ACRM
AMERICAN CONGRESS OF REHABILITATION MEDICINE

Archives of Physical Medicine and Rehabilitation

journal homepage: www.archives-pmr.org

Archives of Physical Medicine and Rehabilitation 2025;106: 1081–91

REVIEW ARTICLE (META-ANALYSIS)

The Power of Prehabilitation, the Reporting of Power Calculations in Randomized Clinical Trials Evaluating Prehabilitation in Cancer Surgery: A Systematic Review and Meta-research Study

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Abstract

Objective: To assess sample size calculation reporting in randomized controlled trials (RCTs) investigating prehabilitation interventions in oncological surgery patients.

Data Sources: A systematic literature search was performed in multiple medical databases from inception to April 2023, including MEDLINE, Embase, The Cochrane Library, CINHAL, AMED, and PsychINFO.

Study Selection: The inclusion criteria used were RCTs evaluating effectiveness of exercise, nutrition, and/or psychological interventions on postoperative outcomes of adult patients undergoing oncological surgery.

Data Extraction: Two authors (DS and SV) extracted information on the sample size calculation parameters, including type I error (α), power (1-β), mean (or mean difference between randomization arms), and variance (eg, standard deviation) for continuous outcomes, and event rates or event rate difference between randomization arms for dichotomous outcomes. When possible, we recalculated the sample size required using the collected data, given a 10% margin of error.

Data Synthesis: Of the 59 included publications (58 RCTs), 26 (44%) reported sufficient information to complete sample size recalculation. Of those that provided sufficient information allowing us to recalculate the required sample size, 11 (42%) were within a 10% margin of the reported sample size, whereas 9 (35%) were >10% higher than reported sample size and 6 (23%) were >10% lower than reported sample size.

Conclusions: Over half of the published RCTs in this field exhibit poor sample size calculation reporting. Most RCTs that report sufficient sample size information were underpowered. More stringent reporting requirements are necessary.

Archives of Physical Medicine and Rehabilitation 2025;106:1081–91

Randomized controlled trials (RCTs) are the criterion standard for evaluating the effect of an intervention in medical research.[1] The number of participants in a trial is an important factor when assessing the quality of RCTs. It is important that a sample size be sufficiently powered to detect a clinically relevant treatment effect.[2] RCTs that do not have a sufficient sample size may not have enough power to determine a clinical effect in the population of interest. Inversely, an overly large sample may recruit more participants than needed, resulting in extra time, cost, and possibly ethical concerns.[2-5]

Recently, several systematic reviews of RCTs in various medical specialities are revealing pitfalls in sample size calculation

Disclosures: none.

0003-9993/$36 - see front matter © 2025 by the American Congress of Rehabilitation Medicine.
https://doi.org/10.1016/j.apmr.2025.01.465

reporting. A study demonstrated that although 91.7% of RCTs in leading anesthesia journals report sample size calculations, for almost one third of these it was not possible to recalculate the sample size.[6] Another study highlighted that only 53% of RCTs published in the 6 highest impact medical journals reported all the required sample size calculation parameters.[7] Many other reviews highlight deficiencies in sample size calculation reporting in other specialities.[8-16]

Some ethics review boards view underpowered trials as unethical because the risk to participants is not weighted against an ability to detect clinically meaningful results.[17] On the other hand, some argue that underpowered trials can be combined in a meta-analysis to address the weakness of insufficient participants, provided that the methodology of the RCTs is homogeneous, outcomes are clearly defined and reported, and publication bias is avoided.[2] However, this concept relies on future work to ameliorate inadequately powered RCTs, which is a suboptimal strategy because meta-analyses are only as robust as the constituent RCTs.

In the field of prehabilitation, there are many RCTs reporting on the effectiveness of preoperative exercise, nutrition, and/or psychological interventions to improve surgical outcomes in patients undergoing oncological surgery. Issues on the quality of these trials, specifically on reporting and defining outcome measures, have been previously reported.[18] However, the integrity of sample size reporting of RCTs investigating prehabilitation interventions has not been determined. Although previous studies have identified widespread deficiencies in sample size reporting across clinical trials, prehabilitation research presents unique challenges that make robust sample size calculation and reporting particularly critical. The complex nature of prehabilitation interventions—often combining exercise, nutrition, and psychological components—introduces multiple outcome measures and potential effect modifiers. Additionally, the presurgical timing of these interventions creates distinct recruitment challenges and can lead to variable patient responses, making effect size estimation more difficult. Despite these challenges, proper sample size calculation is essential to ensure that RCTs investigating prehabilitation interventions are adequately powered to detect clinically meaningful effects and make efficient use of health care resources. Our review specifically examines power analyses in prehabilitation RCTs to address these field-specific concerns and provide guidance for future RCT design and reporting. Therefore, this systematic review and meta-research study aimed to evaluate the reporting of sample size calculations in published RCTs investigating prehabilitation interventions for oncological surgery.

Methods

Design

A protocol for this systematic review and meta-research study is available and was registered on Open Science Framework (https://osf.io/g7t2v). The review protocol adhered to the recommendations by the Preferred Reporting Items for Systematic Review and Meta-Analysis (PRISMA) Protocols.[19] This manuscript followed the recommendations of the PRISMA[20] and the Cochrane Handbook for Systematic Reviews of Interventions for Randomized Controlled Trials.[21]

List of abbreviations:

PRISMA	**Preferred Reporting Items for Systematic Review and Meta-Analysis**
qRCT	**quasi-randomized controlled trial**
RCT	**randomized controlled trial**
SD	**standard deviation**

Search strategy

We searched for RCTs investigating the effectiveness of nutrition, exercise, and/or psychological prehabilitation interventions for participants undergoing surgery for cancer. The search was performed on the following databases from the earliest records to April 2023: MEDLINE, Embase, The Cochrane Library, CINHAL, AMED, and PsychINFO. The search strategy was developed in conjunction with an experienced librarian from The University of Sydney. The search string is provided in supplemental table S12. Forward and backward citation tracking was conducted for additional relevant articles. One review author (DS) conducted the first screening of potentially relevant records based on titles and abstracts using the COVIDENCE systematic review software (Veritas Health Innovation). Subsequently, 2 review authors (DS and SV) conducted independent screening of titles and abstracts of the records.

Eligibility criteria

The included RCTs are based on the following eligibility criteria (fig 1).

Patients

The population of interest was patients undergoing oncological surgery of the thorax, breast, abdomen, and/or pelvic areas. We included publications of RCTs with participants of any age or sex.

Intervention

We included publications reporting the comparisons between preoperative interventions (eg, exercise and/or nutritional and/or psychological). Publications comparing 2 or more active interventions were included (eg, exercise versus nutritional intervention).

Comparator intervention

Groups that received standard treatment, no treatment, placebo, or minimal intervention (eg, an intervention that is unlikely to impact the outcome).

Outcomes

Any postsurgical-related health outcomes.

Study selection

Full-text articles were obtained for each potentially eligible study and assessed to check if the study fulfilled the inclusion criteria. Any disagreement was resolved by discussion. The reasons for exclusion for assessed publications are listed in PRISMA diagram found in figure 1. In our protocol we stated that quasi RCTs (qRCTs) were excluded; we deviated from our protocol and included qRCTs for thoroughness.

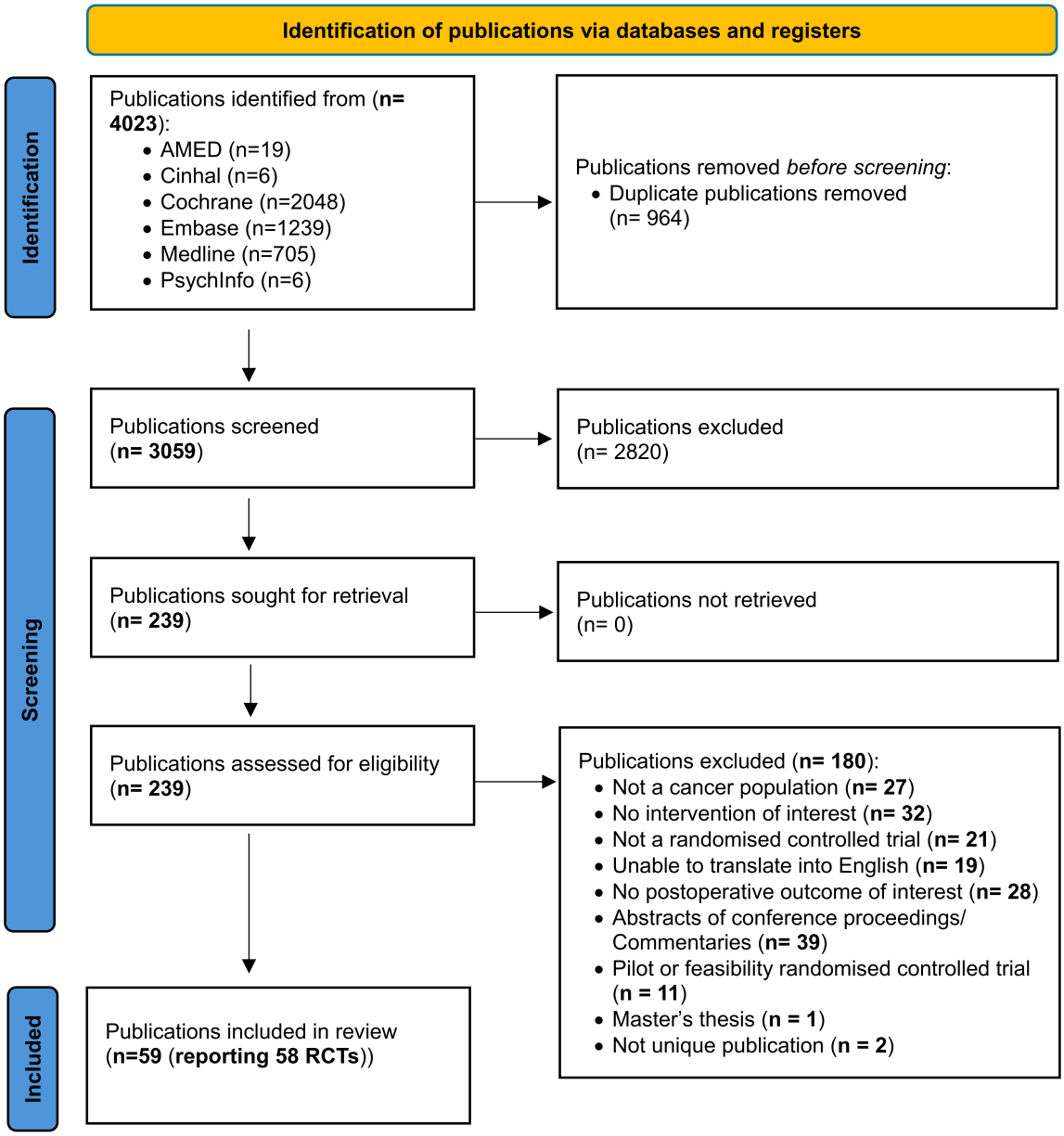

Fig 1 PRISMA flow diagram of publication selection process. This diagram illustrates the identification, screening, eligibility assessment, and inclusion of studies in the systematic review. A total of 4023 records were identified through database searching and other sources. After removing duplicates, 3059 records were screened, of which 239 full-text articles were sought for retrieval and assessed for eligibility. The final analysis included 59 publications reporting 58 RCTs.

Data extraction

Two of the authors (DS and SV) extracted data from the full text of the selected articles relating to the study design and sample size calculations pursuant to the data extraction checklist in supplemental table S2. In addition to metadata relating to the publications, we extracted all relevant variables for sample size calculation as mentioned by study authors in their methodology section or supplementary materials. We additionally extracted primary endpoints and secondary endpoints of the included publications. Differences in data extracted by the 2 review authors were resolved by discussion.

The following main parameters were essential to collect for sample size calculation:

(1) Type I error (alpha) indicates the probability of making a false-positive conclusion. Assuming an α=0.05, investigators allow a maximum 5% chance for drawing false-positive conclusion.

(2) Type II error (beta) indicates the probability of drawing a false-negative conclusion. Assuming a value set at 20%, investigators allow a maximum 20% chance for drawing false-negative conclusions. The study power is derived as a complement of β, ie, $1-\beta$. If assuming a type II error of 20%, the power is 80%.

(3) The effect size, which is the minimal difference that investigators want to detect between study groups. This can be the minimal clinically relevant difference, which is

defined as the smallest improvement considered worthwhile by a patient.[22]

(4) The variability, which usually is the anticipated population variance of a given outcome variable estimated by means of standard deviation (SD).

When calculating the target sample size, we also needed to consider the dropout rate and cross-over rate in the randomization arms.

Data synthesis

Sample size recalculation was completed for RCTs that included sample size parameters in the main publication, supplementary material, the RCT protocol, or cited in a previous publication: type I error (α), power ($1-\beta$), mean (or mean difference between randomization arms), and variance (eg, SD) for continuous outcomes, and event rates or event rate difference between randomization arms, for dichotomous outcomes. If the missing values were α risk, allocation ratio or whether the test was 1- or 2-tailed, we hypothesized an α risk of 0.05, 1:1 ratio, and a 2-tailed test to recalculate the calculation. In determining whether our recalculated sample sizes matched the original sample sizes calculated by the authors of the RCTs, our recalculation includes dropouts expected at the RCT design phase, if available. Where not reported we assume the expected dropout rate to be zero. Sample size loss was calculated by comparing patients analyzed in the final analysis against the required sample size calculated by the authors of the RCTs, not including expected dropout rates.

Sample size recalculations were recalculated using PASS 2023. For a binary endpoint, the recalculation used the formulae adapted for 2-sample Z-Test with unpooled variance, and for a continuous endpoint the recalculation used the formulae adapted for Student's *t* test. In comparing 3 or more groups, unless specified differently in the original publication, for continuous endpoints we used multiarm tests for the difference between treatment and control means allowing unequal variance, and for categorical endpoints we used multiarm tests for treatment and control proportions.

The standardized difference between the reported sample size calculation and the recalculated one is defined by the reported sample size calculation minus the recalculated sample size divided by the reported sample size calculation. We allowed for a 10% margin of error in these calculations.[7] We consider an RCT to be underpowered where it does not analyze sufficient participants as required by the sample size that we recalculate excluding expected dropout rates. We consider sample size calculation information to be insufficient where the basic parameters for recalculating sample size are not presented in the RCT.

Results

Search results

The search yielded 3059 publications (excluding duplicates). After screening citations by title and abstract, 239 full-text articles were assessed. A total of 59 eligible publications (reporting results from 58 RCTs) met the eligibility criteria and were included (fig 1).

Description of included studies

Population

There were a range of different oncological surgeries that the included RCTs focused on, such as: colorectal (N=14), lung (N=11), esophageal (N=6), gastric (N=3), liver (N=3) periampullary (N=1), breast (N=1), gynecologic (N=1), pelvic (N=1), bladder (N=1). Some oncological surgeries were categorized broadly, without further specification, such as "gastrointestinal cancer" (N=3). Many RCTs investigated oncological surgeries for more than 1 cancer type (N=13).

Intervention

Prehabilitation interventions aimed at improving postsurgical outcomes were categorized into either exercise (N=20), nutritional (N=43), and/or psychological interventions (N=6). There were 5 publications with more than 1 intervention.

Comparator intervention

Control groups received standard treatment (N=32), no treatment (N=17), placebo (N=5), or minimal intervention (N=4).

Outcome

Primary outcomes were highly varied, and could be organized into the following categories: complication (N=23), immune (N=13), functional capacity (N=11), length of stay (N=4), metabolic (N=4), analgesia (N=1), quality of life (N=1), temperature (N=1), and not reported (N=1).

The detailed information of the included RCTs is available in table 1.

Both publications by Huang et al (2017)[24,25] (table 1) appear to be based on the same RCT and are considered only once in sample size recalculation.

Reporting quality

Reporting of sample size calculation parameters in publications

One publication reported both dichotomous and continuous primary endpoints and so we included this publication in both groups, bringing the sum of dichotomous and continuous endpoint publications from 59 to 60. However, this publication reported no sample size calculation information.[26] The single eligible qRCT in our review[23] was among those publications that did not report sufficient sample size parameters.

Of the publications measuring continuous variables for the primary endpoint (N=36), 11 (31%) reported all required sample size calculation information. Of the publications measuring dichotomous variables for the primary endpoint (N=24), 15 (63%) reported all required sample size calculation information.

Of the publications reporting type I error (alpha) (N=33), 32 set alpha at 5% (97%), and 1 set alpha at 10% (3%). Of the publications reporting power (N=34), 31 set power at 80% (91%) and 3 set power at 90% (9%). Out of the 59 eligible publications, 9 (15%) included more than 2 groups. The maximum number of groups was 4. Publications with greater than 2 groups did not outline whether adjustment of alpha error was considered to support the additional comparisons.

The reporting of sample size calculations was poor. For instance, among 24 publications exploring dichotomous primary endpoints, 8 (33%) did not report the anticipated incidence of a

Table 1 Characteristics of included publications

Author, Year, No. of Centers, Protocol Available (Y/N)	Intervention (Actual N)	Cancer Type	Primary Endpoint		
			Description	Category	Type
Aiko et al, 2012[40], 1, N	Nutrition (39)	Esophageal	Changes to immune competence	Immune	Continuous
Amraoui et al, 2021[41], 3, Y	Psychological (148)	Breast	Breast pain	Analgesia	Continuous
Ashida et al, 2019[42], 1, Y	Nutrition (20)	Periampullary	Serum IL-6	Immune	Continuous
Ausania et al, 2019[43], 1, N	Exercise (40)	Pancreatic or periampullary	Postoperative complications	Complication	Dichotomous
Barth et al, 2019[44], 2, Y	Nutrition (60)	Colorectal, gallbladder, and primary cholangiocarcinoma	Intraoperative blood loss	Complication	Continuous
Benzo et al, 2011,[26] 2, N*	Exercise (NR)	Lung	Length of hospital stay/postoperative complications	Length of stay/ complication	Continuous/ dichotomous
Blackwell et al, 2020[45], 1, Y	Exercise (40)	Lung	Anaerobic threshold	Functional capacity	Continuous
Braga et al, 2002[47], 1, N	Nutrition (200)	Colorectal	Modulation of immunometabolic variables	Immune	Continuous
Braga et al, 2012[46], 1, Y	Nutrition (36)	Pancreatic, periampullary	Postoperative oxidative stress	Immune	Continuous
Burden et al, 2011[49], 3, N	Nutrition (116)	Colorectal	Postoperative complications	Complication	Dichotomous
Burden et al, 2017[48], 6, N	Nutrition (100)	Colorectal	Postoperative complications	Complication	Dichotomous
Chen et al, 2017[50], 1, N	Nutrition (120)	Gastric	Nutritional status	Metabolic	Continuous
Dunne et al, 2016[52], 1, Y	Exercise (37)	Colorectal, liver	Oxygen uptake at anaerobic threshold	Functional capacity	Continuous
Fan et al, 1989[53], 1, N	Nutrition (40)	Esophageal	Postoperative complications	Complication	Dichotomous
Fang et al, 2013[54], 1, N	Exercise (44)	Lung	Lung function	Functional capacity	Continuous
Fujitani et al, 2012[55], 1, Y	Nutrition (231)	Gastric	Postoperative complications	Complication	Dichotomous
Garcia et al, 2017[89], 1, Y	Exercise (19)	Lung	Exercise capacity at 80% of the workload peak	Functional capacity	Continuous
Giger-Pabst et al, 2013[56], 6, N	Nutrition (108)	Esophageal, gastric, pancreatic, liver, colorectal	Postoperative complications	Complication	Dichotomous
Gil et al, 1997[57], 1, N	Nutrition (41)	Gastric, colorectal, esophageal	Serum albumin	Metabolic	Dichotomous
Gunerhan et al, 2009[58], 1, N	Nutrition (42)	Gastrointestinal	Cellular immunity parameters	Immune	Continuous
Hamamoto et al, 2018[59], 1, Y	Nutrition (64)	Colorectal	Intraoperative esophageal temperature	Temperature	Continuous
Hogan et al, 2020[60], 1, Y	Nutrition (108)	Pelvic	Length of hospital stay	Length of stay	Continuous
Horvat et al, 2010[61], 1, N	Nutrition (68)	Colorectal	Systemic immune response	Immune	Continuous
Huang et al, 2017[24], 1, Y	Exercise (80)	Lung	Postoperative complications	Complication	Dichotomous
Huang et al, 2017[25], 1, N	Exercise (90)	Lung	Postoperative complications	Complication	Dichotomous
Jin et al, 1999[62], 1, N	Nutrition (92)	Gastric, colorectal	Tumor cell kinetics, immunocompetence, and wound healing	Immune	Continuous
Kabata et al, 2014[63], 1, Y	Nutrition (102)	Gastrointestinal	Postoperative complications	Complication	Dichotomous
Kaya et al, 2016[23], 1, N	Nutrition (58)	Lung	Serum albumin, postoperative complications, chest tube drainage	Metabolic	Continuous
Kikuchi et al, 2016[64], 1, Y	Nutrition (77)	Liver	Postoperative complications	Complication	Dichotomous
Kitagawa et al, 2017[65], 1, Y	Nutrition (29)	Esophageal	Postoperative complications	Complication	Dichotomous

(*continued on next page*)

Table 1 (*Continued*)

Author, Year, No. of Centers, Protocol Available (Y/N)	Intervention (Actual N)	Cancer Type	Primary Endpoint		
			Description	Category	Type
Koet et al, 2021[66], 1, N	Psychological (75)	Colorectal	Quality-of-life scores	Quality of life	Continuous
Lai et al, 2019[67], 1, Y	Exercise (NR)	Lung	6-minute-walk-test	Functional capacity	Continuous
Licker et al, 2017[68], 2, Y	Exercise (151)	Lung	Postoperative complications	Complication	Dichotomous
Liu et al, 2020[69], 1, Y	Nutrition; exercise; psychological (73)	Lung	6-minute-walk-test	Functional capacity	Continuous
Lopez-Rodriguez et al, 2021[70], 1, Y	Nutrition; exercise; psychological (20)	Colorectal	Postoperative complications	Complication	Dichotomous
Manzanares et al, 2011[71], 1, Y	Nutrition (84)	Colorectal	Postoperative complications	Complication	Dichotomous
McCarter et al, 1998[72], 1, N	Nutrition (38)	Esophageal, gastric, pancreatic	Immunosuppression	Immune	Continuous
Mikagi et al, 2011[73], 1, N	Nutrition (26)	Liver	Postoperative complications	Complication	Dichotomous
Minnella et al, 2018[75], 1, Y	Nutrition; exercise (51)	Esophageal, gastric	6-minute-walk-test	Functional capacity	Continuous
Minnella et al, 2020[76], 1, Y	Nutrition; exercise; psychological (42)	Colorectal	Oxygen uptake at anaerobic threshold	Functional capacity	Continuous
Minnella et al, 2021[74],1, Y	Nutrition; exercise; psychological (70)	Bladder	6-minute-walk-test	Functional capacity	Continuous
Morano et al, 2013[77],1, Y	Exercise (21)	Lung	Lung function	Functional capacity	Continuous
Moriya et al, 2014[78], 1, Y	Nutrition (85)	Colorectal	Postoperative complications	Complication	Dichotomous
Mueller et al[79], 1982, 1, N	Nutrition (125)	Esophageal, gastric, colorectal, pancreatic	Postoperative complications	Complication	Dichotomous
Nakamura et al, 2005[80], 1, N	Nutrition (26)	Bile duct, pancreatic, gastric, esophageal	Inflammatory and immune response markers	Immune	Continuous
Okamoto et al, 2009[81], 2, N	Nutrition (60)	Gastric	Postoperative complications	Complication	Dichotomous
Ozdemir et al, 2019[82], 1, Y	Exercise (85)	Gynecologic	Mean time to first flatus	Functional capacity	Continuous
Pehlivan et al, 2011[83], 1, N	Exercise (60)	Lung	Length of hospital stay	Length of stay	Continuous
Pexe-Machado et al, 2013[84], 1, Y	Nutrition (22)	Gastrointestinal	Length of hospital stay	Length of stay	Continuous
Polakowski et al, 2019[85], 1, N	Nutrition (73)	Colorectal	Postoperative complications	Complication	Dichotomous
Reis et al, 2019[86], 1, N	Nutrition (33)	Colorectal	Length of hospital stay	Length of stay	Continuous
Rizvanovic et al, 2019[87], 1, Y	Nutrition (50)	Colorectal	Perioperative insulin resistance	Metabolic	Continuous
Russell et al, 2019[88], 1, Y	Nutrition (32)	Liver	Postoperative inflammatory response	Immune	Continuous
Torrinhas et al, 2013[51], 1, N	Nutrition (63)	Gastric, colorectal	Postoperative inflammatory response	Immune	Continuous
Uno et al, 2016[90], 1, Y	Nutrition (40)	Bile duct, gall bladder	Postoperative complications	Immune	Continuous
Valkenet et al, 2018[91], 9, Y	Exercise (241)	Esophageal	Postoperative complications	Complication	Dichotomous
Xu et al, 2006[92], 1, N	Nutrition (60)	Colorectal, gastric	Postoperative complications	Complication	Dichotomous
Yamana et al, 2015[93], 1, Y	Exercise (60)	Esophageal	Postoperative complications	Complication	Dichotomous
Zelic et al, 2012[94], 1, N	Nutrition (40)	Colorectal	Changes to serum IL-6	Immune	Continuous

* This publication reported 2 RCTs.

Table 2 Details of sample size reporting for publications

Parameters	Number of Publications (% of Total Publications, n=59)
Allocation ratio	34 (58)
Type I error (α)	
0.5	32 (54)
0.1	1 (2)
Power	
80%	31 (53)
90%	3 (5)
Expected dropout rate	18 (31)
Observed dropout rate	46 (78)
Effect size*	3 (5)
Standard deviation†	11 (31)
Enough information for sample size recalculation	
Continuous primary endpoint	11 (19)
Dichotomous primary endpoint	15 (25)

* If it has been explicitly stated.
† The percentage is calculated with the denominator being the number of publications with continuous variable primary endpoints (n=36).

Table 3 Details of sample size reporting for RCTs

Recalculated Sample Size*	Number of RCTs (% of RCTs With Sufficient Information)
Within a 10% range of what was reported	11 (42)
>10% higher than reported	9 (35)
>10% lower than reported	6 (23)

* Only for RCTs where sufficient information is available (n=26).

given outcome for intervention and control arms. Expected dropout rates were reported in 9 (38%) of the publications reporting dichotomous primary endpoints. Reporting was worse among the 36 publications exploring continuous endpoints; 22 (51%) did not report the anticipated mean and SD of the control arm, and 23 (64%) did not report the anticipated mean of the intervention arm. Expected dropout rates were reported in 9 (25%) of the publications reporting continuous primary endpoints. For detailed sample size calculation reporting data for the included publications, refer to table 2 and supplemental tables S3 and S4.

Of all included publications, 9 (15%) experienced sample size loss (supplemental tables S3 and S4). All included publications compensated for sample size loss in their analysis, if any, using complete case analysis.

Sample size recalculations for RCTs

The available sample size-calculation information reported for each RCT was assessed for the possibility of recalculation. Of the 58 RCTs, 32 (55%) reported either none or insufficient sample size calculation information, and 26 (45%) reported sufficient information allowing us to recalculate the required sample size. Of those that provided sufficient information, 11 (42%) reported variables that allowed for sample size recalculations within a 10% range of the reported sample size, whereas 9 (35%) were >10% higher than reported, and 6 (23%) were >10% lower than reported (table 3).

Discussion

Main findings

Most RCTs investigating prehabilitation interventions insufficiently reported sample size calculation parameters. Even when RCTs provided sufficient sample size calculation information, most were deemed underpowered. For instance, out of the 26 trials for which we could recalculate the required sample size, only 9 (35%) had recruited sample sizes in line with the recalculated sample sizes. The remaining 17 (65%) trials were underpowered and did not have an adequate number of participants. This may result in patients receiving suboptimal care; underpowered or RCTs that are too small could be misleading by missing small but clinically meaningful differences between interventions, or by overestimating the impact of an intervention and finding it significant merely by chance.[5]

Some of the identified trials were poorly reported that made our interpretation very difficult. For example, there were 4 RCTs registered with the same clinical trial registration number that appeared to be slightly different trials, but with extensive overlap of recruiting periods, participants, number of groups, and intervention methods.[27,28] In the end we opted to include 2 of the 4 trials with the same clinical trial registration number that we felt were sufficiently distinguished to minimize overlap.[24,25]

Generally, the data that are lacking from reporting sample size calculation are the anticipated mean and SD of the control group in continuous endpoint RCTs (table 2 and supplemental tables 3 and 4).

Comparing the findings with other literature

Our findings were consistent with numerous systematic reviews across various medical fields that have highlighted worrying issues regarding adherence to the CONSORT guidelines.[6,11,16,17,29,30] One study evaluated same size calculation reporting in the 10 highest impact factor anesthesia journals in 2013, and concluded that the basic elements of sample size calculation were not consistently provided.[6] Across 194 RCTs, it was possible to recalculate the sample size of only two thirds of trials (N=97 [67.8%]).[6]

In addition to recalculating sample size, many reviews perform a post hoc power analysis, revealing that a significant amount of RCTs are underpowered.[6,16,17,31,32] A study aimed to assess the quality of sample size reporting across a total of 228 surgical RCTs from 1999 (N=74) and 2009 (N=121), finding that only two thirds of the trials from each year were adequately powered (42 trials [56.8%] for 1999 and 82 trials [67.8%] for 2009).[31]

Prehabilitation is generally in need of higher quality RCTs. One review from our research center reported that across 74 RCTs,[18] 55% had a high risk of bias for deviating from intended intervention, and 32% had a high risk of bias because of missing outcome data. Only 9 of 13 Template for Intervention Description and Replication[33] items were adequately reported, and only 12% of the RCTs provided information on intervention modifications.[18] Fifty-five percent of trials had a publicly available protocol, with 68% altering primary and/or secondary aims.[18] This review concluded the generalizability of prehabilitation of RCTs is limited because of their high risk of bias as a result of poor reporting quality.[18]

Another review from our center demonstrated the challenges in recruitment for RCTs investigating prehabilitation interventions.[34] providing background on why RCTs in this field may remain underpowered and struggle to recruit required sample sizes. The process of consenting and randomly dividing patients is a challenge; it was found that a median of 3.6 days was required to recruit 1 patient and a median of 13.7 days was required to randomly divide 1 patient.[34] Patient retention was less of a problem; over the duration of a trial, the median dropout rate was 7.9%.[34]

Our study extends these findings by specifically examining power analyses in RCTs investigating prehabilitation interventions. We found that the inadequacies in sample size reporting and power demonstration were not only prevalent but potentially more pronounced in this field. This may be attributed to the multifaceted nature of prehabilitation interventions, which often involve multiple outcome measures and potential effect modifiers. The abovementioned presurgical timing constraints likely further compound these challenges, potentially contributing to the underpowered studies we observed. Our results underscore the need for tailored guidance on sample size calculation and reporting in prehabilitation research to address these field-specific complexities.

Study limitations

Our systematic review had several strengths. To reduce selection bias by capturing as many RCTs as possible to ensure that the included trials were representative of the population, 6 databases were searched, and no publication date restrictions were applied. To strengthen the reliability of our findings, data extraction and sample size recalculation was independently checked by a total of 3 authors (DS, SV, XL).

Limitations may have been present. Some RCTs (N=19) were excluded based on an English-translated version being unavailable. qRCTs were also included (N=1), which may have reduced the quality of the evidence. Because of variations in sample size recalculations, and based on the reporting methods of previous literature,[6,7,17] we have used a margin of error of 10% between our calculated value and the value provided by the RCTs, which may reduce the accuracy of the results; but on the other hand, this choice reduced the risk of falsely accusing these publications.

Implications (clinical and research)

To our knowledge, this was the first systematic review and meta-research study assessing the quality of sample size reporting in RCTs investigating prehabilitation interventions for oncological surgery. Although there are numerous publications already mentioned assessing the quality of sample size reporting in the RCTs of various medical specialities, our study may be more generalizable because we assessed RCTs on participants with a wide range of cancers and outcomes.

The deficiencies in prehabilitation literature underscore the need for stricter sample size reporting requirements in RCTs, including accurate reporting of sample size calculation variables.[35] Our findings indicate that even when RCTs provide sample size calculations, these are often unjustifiable based on their components, as we struggled to recalculate sample sizes for most RCTs with adequate parameters. These RCTs reveal a high frequency of inadequacies adhering to the CONSORT statement. Historically, adherence to all elements of the CONSORT checklist in journal author guidelines has been inconsistent,[36,37] which may contribute to this issue.

By producing a high volume of underpowered RCTs, researchers are inflating time and cost, while also raising potential ethical concerns because they are recruiting participants for trials that will be unable to answer their hypothesis.[38] The results of these trials are inconclusive; they may claim there is no difference in an intervention, when a significant difference would have been observable with an adequately powered trial.[39] By focusing on achieving a sufficient sample size to adequately power their RCTs, while also reporting sufficient sample size calculation information, researchers should report the sample size calculation accordingly to the CONSORT requirements, increasing the quality of evidence, and reducing waste.[31]

This systematic review and meta-research study only assessed the quality of sample size calculation reporting. Future research in the field of prehabilitation could compare deviations between the RCT protocols and the final reported sample size calculations; conduct a quantitative statistical analysis between RCT characteristics and quality of sample size calculation reporting; analyze the impact of the introduction of the CONSORT statement on the reporting quality of sample size calculation in RCTs investigating prehabilitation interventions; analyze the barriers and enablers to sufficient sample size recruitment in prehabilitation trials via survey; and compare sample size reporting quality between RCTs of different interventions (for example, comparing a nutrition-based against an exercise-based intervention). Additionally, future research could investigate the alignment between the information used in sample size calculations and the references cited by the authors, as this could provide valuable insights into the accuracy and reliability of foundational assumptions for sample size calculations in RCTs. Furthermore, a detailed analysis of how RCT authors compensate for dropout rates in their final analyses could offer important insights into the robustness of results and the effectiveness of different compensation strategies.

Conclusions

RCTs investigating prehabilitation interventions in patient populations with cancer poorly report sufficient sample size calculation information so it is not clear whether these RCTs are sufficiently powered; if these RCTs are underpowered they are providing inconclusive results. Increased adherence to the CONSORT statement checklist is required, as most identified RCTs either do not report any sample size calculation information, or it is not possible to recalculate the required sample size because of incomplete information. Further research in this area may assist with eventually developing RCTs with sufficient power and sample size calculation reporting.

Keywords

Cancer; Power: Prehabilitation; Randomized controlled trials; Rehabilitation; Sample size; Surgery

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