powerful than the original 200, would be sufficient to assess the ETEC attack rate in placebo recipients...24 (22%) of 111 placebo recipients had diarrhoea, of whom 11 (10%) had ETEC diarrhoea.”[75]

Example 2 (stopped at end of recruitment but did not provide sufficient data)

“Recruitment rates were lower than expected which led to the study being expanded to further areas and opened to self-referral via advertisement. However, because of better management of hypertension due to changes in the UK Quality and Outcomes Framework guidelines for blood pressure treatment, few eligible patients were identified and the study closed at the end of the recruitment period, with 13 participants consenting, but 12 failing screening resulting in one recruited participant.”[76]

Explanation

When pilot trials end or are stopped, it is important to state why because this may affect the feasibility of the future definitive trial. In Example 1 the investigators had run out of time, and thought they would have sufficient participants to estimate the rate of diarrhoea, so as to inform future studies. It is not uncommon for changes in the clinical environment to occur, leading to fewer patients with unmanaged disease, and this can lead to major studies, not just pilot studies, failing to recruit. This illustrates a benefit of a pilot study to assess the likely accrual for a future definitive trial. In Example 2 the reason for stopping was simply a failure to recruit, and the reasons for this are clearly stated. Other potential reasons for stopping include the intervention being impossible to implement, other studies indicating that the research has become irrelevant, and difficulties with funding. It is also helpful to know who made the decision to stop early. In definitive RCTs a data monitoring committee

often makes recommendations to stop the trial. It may not be necessary to have data monitoring committees for all pilot trials but investigators should give some thought as to how the decision to stop should be made.

Item 15

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

Example

Table 3: Example of baseline information for each group (from Seebacher et al.)[67]

Parameter	Group A	Group B	Group C
	Music cued motor imagery	Metronome cued motor imagery	Control group
	<i>n</i> = 10	<i>n</i> = 10	<i>n</i> = 10
Females to males	10:0	7:3	5:5
Age (years) ^a	47.3 (38.4, 56.2)	41.8 (34.8, 48.8)	46.1 (39.8, 52.5)
EDSS ^b	3 (1.5, 4.5)	2.5 (1.5, 4.5)	2.5 (1.5, 4.0)
MFIS total score ^b	35 (3, 67)	32 (17, 50)	33.5 (0, 48)
Participants with fatigue (MFIS total score ≥ 38)	4/10	2/10	4/10
T25FW (s) ^a	6.1 (4.5, 7.6)	5.4 (4.5, 6.2)	5.2 (4.3, 6.1)
6MWT (m) ^a	453.1 (365.0, 541.1)	428.2 (352.8, 503.6)	484.7 (399.5, 569.8)

EDSS Expanded Disability Status Scale, MFIS Modified Fatigue Impact Scale, T25FW Timed 25-Foot Walk, s seconds, 6MWT 6-Minute Walk Test, m metres

^a Mean (95 % confidence interval)

^b Median (range)

Explanation

In an RCT evaluating the effect of an intervention a table of baseline characteristics is important to indicate whether there are any differences between intervention groups that could affect the face validity of the trial. In a pilot trial the number of participants is likely to be smaller and baseline imbalances may therefore be more likely. Similar to a definitive trial, imbalance does not suggest bias, and in any case, bias is not an issue in the same way as it is in a definitive trial because an assessment of the effect of an intervention is not the primary concern. Nevertheless, baseline data are important to aid interpretation of the results, including a consideration of generalisability, and a table is the best way of doing this.

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 pilot trials: for each objective, number of participants (denominator) included in each analysis. If relevant, these analyses should be by randomised group

Example 1 (number of sites contacted)

“A research assistant made 41 introductory phone calls to contact the medical directors of the 21 eligible FMGs [Family medicine groups] over a four-week period. One director could not be contacted. Information leaflets were faxed to the 20 contacted FMGs.”[33]

Example 2 (number of practitioners taking part within sites)

“Out of the 52 eligible family physicians working in the five participating FMGs [Family medicine groups], 39 (75%) agreed to participate in the study.”[33]

Explanation

In RCTs evaluating the effect of an intervention, outcomes are usually measured on participants, and therefore denominators are numbers of participants. However, because of the potential variety of objectives in a pilot trial, the denominators for measures that assess feasibility according to these objectives may be organisations, health practitioners, patients or in some cases episodes or events. In the interests of simplicity we have not changed the word “participants” in this item but the item should be interpreted in the light of the particular objective and associated measure or assessment. The two examples above are taken from the same trial. One objective was to assess the feasibility of recruitment. Participants for that objective are FMGs (Example 1) and family physicians (Example 2). The denominators of 21(FMGs) and 52 (family physicians) indicate numbers approached and therefore the effort involved in recruiting. In this example analysis by randomised group is not relevant.

Item 17a

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

Extension for pilot trials: for each objective, results including expressions of uncertainty (such as 95% confidence interval) for any estimates. If relevant, these results should be by randomised group

Example 1 (feasibility outcome)

“The ABSORB [A bioabsorbable everolimus-eluting coronary stent system for patients with single de-novo coronary artery lesions] study aimed to assess the feasibility and safety of the BVS [bioabsorbable everolimus-eluting stent] stent in patients with single de-novo coronary artery lesions...Procedural success was 100% (30/30 patients), and device success 94% (29/31 attempts at implantation of the stent).”[77]

Example 2 (proposed outcome in future definitive trial)

“Rates of initiation of lifestyle change also favoured the individualized assessment arm but less clearly. At 3 months, 75% of the individualized assessment arm and 68% of the usual assessment arm had initiated changes in their lifestyle (unadjusted odds ratio, 1.38 [95%CI, 0.55 to 3.52]). At 6 months, the percentages were 85% and 75%, suggesting increased initiation of change over time in both arms, with the gap widening slightly (unadjusted odds ratio, 1.86 [95% CI, 0.64 to 5.77]).... Wide CIs again point to the degree of uncertainty around this conclusion”[39]

Explanation

It is important that the reported results of a pilot trial reflect the objectives. Results may include, for example, recruitment, retention or response rates or, other sorts of rates as in Example 1 above. Because the sample size in a pilot trial is likely to be small, estimates of these rates will be imprecise and this imprecision should be recognised, for example by calculating a confidence interval around the estimate. Commonly authors do not give such a confidence interval (CI) but if the numerator and

denominator are given the CI can be calculated. For example, in Example 1 the Wilson 95% CI for 100% (30/30) is 88.65 to 100% and for 94% (29/31) it is 79.78 to 98.21% (OpenEpi Seattle).[77] If authors do report differences between trial arms (and this is not necessary if it is not consistent with the objectives of the trial) then confidence intervals again provide readers with an assessment of precision (Example 2) which usually indicates considerable uncertainty. If samples in the pilot trial and future definitive trial are drawn from slightly different populations, confidence intervals calculated from the pilot will not directly indicate the likely upper and lower bounds of the relevant measure in the future definitive trial, but can nevertheless highlight the lack of precision effectively.

Item 17b

Standard CONSORT item: for binary outcomes, presentation of both absolute and relative effect sizes is recommended

Extension for pilot trials: not applicable

Explanation

This item is included in the 2010 CONSORT Statement because when considering clinical implications, neither the relative nor absolute measures of effect size for binary outcomes give a complete picture of the effect of an intervention. For example, relative risks are less affected than absolute risks by differences in baseline populations across studies but, on the other hand, can sometimes be misinterpreted in terms of population benefit. In addition, different audiences (clinical, policy, patient) prefer to use one or the other measure. However, in pilot trials the situation is different. Because of the imprecision of estimates from these trials and the fact that samples in these trials can be unrepresentative (see item 17a), we caution against any reliance on estimates of effect size from pilot trials for clinical implications (see also Introduction, Scope, Box 2). Information from outcome data, however, can be legitimately used for other purposes such as estimating inputs for sample size for the future definitive trial (see item 7a). Thus item 17b, which is underpinned by rationale around clinical implications, is not applicable.

Item 18

Standard CONSORT item: results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory

Extension for pilot trials: results of any other analyses performed that could be used to inform the future definitive trial

Example

“Sensitivity analysis

At both six and 12 weeks, findings were insensitive to the exclusion of those catheterised throughout their hospital stay (and also to the exclusion of those who were never incontinent following the removal of a catheter). However, at both time points, odds ratios reduced when those with pre-stroke incontinence were excluded....”[78]

Explanation

It is possible that the results of analyses that were not initially planned may have important implications for the future definitive trial. Such findings should be reported and discussed in relation to how they might inform the future definitive trial. In the Example, although numbers were small, the authors inferred from the unplanned sensitivity analyses that those with pre-stroke incontinence were at least as, or more likely to benefit from the intervention than those continent pre-stroke, and concluded that therefore this group of patients should be included in the full trial.

Item 19

Standard CONSORT item: all important harms or unintended effects in each group (for specific guidance see CONSORT for harms)[7]

Example 1 (potential harm)

“Intervention and usual treatment groups were similar in terms of age, gender, and marital status, but those in the intervention group were more likely to be unemployed (69% v. 59%), to use methods other than poisoning (23% v. 9%), to have a past history of self-harm (67% v. 53%) and to have had previous psychiatric treatment (64% v. 53%).

Online Table DS1 shows self-harm repetition and resource use in the two groups. The 12-month repeat rate for individuals in the intervention group was 34.4% v. 12.5% for the usual treatment group (odds ratio (OR) 3.67, 95% CI 1.0–13.1...) ... Adjusting for baseline clinical factors (centre, method of harm (self-poisoning v. other), previous self-harm, previous psychiatric treatment), the odds ratio for repetition and incidence rate ratio for number of repeat episodes remained elevated...”[79]

Example 2 (unintended effect/potential harm)

“An unanticipated finding in this study was a 4-kg weight loss, on average, favouring the intervention group, although we recognized that there were some differences in weight between groups at study commencement that may have had an effect on our results... Thus, there is a clear role for dietary considerations in any study that aims to positively influence body weight. Although we provided one educational session on nutrition during a tour of a local grocery store with a dietitian and modelled healthy food choices with the lunches provided, dietary behaviors and body weight were not the focus of the study.”[21]

Explanation

It is crucial to report all important or potential harms or unintended effects on individual participants in each group to enable the study design for the future definitive trial to be changed either to avoid these effects or to put in place effective processes for monitoring potential harms. In Example 1, it was not clear whether the unexpected increased risk of repeated self-harm in the intervention group was real, or a consequence of baseline covariate imbalance, or peculiar to the particular setting. This led to a proposal to change the design to use stratified randomisation in the future definitive trial. In Example 2, the unintended effect of weight loss in elderly participants led to the decision to include a dietary component in the intervention to avoid potential harm in the future definitive trial. This information may also be useful to other researchers planning similar studies.

Item 19a

Extension for pilot trials: if relevant, other important unintended consequences

Example (unintended consequence)

“Twelve of the 13 active, and 11 of the 13 traditional practices recruited a total of 231 participants in the 12 months from mid-April 1998. Active practices recruited 165 (average practice recruitment rate of 1.71 per 1000 registered patients, i.e., 141% of expected) while traditional practices recruited only 66 (0.57 per 1000 i.e. 54% of expected) (Figure 1). On average active practices recruited 12.7 participants (range 0-39), while traditional practices recruited only 5.1 participants (range 0-18) (Table 2). Although both types of practices recruited similar percentages of those identified (13% in active; 16% in traditional), active identified 1257, far more than the 416 by traditional practices. The extreme difference in recruitment rates led to an investigation of baseline characteristics of participants in the two groups (Table 3). Participants recruited by active practices were more likely to

be working full-time and to have had further education since leaving school. They were also suffering from milder back pain, less limited physically and less depressed.”[80]

Explanation

This is a new item reflecting the importance of reporting unintended consequences that do not directly affect individual participants but may have implications for the validity of the future definitive trial if not addressed in that trial. By *unintended* consequences we mean things that happened in the pilot trial that the investigators did not intend to look for but that would have such implications. In the Example, the design of the pilot trial included practice-level randomisation with participant recruitment *following* that randomisation. This had unintended consequences in the balance of recruited participants between arms and the researchers abandoned randomisation at the practice level in the main study.

Discussion

Item 20

Standard CONSORT item: trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses

Extension for pilot trials: pilot trial limitations, addressing sources of potential bias and remaining uncertainty about feasibility

Example 1 (pilot trial limitations)

“In some cases, platelet mass was calculated on an MPV [mean platelet volume] that was up to 72 hours old based on our previous research on the relationship between platelet mass and IVH [intraventricular hemorrhage]. We cannot rule out the possibility that during acute thrombocytopenia changes in MPV may be more acute. Because platelet counts were not confirmed by manual count, we cannot exclude the unlikely possibility that some infants may have had pseudothrombocytopenia.”[81]

Example 2 (potential bias)

“Fourth, the house staff at the two academic centers in the study may have been a source of contamination. Additional house staff occasionally provided overnight coverage at the intervention group academic center. These additional house staff were not formally educated about the study, so they effectively functioned as if they were in the control group. Conversely, additional house staff who provided overnight coverage at the control group academic center may have been previously educated about our study while working at the intervention group academic center. Thus, they effectively functioned as if they were in the intervention group.”[82]

Example 3 (remaining uncertainty)

“The integration of a nested, internal pilot in the definitive trial should also be considered to allow continued monitoring of the feasibility, in particular, the assessment of using different inclusion criteria and the recommended changes to the data collection methods, particularly within the first year of recruitment. The use of a qualitative element to assess the participants’ views on data collection methods would also be beneficial.”[83]

Explanation

Identifying and discussing the limitations of a study helps to provide a better context for understanding the significance of its findings. In a pilot trial it may also be helpful to distinguish between limitations that can be overcome in a future definitive trial, and those that cannot. In

Example 1 authors explain the limitation of a method of measurement although they do not say whether they think this could be overcome in a future definitive trial.

In a future definitive trial investigators will want, as far as possible, to avoid sources of bias that may affect treatment effect estimates. In a pilot trial we are not primarily interested in treatment effect so these biases will not be of so much concern but it will still be useful to identify *potential* biases that could affect the treatment effect in the future definitive trial so that investigators have a better chance of avoiding these. In Example 2 a potential source of bias in the future definitive trial is identified.

If substantial areas of uncertainty about feasibility remain at the end of the pilot trial that prevent investigators from proceeding with a future definitive trial or warrant investigation in an internal pilot then, for clarity, these should be reported as in Example 3.

Lastly, although we do not recommend this, if underpowered tests are performed and reported then investigators should always point out this limitation to avoid misinterpretation of results (see item 2b).

Item 21

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

Extension for pilot trials: generalisability (applicability) of pilot trial methods and findings to future definitive trial and other studies

Example 1 (generalisability of findings)

“We accommodated variability in choice and duration of standard treatments to enhance generalizability of the results and had high rates of follow-up.”[64]

Example 2 (generalisability to other pilot trials)

“Our data reflect the activities of only one pilot trial; however, we hope that the methods may serve as a template for analyzing other pilot studies with different designs in other settings.”[62]

Example 3 (generalisability concerns)

“Although safety issues must remain paramount in practice and clinical research, common overstringent exclusion criteria may increase perceived trial safety yet limit the generalizability of trial results and delay answers to important clinical questions. Reevaluation of the PROTECT Pilot exclusion criteria will enhance the applicability of the larger PROTECT study.... The PROTECT Pilot indicated the need for another pilot study (DIRECT) to determine the safety of dalteparin 5000 IU SC OD among patients with severe renal insufficiency (creatinine clearance, ≤ 30 mL/min).”[62]

Explanation

Generalisability (applicability) is the extent to which aspects of a study can be applied to other circumstances. Generalisability is not absolute and is a matter of judgement. In a definitive trial readers are usually interested in the generalisability of *findings* to situations outside research settings, for example routine clinical practice. However, in pilot trials this is not the case because the size of these studies does not allow this. Nevertheless, it may be important to consider generalisability at the pilot stage because this may be important for the generalisability of the future definitive trial (Example 1), the findings and the methods may be applied in research settings other than the future definitive trial (Example 2) or there may be concerns about the generalizability of results from a future definitive trial conducted in an identical way to the pilot trial that may lead to changes in the design of the future definitive trial or further piloting (Example 3).

Item 22

Standard CONSORT item: interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence

Extension for pilot trials: interpretation consistent with pilot trial objectives and findings, balancing potential benefits and harms, and considering other relevant evidence

Example 1 (consistency with objectives and findings)

“One of the goals of this pilot study was to investigate the feasibility of using platelet transfusion guidelines based on platelet mass. In five infants, MPV [mean platelet volume] was not available within 72 hours preceding the diagnosis of thrombocytopenia. A lack of immediately available MPV may limit the clinical utility and generalizability of this transfusion strategy at some institutions...In our study approximately half of the families at the Christiana Hospital site did not consent to the study. This information is important for planning future studies on platelet transfusion. Many families were unable to decide on enrollment at a time when their infant was thrombocytopenic and facing transfusion. An alternative study design for platelet transfusion study may involve enrolling a larger number of infants on admission, regardless of platelet count, with transfusion guidelines to apply only if they actually become thrombocytopenic. This approach may limit the stress on families of being approached about the need for transfusion and a transfusion related study simultaneously.”[81]

Example 2 (considering other relevant evidence)

“As far as we know, our participants were able to perform motor imagery. Our results seem to be in contrast to previous studies demonstrating a lower capacity for motor imagery in people with MS. However, these authors linked impaired motor imagery in this population particularly to cognitive dysfunction and depression. Therefore, persons with cognitive impairment and depression were excluded from our study. Several studies used patient-rated questionnaires, such as the Kinaesthetic and Visual Imagery Questionnaire to assess the motor imagery ability in their participants. Our study could have used this patient-rated questionnaire, but our participants were called weekly to ask for any problems with kinaesthetic motor imagery, and they were supported accordingly. In addition, all motor imagery ability studies in people with MS were experimental studies with no long-term training effects, in contrast to our 4 weeks duration study with 24 training sessions which might have enhanced the mental representation.”[67]

Example 3 (consistency with findings in relation to decision criteria)

“Investigators had established *a priori* threshold for specific feasibility and acceptability criteria. These were the following: (a) the proportion of contacted FMGs participating in the pilot study would be 50% or greater, (b) the proportion of recruited family physicians participating in all three workshops would be 70% or greater, (c) the mean level of satisfaction from family physicians regarding the workshops would be 65% or greater, and (d) the proportion of missing data in each completed questionnaire would be less than 10%. Not reaching the pre-established criteria does not necessarily indicate unfeasibility of the trial but rather underlines changes to be made to the protocol.”[33]

Explanation

Interpretation of findings helps increase understanding of the importance of the results. In Example 1, in addition to matching their interpretation to one of the goals of the study, the authors draw out the issue of redesign to reduce stress in families approached and so increase recruitment – and hopefully eventually positive benefit for the children involved. This observation could be helpful to others planning similar studies. As for definitive trials, readers will want to know how the evidence presented in the report of a pilot trial relates to evidence from other sources (Example 2). These sources may be other feasibility studies carried out by the authors or studies by different authors in the

same or similar settings or with similar patients. If a priori decision criteria have been used (item 6c) then interpretation should be made with reference to these criteria (Example 3).

Item 22a

Extension for pilot trials: implications for progression from pilot to future definitive trial including any proposed amendments

Example 1 (proposed amendments to improve recruitment)

“The target of recruit to time was met but this did not translate to the expected number of eligible patients being recruited. Eligibility of the screened population was much lower than expected, indicating that the inclusion criteria may have been too stringent. The exclusion criteria of BMI ≤ 22 kg/m² was based on published evidence that a BMI at the lower end of the normal range can increase mortality in the haemodialysis population ... However, body composition is thought to play a much greater role in the protective effects of a greater BMI, than the BMI itself ... The use of BMI as a screening tool was a quick and easy measure but the level of ≤ 22 kg/m² should be reassessed prior to a definitive trial. If the BMI was raised to ≤ 24 kg/m² then this would have increased potential recruitment by 10%.”[83]

Example 2 (proposed amendments to improve cooperation)

“Six homes declined to actively participate before even beginning the intervention. To ensure cooperation by the entire team and avoid early withdrawal, a short presentation to the Professional Advisory Committee team could potentially boost recruitment/retention. Obtaining initial consent from both the medical director and director of care may also be beneficial. Furthermore, to overcome logistical challenges, particularly for homes in the far north, providing an opportunity to view modules on a Web site or participate remotely may improve participation.”[84]

Example 3 (implications for progression to future definitive trial)

“Hospitals that were allocated to receive our multicomponent intervention comprising education, standardized paper-based physician orders, and group audit and feedback did not have a higher rate of hospitalized medical patients appropriately managed for thromboprophylaxis within 24 hours of admission than did hospitals that were not allocated to this strategy (63% vs. 67%). This finding, coupled with the problems associated with ensuring preprinted orders were placed in all medical charts led us to conclude that this intervention should not be provided on a larger scale without major revision and testing. That is, it was not feasible.”[82]

Explanation

This is a new item. In order to progress from a pilot trial to a future definitive trial, it is important to understand how the implications of the findings in the pilot carry over to the future definitive trial. To aid clarity a simple statement as to whether the future definitive trial will be planned without any changes from the pilot trial, planned with changes from the pilot trial (Examples 1 and 2), or not planned because of major issues with feasibility (Example 3), is sufficient. If it is proposed to plan the future definitive trial with specific changes from the pilot trial, these should be stated.

Other information

Item 23

Standard CONSORT item: registration number and name of trial registry

Extension for pilot trials: registration number for pilot trial and name of trial registry

Example

“Trial registration number: Clinical Trials, protocol registration system: NCT01695070.”[85]

Explanation

It is just as important for a pilot trial to be registered with a unique identifier as it is for a definitive trial. Registration ensures transparency and accountability and in the UK is now a requirement for all clinical trials prior to approval from UK Ethics committees.[86, 87] It ensures all ongoing work is in the public domain, and subsequent publication (and therefore access to findings for the greater good) confirmed. The World Health Organisation states that “the registration of all interventional trials is a scientific, ethical and moral responsibility”.[88] The ICMJE requires all trials to be registered as a criterion for publication and lists suggested registries.[89]

Item 24

Standard CONSORT item: where the full trial protocol can be accessed, if available

Extension for pilot trials: where the pilot trial protocol can be accessed, if available

Example 1 (reference to published protocol)

“The Healthy Hospital Trial is a single-center, randomized controlled, 2-arm, parallel-group, unblinded feasibility trial that was conducted on 2 cardiology wards at the Leeds Teaching Hospitals Trust. Its primary aim was to explore the feasibility of individualized lifestyle referral assessment, estimate the rate of recruitment, and explore the feasibility of collecting the data and follow-up of participants to inform the sample size of a definitive trial. A secondary aim was to test the concept that an individually tailored assessment improves uptake of life-style change compared with usual assessment. The trial protocol has been published elsewhere.”[39]

Example 2 (protocol as supporting information)

“The protocol for this trial and supporting TREND checklist are available as supporting information; see Checklist S1 and Protocol S1.”[90]

Example 3 (protocol available from authors on request)

“Participants in the control arm (but not the other two arms) received a 16-page informational booklet relevant to education, medical care, housing, employment, and community resources (protocol available from authors upon request).”[91]

Explanation

Access to the full protocol for the pilot trial is important as it will pre-specify all the main components of the trial. The SPIRIT statement defines an evidence-based set of items which would be included.[92] Accessibility of the protocol will allow subsequent output to be checked for completeness, and reduce the chance of selective reporting to suggest “better” results. The above examples illustrate the different ways in which protocols may be made available, such as prior publication as in Example 1, as an addendum to the report of the pilot trial as in Example 2, or on request from the authors, as in Example 3. Options where the protocol is already in the public domain, such as prior publication are to be preferred. Other methods that could be used to achieve this would include publication on a study website. Trial registries (see item 23) also include some core protocol items.

Item 25

Standard CONSORT item: sources of funding and other support (such as supply of drugs), role of funders

Example

Funding: “This trial was funded through grants from Academic Health Science Centres Alternative Funding Plan Innovation Fund of Ontario and Octapharma Canada. The trial funders had no role in the design of the study, the collection, analysis or interpretation of data, the writing of the report, or the decision to submit the article for publication,”[93]

Explanation

Reporting the sources of all funding for a pilot trial (that is both the main research award and any other support such as supply of equipment) allows the reader to judge the potential influence of the funding body on the design, conduct, analysis and reporting of the trial. If no specific funding was made to support the pilot trial this should also be stated. As reported in the main CONSORT paper, a systematic review has shown that research funded by the pharmaceutical industry is more likely to report findings in their favour, compared to reports of research funded from independent funding bodies.[2, 60] Where funders have had no involvement in any aspect of trial conduct or reporting this should be explicitly stated.

Item 26

Extension for pilot trials: ethical approval/research review committee approval confirmed with reference number

Example

“The Regional Ethical Review Board at the Karolinska Institute approved the study, no. 2007/1401-31/3.”[94]

Explanation

This is a new item which has been added to the CONSORT checklist because of the need to emphasise that all research, including pilot trials, should only be conducted within an ethical framework and with all ethical and other approvals in place prior to commencement. Of particular relevance to pilot trials is the need also to be aware of any restrictions imposed by the reviewing ethical committee, because these would have implications for the design and conduct of the future definitive trial.

COMMENT

Reports of RCTs need to include key information on the methods and results so that readers can accurately interpret the contents of the report. This is as true for pilot trials as it is for any other RCTs. The CONSORT 2010 Statement provides the latest recommendations from the CONSORT Group on essential items to be included in the report of an RCT.[2, 60] However, pilot trials differ from other randomised trials in their aims and objectives, focusing on assessing feasibility rather than effectiveness or efficacy. Therefore, though much of the information to be reported in these trials is similar to that which needs to be reported in any other randomised trial, there are some key differences in the type of information and in the appropriate interpretation of standard CONSORT reporting items.

In this article we introduce and explain these key differences in an extension to the CONSORT checklist specific to pilot trials. In the Scope section we discuss a number of other types of feasibility study, and “proof of concept” trials. Other researchers have begun to look at the transfer of ideas between these different type of study, for example Wilson et al.[95] It is our expectation that some of the principles of reporting outlined in this extension can be adapted for other types of feasibility and/or “proof of concept” studies.

Use of the CONSORT Statement for the reporting of two group parallel trials is associated with improved reporting quality.[96] We believe that the routine use of this proposed extension to the CONSORT Statement will eventually result in similar improvements in reporting of pilot trials. When reporting a pilot trial, authors should address each of the 26 items on the CONSORT extension checklist using this document, referring to the main CONSORT guidelines as appropriate. Adherence to the CONSORT Statement and extensions can also 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. A tool is currently being developed to support journals in doing this.[97] The most up to date versions of all CONSORT recommendations can be found at www.consort-statement.org.

Competing interests:

All authors have completed the ICMJE disclosure form at http://www.icmje.org/doi_disclosure.pdf and declare support from the following organisations for the submitted work – Queen Mary University of London, University of Sheffield, NIHR Research Design Services London and South East, NIHR statisticians Group, Chief Scientist Office Scotland. Elaine McColl received travel and accommodation expenses from Oxford University for the expert panel meeting in the development of this paper. Authors also declare financial relationships with NIHR, MRC, EC FP7, Canadian Institute for Health Research, Wiley, and Novartis Frank Bretz). Gillian Lancaster is editor in chief of the new BioMed central journal Pilot and Feasibility Studies proposed by Daniel R Shanahan which was created out of this work. Daniel Shanahan is employed by BioMed Central, and Trish Groves is an editor of the BMJ but will not play any part in the peer review process or editorial decision making.

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Contributorship statement:

SE, MJC, GL, CB, LT, SH conceived the study. SE led its development and execution. SE, MJC, GL, CB, LT, SH, and CC all contributed to various aspects of the empirical work, and the PAFS consensus group provided feedback on the proposed items to be included in the guidelines via a consensus meeting and by email. SE, MJC, GL, CB, LT, SH, and CC drafted the manuscript and all authors reviewed it and approved the final version. SE is the guarantor of this work.

Transparency declaration:

The lead author (the manuscript's guarantor) affirms that the manuscript is an honest, accurate, and transparent account of the study being reported; that no important aspects of the study have been omitted; and that any discrepancies from the study as planned (and, if relevant, registered) have been explained.

Ethics committee approval:

Ethical approval for the Delphi study was granted by the ScHARR research ethics committee at the University of Sheffield.

Funding:

We received small grants from Queen Mary University of London (£7495), University of Sheffield (£8000), NIHR RDS London and South East (£2000), NIHR Statisticians Group (£2400), and Chief Scientist Office Scotland (£1000). Claire Chan (nee Coleman) was funded by a National Institute for Health Research (NIHR) Research Methods Fellowship. This article presents independent research funded by the NIHR. The views expressed are those of the authors and not necessarily those of the NHS, the NIHR or the Department of Health. Marion Campbell works at the Health Services Research Unit, University of Aberdeen, and the Unit receives core funding from the Scottish Government Health and Social Care Directorates; however, the opinions expressed are those of the authors alone. The funders had no role in this study.

Data sharing statement:

Due to a requirement by the ethics committee that the authors specified when the data from the Delphi study will be destroyed, the authors are not able to give unlimited access to the Delphi study quantitative data. These data are available from Professor Sandra Eldridge. Data will be available upon request to all interested researchers. Qualitative data from the Delphi study are not available because the authors do not have consent from participants for wider distribution of this more sensitive data.

Acknowledgements:

During the development of this work we presented our thinking at workshops and open meetings and would like to thank all participants for their valuable input and views: Society for Clinical Trials May 2013 Boston; Clinical Trials Methodology Conference Edinburgh November 2013; Royal Statistical Society October 2013; UK National Institute for Health Research (NIHR) Research Design Service London & South East March 2014 London; UK NIHR Statisticians April 2015 London; Society for Academic Primary Care Annual Scientific Meeting July 2015 Oxford; HSRPP Health Services Research & Pharmacy Practice Conference April 2016. We also thank Colin Begg for feedback on the proposed items to be included in the guidelines during the two-day consensus meeting in Oxford.

FIGURES

Figure 1a: Example of abstract for report of pilot trial,[21] shown alongside the CONSORT for Abstracts extension for pilot randomised trials

Figure 1b: Revised version of example abstract for report of pilot trial,[21] shown alongside the CONSORT for Abstracts extension for pilot randomised trials

Figure 1c: Track changes version of example abstract for report of pilot trial,[21] showing the changes between Figure 1a and Figure 1b

Figure 2: Flow diagram of a randomised pilot trial of pharmacist-led management of chronic pain in primary care (reproduced from Bruhn et al.[68])

Figure 3: Recommended flow diagram of the progress through the phases of a parallel randomised pilot trial of two groups (that is, screening, enrolment, intervention allocation, follow-up, and assessed for each pilot trial objective (adapted from Moher et al.[2]).

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