

ROLE OF PILOT TRIALS IN RCT QUALITY AND FEASIBILITY

by
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Abstract

Large randomized controlled trials are crucial for assessing intervention efficacy but often fail or remain incomplete due to feasibility issues. Pilot trials may increase full-scale trial success but come with methodological challenges, including the debate over estimating efficacy. Design modifications post-pilot are common but their impact on feasibility is not fully understood. Furthermore, the association between pilot trials and the quality of full-scale trials is underexplored.

This meta-epidemiological study tackles these challenges by assembling two datasets. We searched PubMed for pilot trials published between 2005 and 2018 and their subsequent full-scale trials. The meta-analysis of the full-scale trials was then used to identify other full-scale trials on the same research topic but without a pilot trial.

In Paper 1, we analyzed 248 pairs of pilot and full-scale trials. Full-scale trials with a significant pilot trial were 2.72 times more likely to find a significant result for the primary efficacy outcome than those with a non-significant pilot trial. In 73% of the pairs, the pilot trial yielded a larger point estimate than the full-scale trial, yet in 87% of cases, the pilot's 95% confidence interval encompassed the full-scale point estimate. Paper 2 analyzed 249 pilot and full-scale trial pairs. Using feasibility progression criteria in pilot trials and maintaining the same masking status as the full-scale trial may improve the chances of successful screening, whereas adding extra content to the intervention, changing to active or more frequent control, and altering follow-up lengths and visits may decrease the chances of retaining participants in full-scale trials. In Paper 3, 58 full-scale trials with a pilot trial and 151 full-scale trials without were identified from 47 meta-analyses. A pilot trial's presence was associated with lower risk of bias in full-scale trial random sequence generation, allocation concealment, and participants/researchers masking, but not outcome assessment masking, incomplete outcome data, and selective reporting.

Pilot trials can offer early signals on intervention efficacy. Researchers and funders should weigh both the data from pilot trials and proposed design modifications when evaluating full-scale trials. Pilot trials may improve the quality of ensuing full-scale trials and warrant more frequent consideration.

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Chapter 1 Introduction

1. Impact of Unsuccessful RCTs

Randomized controlled trials (RCTs) are crucial for assessing the effectiveness or efficacy of interventions. However, conducting large-scale trials, such as pivotal drug development trials, demands significant time and resources. The entire process, encompassing planning, conducting, analyzing, and reporting of clinical trials, often spans years. A study of 138 pivotal trials for 59 FDA-approved drugs revealed a median cost of \$19 million per trial and roughly \$40,000 per patient in 2015. This cost can fluctuate dramatically, with estimates varying up to 100-fold ¹⁻³.

Despite the high investment, approximately 45% of trials with results posted on ClinicalTrials.gov from 2000 to 2019 were not completed ⁴. The feasibility of a trial heavily relies on a viable protocol, characterized by successful recruitment, participant adherence, and retention. Yet, approximately 45% of trials fail to meet the planned enrollment target, a figure that has remained fairly constant over time ⁵. Noncompliance with prescribed medication is alarmingly high in real-world practice, with rates estimated at around 50%, leading to roughly 89,000 premature deaths and annual costs exceeding \$100 billion ⁶. Trial drop-outs are common, occurring in 81-95% of trials ⁷, and in certain fields like obesity interventions, the attrition rate can surge to 80% ⁸. Even among trials that reach completion, a significant portion does not demonstrate efficacy. For instance, literature suggests that success rates for oncology drug trials hover between 3.4% and 6.7% ^{9,10}.

Unsuccessful trials contribute to research inefficiencies by delaying results, inflating costs, and potentially biasing findings. The implications of a failed definitive trial extend beyond the direct trial costs and also encompass opportunity costs and expenses associated with previous

research efforts. After factoring in the probability of failure and opportunity costs, the research and development costs for drugs are estimated to lie between \$200 million and \$2.9 billion ¹¹.

2. Utilizing Pilot Trials to Boost RCT Success Rates

A pilot or feasibility study is “a small-scale investigation designed either to test the feasibility of methods and procedures for later use on a large scale, or to search for possible effects and associations that may be worth following up in a subsequent larger study ¹².” In particular, external randomized pilot studies or pilot trials are stand-alone pilot studies that incorporate a randomization procedure ¹³. Although a pilot or feasibility study does not ensure success in the subsequent main study, it is generally perceived to enhance the likelihood of success, efficiency, and validity ¹⁴.

Over the past two decades, pilot and feasibility studies have gained increased attention ^{15,16}. In 2015, a new journal titled 'Pilot and Feasibility Studies' was launched, dedicated solely to these types of studies ¹⁷. Additionally, two reporting guidelines for randomized and nonrandomized pilot and feasibility studies have been recently made available ^{18,19}.

There has been a noticeable surge in the publication of pilot and feasibility studies. Data from PubMed shows an increase in articles containing the MeSH terms "pilot project" or "feasibility study", from 3,430 in 2000 to 12,563 in 2021. The ratio of pilot-to-main trials has also risen significantly from 1.4/100 in 2000 to 15.7/100 in 2022. Notably, approximately 33% to 42% of these published studies on an annual basis were pilot trials.

3. The Central Debate: Efficacy Estimation in Pilot Trials

Historically, the primary emphasis of pilot trials was on efficacy estimation. Hypothesis testing was conducted in 81% of pilot randomized controlled trials published in seven high-impact medical journals during 2007 and 2008 ²⁰. In a similar vein, 69 out of 93 (67.7%) pilot studies

published in Indian journals in 2013 employed at least one statistical test to discern any significant intergroup differences ²¹.

However, these practices have raised methodological concerns due to the potential for underpowered hypothesis testing, leading to a general discouragement of efficacy estimation in pilot trials ¹⁸. Current consensus leans more towards the view that pilot studies or trials are typically not designed to gauge intervention efficacy, but rather to inform study processes and feasibility measures which will impact the design and execution of a larger, subsequent study ^{22,23}.

Yet, a need often arises for preliminary efficacy evidence before committing significant investments in definitive trials. This begs the question: given the limited sample size of pilot trials, how informative can they be regarding efficacy estimation for a larger trial?

4. The Unsettled Question: How Valuable are Pilot Trials for Assessing Feasibility after Design Modifications?

The primary purpose of conducting a pilot trial, as advocated by many, has become feasibility estimation. This process inherently assumes that the feasibility parameters derived from the pilot trial are reliable indicators of the feasibility of the forthcoming trial. Yet, even if the only difference between the pilot and the full trial is the scale of the study, it might be overly optimistic to anticipate the larger-scale trial to reproduce the results of the smaller-scale pilot trial ²⁴.

Furthermore, if alterations are made to the study design (for example, eligibility criteria), the pilot trial's feasibility outcomes may not necessarily extend to the main trial ²⁵.

Cooper et al. identified a systematic bias and substantial variations between the feasibility parameters (namely, randomization and attrition proportions) predicted in the pilot trials and those observed in the definitive trials ²⁶. These differences could be due to modifications in the trial design following the pilot trial. However, the study was based on a relatively small sample of

16 pairs of pilot and corresponding full-scale trials, which restricts further investigations into how trial modifications influence feasibility.

It is quite common for researchers to alter trial design after the pilot trial. In fact, such modifications are often necessary to enhance the feasibility of the trial or the efficacy of the intervention. Beets et al found that 75% of full trials were different from the pilot trial in at least one domain, including intervention intensity and implementation support ²⁷. Regrettably, these alterations are often implemented without a clear understanding of their potential impact on trial feasibility, considering that it would be impractical to rerun the pilot trial for another round of feasibility estimation. Consequently, there is an urgent requirement for evidence regarding the influence and extent to which trial modifications might impact trial feasibility. This would optimize the utilization of pilot trials in guiding the feasibility of full-scale trials.

5. The Gap in Evidence: Do Pilot Trials Enhance the Quality of Full-scale Trials?

Trial quality, defined as the "absence of errors that matter to decision-making" ²⁸, is vital not only for achieving scientific objectives but also for safeguarding the rights and well-being of participants. Since errors can be categorized into random errors and systematic errors or bias, the absence of bias is considered a cornerstone of trial quality. Unfortunately, a significant number of randomized controlled trials to date have suffered from poor methodological quality or a high risk of bias. A study that analyzed over 170,000 RCTs published between 1966 and 2018 revealed a positive trend in trial quality over time ²⁹. However, there remains an urgent need for enhancement, given the persistently high probabilities of bias in treatment allocation, randomization, and masking processes. A similar conclusion was drawn in an earlier 2015 study, which found that 43% of trials had a high risk of bias in at least one domain of the Cochrane Risk of Bias (RoB) Assessment tool ³⁰. Simulations conducted in the study showed

that 50% of these biases and the associated wastage of resources could have been circumvented.

In the last decade, the focus on trial quality has shifted from being a secondary, retrospective aspect of trial science to becoming a central part of trial design³¹. The idea is that reliance on retrospective quality audits should be reduced, with the trial protocol acting as the blueprint for quality³². Since information from pilot trials is incorporated into the design of the main trial, it is reasonable to anticipate that conducting a pilot trial would enhance the main trial's quality. However, the role of pilot trials in improving the quality of the subsequent main trial has been sparingly discussed. Such evidence could carry significant implications for researchers and funders when distributing resources, as well as for reviewers and journal editors during the peer-review process of papers.

6. Objectives of the Study

AIM 1. To evaluate the role of pilot trials in informing full-scale trials' efficacy estimation.

Specifically, we examine (1) the agreement in efficacy estimates between pilot and full-scale trials, (2) the impact on the full-scale trials' power when parameters estimated from pilot trials are used for sample size calculation, and (3) the association between the statistical significance and other characteristics of pilot trials with the efficacy results of full-scale trials.

Hypothesis: (1) Pilot trials tend to overestimate the effect size; (2) Using the effect size or standard deviation derived from pilot trials directly for sample size calculation can result in underpowered full-scale trials; (3) Achieving statistical significance in a pilot trial is independently associated with a "positive" outcome in the main trial.

AIM 2. To contrast feasibility estimates between pilot and full-scale trials and to investigate whether characteristics of pilot trials and subsequent modifications are associated with equivalent or improved feasibility in full-scale trials.

AIM 3. To examine the association between the presence of a pilot trial and the methodological quality of the main trial as assessed by the Cochrane Risk of Bias Assessment tool.

Hypothesis: The conduct of a pilot trial is associated with an enhanced quality of the main trial.

Chapter 2 Paper 1

Re-evaluating the role of pilot trials in informing effect and sample size estimates for full-scale trials: a meta-epidemiological study

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Abstract

Background Some have argued that pilot trials have little value for informing the expected effect size of a subsequent large trial. This study aims to empirically evaluate the roles of pilot trials in informing the effect and sample size estimates of a full-scale trial.

Methods We conducted a search in PubMed on 19 February 2022, for all pilot trials published between 2005 and 2018 and their subsequent full-scale trials. We analyzed the agreement in results by comparing the direction and magnitude of the effect size in the pilot trial and full-scale

trial. Logistic regression was used to explore whether a significant pilot trial and other characteristics were associated with a significant full-scale trial.

Results A total of 248 pairs of pilot and full-scale trials were analyzed. Full-scale trials with a significant pilot trial were 2.72 times more likely to find a significant result for the primary efficacy outcome than those with a non-significant pilot trial (95%CI 1.52 to 4.86, $p=0.001$). The association remained significant irrespective of changes made to the trial design. In 73% of the pairs, the pilot trial produced a larger point estimate than the subsequent full-scale trial, but 87% of pairs had a 95%CI estimated by the pilot trial that covered the full-scale trial point estimate. Full-scale trials with a sample size estimated using the SD from the pilot trial were less likely to yield a significant result (OR=0.26, 95%CI 0.10 to 0.65, $p=0.004$).

Conclusion Pilot trials can provide strong signals on intervention efficacy. When determining the sample size for full-scale trials, using the CI bounds from the pilot trials instead of the point estimate may improve power estimation.

Introduction

Large randomized controlled trials are time consuming and resource consuming. Careful planning is needed before substantial investment in a well-powered trial. This is critical not only for scientific reasons but also to protect the rights and well-being of participants. A major consideration is whether the intervention is efficacious enough so that it is worthwhile to perform a full-scale trial. Such information may be provided by preliminary studies^{1 2}. In a typical drug development process, phase II trials provide preliminary evidence on efficacy, which is subsequently used to justify larger definitive phase III trials. For non-pharmaceutical interventions, trialists often perform a pilot trial to test the potential effects of the intervention. A pilot trial is a type of pilot study that uses a randomized controlled design³.

However, many have questioned the usefulness of pilot trials in informing a subsequent large trial about the magnitude of the effect of an intervention. It has also been argued that the effect size or SD estimated from the pilot data should not be used for full-scale trial sample size calculation⁴. Because pilot trials are usually small in size, the efficacy signals can be missed or exaggerated, leading to false conclusions. For instance, in one study using statistical simulations, even when the true effect size was moderate or large and the sample size was determined using the effect size from the pilot trial, the full-scale trial was found to be underpowered 32% and 23% of the time, respectively⁵. Furthermore, trialists might modify trial design after the pilot trial to improve key aspects, but very few have investigated how those modifications influenced the subsequent efficacy estimates^{6,7}.

The aim of this meta-epidemiological study is to evaluate the role of pilot trials in informing full-scale trials' efficacy estimation. Specifically, we examine (1) the agreement in efficacy estimates between pilot and full-scale trials, (2) the impact on the power of full-scale trials when parameters estimated from pilot trials are used for sample size calculation and (3) the association between the statistical significance and other characteristics of pilot trials with the efficacy results of full-scale trials. We follow guidelines for reporting meta-epidemiological methodology research⁸.

Methods

Literature search

PubMed was searched on 19 February 2022 to identify pilot trials. The search strategy included three concepts: pilot or feasibility study, randomized controlled trial and feasibility parameters. All concepts were searched as MeSH terms and keywords (eTable 1 in Appendix A). A date restriction of 2005–2018 was imposed. This was because few pilot trials were published before 2005 and this ensured that a time window was left after the pilot trial for the full-scale trials to be

conducted and published. The search was restricted to the English language. To maximize the number of pilot full-scale trial pairs, we did not limit the search to a specific research area or disease type.

Study selection

Any pilot trials using a randomized controlled design were eligible for inclusion, except for internal pilot trials in which the data were pooled with the full-scale trial data in the final analysis. We then identified the subsequent full-scale trial by screening similar articles noted by PubMed and papers citing the pilot trial. A study was considered as the full-scale trial of the pilot if the study's result paper acknowledged the pilot trial as a foundation or basis for its design or execution. It was also required that the full-scale trial be conducted by the same research team and have at least one arm that was the same or similar to the pilot trial. Moreover, there had to be an overlap in population characteristics between the pilot and full-scale trials. In cases where the full-scale trial paper did not directly cite the pilot trial, but referenced a study protocol (published or provided as an appendix), we reviewed the protocol to determine if it was based on the pilot trial. We conducted iterative screenings of citations until we located the full-scale trial or determined its non-existence. We excluded full-scale trials that were informed by multiple pilot trials simultaneously to avoid ambiguity in isolating one specific pilot full-scale pair. However, if pilot trials were conducted sequentially and the full-scale trial was primarily informed by the final pilot trial, we included the pair consisting of the final pilot trial and the full-scale trial. We also included pairs where the full-scale trial was informed by one pilot trial alongside other preliminary work. The initial identification of pilot trials and their subsequent full-scale trials was carried out by one investigator (XY) following a predetermined plan. The selection of the final sample was subsequently discussed and agreed on by two investigators (XY and SE).

Data collection and preparation

One investigator (XY) performed data extraction in Covidence⁹ using a pilot-tested form. For each identified pair of pilot and full-scale trials, trial characteristics and results (eg, effect size, p value and CI) were extracted from trial result papers, protocols, statistical analysis plans and trial registries. In the few cases where information was contradictory across different sources, priority was given to protocols and published trial result papers.

Trial characteristics of the full-scale trials were compared with their pilot trials to determine if any modifications had been made to the participant eligibility, intervention, control or outcome. Specifically, we examined whether the intervention and control groups in both trials had the same content, duration and frequency. An intervention or control was classified as the same across both studies if all three aspects were unchanged. We regarded the intervention or control as modified if the intervention or control of the full-scale trial contained more or less content, was longer or shorter in duration or was more or less frequent than it was in the pilot trial. If the changes extended beyond simple additions or reductions in content, the intervention or control was categorized as having other differences. Appendix A eTable 2 provides further definitions and examples of what was considered a modification.

Effect sizes (eg, mean difference, Cohen's d, OR, etc) were extracted from each pilot trial and full-scale trial only if they referred to the same efficacy endpoint, measured using the same methodology and taken at the same or nearest time points. If effect sizes were not directly reported, we extracted the necessary information for their calculation. For continuous outcomes, we calculated Cohen's d based on the appropriate SD for the given design^{10,11}. For binary or time-to-event data, relative measures of association were calculated. When feasible, we employed the same analytic approach for both pilot and full-scale trials to ensure comparability of the estimates. The effect size was considered medium to large in magnitude if Cohen's d >0.5 or ratio >2.74 for a positive association and if Cohen's d <-0.5 or ratio <0.36 for a negative association¹².

A full-scale trial was deemed 'positive' if the stated primary hypothesis of the trial was met. A pilot trial was considered statistically significant if the p value was less than 0.05 for the between-group comparison on the efficacy endpoint that was subsequently used as the primary endpoint in the full-scale trial. We performed appropriate statistical tests on efficacy if the pilot trial did not test for differences between groups.

Statistical analysis

We compared the direction and magnitude of the point estimate of the effect in the full-scale trial to that of the pilot trial. We assessed whether the 95% CI of the effect estimated in the pilot trial included the point estimate of the effect in the full-scale trial. For point estimates sharing the same direction, we calculated the absolute difference by deducting the value of the full-scale trial from that of the pilot trial (ie, pilot – full-scale). Subsequently, we determined the relative difference by dividing this absolute difference by the effect size of the full-scale trial. This relative difference measure facilitated a comparison of the magnitude of the discrepancy relative to the actual value under examination. In cases where both associations were negative, we used the absolute value of Cohen's d or the inverse of the ratio in our calculations. This ensured that a positive difference would always be indicative of a larger magnitude of association in the pilot trial than in the full-scale trial, regardless of the direction of the association.

Since each pair of pilot and subsequent full-scale trial is clustered by the research team and topic, but the study sample in the pilot and full-scale trials are independent from one another, the statistical significance and other characteristics of pilot trial results were analyzed using logistic regression analyses with robust variance estimates to estimate whether one or more factors were predictive of positive full-scale trials¹³. Both univariable and multivariable analyses were conducted for the primary exposure of interest (ie, statistical significance of the full-scale trial's primary endpoint in the pilot trial), with the multivariable analysis model being adjusted for the characteristics of the pilot trial that were associated with the significance of the full-scale

trial. For the remaining trial design and modification characteristics, we reported the estimates while adjusting for intervention type. Additionally, we conducted subgroup analyses based on trial design modification status and other characteristics to explore their impact on the association between pilot trial significance and full-scale trial significance.

All analyses were performed using Stata (V.16; StataCorp, TX, USA) and RStudio (V.2022.12.0+353).

Patient and public involvement

Patients and/or the public were not involved in the design, or conduct, or reporting, or dissemination plans of this research.

Results

Trial characteristics

The initial search yielded 8739 unique citations of potential pilot trials, with the detailed numbers and reasons for exclusion depicted in figure 1. We identified 249 pairs of pilot and full-scale trials (full list available in eTable 3 in Appendix A). One full-scale trial published baseline characteristics, but the primary endpoint was not yet reached. This pair was excluded from the sample, resulting in 248 pairs in the analysis. Two hundred pilot trials assessed and reported efficacy outcomes, and 46% of those trials found a statistically significant between-group difference. One hundred and twenty nine (52%) full-scale trials were positive.

The pilot trial characteristics and modifications are summarized in table 1 and table 2. Most pilot trials (69%) investigated behavioral interventions. The mean number of participants enrolled in pilot trials was 121 (range=7–3318), and the mean pilot-to-full-scale sample size ratio was 24% (IQR=12%–32%). On average, the full-scale trials were published 5 years (IQR=3–7 years) after

the publication of the pilot trial. Sixty-nine per cent, 40% and 15% of trials were modified after the pilot trial on participant eligibility, intervention and control, respectively.

Agreement with full-scale trial effect size

Among the 125 pairs of pilot and full-scale trials that had effect size estimates available for agreement analysis, 26 pairs (21%) had point estimates of effect in the opposite direction.

Among the remaining 99 pairs with point estimates in the same direction, 72 (73%) of the pilot trials yielded a larger point estimate than their subsequent full-scale trials (average absolute difference: 0.37, 95%CI -0.96 to 1.70; figure 2A). Approximately half of the pilot trials had a point estimate 53% larger than the full-scale trial (median relative difference: 53%, IQR=-2% to 187%; figure 2B). The magnitude and variation of the relative differences appeared to decrease when both pilot and full-scale trials had larger sample sizes (figure 2B). Approximately 87% (109 out of 125) of the point estimates from the full-scale trial fell within the 95%CI estimated by the pilot trial.

Association with full-scale trial significance

Pilot trial characteristics and modifications

Tables 3 and 4 show the associations of pilot trial characteristics and subsequent modifications with the full-scale trial significance. Pharmaceutical interventions were less likely to have a positive full-scale trial than behavioral interventions (unadjusted OR=0.27, 95%CI 0.13 to 0.58, $p=0.001$). When the sample size per arm of the pilot trial was more than 15% of the sample size per arm of the full-scale trial, the odds of a significant outcome for the full-scale trial was higher (adjusted OR=1.86, 95%CI 1.09 to 3.18, $p=0.023$). Using the pilot trial's SD for the full-scale trial sample size calculation was associated with reduced odds of the full-scale trial being positive (adjusted OR=0.26, 95%CI 0.10 to 0.65, $p=0.004$). If the full-scale trial increased the length of

the intervention relative to the pilot trial, we observed an increased odds of a positive trial result (adjusted OR=2.11, 95%CI 1.22 to 3.66, p=0.008).

Pilot trial statistical significance

Positive pilot trials were more likely to lead to positive full-scale trials (68% vs 44%, unadjusted OR=2.72, 95%CI 1.52 to 4.86, p=0.001). The OR was 2.41 (95% CI 1.32 to 4.42, p=0.004) after adjusting for pilot trial characteristics significantly linked to the likelihood of a positive full-scale trial, including intervention type, cluster randomized design, participant masking status and ratio of sample size per arm.

We investigated the effect of post-pilot trial modification on the association between pilot trial significance and full-scale trial significance by stratifying the pairs into two subgroups: with modifications and without modifications. Our findings revealed a significant strong association in the no modification subgroup (unadjusted OR=4.78, 95% CI 1.10 to 20.72, p=0.036; adjusted OR=6.87, 95% CI 1.07 to 44.12, p=0.042). We also observed a significant association in the modification subgroup (unadjusted OR=2.33, 95% CI 1.23 to 4.43, p=0.010; adjusted OR=1.99, 95% CI 1.02 to 3.88, p=0.044). The results of subgroup analyses based on other trial design characteristics can be found in figure 3. It is worth noting that the association between pilot trial significance and full-scale trial significance was no longer statistically significant when the analyses were limited to subgroups where the effect size or SD estimated from the pilot trial was used for the calculation of the full-scale trial's sample size (both p>0.05).

Discussion

Our analysis revealed that pilot trials, while not designed for definitive evidence, can still provide strong signals of efficacy. A moderate to strong association was found between pilot and full-scale trial statistical significance, and in most cases, the pilot trial's 95% CI covered the point estimate of the full-scale trial.

Concerns about the usefulness of pilot trials in determining intervention efficacy include modifications made to the trial design and unreliable estimates due to small sample sizes. However, our analysis found that even for pairs with modifications, there was a significant association between pilot and full-scale trial significance, with increased intervention length being the only significant modification. These results suggest that pilot trials remain informative regardless of modifications made to the trial design. While pilot trials tended to produce a larger point estimate than the full-scale trial, most pairs had a 95% CI that covered the full-scale trial point estimate, suggesting that using the CI bound for sample size calculation may be more reasonable than the point estimate of effect size or SD. Statistical methods have been developed to adjust sample size based on pilot estimates, with some advocating for the use of the CI bound ¹⁴⁻¹⁶.

The required sample size for pilot trials has been discussed in the literature. Rules of thumb include 12 participants per arm,¹⁷ 30¹⁴ and 70 overall,¹⁸ and calculation methods for different study objectives are available ¹⁹⁻²¹. Our results suggest that the pilot-to-full-scale sample size ratio rather than the pilot trial sample size itself predicted whether the full-scale trial showed a positive result. Moreover, full-scale trials appeared to be less likely to be significant when the pilot trial had a larger sample size (70+ vs < 30). This was because a higher proportion of non-significant full-scale trials had pilot trials that were cluster randomized trials with typically larger sample sizes than individual randomized controlled trials. Nonetheless, when pilot trial results are used to inform the full-scale trial, the relative size of pilot trials to full-scale trials may be a more relevant metric. Larger pilot trials can yield more precise estimates, leading to a smaller sample size needed for the full-scale trial ^{22 23}. Therefore, an optimal sample size for pilot trials (or the pilot-to-full-scale trial sample size ratio) might exist, which minimizes the total sample size of pilot and full-scale trials combined. Other authors also proposed that the pilot trial's

sample size should be chosen based on the full-scale trial's possible sample size, and the recommended percentage is at least 9% ¹⁹.

It is worth noting that most trials included in our analysis investigated behavioral interventions. An earlier study reported that a positive phase II trial was predictive of success in a phase III trial for pharmaceutical cancer therapies ²⁴. Our study relies on published literature, which raises concerns about potential selection bias due to publication bias. However, we believe that this might bias our results toward the null. This is because pairs of negative pilot and negative full-scale trials are the least likely to be published among the four combinations of pilot and full-scale trial publication status. Consequently, these pairs are undersampled (ie, cell d in the two-by-two table) in our study, which may lead to an underestimation of the OR. The current study was not preregistered but had a prespecified protocol.

Conclusion

While pilot trials do not typically provide definitive evidence on intervention efficacy, they can offer important preliminary evidence and strong signals regarding efficacy. When calculating the full-scale trial sample size, it may be more reasonable to use the CI bound than the point estimate of effect size or SD.

(Paper 1) Table 1 Characteristics of pilot trials (N=248)

Variables	N (%)
Publication year	
2004-2009	74 (30%)
2010-2014	103 (42%)
2015-2019	71 (29%)
Disease	
Addiction	24 (10%)
Mental health	34 (14%)
Obesity & physical activity	26 (10%)
Oncology	21 (8%)
Other ¹	143 (58%)
Intervention	
Behavioral	171 (69%)
Pharmaceutical	41 (17%)
Other ²	36 (15%)
Funding source	
Non-industry	219 (88%)
Industry	6 (2%)
None or not reported	23 (9%)
Cluster randomization	
No	232 (94%)
Yes	16 (6%)
Non-inferiority hypothesis	
No	246 (99%)
Yes	2 (1%)
Participants masked	
No	208 (84%)
Yes	40 (16%)
Caregiver/investigator masked	
No	215 (87%)
Yes	33 (13%)
Evaluator masked	
No	173 (70%)
Yes	75 (30%)
Analyst masked	
No	236 (95%)
Yes	12 (5%)
Number of parties masked	
0	138 (56%)
1	74 (30%)
2	23 (9%)

3	12 (5%)
4	1 (0%)
Pilot sample size, mean±SD	121±300
Pilot sample size, median (IQR)	53 (31, 100)
Pilot sample size per arm, mean±SD	56±147
Pilot sample size per arm, median (IQR)	25 (15, 46)
Sample size ratio (pilot/full-scale), %, mean±SD	24±17
Sample size ratio (pilot/full-scale), %, median (IQR)	20 (12, 32)
Sample size per arm ratio (pilot/full-scale), %, mean±SD	24±17
Sample size per arm ratio (pilot/full-scale), %, median (IQR)	20 (13, 31)
Effect size used for sample size calculation	
No	183 (74%)
Yes	47 (19%)
Yes, but adapted	18 (7%)
Standard deviation used for sample size calculation	
No	219 (88%)
Yes	25 (10%)
Yes, but adapted	4 (2%)
Pilot purpose: assess efficacy	
No	47 (19%)
Yes	153 (81%)
Pilot purpose: assess trial feasibility	
No	155 (77%)
Yes	45 (23%)
Pilot purpose: assess intervention feasibility	
No	96 (48%)
Yes	104 (52%)

SD: standard deviation; IQR: interquartile range

¹ Other include HIV (n=11), pain (n=9), stroke (n=7), diabetes (n=7), heart disease (n=7), and so on.

² Other includes interventions related to devices (n=19), complementary therapies (n=11), occupational therapies (n=4), surgical treatments (n=3), and psychotherapies (n=2).

(Paper 1) Table 2 Characteristics of trial modifications (N=248)

Variables	N (%)
Publication gap year, median (IQR)	5 (3, 7)
Eligibility criteria modification	
Same	76 (31%)
Modified	172 (69%)
Disease criteria	
Same	169 (68%)
Less severe	30 (12%)
More severe	49 (20%)
Other criteria (e.g., age)	
Same	171 (69%)
Less stringent	59 (24%)
More stringent	18 (7%)
Intervention modification	
Same	137 (55%)
Modified	99 (40%)
Other difference	12 (5%)
Intervention content	
Same	176 (71%)
Added content	52 (21%)
Reduced content	8 (3%)
Missing	12 (5%)
Intervention duration	
Same	181 (73%)
Longer duration	48 (19%)
Shorter duration	7 (3%)
Missing	12 (5%)
Intervention frequency	
Same	227 (92%)
More frequent	6 (2%)
Less frequent	3 (1%)
Missing	12 (5%)
Control modification	
Same	165 (67%)
Modified	36 (15%)
Active in main, placebo in pilot	20 (8%)
Placebo in main, active in pilot	10 (4%)
Other difference	17 (7%)
Control content	
Same	178 (72%)
Added content	16 (6%)

Reduced content	7 (3%)
Missing	47 (19%)
Control duration	
Same	189 (76%)
Longer duration	7 (3%)
Shorter duration	5 (2%)
Missing	47 (19%)
Control frequency	
Same	196 (79%)
More frequent	2 (1%)
Less frequent	3 (1%)
Missing	47 (19%)
Length of follow-up	
Same	73 (29%)
Longer	134 (54%)
Shorter	26 (10%)
Missing	15 (6%)

IQR: interquartile range

(Paper 1) Table 3 Associations of pilot trial characteristics with full-scale trial efficacy outcome by logistic regressions with robust variance estimate

	OR	95%CI	P value
<i>General characteristics</i>			
Publication year			
2004-2009	Ref		
2010-2014	1.4	0.76 - 2.55	0.278
2015-2019	0.82	0.43 - 1.58	0.553
Disease			
Addiction	Ref		
Mental health	1.62	0.56 - 4.66	0.375
Obesity & physical activity	1.00	0.33 - 3.04	1
Oncology	1.33	0.41 - 4.34	0.633
Other ¹	0.99	0.41 - 2.35	0.975
Intervention			
Behavioral	Ref		
Pharmaceutical	0.27	0.13 - 0.58	0.001
Other ²	0.93	0.45 - 1.92	0.847
Funding source			
Non-industry	Ref		
Industry	0.18	0.02 - 1.58	0.122
None or not reported	1.18	0.49 - 2.80	0.715
<i>Design characteristics³</i>			
Cluster randomization			
No	Ref		
Yes	0.22	0.07 - 0.72	0.012
Participants masked			
No	Ref		
Yes	0.68	0.30 - 1.50	0.338
Caregiver/investigator masked			
No	Ref		
Yes	0.78	0.33 - 1.84	0.57
Evaluator masked			
No	Ref		
Yes	0.96	0.55 - 1.68	0.889
Analyst masked			
No	Ref		
Yes	0.39	0.11 - 1.34	0.135
Number of parties masked			
0	Ref		

1	0.58	0.33 - 1.03	0.063
2	1	0.36 - 2.74	0.996
>2	0.65	0.17 - 2.50	0.529
Effect size used for sample size calculation			
No	Ref		
Yes	1.52	0.78 - 2.93	0.217
Yes, but adapted	0.56	0.20 - 1.62	0.287
Standard deviation used for sample size calculation			
No	Ref		
Yes	0.26	0.10 - 0.65	0.004
Yes, but adapted	0.64	0.09 - 4.65	0.662
Pilot sample size			
≤30	Ref		
30-70	0.92	0.47 - 1.79	0.799
>70	0.55	0.28 - 1.09	0.086
Sample size ratio (pilot/full-scale) ⁴			
<15%	Ref		
>15%	1.58	0.93 - 2.67	0.089
Pilot sample size per arm			
≤15	Ref		
15-30	0.86	0.44 - 1.69	0.665
>30	0.59	0.31 - 1.13	0.11
Sample size per arm ratio (pilot/full-scale) ⁴			
<15%	Ref		
>15%	1.86	1.09 - 3.18	0.023
Pilot purpose: assess efficacy			
No	Ref		
Yes	1.76	0.98 - 3.19	0.061
Pilot purpose: assess trial feasibility			
No	Ref		
Yes	0.58	0.32 - 1.04	0.069
Pilot purpose: assess intervention feasibility			
No	Ref		
Yes	0.63	0.37 - 1.08	0.091

¹ Other include HIV (n=11), pain (n=9), stroke (n=7), diabetes (n=7), heart disease (n=7), and so on.

² Other includes interventions related to devices (n=19), complementary therapies (n=11), occupational therapies (n=4), surgical treatments (n=3), and psychotherapies (n=2).

³ Estimates are adjusted for intervention type.

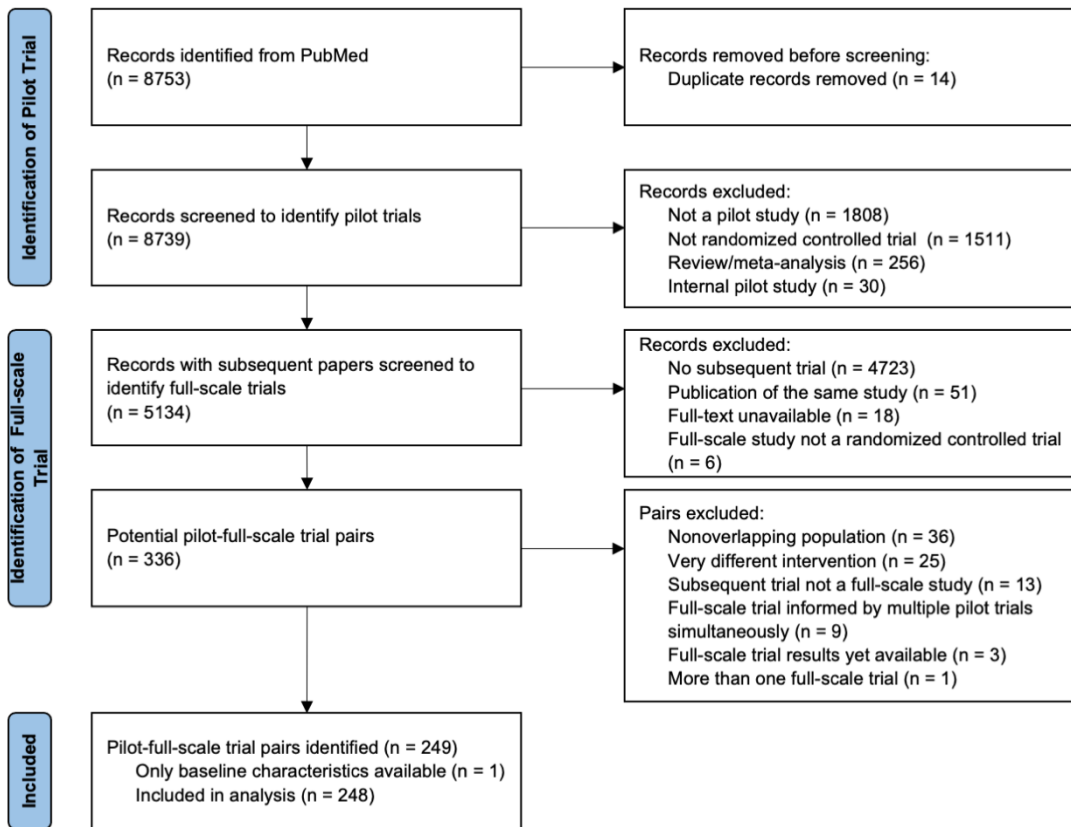
⁴ The 15% cutoff is the optimal value that maximizes the product of sensitivity and specificity when using the sample size ratio to predict the statistical significance of the full-scale trial.

(Paper 1) Table 4 Associations of trial modification characteristics with full-scale trial efficacy outcome by logistic regressions with robust variance estimate and adjusted for intervention type

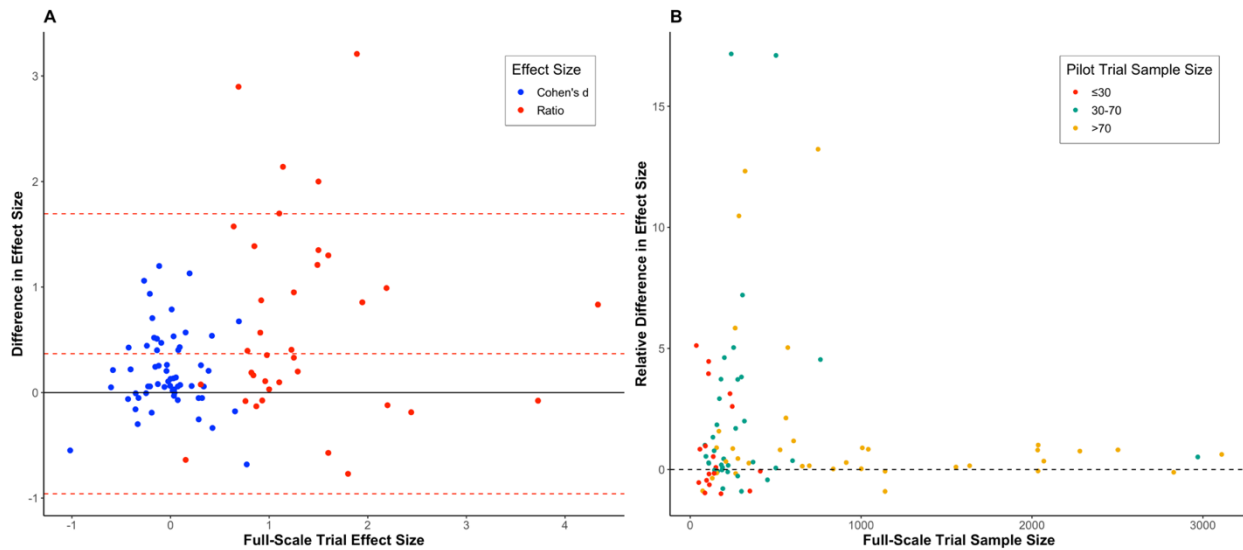
	OR	95%CI	P value
Publication gap year	0.98	0.89 - 1.08	0.656
Eligibility criteria modification			
Same	Ref		
Modified	1.18	0.67 - 2.05	0.569
Disease criteria			
Same	Ref		
Less severe	0.59	0.25 - 1.39	0.229
More severe	1.31	0.69 - 2.49	0.418
Other criteria (e.g., age)			
Same	Ref		
Less stringent	1.59	0.85 - 2.98	0.146
More stringent	0.86	0.31 - 2.38	0.771
Intervention modification			
Same	Ref		
Modified	2.11	1.22 - 3.66	0.008
Other difference	2.15	0.61 - 7.64	0.236
Intervention content			
Same	Ref		
Added content	1.23	0.64 - 2.36	0.531
Reduced content	1.37	0.31 - 6.00	0.674
Intervention duration			
Same	Ref		
Longer duration	3.34	1.60 - 6.97	0.001
Shorter duration	1.96	0.35 - 10.88	0.443
Intervention frequency			
Same	Ref		
More frequent	0.75	0.15 - 3.85	0.729
Less frequent	0.38	0.03 - 4.27	0.433
Control modification			
Same	Ref		
Modified	1.90	0.91 - 3.96	0.086
Active in main, placebo in pilot	1.50	0.56 - 4.05	0.422
Placebo in main, active in pilot	1.50	0.35 - 6.38	0.582
Other difference	1.38	0.46 - 4.10	0.568
Control content			
Same	Ref		
Added content	1.18	0.44 - 3.17	0.739

Reduced content	-	-	-
Control duration			
Same	Ref		
Longer duration	1.16	0.28 - 4.76	0.84
Shorter duration	5.32	0.76 - 37.30	0.093
Control frequency			
Same	Ref		
More frequent	-	-	-
Less frequent	-	-	-
Length of follow-up			
Same	Ref		
Longer	0.98	0.54 - 1.77	0.947
Shorter	0.52	0.20 - 1.36	0.184

(Paper 1) Figure 1 Flowchart

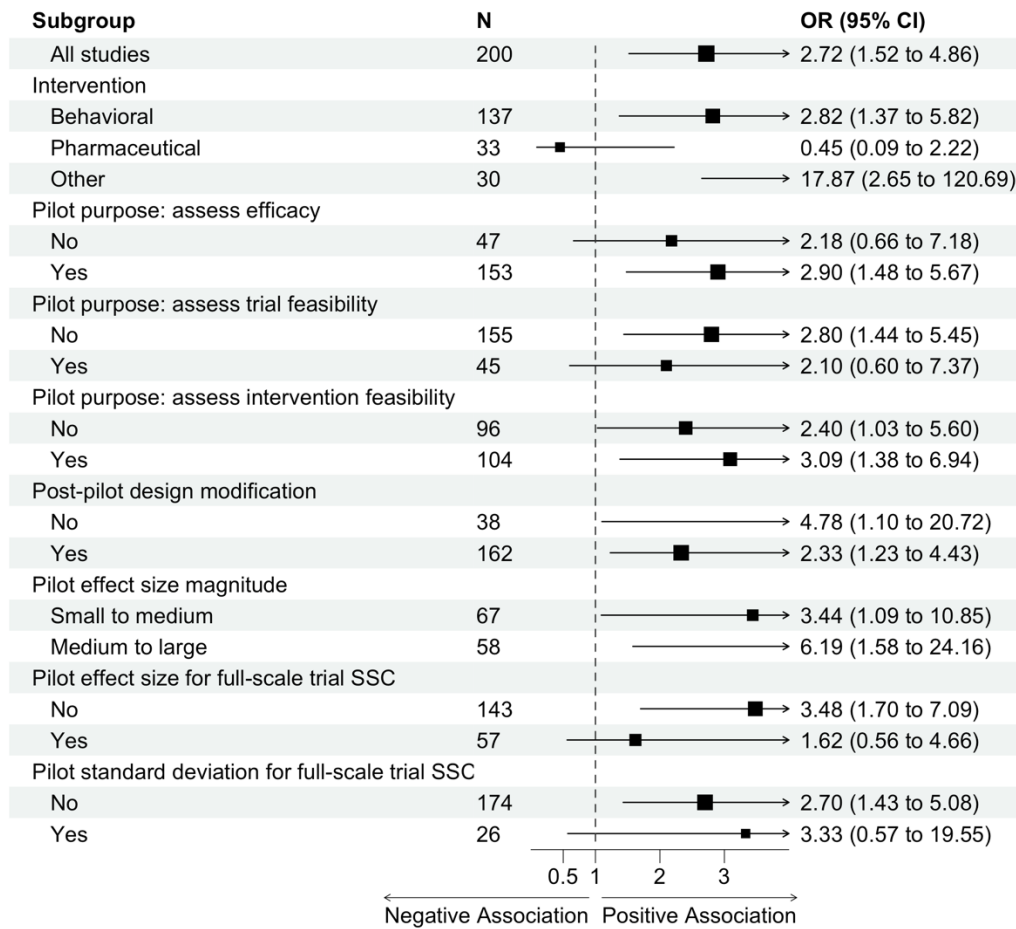


(Paper 1) Figure 2 Bland-Altman plot (A) and scatterplot (B) of differences in effect size estimates between pilot and full-scale trials



(A) The y-axis represents the absolute difference in effect size between the pilot and full-scale trials, obtained by subtracting the value of the full-scale trial from the pilot trial (ie, pilot – full-scale). The three red dashed lines, presented from top to bottom, indicate the upper bound of the 95% CI of the average absolute difference (1.70), the average absolute difference (0.37) and the lower bound of the 95% confidence interval of the average absolute difference (–0.96). To enhance clarity and focus on the main distribution of data points, three outlier pairs were excluded from the graph. These outliers consisted of 2 pairs where the pilot trial estimated an odds ratio larger than 20 and 1 pair where the full-scale trial estimated a Cohen’s d larger than 3. (B) The y-axis represents the relative difference in effect size between the pilot and full-scale trials, obtained by dividing the absolute difference by the value of the full-scale trial (ie, (pilot – full-scale)/full-scale). To enhance clarity and focus on the main distribution of data points, eight outlier pairs were excluded from the graph. These outliers consisted of 2 pairs where the relative difference is larger than 20 and 6 pairs where the full-scale trial sample size is larger than 5000.

(Paper 1) Figure 3 Analyses of the association between the significance of a pilot trial and that of a full-scale trial by subgroups of pilot trial characteristics



SSC, sample size calculation. *Other intervention includes interventions related to devices (n=19), complementary therapies (n=11), occupational therapies (n=4), surgical treatments (n=3) and psychotherapies (n=2).

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Chapter 3 Paper 2

Pilot trial characteristics, postpilot design modifications, and feasibility of full-scale trials

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Abstract

Importance Pilot trials often lead to study design changes in subsequent full-scale trials. Yet, it remains unclear whether these modifications improve the feasibility of the larger trial.

Objective To compare feasibility estimates between pilot and full-scale trials and identify pilot trial characteristics and modifications associated with equivalent or improved feasibility in the full-scale trial.

Design Cohort study

Setting PubMed searched on February 19th, 2022.

Participants Pilot trials published between 2005 and 2018 and their corresponding full-scale trials.

Exposures Pilot trial characteristics and post-pilot trial design modifications.

Main outcomes and measures The outcome of interest was difference in three feasibility parameters: successful screening probability, enrollment rate, and retention probability. We defined these metrics as equivalent or improved if the full-scale trial's estimate was within or exceeding 10% of the pilot trial's estimate.

Results Two hundred forty-nine pairs of trials were analyzed, with 43%, 77%, and 82% of full-scale trials having equivalent or improved successful screening probabilities, enrollment rates, and retention probabilities, respectively. When pilot trials employed feasibility progression criteria (RR=1.94, 95% CI: 1.02-5.97) and maintained masking for participants (RR=1.82, 95% CI: 1.04-4.33) or healthcare practitioners (RR=1.81, 95% CI: 1.03-3.97) consistent with the full-scale trial, the likelihood of achieving equivalent or improved screening success in full-scale trials increased. Increasing study sites post-pilot was associated with higher likelihood of equivalent or improved enrollment rates (RR=1.03, 95% CI: 1.01-1.08). Adding extra content to the intervention (RR=0.82, 95% CI: 0.66-0.98), changing to active control (RR=0.74, 95% CI: 0.48-0.99), administrating the control treatment more frequently (RR=0.60, 95% CI: 0.29-0.93), different follow-up lengths (RR=0.85, 95% CI: 0.73-0.97), and more follow-up visits (RR=0.86, 95% CI: 0.75-0.98) were associated with lower likelihood of equivalent or improved retention probability in the full-scale trial.

Conclusions and relevance In this cohort study of pilot and full-scale trial pairs, pilot trial characteristics and post-pilot modifications had varying association with full-scale trial's feasibility. If full-scale trials planned for masking, it was desirable to use it in the pilot. Modifications increasing participant burden might decrease full-scale trial feasibility. Trialists and funders should consider both pilot trial data and proposed design changes when assessing full-scale trials.

Introduction

In the past two decades, there has been growing attention on pilot studies. A basic PubMed search using the term "pilot study" yielded 668 articles in 2000 and 5484 articles in 2020.

Traditionally, pilot studies served the purpose of evaluating feasibility and providing preliminary evidence on efficacy ¹. However, the appropriateness of pilot studies in evaluating efficacy has been questioned due to their small sample sizes ²⁻⁶. It has been recommended that pilot studies should focus primarily on feasibility estimation, such as calculating probabilities of recruitment, randomization, intervention adherence, and attrition ⁷.

Pilot trials are a specific type of pilot study that utilizes a randomized controlled design ^{8,9}.

Although the emphasis is on using pilot studies or pilot trials for feasibility, few studies have examined the accuracy of their estimates in predicting parameters for full-scale trials. A recent empirical analysis of 16 pairs found that, on average, pilot trials provided variable but unbiased estimates for randomization and attrition probabilities ¹⁰. The authors speculated that the differences could be due to remedial action taken in the full trial to address issues identified in the pilot ¹⁰.

It is not uncommon for trials to modify their designs after the pilot trial, as identifying areas requiring modification is one of the key objectives of conducting a pilot trial. A recent analysis found that 75% of full-scale intervention trials on obesity differed from the pilot trial in at least one domain, such as intervention intensity and implementation support ¹¹. However, it is often unclear how those modifications will impact the feasibility of conducting the full-scale trial, especially when multiple aspects of the trial are being modified, which adds an extra layer of complexity. Ideally, a new pilot trial incorporating those changes would provide the most current feasibility data, but this comes with additional resource demands and potential delays in

generating definitive evidence ¹². Moreover, repeating this approach may not be practical should further modifications be required after the new pilot.

This study therefore aims to compare feasibility estimates between pilot and full-scale trials and explore whether certain pilot trial characteristics and modifications are associated with equivalent or improved feasibility in full-scale trials.

Methods

We followed the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) reporting guideline for cohort studies ¹³. Since the analysis was conducted at the study level without involving human participants, it did not require ethics approval or informed consent.

Literature search and study selection

A systematic search in PubMed was conducted on February 19th, 2022, to identify pilot trials published between January 2005 and December 2018. The search was restricted to English and included three concepts: pilot or feasibility study, randomized controlled trial, and feasibility parameters (eTable 1 in Appendix B). A pilot study was defined as a small-scale investigation aimed at testing feasibility of methodologies for large-scale application, or exploring potential effects and associations to be examined in a future larger study ¹. Stand-alone pilot studies that utilized a randomized controlled design were considered for inclusion. We employed these inclusive early definitions to cover the timeline and to account for the varied use of the term "pilot trial" in literature.

To identify the subsequent full-scale trial that was conducted by the same research team and had an overlap in population characteristics with the pilot, we screened articles that cited the pilot trial. A full-scale trial was included if it had at least one arm that was the same or similar to the pilot. We excluded the full-scale trial if it was informed by multiple pilot trials simultaneously.

Data extraction

We gathered information on trial characteristics, feasibility and efficacy estimates for each pilot and full-scale trial pair in Covidence¹⁴ using a form that had been pilot-tested. Our selection of these characteristics was guided by research on factors influencing trial generalizability¹⁵⁻¹⁷ or participant recruitment and retention¹⁸⁻²⁴. To ensure data accuracy and minimize missingness, we extracted and cross-checked information from trial reports, protocols, and registries, prioritizing trial reports in case of discrepancies. Protocols were crucial for supplementary details when the trial report did not adequately describe elements such as intervention procedures and outcome measurements. We compared the trial characteristics of the full-scale trials with their pilot trials to identify any modifications made to trial design, participant eligibility, intervention, control, and outcome measurement.

Feasibility parameters

The study examined three feasibility parameters: probability of successful screening, enrollment rate, and retention probability.

Successful screening means that a study participant is both eligible and willing to be randomized. We calculated the probability of successful screening by dividing the number of randomized participants by the total number of participants screened.

The enrollment rate was calculated by dividing the number of participants randomized by the duration of recruitment in weeks. A site-average rate was also computed by dividing this overall rate by the number of sites. Unless specifically mentioned otherwise, any reference to 'enrollment rate' in this paper pertains to the overall, not per-site, estimate.

For the probability of retention, we divided the number of participants who completed the study by the number of participants who were initially randomized. Noncompletion can be caused by competing events, withdrawal, loss to follow-up, and protocol deviations. To maintain

consistency, we used the same definition of dropout within each pair of pilot and full-scale trials, as different studies had varying definitions. Whenever possible, we calculated the retention probabilities at the same timepoint in both the pilot and full-scale trials.

Statistical analysis

We described the feasibility estimates from pilot and full-scale trials using either the mean and standard deviation (SD) or median and interquartile range (IQR) if the estimate was heavily skewed. To evaluate the agreement between the pilot and full-scale trials, we calculated the percentage difference by dividing the difference between the two studies (i.e., pilot - full-scale) by their mean ²⁵.

All pilot trials in our sample progressed to full-scale trials, indicating that the trialists deemed the full-scale trial feasible, either initially or after making protocol modifications. We considered the full-scale trial's feasibility estimate to be equivalent if it fell within 10% of the pilot trial's estimate, in either direction, or improved if it was more than 10% greater than the pilot trial estimate. We chose the 10% threshold because it accounted for possible random fluctuations and was commonly used in sample size calculations to adjust for dropouts. We used logistic regression to identify characteristics and modifications of pilot trials associated with equivalent or improved feasibility in the full-scale trial. The resulting odd ratios were converted to relative risk (RRs), and corresponding percentile-based confidence intervals (CIs) were calculated using 10000 bootstrap replications ²⁶.

All analyses were performed using Stata (Version 16; StataCorp, TX) and RStudio (Version 2022.12.0+353). A two-sided P value smaller than .05 was considered statistically significant.

Results

Study characteristics

A total of 249 pairs of pilot and subsequent full-scale trials were identified (eFigure 1 in Appendix B). These pairs investigated a range of diseases (eTable 2 in Appendix B), with the majority (69%) focusing on behavioral interventions (Table 1). Most pilot trials (75%) were conducted in a single center, while more than half (54%) of full-scale trials were multicenter. The proportion of trials with two arms was the same for both pilot and full-scale trials (84%). On average, 121 individuals (SD: 300, median: 53) were randomized in pilot trials, while full-scale trials randomized an average of 1164 individuals (SD: 4111, median: 264). The average and median follow-up duration in full-scale trials were approximately twice as long as in pilot trials (321 vs. 166 days for average and 182 vs. 91 days for median).

Data on successful screening probability, enrollment rate, and retention probability were available in 183, 177, and 238 pairs of pilot and full-scale trials, respectively. Comparisons of characteristics between pairs with and without missing data for these parameters are provided in Supplemental text, eTable 3, and eTable 4 in Appendix B.

Successful screening

On average, the successful screening proportion (n=183) was 47% (SD: 27%) for pilot trials and 41% (SD: 27%) for full-scale trials. The mean percentage difference between pilot and full-scale trials was 15% (SD: 60%; median: 14%; IQR: -21% – 47%). As shown in Figure 1, the percentage differences are symmetrically distributed around the mean, with a tendency for both the magnitude and variability to decrease as the sample size of the pilot trial increases.

The full-scale trial showed equivalent (n=35) or improved (n=54) successful screening in 89 of the 183 pairs (43%). The likelihood of achieving equivalent or improved successful screening in the full-scale trials were higher when the pilot trial utilized masking/blinding (RR=1.41, 95% CI: 1.05 – 1.93) and feasibility progression criteria (RR=1.94, 95% CI: 1.02 – 5.97) (Table 2). When the pilot trial was single-center, the full-scale trial had higher likelihood of achieving an

equivalent or improved successful screening if it was also conducted at a single center (RR=1.50, 95% CI: 1.04 – 2.31) (Table 3). When participants or healthcare practitioners were masked in the full-scale trials, the likelihood of observing an equivalent or improved successful screening probability were higher if the pilot trial also masked the participants or healthcare practitioners compared to situations where they were unmasked (RR=1.82, 95% CI: 1.04 – 4.33; RR=1.81, 95% CI: 1.03 – 3.97, respectively) (Table 3).

Enrollment rate

The median overall enrollment rate (n=177) was 1.7 participants per week (IQR: 0.6 – 5.4) for pilot trials and 2.9 participants per week (IQR: 1.3 – 8.5) for full-scale trials. The mean percentage difference between the two was -52% (SD: 85%, median: -59%, IQR: -121% – 4%). For the site-average enrollment rate, pilot trials had a median rate of 1.2 participants per week per site (IQR: 0.5 – 3.3), while full-scale trials had a median rate of 1.0 participants per week per site (IQR: 0.4 – 3.3). The mean percentage difference was 7% (SD: 92%, median: 7%, IQR: -56% – 83%).

Out of 177 pairs, 136 (77%) had equivalent (n=9) or improved (n=127) overall enrollment rates in the full-scale trial compared to the pilot trial. Having one more study site in the full-scale trial was associated with 1.03 times higher likelihood of equivalent or improved enrollment rates (95% CI: 1.01 – 1.08) (Table 3). When healthcare practitioners were unmasked in the pilot trial, the full-scale trial had higher likelihood of achieving an equivalent or improved enrollment rate if trialists did not change this design feature (RR=1.51, 95% CI: 1.02 – 2.83) (Table 3). However, modifying the intervention was associated with lower likelihood of equivalent or improved enrollment rates (RR=0.84, 95% CI: 0.70 – 0.99), as was extending the length of follow-up in the full-scale trial (RR=0.81, 95% CI: 0.68 – 0.96) (Table 4).

Retention probability

The retention probability (n=238) was found to be similar for both pilot and full-scale trials, with an average of 83.5% (SD: 15%) and 84.2% (SD: 13%), respectively (mean percentage difference: -1%, SD: 19%, median: 0%, IQR: -9% – 6%) (eFigure 2 in Appendix B).

Approximately 82% (194/238) of full-scale trials achieved an equivalent (n=138) or improved (n=56) retention probability. If the pilot trial had a sample size between 30 and 50, the retention probability had higher likelihood of being equivalent or improved compared to pilot trials with a sample size below 30 (RR=1.21, 95% CI: 1.03 – 1.44) (Table 2). The likelihood of having an equivalent or improved retention probability were lower if the full-scale trial added extra content to the intervention (RR=0.82, 95% CI: 0.66 – 0.98), changed the comparison group from placebo or no treatment to active control as opposed to simple modification (RR=0.74, 95% CI: 0.48 – 0.99), administrated the control intervention more frequently (RR=0.60, 95% CI: 0.29 – 0.93), had a different length of follow-up (RR=0.85, 95% CI: 0.73 – 0.97), or conducted more follow-up visits (RR=0.86, 95% CI: 0.75 – 0.98) (Table 4).

Discussion

This study first compared feasibility estimates between pilot and full-scale trials. On average, screening success was slightly lower (7%) in full-scale trials, with only 43% of trials showing improved screening. However, 77% of full-scale trials had better enrollment rates (average increase of 52%). Estimated retention probability had good agreement between pilot and full-scale trials, with a 1% difference and over half of the values within the 10% equivalence margin. This aligns with a previous study comparing 16 pairs of pilot and full-scale trials ¹⁰.

The observed decrease in screening proportion and increase in enrollment rates in full-scale trials could be attributed to the greater number of study sites compared to pilot trials. While multi-site trials can expedite enrollment through simultaneous recruitment at different sites, they also face a more diverse participant pool. This diversity may lower the screening proportion as

not all seemingly eligible participants ultimately qualify. Our associational analysis indeed showed that trials with more sites than their pilot often achieved higher enrollment, but multi-center full-scale trials following single-center pilots had lower likelihood of similar or improved screening success. Therefore, researchers conducting full-scale trials at multiple sites may anticipate faster recruitment but should also prepare for a larger screening pool to reach the target sample size.

Masking has been widely recognized as a factor that can hinder study recruitment^{18,27-29}. We found that masking was one of the few design features in pilot trials that was associated with an equivalent or improved probability of successful screening. Our results also suggest that if masking is envisioned in the full-scale trial, it is desirable to use it in the pilot trial. We recommend that the pilot and full-scale trials be consistent in terms of masking to maximize recruitment feasibility.

Our analyses also found that protocol modifications may decrease the feasibility of full-scale trials if they impose a greater burden on participants. Such modifications include additional intervention content, changing the comparator from placebo or no treatment to active treatment, administering the control treatment more frequently, prolonged follow-up periods, and increasing the number of follow-up visits. Previous qualitative and quantitative evidence has suggested that potential trial participants may perceive high time commitments and demanding follow-up schedules as too burdensome³⁰⁻³³, leading to increased screen failure and dropouts³⁴. Quantifying participant burden and incorporating it into study protocol to evaluate feasibility has been suggested³⁵⁻³⁷.

It has been recommended that pilot trials incorporate prespecified progression criteria to aid in the decision-making process for proceeding with a full-scale trial⁷. Typically, these criteria set a threshold above which the full-scale trial is deemed feasible. The decision to proceed can be made in a binary fashion by comparing the feasibility parameter's point estimate to the threshold

or by testing if the CI around the estimate includes the threshold. While progression criteria have been used in research practice ³⁸, few studies have investigated whether their use improves the performance of pilot trials in informing the feasibility of full-scale trials. Our analysis suggests that using feasibility progression criteria in the pilot trial may result in an equivalent or improved probability of successful screening in the full-scale trial. However, we did not observe a similar association for recruitment rate or retention probability. Further examination of the data revealed that this difference may be attributed to subsequent modifications made to the trial design. These modifications were associated with worse retention probability and recruitment rate, while maintaining or enhancing screening probability. Our findings imply that the utility of progression criteria might be undermined by modifications made after the pilot phase.

In the current study, we adopted a broad definition of pilot trials, not excluding studies solely due to the implementation of effect size estimation or hypothesis testing, despite concerns have been raised about these practices ²⁻⁶. This approach is partially based on the understanding that treatment efficacy could affect trial retention and participant recruitment. We also presumed that studies, even if not explicitly assessing feasibility, inherently do so during execution.

Nonetheless, pilot trials primarily focusing on efficacy estimation were excluded at the analysis stage if they did not report the three feasibility parameters of interest.

This study has certain limitations. First, we did not differentiate between “true” pilot trials and those potentially mislabeled. However, we posit that post-hoc mislabeling of studies as pilot trials to excuse small sample sizes, low methodological quality, or incomplete studies is less likely in our dataset, considering all studies informed a full-scale trial. Second, by excluding pilot trials not followed by full-scale trials, we may have observed an attenuated association. The absence of a full-scale trial may indicate its infeasibility even with significant modifications. The association between trial modifications and feasibility would be stronger in such cases because

the modifications altered the full-scale trial from being infeasible to feasible. Thirdly, we employed a complete case analysis, excluding pairs with missing feasibility estimates. This non-reporting indicates a lack of adherence to the Consolidated Standards of Reporting Trials (CONSORT) guidelines³⁹ and possibly inferior methodological quality, as reporting quality often proxies for methodological quality⁴⁰. Fourth, there are other important factors that can influence trial recruitment and retention, such as the use of incentives and the follow-up format^{18,19}, which we were not able to examine in our study. Fifth, multiple trial aspects may be modified simultaneously, and these modifications may influence the feasibility of the full-scale trial in different ways and magnitudes. Lastly, the current study examined various characteristics. However, per nature of its design, the width of the confidence intervals was not adjusted for multiple comparisons, and the results should be viewed as exploratory.

Using pilot trial estimates to inform the full-scale trial's feasibility can be challenging due to biases introduced by modifications and random errors magnified by the small sample size. While the agreement between pilot and full trials may improve with larger sample sizes, systematic errors may still persist. Trialists and funders should consider potential impacts of protocol modifications on feasibility when planning or assessing a full-scale trial. On average, full-scale trials had slightly lower screening success, better enrollment rates, and comparable retention probabilities than the pilot trial. Consistency in masking is desirable, and the pilot trial's use of feasibility progression criteria might improve full-scale trial feasibility. Modifications that increase participant burden may make full-scale trials less feasible.

(Paper 2) Table 1 Key characteristics shared by pilot trials and subsequent full-scale trials

	No. (%)	
	Pilot trial (n=249)	Full-scale trial (n=249)
Disease ^a		
Addiction	24 (10)	
Mental health	34 (14)	
Obesity & physical activity	27 (11)	
Oncology	21 (8)	
Other	143 (57)	
Intervention		
Behavioral	172 (69)	
Pharmaceutical & other	77 (31)	
Publication year		
2004-2009	74 (30)	6 (2)
2010-2014	103 (41)	51 (20)
2015-2019	72 (29)	123 (49)
2020-2022	0 (0)	69 (28)
Funding source		
Non-industry	220 (88)	230 (92)
Industry	6 (2)	12 (5)
None or not reported	23 (9)	7 (3)
Cluster randomization		
No	233 (94)	211 (85)
Yes	16 (6)	38 (15)
No. of sites		
Single center	186 (75)	115 (46)
Multicenter	63 (25)	134 (54)
No. of arms		
2	210 (84)	210 (84)
>2	39 (16)	39 (16)
Sample size		
Mean (SD)	121 (300)	1164 (4111)
Median (IQR)	53 (31, 100)	264 (143, 600)
Masking used		
No	139 (56)	90 (36)
Yes	110 (44)	159 (64)
Primary length of follow-up (days)		
Mean (SD)	166 (237)	321 (428)
Median (IQR)	91 (42, 182)	182 (91, 365)
Intervention efficacy		
Not statistically significant	109 (44)	119 (48)
Statistically significant	92 (37)	129 (52)
Not evaluated	48 (19)	1 (0)

Abbreviations: SD, standard deviation; IQR, interquartile range.

^a The diseases listed represent the top four most frequently occurring within the dataset. All other disease types are grouped under the category labeled as "other." A complete list of diseases is available in eTable 2 in Appendix B.

(Paper 2) Table 2 Association of pilot trial characteristics with concordance in feasibility estimates

	Successful screening probability (n=183)	Enrollment rate per week (n=177)	Retention probability (n=238)
	Relative risk (95%CI)	Relative risk (95%CI)	Relative risk (95%CI)
<i>General characteristics</i>			
Disease ^b			
Addiction	1 [Reference]	1 [Reference]	1 [Reference]
Mental health	1.46 (0.75 - 3.75)	1.21 (0.85 - 1.85)	1.07 (0.86 - 1.38)
Obesity & physical activity	1.29 (0.59 - 3.36)	0.84 (0.41 - 1.43)	0.92 (0.67 - 1.24)
Oncology	1.58 (0.70 - 4.16)	0.92 (0.54 - 1.52)	1.08 (0.84 - 1.38)
Other	1.32 (0.78 - 3.27)	1.20 (0.91 - 1.82)	0.96 (0.81 - 1.24)
Intervention			
Behavioral	1 [Reference]	1 [Reference]	1 [Reference]
Pharmaceutical & others	1.18 (0.85 - 1.93)	1.03 (0.91 - 1.46)	1.30 (1.02 - 2.41) ^a
Publication year			
2004-2009	1 [Reference]	1 [Reference]	1 [Reference]
2010-2014	1.22 (0.81 - 1.97)	1.07 (0.88 - 1.33)	1.09 (0.94 - 1.28)
2015-2019	1.64 (1.11 - 2.62) ^a	0.95 (0.76 - 1.21)	1.07 (0.91 - 1.28)
Funding source			
Non-industry	1 [Reference]	1 [Reference]	1 [Reference]
Industry	1.03 (0.39 - 1.74)	NA	NA
None or not reported	1.03 (0.53 - 1.60)	0.90 (0.54 - 1.20)	0.93 (0.69 - 1.15)
Cluster randomization			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	0.93 (0.32 - 1.61)	1.11 (0.78 - 1.29)	0.89 (0.59 - 1.14)
<i>Recruitment number</i>			
No. of sites			
Single center	1 [Reference]	1 [Reference]	1 [Reference]
Multicenter	0.93 (0.73 - 1.37)	1.02 (0.90 - 1.46)	1.14 (0.97 - 1.89)
No. of arms			
2	1 [Reference]	1 [Reference]	1 [Reference]
>2	1.14 (0.85 - 2.30)	1.07 (0.94 - 3.64)	0.97 (0.95 - 1.47)
Sample size			
≤30	1 [Reference]	1 [Reference]	1 [Reference]
30-50	0.93 (0.53 - 1.62)	0.98 (0.74 - 1.29)	1.21 (1.03 - 1.44) ^a
50-100	1.38 (0.94 - 2.23)	1.07 (0.86 - 1.36)	1.10 (0.92 - 1.33)

>100	1.04 (0.63 - 1.76)	1.00 (0.79 - 1.28)	1.05 (0.86 - 1.30)
Sample size per arm			
≤15	1 [Reference]	1 [Reference]	1 [Reference]
15-45	1.12 (0.78 - 1.72)	0.99 (0.81 - 1.24)	1.12 (0.97 - 1.31)
>45	1.03 (0.64 - 1.65)	1.04 (0.84 - 1.31)	1.05 (0.87 - 1.26)
<i>Masking usage</i>			
Masking used			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.41 (1.05 - 1.93) ^a	1.03 (0.87 - 1.21)	1.04 (0.92 - 1.17)
Participants masked			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.64 (1.20 - 2.19) ^a	0.96 (0.75 - 1.17)	1.05 (0.88 - 1.20)
Healthcare practitioner masked			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.04 (0.39 - 1.76)	NA	0.87 (0.40 - 1.10)
Assessor masked			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.10 (0.79 - 1.48)	1.10 (0.94 - 1.30)	1.00 (0.87 - 1.13)
Analyst masked			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.91 (1.24 - 2.24) ^a	0.75 (0.35 - 1.11)	1.00 (0.67 - 1.16)
<i>Outcome and aims</i>			
Primary length of follow-up (months)	1.01 (0.97 - 1.03)	1.01 (1.00 - 1.04)	1.00 (0.99 - 1.01)
Intervention efficacy			
Not statistically significant	1 [Reference]	1 [Reference]	1 [Reference]
Statistically significant	1.16 (0.83 - 1.61)	1.12 (0.94 - 1.35)	1.00 (0.88 - 1.14)
Not evaluated	1.02 (0.59 - 1.56)	1.01 (0.78 - 1.27)	0.90 (0.73 - 1.08)
Pilot aim: efficacy			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	0.96 (0.70 - 1.37)	0.91 (0.77 - 1.08)	1.07 (0.93 - 1.26)
Pilot aim: safety			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.17 (0.84 - 1.75)	1.05 (0.88 - 1.27)	0.95 (0.85 - 1.08)
Pilot aim: feasibility			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.03 (0.51 - 1.61)	1.03 (0.76 - 1.26)	1.07 (0.87 - 1.22)

Feasibility progression criteria used			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.94 (1.02 - 5.97) ^a	0.96 (0.85 - 1.61)	0.94 (0.87 - 1.26)

Abbreviations: NA, not available.

^a P<.05.

^b The diseases listed represent the top four most frequently occurring within the dataset. All other disease types are grouped under the category labeled as "other." A complete list of diseases is available in eTable 2 in Appendix B..

(Paper 2) Table 3 Association of modifications on recruitment number and masking usage with concordance in feasibility estimates

Modifications compared to pilot	Successful screening probability (N=183) Relative risk (95%CI)	Enrollment rate per week (N=177) Relative risk (95%CI)	Retention probability (N=238) Relative risk (95%CI)
<i>Recruitment number</i>			
Ratio of sample size per arm (pilot/full-scale)			
<50%	1 [Reference]	1 [Reference]	1 [Reference]
>50%	1.36 (0.78 - 1.95)	0.70 (0.31 - 1.08)	1.10 (0.87 - 1.22)
Effect size used for sample size calculation			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.28 (0.93 - 1.72)	1.02 (0.83 - 1.21)	1.04 (0.91 - 1.18)
Standard deviation used for sample size calculation			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	0.81 (0.61 - 1.30)	1.02 (0.88 - 1.72)	1.00 (0.90 - 1.61)
Difference in no. of sites	1.00 (0.98 - 1.00)	1.03 (1.01 - 1.08) ^a	1.00 (1.00 - 1.02)
No. of sites			
Pilot single-center, full-scale multi-center	1 [Reference]	1 [Reference]	1 [Reference]
Both single-center	1.50 (1.04 - 2.31) ^a	0.90 (0.74 - 1.10)	0.95 (0.82 - 1.11)
Both multi-center	1.20 (0.74 - 1.94)	0.97 (0.79 - 1.18)	1.06 (0.91 - 1.23)
No. of countries			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More	1.66 (0.75 - 5.94)	1.17 (0.88 - 2.15)	NA
<i>Masking usage</i>			
Number of parties masked			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More	0.97 (0.68 - 1.35)	1.00 (0.85 - 1.19)	1.05 (0.92 - 1.19)
Fewer	1.56 (0.997 - 2.19)	0.86 (0.52 - 1.18)	0.95 (0.69 - 1.18)
Participant masking status			
Pilot unmasked, full-scale masked	1 [Reference]	1 [Reference]	1 [Reference]
Both unmasked	1.15 (0.69 - 2.82)	1.36 (0.96 - 2.38)	0.90 (0.79 - 1.07)
Both masked	1.82 (1.04 - 4.33) ^a	1.39 (0.93 - 2.46)	0.94 (0.77 - 1.16)
Pilot masked, full-scale unmasked	2.06 (0.80 - 4.50)	0.85 (0.28 - 1.80)	0.98 (0.60 - 1.15)
Healthcare practitioner masking status			

Pilot unmasked, full-scale masked	1 [Reference]	1 [Reference]	1 [Reference]
Both unmasked	0.99 (0.60 - 2.20)	1.51 (1.02 - 2.83) ^a	0.95 (0.82 - 1.17)
Both masked	1.81 (1.03 - 3.97) ^a	1.55 (0.89 - 2.90)	1.08 (0.80 - 1.30)
Pilot masked, full-scale unmasked	NA	NA	NA
Assessor masking status			
Pilot unmasked, full-scale masked	1 [Reference]	1 [Reference]	1 [Reference]
Both unmasked	0.91 (0.62 - 1.35)	1.01 (0.82 - 1.27)	0.92 (0.80 - 1.06)
Both masked	0.98 (0.64 - 1.49)	1.07 (0.85 - 1.35)	1.00 (0.86 - 1.16)
Pilot masked, full-scale unmasked	1.23 (0.63 - 1.99)	NA	0.75 (0.46 - 1.03)
Analyst masking status			
Pilot unmasked, full-scale masked	1 [Reference]	1 [Reference]	1 [Reference]
Both unmasked	0.75 (0.52 - 1.24)	0.85 (0.75 - 1.06)	1.10 (0.89 - 1.47)
Both masked	1.11 (0.42 - 1.78)	0.74 (0.28 - 1.05)	0.80 (0.29 - 1.26)
Pilot masked, full-scale unmasked	NA	0.56 (0.19 - 0.96) ^a	NA

Abbreviations: NA, not available.

^a P<.05.

(Paper 2) Table 4 Association of modifications on PICO components with concordance in feasibility estimates

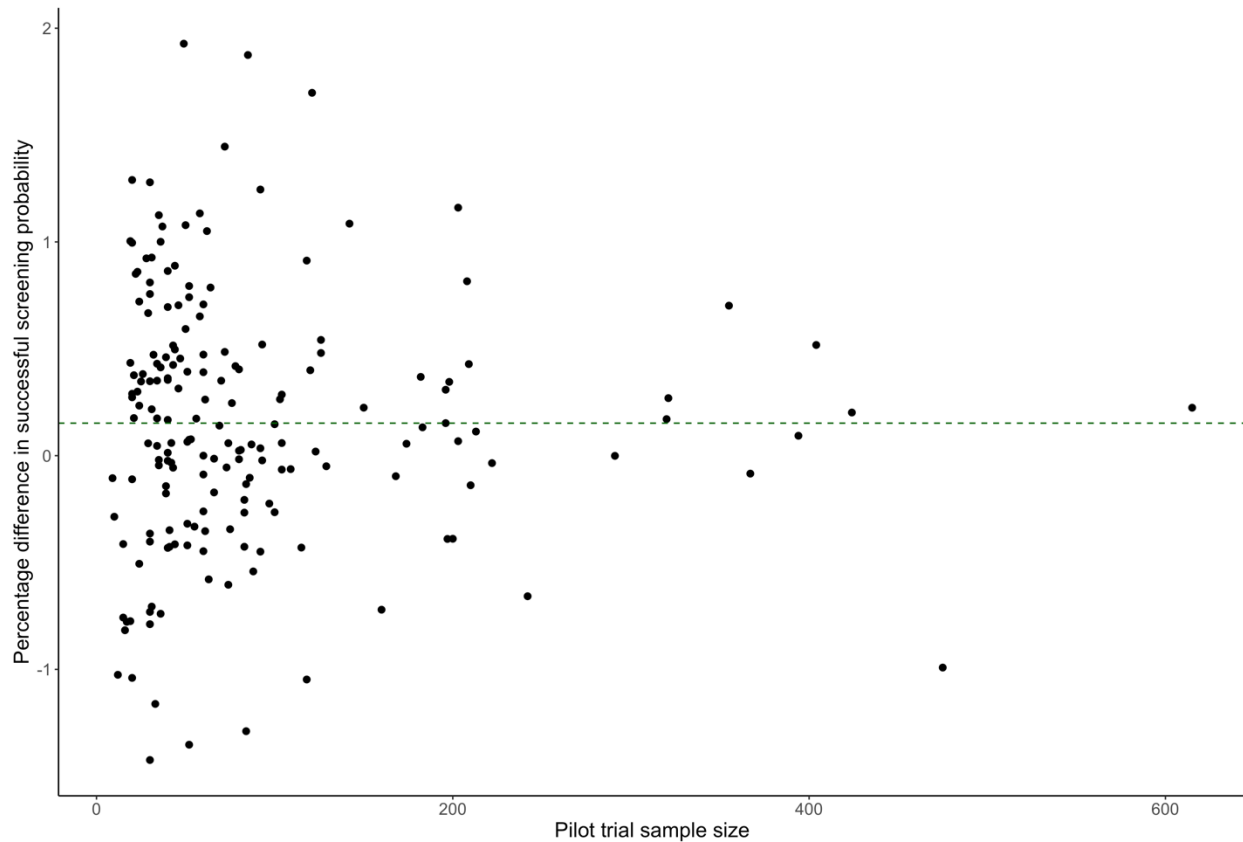
Modifications compared to pilot	Successful screening probability (N=183)	Enrollment rate per week (N=177)	Retention probability (N=238)
	Relative risk (95%CI)	Relative risk (95%CI)	Relative risk (95%CI)
<i>Population (P)</i>			
Eligibility modified			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	0.83 (0.61 - 1.13)	0.98 (0.83 - 1.17)	0.99 (0.88 - 1.14)
Eligibility modification type			
Broader	1 [Reference]	1 [Reference]	1 [Reference]
Narrower	0.89 (0.51 - 1.39)	0.91 (0.70 - 1.14)	1.04 (0.88 - 1.23)
Same	0.96 (0.68 - 1.39)	0.92 (0.77 - 1.10)	1.00 (0.87 - 1.17)
<i>Intervention (I)</i>			
No. of arms			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More	0.78 (0.33 - 1.33)	1.08 (0.79 - 1.29)	1.00 (0.78 - 1.18)
Fewer	1.01 (0.57 - 1.49)	0.96 (0.63 - 1.23)	0.98 (0.76 - 1.17)
Intervention modification type			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Modified	0.93 (0.67 - 1.26)	0.84 (0.70 - 0.99) ^a	0.97 (0.84 - 1.09)
Other difference	0.43 (0.16 - 1.07)	0.85 (0.46 - 1.12)	0.99 (0.65 - 1.16)
Intervention modified			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	0.88 (0.63 - 1.18)	0.84 (0.70 - 0.99) ^a	0.97 (0.85 - 1.09)
Intervention content			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Added content	0.74 (0.45 - 1.07)	1.00 (0.81 - 1.20)	0.82 (0.66 - 0.98) ^a
Reduced content	0.37 (0.21 - 1.22)	0.51 (0.19 - 1.01)	1.03 (0.64 - 1.12)
Intervention duration			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Longer duration	1.12 (0.75 - 1.54)	0.85 (0.63 - 1.08)	1.00 (0.84 - 1.15)
Shorter duration	1.24 (0.44 - 1.86)	0.94 (0.40 - 1.11)	NA
Intervention frequency			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More frequent	0.80 (0.29 - 1.60)	0.64 (0.30 - 1.00)	1.02 (0.51 - 1.13)
Less frequent	0.99 (0.46 - 1.57)	0.64 (0.30 - 1.00)	NA

<i>Comparison (C)</i>			
Control modified			
No	1 [Reference]	1 [Reference]	1 [Reference]
Yes	1.15 (0.83 - 1.56)	1.01 (0.84 - 1.19)	0.99 (0.86 - 1.12)
Control modification type			
Modified	1 [Reference]	1 [Reference]	1 [Reference]
Same	0.89 (0.60 - 1.50)	0.97 (0.80 - 1.23)	0.93 (0.82 - 1.09)
Active in main, placebo in pilot	0.96 (0.44 - 1.83)	1.03 (0.66 - 1.35)	0.74 (0.48 - 0.99) ^a
Placebo in main, active in pilot	1.28 (0.50 - 2.25)	0.63 (0.23 - 1.08)	0.71 (0.29 - 1.03)
Other difference	1.04 (0.46 - 1.98)	0.97 (0.63 - 1.33)	1.07 (0.86 - 1.22)
Control content			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Added content	1.33 (0.69 - 1.99)	1.11 (0.79 - 1.29)	1.06 (0.82 - 1.20)
Reduced content	0.72 (0.36 - 1.68)	1.05 (0.44 - 1.20)	1.01 (0.49 - 1.11)
Control duration			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Longer duration	0.85 (0.32 - 1.70)	0.64 (0.22 - 1.08)	NA
Shorter duration	1.41 (0.51 - 1.88)	NA	NA
Control frequency			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More frequent	NA	0.65 (0.31 - 1.01)	0.60 (0.29 - 0.93) ^a
Less frequent	NA	NA	NA
<i>Outcome (O)</i>			
Length of follow-up (longest)			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Longer	0.95 (0.68 - 1.38)	0.81 (0.68 - 0.96) ^a	0.93 (0.81 - 1.06)
Shorter	0.78 (0.33 - 1.38)	0.94 (0.71 - 1.14)	0.99 (0.78 - 1.16)
Length of follow-up (primary)			
Same	1 [Reference]	1 [Reference]	1 [Reference]
Different	1.32 (0.98 - 1.78)	0.84 (0.69 - 0.99) ^a	0.85 (0.73 - 0.97) ^a
No. of follow-up visits			
Same	1 [Reference]	1 [Reference]	1 [Reference]
More	0.93 (0.66 - 1.27)	1.01 (0.84 - 1.22)	0.86 (0.75 - 0.98) ^a
Fewer	0.87 (0.40 - 1.43)	1.20 (0.94 - 1.41)	0.94 (0.74 - 1.10)

Abbreviations: PICO, patient/population/problem, intervention, comparison, outcome; NA, not available.

^a P<.05.

(Paper 2) Figure 1 Scatterplot of percentage difference in successful screening probability vs pilot trial sample size



Dots represent the percentage difference, calculated by dividing the difference between the two studies (i.e., pilot - full-scale) by their mean value. The dashed line represents the average percentage difference.

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Chapter 4 Paper 3

Pilot trials may improve the quality of full-scale trials: a meta-research study

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Abstract

Objectives

Evidence on the value of pilot trials for subsequent trial's quality is scarce. This study aims to determine if a pilot trial improves the quality of the full-scale trial.

Study Design and Setting

We searched PubMed for pilot trials and their subsequent full-scale trials. The meta-analysis of the full-scale trials was used to identify other full-scale trials on the same research topic but without a pilot trial. Markers of trial quality included publication outcomes and Cochrane Risk of Bias (RoB) assessment.

Results

Fifty-eight full-scale trials with a pilot trial and 151 full-scale trials without were identified from 47 meta-analyses. Trials with a pilot trial were published 0.9 years sooner (mean \pm standard deviation: 1.7 ± 1.0 vs. 2.6 ± 2.0 , $P = 0.005$) and in peer-reviewed journals with higher impact

factors (60.9 ± 75.0 vs. 24.8 ± 50.3 , $P < 0.001$). A pilot trial's presence was associated with lower risk of bias in full-scale trial random sequence generation (OR [95% CI]: 4.05 [1.27–12.91]), allocation concealment (2.89 [1.07–7.83]), and participants/researchers masking (4.31 [1.37–13.50]), but not outcome assessment masking (1.03 [0.49–2.18]), incomplete outcome data (1.27 [0.47–3.42]), and selective reporting (1.23 [0.44–3.46]).

Conclusion

Conducting a pilot trial may enhance the quality of the subsequent full-scale trial.

Introduction

Trial quality is a concept that is often discussed but hard to define. It is multidimensional and relates to the design, conduct, and analysis of a trial ¹. The Clinical Trials Transformation Initiative defines that quality is the “absence of errors that matter to decision-making” ². Hence, lack of bias is one mainstay of trial quality. However, until now, a considerable number of randomized controlled trials (RCTs) have suffered from poor methodological quality, including a high risk of bias. One study analyzed over 170,000 RCTs published between 1966 and 2018 ³. A positive time trend in trial quality was found, but there existed an urgent need for improvement, as relatively high probabilities of bias remained in the processes of treatment allocation, randomization, and masking. Similar results were found in an earlier paper published in 2015, where colleagues found that 43% of trials had a high risk of bias in at least one domain of the Cochrane Risk of Bias (RoB) assessment tool ⁴. Pilot and feasibility studies are designed to generate sufficient evidence that researchers and funding agencies may use to assess whether it is worthwhile or feasible to carry out a larger trial ⁵.

This study aims to assess whether conducting a pilot trial is associated with improved quality of full-scale trials. As randomization is a critical aspect of conducting RCTs, we will limit our focus

to external pilot trials that utilize a randomized controlled design. These pilot studies serve as stand-alone studies that precede the full-scale trial ⁶.

Materials and methods

Literature search and study selection

We searched PubMed on February 19, 2022, for pilot trials published between 2005 and 2018.

The search strategy and study selection criteria are detailed in Appendix C, and the study selection process is illustrated in Figure 1. We identified stand-alone pilot trials that utilized a randomized controlled design and searched for subsequent full-scale trials conducted by the same research team by reviewing papers that cited the pilot trial. We then looked for meta-analyses on the primary endpoint of the full-scale trial by reviewing papers that cited the trial.

When multiple meta-analyses were available, the most relevant or recently published one was chosen, with a preference for Cochrane meta-analyses.

Using the meta-analysis, we identified other full-scale trials on the same research topic but without a pilot trial. Systematic reviews alone were not used as a source to identify other trials, because the inability to perform a meta-analysis often indicates substantial heterogeneity. To ensure the comparability of trials, we manually reviewed the patient, population or problem, intervention, comparison, and outcome (PICO) components of each trial in the meta-analyses. Only trials that were most similar to our trial with a preceding pilot were included for comparison. In most cases, we relied on the subgroup that had already been defined by the meta-analysis author. The full-text of the similar trials was reviewed to determine if they were full-scale trials and if they were informed by a pilot trial or any other forms of preliminary work. Trials were excluded if they were informed by any preliminary work or if the information available was insufficient to determine even after consulting trial protocol and registration information. A full-scale trial was considered to have a preceding pilot trial if explicitly stated in the paper,

regardless of whether the pilot trial was published or not. Any trial newly found to be informed by a pilot trial was moved to the group of prepiloted full-scale trials. We included both individual and cluster randomized trials. The initial study selection was carried out independently of the outcome status and according to a predetermined plan by one investigator (XY). A second investigator (SE) reviewed the selected studies. Both investigators agreed on the final sample.

Data collection

Two-level data were collected by one investigator (XY) from the meta-analysis, original trial paper, and other sources, such as trial registry. Meta-analysis level characteristics included publication information, study field, intervention format, and PICO components. Individual trial level variables included trial design features, research team experience, publication outcomes, intervention efficacy results, trial feasibility measures, and Cochrane RoB assessment results. To maintain the independence of outcome adjudication, we mainly relied on the RoB assessment made by the meta-analysis authors. We cross-checked the meta-analysts' judgment on the incomplete outcome data domain with the dropout rate extracted from the original trial. Five meta-analyses did not rate all domains. We made these assessments without knowledge of pilot trial status and checked for consistency between our assessment and that of other meta-analyses, if available.

Measurement of trial quality

Both publication outcomes and RoB results were proxies of trial quality. We calculated the time lag between study completion and print publication date. We obtained publishing journal impact factors (IF) from Clarivate's Web of Science Journal Citation Reports for the year in which the trial was published, as well as for the most recent year of available data as of our search date (2021). Additionally, we recorded the corresponding author's H-index as of January 2023 from the Web of Science ⁷. The original version of the Cochrane RoB assessment tool was used to

measure the methodologic quality of the trials. Although a revised version of this tool was proposed recently ⁸, the original version is still by far the most commonly used. Results across these two versions are largely consistent ⁹.

Statistical analysis

We recalculated the I-squared statistic for each set of similar trials as a simple check of between-trial heterogeneity. We examined the crude associations between the presence of pilot trials and the statistical significance of intervention efficacy, feasibility outcomes, funding status, and publication outcomes of the full-scale trial.

The original Cochrane RoB assessment tool included seven domains, namely, random sequence generation (selection bias), allocation concealment (selection bias), masking of participants and personnel/researchers (information bias), masking of outcome assessment (information bias), incomplete outcome data (selection bias), selective reporting (reporting bias), and other bias ¹⁰. Each domain was analyzed separately. The last domain of other bias was not assessed in most meta-analyses and therefore was not included in our analyses. Domains were rated as having a low, high, or unclear risk of bias.

We compared the dichotomized risk of biases (low vs. high or unclear) from full-scale trials with and without a pilot trial using random-effects logit models with a clustered sandwich estimator of variances, considering that the trials are clustered within the meta-analysis. Default adaptive quadrature was used in the analysis with a quadrature check conducted after each random-effects model. If needed, the number of quadrature points was increased to ensure reliable model fit ¹¹. We also performed logistic regressions with robust variance estimators (RVE), treating trials within each meta-analysis independently as a comparison to the random-effects model results.

Adjusted and unadjusted analyses were conducted, with adjustment variables selected based on prior knowledge, statistical significance, and magnitude of associations in univariable analyses. All adjusted models included study region, number of authors, presence or absence of group author, corresponding author's H-index 1 year before the (pilot) trial, study field, and year of study initiation (before or after 2005). Number of authors and corresponding author's H-index were continuous variables and quadratic terms were added to the model to account for nonlinear associations if needed. Additionally, the analysis of the domain "masking of participants and personnel" accounted for sample size and intervention format, while "masking of outcome assessment" was analyzed by adjusting for sample size. Finally, the length of follow-up was included in the model for "incomplete outcome data" and "selective reporting".

All statistical analyses were performed using Stata (Version 16; StataCorp, TX) with a two-sided P value <0.05 considered statistically significant.

Results

Study characteristics

A total of 47 meta-analyses were retrieved (Table 1). Half of them were published in 2020 and onward (range: 2013–2022). Approximately one-third of them were Cochrane reviews and meta-analyses. The most frequently examined intervention type was behavioral intervention (31.9%), followed by psychological intervention (29.8%) and pharmacological intervention (17.0%).

From those meta-analyses, 58 full-scale trials had a preceding pilot trial, and 151 full-scale trials did not (Table 2). Participant average age and gender distribution were similar between trials with and without a pilot. The median sample size was significantly larger in trials with a pilot than in those without (341.0 vs. 229.0, $P = 0.018$). Full-scale trials with a pilot trial were more recently conducted, more recently published, had more authors, and were more likely to have group authorship (all $P < 0.05$). Regardless of the presence or absence of a pilot trial, researchers

were at approximately the same stage of their career before the pilot or full-scale trial started (median H-index of corresponding author: 8.0 vs. 8.0, $P = 0.910$).

Efficacy, feasibility, funding status, and publication outcomes

Among full-scale trials with and without a pilot trial, 31.0% and 39.7%, respectively, found a statistically significant difference in the primary efficacy endpoint (Table 3). Two trials with a pilot trial stopped early for futility, and three trials without a pilot trial were terminated early for reasons of futility ($n = 1$), efficacy ($n = 1$), and failure to enroll ($n = 1$). Regardless of the presence or absence of a pilot trial, full-scale trials spent similar time on recruitment (mean: 2.1 vs. 2.2 years) and had comparable recruitment rates (median: 18.8 vs. 16.0 participants per month) and dropout percentages (mean: 12.6% vs. 13.5%). There was a higher proportion of full-scale trials with a pilot trial receiving government funding (82.8% vs. 56.3%, $P < 0.001$) and multiple sources of funding (24.1% vs. 17.9%, $P = 0.041$) compared to those without a pilot trial. Trials with a pilot trial were on average published 0.9 years sooner after trial completion (1.7 ± 1.0 vs. 2.6 ± 2.0 years, $P = 0.005$) and were published in peer-reviewed journals that had a higher IF (60.9 ± 75.0 vs. 24.8 ± 50.3 as of 2021, $P < 0.001$) than trials without a pilot trial.

Risk of bias

Most full-scale trials fell under the low risk of bias category (Table 4). The exception was that for the domain of masking of participants and researchers, only 31 (20.5%) full-scale trials without a pilot trial and 20 (34.5%) full-scale trials with a pilot trial were rated as having a low risk of bias. Across all domains, the proportion of being categorized as “low risk of bias” appeared to be higher among trials with a pilot trial than trials without a pilot.

Table 5 shows the results of the unadjusted and adjusted regression analyses for the associations of pilot trials with the risk of bias in the subsequent full-scale trial. Logistic regression with RVE indicated that the presence of a pilot trial was significantly associated with

a higher probability of achieving a low risk of bias in the random sequence generation of the full-scale trial (OR = 3.2, 95% CI: 1.18–8.64; adjusted OR = 4.05, 95% CI: 1.27–12.91), but this association was not observed in the random-effects model. The presence of a pilot trial was also associated with a low risk of bias in the full-scale trial's allocation concealment (OR = 4.95, 95% CI: 1.94–12.68; adjusted OR = 2.89, 95% CI: 1.07–7.83) and masking of participants and researchers (OR = 4.2, 95% CI: 1.80–9.79; adjusted OR = 4.31, 95% CI: 1.37–13.50) both before and after adjustment. No significant association was found in either model for masking of outcome data or incomplete outcome data. An unadjusted analysis indicated a positive association between pilot trials and selective reporting in the full-scale trial (OR = 2.78, 95% CI: 1.04–7.45), but this association became null after the adjustment (OR = 1.23, 95% CI: 0.44–3.46).

Discussion

Pilot studies are designed to answer the question “Can we do this?” by testing the performance characteristics of study designs, outcome measures, procedures, recruitment criteria, and operational strategies ^{12,13}. Our analyses found that RCTs with pilot trials were published sooner and in higher-impact peer-reviewed journals. We also saw strong positive associations between the pilot trial and a lower risk of bias in the full-scale trial's allocation concealment and masking. This may be attributed to the fact that allocation concealment and masking procedures have been tested during the pilot, and can therefore be more smoothly implemented during the full-scale trial.

The logistic regression with RVE found statistically significant associations between conducting a pilot trial and the domain of random sequence generation in both unadjusted and adjusted analyses. However, the random-effects logit models did not show statistically significant associations. It is worth noting that in these models, the point estimate for the association

(unadjusted OR = 3.37, adjusted OR = 4.93) was large enough to be considered substantively important. Nevertheless, due to the wide confidence interval (CI) that included the null value, the evidence supporting the association was not sufficiently strong.

The smallest point estimate was found for masking of outcome assessment across all domains. This is likely due to the fact that the ability to mask outcome assessment is largely determined by the nature of the outcome. In our sample, both RCTs with and without a preceding pilot trial shared the same outcome. If the outcome assessment cannot be masked by nature (e.g., patient-reported outcomes where patients cannot be masked), conducting a pilot trial may not make a significant difference.

In our unadjusted analyses, we observed a lower risk of bias in selective reporting among full-scale trials that conducted pilot trials. It is possible that researchers used the pilot trials to test various outcomes of interest and selected the most reliable and valid ones for use in full-scale trials, thereby reducing the likelihood of selective reporting. However, this significant association was no longer present after adjusting for whether the study was initiated before or after 2005, the year when journals began mandating trial protocol registration. This finding suggests that pilot trials may not have as significant an impact on selective reporting, or at least not as significant as the trial registration requirement.

Theoretically, the presence of a pilot trial may improve the feasibility of a full-scale trial, resulting in fewer incomplete outcome data. However, the point estimate (OR = 1.27) for the incomplete outcome data domain may be too small to be of importance, although the confidence interval extends to values (3.42) that could be significant. These findings align with the null associations of pilot trials with the recruitment rate and attrition proportion of full-scale trials in our study, which are two other crucial feasibility parameters.

It is possible that the point estimate for domain incomplete outcome data is confounded by indication, as the presence of a pilot trial may indicate feasibility concerns. Without a pilot trial, the full-scale trial may have faced more feasibility issues. To eliminate “confounding by indication,” we restricted comparisons to full-scale trials on the same/similar research question and adjusted for factors such as trial size, context, research team experience, and design features like length of follow-up in the analysis. However, residual confounding may still exist. Additionally, our analyses were based solely on published papers, which could be another source of bias, as studies that failed to complete due to feasibility issues may be less likely to be published in peer-reviewed journals. Therefore, our study may have observed an underestimated association between pilot trials and full-scale trial feasibility by undersampling full-scale trials that did not have a pilot trial and failed to complete and publish due to feasibility issues.

Furthermore, the generalizability of pilot trial feasibility results to a full-scale trial may be limited by modifications made to the trial design after the pilot phase ¹⁴. An analysis of 16 pairs of pilot and full-scale trials found that pilot trials underestimated attrition and overestimated enrollment capacity as compared to their full-scale trials ¹⁵. Another recent analysis revealed that 75% of full-scale trials differed from the pilot trial in at least one domain, such as intervention intensity and implementation support ¹⁶. There is a need for methods that can better predict the feasibility of a full-scale trial based on pilot data. In particular, trialists need to understand how modifications based on pilot-trial data change key trial performance metrics of full-scale trials.

There are several limitations inherent in the current study. Firstly, approximately one-third of the trials investigated nondrug interventions, which may impact the generalizability of our findings to pharmaceutical interventions, particularly in masking-related domains. Secondly, some of the included studies were cluster randomized trials, which require specific risk of bias considerations in addition to those for individual randomized trials. Thirdly, an overall risk of bias

score was not computed or compared as this is discouraged when using the original RoB assessment tool ¹⁷. Finally, while our study had a prespecified protocol, it was not preregistered.

Conclusion

The study sheds light on the association between pilot trials and the quality of subsequent full-scale trials. Full-scale trials with a pilot trial were published sooner and in journals with higher impact factors. A published pilot trial was associated with a higher likelihood of a low risk of bias particularly in allocation concealment and masking. These findings have important implications for researchers and funders when allocating resources, as well as for reviewers and journal editors during the paper peer-review process.

(Paper 3) Table 1 Characteristics of the 47 meta-analyses that contributed 58 full-scale trials with a pilot trial and 151 full-scale trials without a pilot trial

	N (%)
Publication year, median (range)	2020 (2018, 2021)
Journal impact factor, median (range)	10.3 (4.6, 12.0)
Cochrane review	
No	32 (68.1%)
Yes	15 (31.9%)
Research field	
Clinical medicine	23 (48.9%)
Epidemiology and health behavior	16 (34.0%)
Healthcare services	8 (17.0%)
Patient, population or problem	
Cancer	6 (12.8%)
Cardiovascular conditions	3 (6.4%)
Endocrinal, nutritional and metabolic disorders	4 (8.5%)
Healthcare delivery	7 (14.9%)
Infections	3 (6.4%)
Lifestyle and wellbeing	7 (14.9%)
Mental health and behavioral conditions	9 (19.1%)
Musculoskeletal conditions	6 (12.8%)
Other	2 (4.3%)
Intervention format	
Medical procedure or device	3 (6.4%)
Medicine	9 (19.1%)
Program	35 (74.5%)
Intervention content	
Behavioral therapies	15 (31.9%)
Complementary and alternative therapies	3 (6.4%)
Healthcare delivery interventions	6 (12.8%)
Medical devices	1 (2.1%)
Pharmacological interventions	8 (17.0%)
Psychological therapies	14 (29.8%)
Outcome	
Compliance with treatment	3 (6.4%)
Function	2 (4.3%)
Infection	2 (4.3%)
Mental health	8 (17.0%)
Mortality	4 (8.5%)
Other	10 (21.3%)
Pain	7 (14.9%)
Resource use	3 (6.4%)

Smoking cessation	2 (4.3%)
Weight/physical activity	6 (12.8%)
I-squared	
>50	17 (39.5%)
≤50	26 (60.5%)

SD: standard deviation

(Paper 3) Table 2 Characteristics of 209 full-scale trials by the presence or absence of a pilot trial

	With pilot (N=58)	Without pilot (N=151)	P-value*
Age of study participants (average \pm SD)	41.8 \pm 22.2	42.8 \pm 24.4	0.79
Percent women (average \pm SD)	60.7 \pm 25.4	57.8 \pm 25.1	0.46
Study region			0.13
Africa	0 (0.0%)	5 (3.3%)	
America	20 (34.5%)	48 (31.8%)	
Asia	3 (5.2%)	21 (13.9%)	
Australia	7 (12.1%)	11 (7.3%)	
Europe	21 (36.2%)	58 (38.4%)	
International	7 (12.1%)	8 (5.3%)	
Cluster randomized controlled trial			0.50
No	46 (79.3%)	113 (74.8%)	
Yes	12 (20.7%)	38 (25.2%)	
Number of participants randomized			
Average \pm SD	878.0 \pm 1291.3	934.2 \pm 2310.6	0.86
Median (range)	341.0 (205.0, 755.0)	229.0 (100.0, 702.0)	0.018
Time from baseline to last follow-up, months			
Average \pm SD	7.6 \pm 6.1	10.5 \pm 11.5	0.071
Median (range)	6.0 (3.0, 12.0)	7.0 (3.0, 12.0)	0.17
Study start year			
1989-2005	3 (5.3%)	55 (40.4%)	<0.001
2006-2018	54 (94.7%)	81 (59.6%)	
Publication year			<0.001
1990-2010	6 (10.3%)	52 (34.4%)	
2011-2021	52 (89.7%)	99 (65.6%)	
Number of authors			
Average \pm SD	11.3 \pm 7.0	7.4 \pm 4.2	<0.001
Median (range)	9.0 (7.0, 13.0)	6.0 (4.0, 9.0)	<0.001
Group author			0.007
No	45 (77.6%)	138 (91.4%)	
Yes	13 (22.4%)	13 (8.6%)	
H-index of corresponding author			
Average \pm SD	12.3 \pm 13.6	14.8 \pm 18.5	0.34
Median (range)	8.0 (3.0, 18.0)	8.0 (1.0, 23.0)	0.91

SD: standard deviation

* P values are derived from Student's t tests for means, Wilcoxon rank-sum tests for medians, Pearson's Chi-squared tests for frequencies if all cell counts exceed 5 or Fisher's Exact tests if at least one cell count is less than or equal to 5.

(Paper 3) Table 3 Intervention efficacy, feasibility, funding status, and publication outcomes of full-scale trials by the presence or absence of a pilot trial

	With pilot (N=58)	Without pilot (N=151)	P-value*
Efficacy significant			0.24
No	40 (79.0%)	91 (60.3%)	
Yes	85 (31.0%)	60 (39.7%)	
Trial early stop			0.62
No	56 (96.6%)	148 (98.0%)	
Yes	2 (3.4%)	3 (2.0%)	
Recruitment length, years			
Average \pm SD	2.1 \pm 1.4	2.2 \pm 1.7	0.81
Median (range)	1.9 (1.2, 2.7)	1.6 (1.0, 2.9)	0.84
Recruitment rate, per month			
Average \pm SD	95.6 \pm 274.5	217.9 \pm 1049.2	0.49
Median (range)	18.8 (7.4, 45.5)	16.0 (4.4, 46.4)	0.50
Dropout, %			
Average \pm SD	12.6 \pm 12.4	13.5 \pm 12.0	0.62
Median (range)	10.5 (2.5, 19.6)	11.0 (4.4, 22.0)	0.57
Industry funding			0.16
No	56 (96.6%)	136 (90.1%)	
Yes	2 (3.4%)	15 (9.9%)	
Government funding			<0.001
No	10 (17.2%)	66 (43.7%)	
Yes	48 (82.8%)	85 (56.3%)	
Organization funding			0.66
No	38 (65.5%)	94 (62.3%)	
Yes	20 (34.5%)	57 (37.7%)	
Number of funding sources			0.041
None/not reported	2 (3.4%)	23 (15.2%)	
1	42 (72.4%)	101 (66.9%)	
2 or more	14 (24.1%)	27 (17.9%)	
Journal impact factor (publication year)			
Average \pm SD	19.1 \pm 20.7	7.9 \pm 12.7	<0.001
Median (range)	8.1 (4.5, 21.7)	3.9 (2.5, 5.6)	<0.001
Time from study completion to publication, years			
Average \pm SD	1.7 \pm 1.0	2.6 \pm 2.0	0.005
Median (range)	1.6 (1.0, 2.1)	2.3 (1.4, 3.2)	0.003
Journal impact factor (2021)			
Average \pm SD	60.9 \pm 75.0	24.8 \pm 50.3	<0.001
Median (range)	17.5 (6.5, 96.2)	5.6 (3.8, 10.7)	<0.001
H-index of corresponding author (2023)			

Average ± SD	36.3±25.0	32.4±27.6	0.34
Median (range)	29.5 (20.0, 44.0)	25.0 (11.0, 45.0)	0.090

SD: standard deviation

*P values are derived from Student's t tests for means, Wilcoxon rank-sum tests for medians, Pearson's Chi-squared tests for frequencies if all cell counts exceed 5 or Fisher's Exact tests if at least one cell count is less than or equal to 5.

(Paper 3) Table 4 Assessment of risk of bias of the full-scale trials by the presence or absence of a pilot trial

	With pilot (N=58)	Without pilot (N=151)	P-value*
Random sequence generation (selection bias)			0.043
Low risk of bias	53 (91.4%)	116 (76.8%)	
High risk of bias	0 (0.0%)	6 (4.0%)	
Unknown risk of bias	5 (8.6%)	29 (19.2%)	
Allocation concealment (selection bias)			<0.001
Low risk of bias	49 (84.5%)	81 (53.6%)	
High risk of bias	0 (0.0%)	22 (14.6%)	
Unknown risk of bias	9 (15.5%)	48 (31.8%)	
Masking of participants and researchers (information bias)			0.013
Low risk of bias	20 (34.5%)	31 (20.5%)	
High risk of bias	31 (53.4%)	75 (49.7%)	
Unknown risk of bias	7 (12.1%)	45 (29.8%)	
Masking of outcome assessment (information bias)			0.51
Low risk of bias	38 (65.5%)	88 (58.3%)	
High risk of bias	9 (15.5%)	23 (15.2%)	
Unknown risk of bias	11 (19.0%)	40 (26.5%)	
Incomplete outcome data (selection bias)			0.32
Low risk of bias	44 (75.9%)	102 (67.5%)	
High risk of bias	9 (15.5%)	24 (15.9%)	
Unknown risk of bias	5 (8.6%)	25 (16.6%)	
Selective reporting (reporting bias)			0.014
Low risk of bias	46 (79.3%)	87 (57.6%)	
High risk of bias	3 (5.2%)	15 (9.9%)	
Unknown risk of bias	9 (15.5%)	49 (32.5%)	

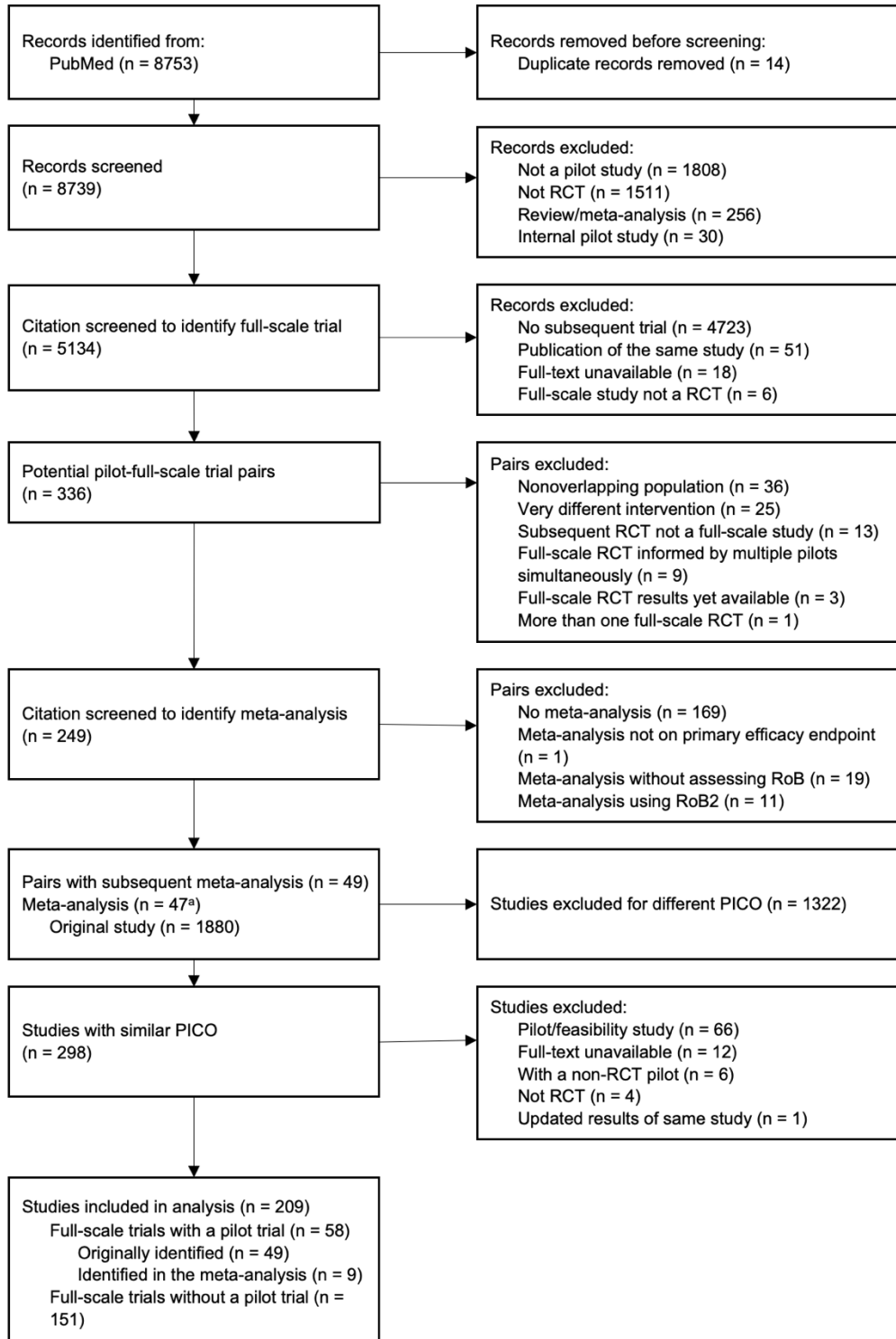
*P-values are calculated for the 3-level risk of bias assessment results by Pearson's Chi-squared tests if none of the cell count <5 or Fisher's Exact tests if at least one cell count ≤5

(Paper 3) Table 5 Association of pilot trials with risk of bias in the full-scale trials

	Unadjusted analyses		Adjusted analyses	
	Random-effects logit model	Logistic regression with RVE	Random-effects logit model	Logistic regression with RVE
Random sequence generation (selection bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	3.37 (0.87 - 13.07)	3.2 (1.18 - 8.64)	4.93 (0.92 - 26.36)	4.05 (1.27 - 12.91)
Allocation concealment (selection bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	4.95 (1.94 - 12.68)	4.71 (2.15 - 10.28)	2.89 (1.07 - 7.83)	3.09 (1.09 - 8.77)
Blinding of participants and researchers (information bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	4.2 (1.80 - 9.79)	2.04 (1.04 - 3.99)	4.31 (1.37 - 13.50)	2.74 (1.001 - 7.48)
Blinding of outcome assessment (information bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	1.59 (0.80 - 3.12)	1.36 (0.72 - 2.56)	1.03 (0.49 - 2.18)	1.09 (0.52 - 2.29)
Incomplete outcome data (selection bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	1.45 (0.66 - 3.16)	1.51 (0.76 - 3.02)	1.27 (0.47 - 3.42)	1.23 (0.54 - 2.84)
Selective reporting (reporting bias)				
High risk of bias	Reference	Reference	Reference	Reference
Low risk of bias	2.78 (1.04 - 7.45)	2.82 (1.38 - 5.76)	1.23 (0.44 - 3.46)	1.77 (0.70 - 4.49)

RVE: robust variance estimate

(Paper 3) Figure 1 Process of identifying full-scale trials with or without a pilot trial



Abbreviations: RCT, randomized controlled trial; RoB, risk of bias; PICO, patient, population or problem, intervention, comparison, and outcome. ^aFour pairs shared the same two meta-analyses.

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Chapter 5 Conclusion

1. Summary of Findings and Implications

In our first paper, we found that pilot trials tend to overestimate the point estimate of the effect size on average. Yet, most 95% confidence interval surrounding this estimate did incorporate the point estimate from the full-scale trial. Moreover, having a significant pilot trial appeared to be a moderate to strong indicator for the full-scale trial's significance. It is important to clarify that this discovery does not endorse the pursuit of statistical significance in pilot trials. Instead, we aim to prompt the scientific community to reconsider the relevance of statistical testing within these trials.

From our findings, we propose several recommendations. Significance testing regarding efficacy can be performed in pilot trials, though its results should not be perceived as definitive proof of efficacy. Provided that the evidence is deemed preliminary, a significant difference in efficacy between the intervention and control groups typically signifies a large effect size within the pilot trial. As a result, there is a high likelihood that a full-scale trial will yield significant findings, indicating that further testing of the intervention is warranted.

If the pilot trial does not produce a significant p-value, we suggest researchers examine the confidence interval around the point estimate to determine if it includes values of potential clinical relevance. This objective analysis should be supplemented with a subjective understanding of the intervention's potential effect. By combining these evaluations, researchers may be better equipped to make informed decisions about the necessity of a definitive trial to further assess the intervention's efficacy.

Paper 2 concluded that the characteristics of pilot trials and subsequent modifications might influence the feasibility of full-scale trials to varying degrees. Implementing feasibility

progression criteria in pilot stages and retaining the same masking status as in the full-scale trial can enhance the likelihood of successful screening. While adding more sites after the pilot phase correlated with quicker recruitment, it might diminish the probability of successful screening. Changes that increase the burden on participants may undermine the feasibility of a full-scale trial. For example, expanding intervention content, shifting to an active or more frequently administered control, and increasing the frequency of follow-ups can lead to decreased participant retention in full-scale trials.

While our findings echo established evidence from non-pilot contexts about factors influencing participant recruitment and retention in RCTs^{33,34}, to our understanding, this was the first exploration of such evidence within the context of pilot RCTs. The distinctive nature of pilot trials, which offers a unique setting for testing trial procedures, could have shifted the impact of these factors. Our data underscore the importance for both trialists and funders to review pilot trial results and post-pilot design alterations when gauging the feasibility of full-scale trials. Additionally, as researchers make changes to trial designs, they can draw upon the detailed evidence from this study to make well-informed decisions on feasibility. This may hold true even when various elements of the trial are being modified, as understanding the specific impacts of different changes can guide an overarching assessment. As a result, a new pilot trial with updated design features might not always be essential to reassess feasibility, sparing both time and the resources a new trial demands.

Paper 3 reported that conducting a pilot trial could decrease bias in a subsequent full-scale trial, particularly in the generation of random sequences, allocation concealment, and participant masking. This finding aligns with the purpose of pilot trials, which serve as preparatory studies designed to evaluate study procedures and operational strategies for later, typically larger, investigations^{35,36}.

Many research grant applications necessitate information regarding any preliminary work done before the current proposal. Our study provides empirical support for this prerequisite by demonstrating pilot trials may enhance the quality of full-scale trials. The results suggest that pilot trials might warrant more frequent consideration by researchers and funding bodies. This is particularly relevant for early-career researchers who might lack the experience of seasoned researchers in executing a randomized controlled trial, or in instances where the trial is expected to face practical challenges during the study implementation.

2. Future Directions

This dissertation offers several potential avenues for further research.

Our current investigation focused on pilot trials implementing a randomization process, but other pilot study forms such as one-arm or non-randomized pilot studies could be explored. Given that randomization frequently presents practical challenges, an examination of this process in pilot trials is needed, particularly when the goal is to enhance feasibility or improve quality. Yet, informing intervention efficacy might not necessitate the use of randomization. With recent advances in Bayesian statistics and the strategy of borrowing information from external controls³⁷, it is plausible that pilot studies could effectively inform efficacy without an accompanying concurrent randomized control group. Therefore, it could be informative to assess whether randomization is required during the pilot phase when the intention is to inform intervention efficacy.

We analyzed pilot trials across a broad range of fields in this work. Future research might narrow its scope to specific subgroups. For instance, as clustered randomized trials are often more susceptible to bias than individual randomized trials^{38,39}, a deeper look into whether pilot trials are especially beneficial for the methodological quality of clustered randomized trials could

be enlightening. It could also be valuable to determine if a pilot trial needs to adopt a clustered randomized design if the subsequent full-scale trial is planned in this format.

Our current findings associate pilot trials with improved quality in the ensuing trial. Future studies might explore whether a pilot trial must maintain a high quality to optimally inform the quality of a full-scale trial. Given the widespread belief that the term 'pilot' is often misused to justify poor study quality, it might also be insightful to investigate whether empirical evidence indeed suggests that pilot trials are of lower quality than definitive trials.

Finally, while our research centered on methodological quality, future studies could consider the reporting quality by assessing adherence of both pilot and full-scale trials to the relevant CONSORT guidelines, as well as how adherence in each may be correlated.

In summary, this dissertation contributes to our understanding of the role of pilot trials in informing efficacy, feasibility, and quality for subsequent full-scale trials. Pilot trials can offer early signals on intervention efficacy. Researchers and funders should weigh both the data from pilot trials and proposed design modifications when evaluating full-scale trials. Pilot trials may improve the quality of ensuing full-scale trials and warrant more frequent consideration. Future research is needed to explore other types of pilot studies and evaluate their impact on full-scale trials.

Appendices

Appendix A: Supplementary Materials for Paper 1

eTable 1. Search strategy

#1	"Pilot Projects"[Mesh] OR "Feasibility Studies"[Mesh]
#2	(Feasib*[Title/Abstract] OR pilot[Title/Abstract]) AND (study[Title/Abstract] OR trial[Title/Abstract])
#3	#1 OR #2
#4	retention[Title/Abstract] OR attrition[Title/Abstract] OR recruitment[Title/Abstract] OR randomization[Title/Abstract] OR participation[Title/Abstract] OR adherence[Title/Abstract] OR compliance[Title/Abstract] OR acceptability[Title/Abstract] OR completion[Title/Abstract] OR attendance[Title/Abstract]
#5	randomized controlled trial[pt] OR controlled clinical trial[pt] OR randomized[tiab] OR placebo[tiab] OR drug therapy[sh] OR randomly[tiab] OR trial[tiab] OR groups[tiab] NOT (animals [mh] NOT humans [mh])
#6	random*[Title/Abstract]
#7	#5 AND #6
#8	#3 AND # 4 AND #7

eTable 2. Illustration of modifications on the content, duration, and frequency of the intervention and comparator

	Pilot trial	Full-scale trial	Example
<i>Content: intervention component (duration and frequency can vary)</i>			
Same	A	A	Pair #2: Pilot: simvastatin 20 mg plus ezetimibe 10 mg daily Full: simvastatin 20 mg plus ezetimibe 10 mg daily
Added content	A	A+B	Pair #25: Pilot: 0.6% vaginal and neonate wipe performed every 4 hours (up to 3 times maximum) until delivery Full: 0.6% vaginal and neonate wipe performed every 4 hours (up to 3 times maximum) until delivery+minimum of one wash at least an hour before delivery
Reduced content	A+B	A	Pair #46: Pilot: 10-week Viewing IDVD at home+peer support group teleconferences (PSGTs)+Usual WIC care Full: 16-week Viewing IDVD at home+peer support group teleconferences (PSGTs)
Other difference	A+B	A+C	Pair #235: Pilot: 12 week wechat articles three to five times a week+12 SMS text message greetings and reminders Full: 12 week wechat articles three to five times a week+physical activity promotion program+ most read articles as booster
	A delivered by P	A delivered by Q	Pair #13: Pilot: clinic-based cognitive-behavioral therapy partially delivered by internet Full: internet-based cognitive-behavioral therapy
	A	Revised A	Pair #95: Pilot: DECISION+ program (3 three-hour on-site interactive workshops, reminders and feedback over a four- to six-month period) Full: revised DECISION+ program (a 2-hour web-based tutorial followed by a 2-hour on-site interactive workshop followed by reminders)
<i>Duration: intervention length, in hours/days/weeks/months (content can vary)</i>			
Same	X	X	Pair #5: Pilot: definitive radiotherapy (70 Gy in 7 weeks) + cisplatin (75 mg/m2) plus

			<p>tirapazamine (290 mg/m²/d) on day 2 of weeks 1, 4, and 7, and tirapazamine alone (160 mg/m²/d) on days 1, 3, and 5 of weeks 2 and 3</p> <p>Full: definitive radiotherapy (70 Gy in 7 weeks) + cisplatin (75 mg/m²) plus tirapazamine (290 mg/m²/d) on day 1 of weeks 1, 4, and 7, and tirapazamine alone (160 mg/m²/d) on days 1, 3, and 5 of weeks 2 and 3</p>
Longer duration	X	>X	<p>Pair #46:</p> <p>Pilot: 10-week Viewing IDVD at home+peer support group teleconferences (PSGTs)+Usual WIC care</p> <p>Full: 16-week Viewing IDVD at home+peer support group teleconferences (PSGTs)</p>
Shorter duration	X	<X	<p>Pair #71:</p> <p>Pilot: injectable hydromorphone over 12 months</p> <p>Full: injectable hydromorphone over 6 months</p>
<i>Frequency: number of sessions (content can vary)</i>			
Same	Y	Y	<p>Pair #5:</p> <p>Pilot: definitive radiotherapy (70 Gy in 7 weeks) + cisplatin (75 mg/m²) plus tirapazamine (290 mg/m²/d) on day 2 of weeks 1, 4, and 7, and tirapazamine alone (160 mg/m²/d) on days 1, 3, and 5 of weeks 2 and 3</p> <p>Full: definitive radiotherapy (70 Gy in 7 weeks) + cisplatin (75 mg/m²) plus tirapazamine (290 mg/m²/d) on day 1 of weeks 1, 4, and 7, and tirapazamine alone (160 mg/m²/d) on days 1, 3, and 5 of weeks 2 and 3</p>
More frequent	Y	>Y	<p>Pair #67:</p> <p>Pilot: 16 weeks weekly 90-minute yoga classes + newsletters on back care</p> <p>Full: 24 weeks twice weekly 90-minute yoga classes</p>
Less frequent	Y	<Y	<p>Pair #11:</p> <p>Pilot: 90-minute after-school Physical Activity Club offered five days a week</p> <p>Full: 90-minute after-school physical activity club offered 3 days/week</p>

eTable 3. List of 248 pilot-full-scale trial pairs

Pair #	Pilot trial	Full-scale trial
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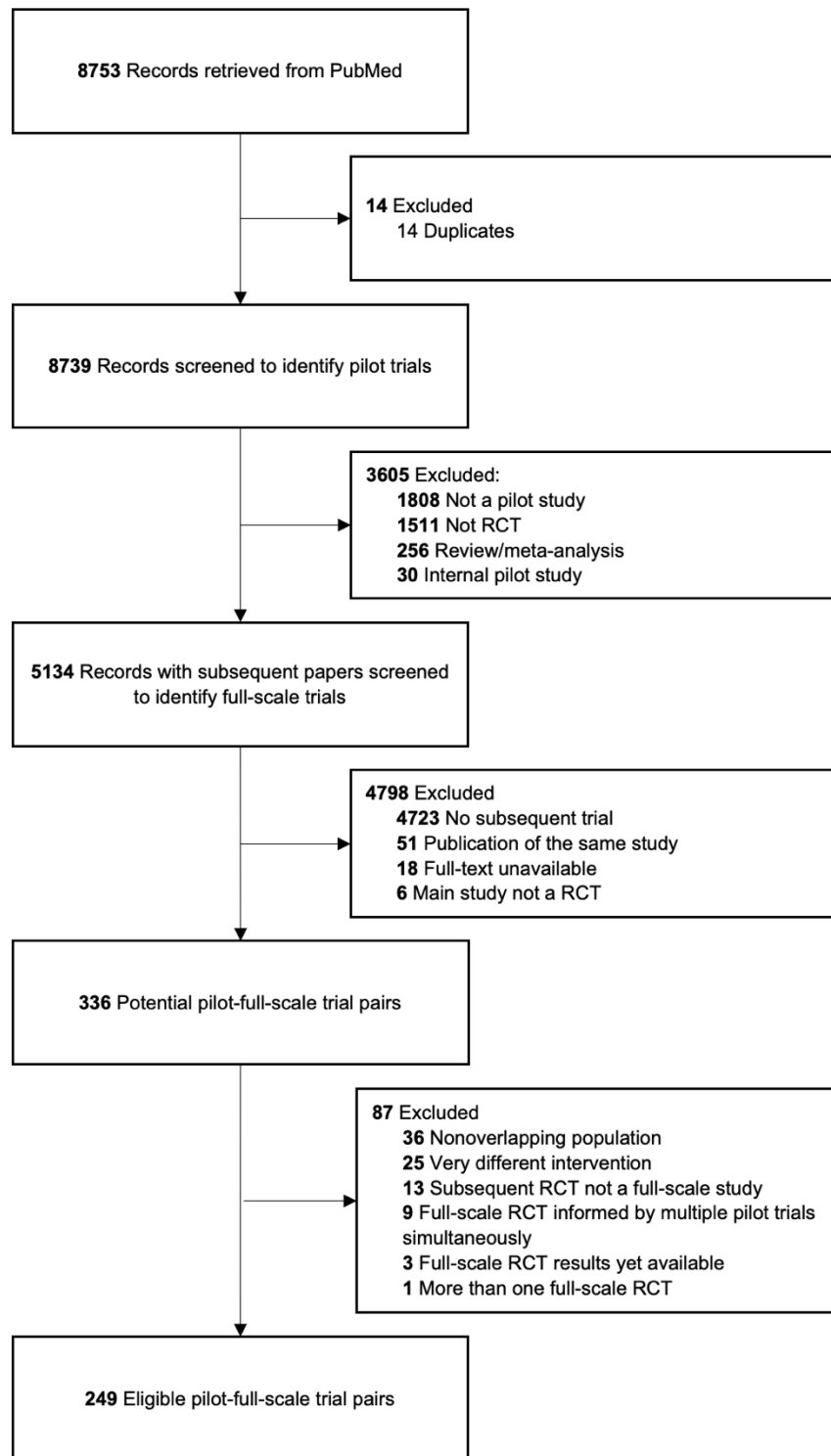
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Appendix B: Supplementary Materials for Paper 2

eTable 1. Search Strategy

#1	"Pilot Projects"[Mesh] OR "Feasibility Studies"[Mesh]
#2	(Feasib*[Title/Abstract] OR pilot[Title/Abstract]) AND (study[Title/Abstract] OR trial[Title/Abstract])
#3	#1 OR #2
#4	retention[Title/Abstract] OR attrition[Title/Abstract] OR recruitment[Title/Abstract] OR randomization[Title/Abstract] OR participation[Title/Abstract] OR adherence[Title/Abstract] OR compliance[Title/Abstract] OR acceptability[Title/Abstract] OR completion[Title/Abstract] OR attendance[Title/Abstract]
#5	randomized controlled trial[pt] OR controlled clinical trial[pt] OR randomized[tiab] OR placebo[tiab] OR drug therapy[sh] OR randomly[tiab] OR trial[tiab] OR groups[tiab] NOT (animals [mh] NOT humans [mh])
#6	random*[Title/Abstract]
#7	#5 AND #6
#8	#3 AND # 4 AND #7

eFigure 1. Flowchart of Study Selection Process



eTable 2. Complete list of diseases

Disease	Freq.	Percent
mental health	34	13.65
addiction	24	9.64
oncology	21	8.43
physical activity	14	5.62
obesity	13	5.22
pain	12	4.82
HIV	11	4.42
stroke	11	4.42
orthopedics	10	4.02
aging	7	2.81
diabetes	7	2.81
heart disease	7	2.81
multiple sclerosis	6	2.41
diet	5	2.01
healthcare	5	2.01
obstetric	5	2.01
sleep	5	2.01
developmental	4	1.61
transplantation	4	1.61
acute respiratory infection	3	1.2
critical care	3	1.2
neuropathy	3	1.2
parenting	3	1.2
renal	3	1.2
chronic obstructive pulmonary disease	2	0.8
dementia	2	0.8
hypertension	2	0.8
metabolic	2	0.8
Duchenne Muscular Dystrophy	1	0.4
acute lung injury	1	0.4
asthma	1	0.4
auditory hallucination	1	0.4
bacteremia	1	0.4
Barrett esophagus	1	0.4
blood donation	1	0.4
chronic fatigue syndrome	1	0.4
cystic fibrosis	1	0.4

diarrhea	1	0.4
domestic violence	1	0.4
gonorrhoeae	1	0.4
irritable bowel syndrome	1	0.4
otitis media prophylaxis	1	0.4
Parkinson	1	0.4
seizure	1	0.4
spinal cord injury	1	0.4
tuberculosis	1	0.4
urinary tract infection	1	0.4
vaccination	1	0.4
vitiligo	1	0.4
Total	249	100

Supplemental text. Missing data description

Information on successful screening probability, enrollment rate, and retention probability was available in 183, 177, and 238 pairs of pilot and full-scale trials, respectively. The primary source of this missing data was the pilot trials. Specifically, successful screening probability was not reported in 57 pilot trials and 30 full-scale trials. Enrollment rate was omitted in 69 pilot trials and 20 full-scale trials, while retention probability was not reported in 11 pilot and 11 full-scale trials. Comparison of pilot and full-scale trial characteristics between pairs with and without missing data is available in eTable 3 and eTable 4.

Pilot trials from pairs with unavailable data on successful screening probability were less likely to have more than two arms (8% vs 19%, $P=.035$), while tending to have larger average sample sizes (212 ± 554 vs 88 ± 93 , $P=.004$) (eTable 3). No significant between-group differences were found in relation to the full-scale trials' characteristics (eTable 4).

Regarding the enrollment rate, pilot trials in pairs lacking data on this metric more commonly examined interventions for obesity or physical activity (18% vs 8%, $P=.047$), were less frequently published post-2015 (14% vs 35%, $P=0.003$), and were less likely to use masking (33% vs 49%, $P=.028$) (eTable 3). The full-scale trials from pairs lacking data on the enrollment rate were also less frequently published after 2015 (18% vs 32%, $P=.031$), had a higher likelihood of being cluster-randomized trials (25% vs 12%, $P=.010$), were less frequently multicenter (38% vs 60%, $P<.001$), and had smaller median sample sizes (203 vs 290, $P=.024$) (eTable 4).

Pilot trials in pairs missing data on retention probability had larger median sample sizes (479 vs 104, $P<.001$) and longer median follow-up lengths (182 vs 91 days, $P=.038$) (eTable 3). Similarly, full-scale trials in pairs missing data on retention probability also had larger median sample sizes (600 vs 264, $P=.016$) and longer median follow-up lengths (365 vs 182 days, $P=.019$) (eTable 4).

eTable 3. Comparison of pilot trial characteristics between pairs with and without missing data on feasibility parameters ^a

	No. (%)		P-value	Enrollment rate per week		P-value	Retention probability		P-value
	Successful screening probability (n=183)	Missing (n=66)		Non-missing (n=177)	Missing (n=72)		Non-missing (n=238)	Missing (n=11)	
Disease ^b			0.45			0.047			0.33
Addiction	19 (10)	5 (8)		19 (11)	5 (7)		23 (10)	1 (9)	
Mental health	26 (14)	8 (12)		23 (13)	11 (15)		34 (14)	0 (0)	
Obesity & physical activity	21 (11)	6 (9)		14 (8)	13 (18)		25 (11)	2 (18)	
Oncology	12 (7)	9 (14)		19 (11)	2 (3)		19 (8)	2 (18)	
Other	105 (57)	38 (58)		102 (58)	41 (57)		137 (58)	6 (55)	
Intervention			0.15			0.11			0.51
Behavioral	131 (72)	41 (62)		117 (66)	55 (76)		163 (68)	9 (82)	
Pharmaceutical & other	52 (28)	25 (38)		60 (34)	17 (24)		75 (32)	2 (18)	
Publication year			0.32			0.003			0.93
2004-2009	50 (27)	24 (36)		46 (26)	28 (39)		70 (29)	4 (36)	
2010-2014	80 (44)	23 (35)		69 (39)	34 (47)		99 (42)	4 (36)	
2015-2019	53 (29)	19 (29)		62 (35)	10 (14)		69 (29)	3 (27)	
Funding source			0.75			0.22			0.45
Non-industry	161 (88)	59 (89)		160 (90)	60 (83)		211 (89)	9 (82)	
Industry	4 (2)	2 (3)		4 (2)	2 (3)		6 (3)	0 (0)	
None or not reported	18 (10)	5 (8)		13 (7)	10 (14)		21 (9)	2 (18)	
Cluster randomization			0.40			0.78			0.17
No	172 (94)	60 (91)		164 (93)	68 (94)		223 (94)	9 (82)	
Yes	11 (6)	6 (9)		13 (7)	4 (6)		15 (6)	2 (18)	
No. of sites			0.92			0.093			0.73
Single center	137 (75)	49 (74)		127 (72)	59 (82)		177 (74)	9 (82)	
Multicenter	46 (25)	17 (26)		50 (28)	13 (18)		61 (26)	2 (18)	
No. of arms			0.035			0.30			0.68
2	149 (81)	61 (92)		152 (86)	58 (81)		201 (84)	9 (82)	
>2	34 (19)	5 (8)		25 (14)	14 (19)		37 (16)	2 (18)	
Sample size									
Mean (SD)	88 (93)	212 (554)	0.004	143 (351)	67 (60)	0.072	104 (213)	479 (1002)	<0.001
Median (IQR)	56 (34, 100)	49 (28, 100)	0.35	60 (32, 115)	48 (30, 82)	0.11	52 (31, 100)	66 (33, 355)	0.32
Masking used			0.96			0.028			0.36
No	102 (56)	37 (56)		91 (51)	48 (67)		131 (55)	8 (73)	

Yes	81 (44)	29 (44)		86 (49)	24 (33)		107 (45)	3 (27)	
Primary length of follow-up (days)									
Mean (SD)	158 (236)	187 (240)	0.39	178 (263)	136 (152)	0.21	163 (240)	220 (144)	0.44
Median (IQR)	91 (45, 182)	91 (30, 274)	0.87	91 (61, 182)	91 (14, 182)	0.22	91 (42, 182)	182 (91, 365)	0.038
Intervention efficacy			0.13			0.78			0.31
Not statistically significant	81 (44)	28 (42)		79 (45)	30 (42)		106 (45)	3 (27)	
Statistically significant	72 (39)	20 (30)		63 (36)	29 (40)		88 (37)	4 (36)	
Not evaluated	30 (16)	18 (27)		35 (20)	13 (18)		44 (18)	4 (36)	

Abbreviations: SD, standard deviation; IQR, interquartile range.

^a P values are derived from Student's t tests for means, Wilcoxon rank-sum tests for medians, Pearson's Chi-squared tests for frequencies if all cell counts exceed 5 or Fisher's Exact tests if at least one cell count is less than 5. Bold text indicates P<.05.

^b The diseases listed represent the top four most frequently occurring within the dataset. All other disease types are grouped under the category labeled as "other." A complete list of diseases is available in eTable 2 in Appendix B.

eTable 4. Comparison of full-scale trial characteristics between pairs with and without missing data on feasibility parameters ^a

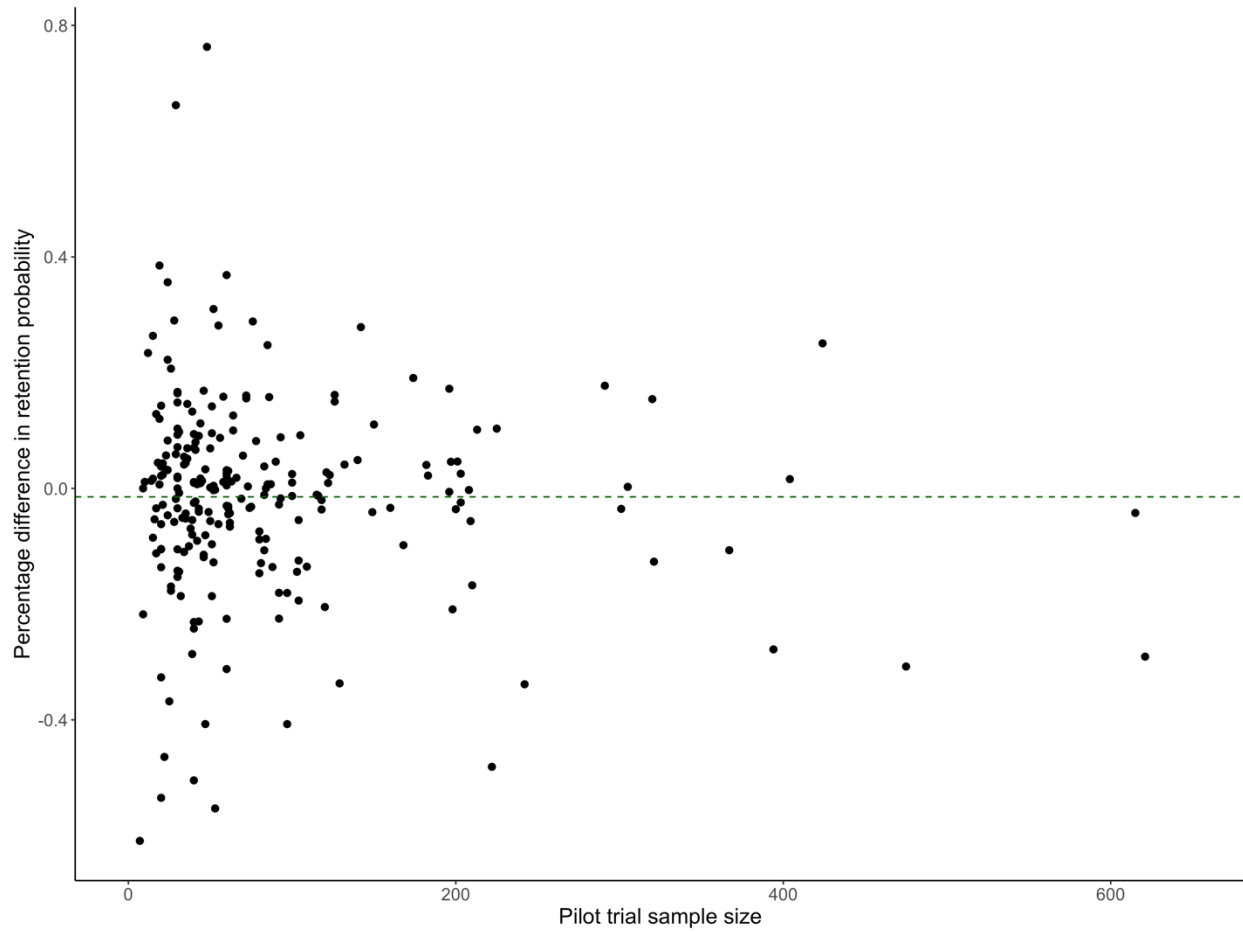
	No. (%)		P-value	Enrollment rate per week		P-value	Retention probability		P-value
	Successful screening probability			Non-missing	Missing		Non-missing	Missing	
	Non-missing (n=183)	Missing (n=66)		(n=183)	(n=66)		(n=183)	(n=66)	
Publication year			0.61			0.031			0.51
2004-2009	4 (2)	2 (3)		2 (1)	4 (6)		6 (3)	0 (0)	
2010-2014	34 (19)	17 (26)		33 (19)	18 (25)		47 (20)	4 (36)	
2015-2019	93 (51)	30 (45)		86 (49)	37 (51)		119 (50)	4 (36)	
2020-2022	52 (28)	17 (26)		56 (32)	13 (18)		66 (28)	3 (27)	
Funding source			0.45			0.55			0.34
Non-industry	171 (93)	59 (89)		165 (93)	65 (90)		220 (92)	10 (91)	
Industry	7 (4)	5 (8)		7 (4)	5 (7)		12 (5)	0 (0)	
None or not reported	5 (3)	2 (3)		5 (3)	2 (3)		6 (3)	1 (9)	
Cluster randomization			0.29			0.010			0.075
No	157 (86)	53 (80)		156 (88)	54 (75)		203 (85)	7 (64)	
Yes	26 (14)	13 (20)		21 (12)	18 (25)		35 (15)	4 (36)	
No. of sites			0.47			<0.001			0.96
Single center	82 (45)	33 (50)		70 (40)	45 (62)		110 (46)	5 (45)	
Multicenter	101 (55)	33 (50)		107 (60)	27 (38)		128 (54)	6 (55)	
No. of arms			0.60			0.51			0.075
2	153 (84)	57 (86)		151 (85)	59 (82)		203 (85)	7 (64)	
>2	30 (16)	9 (14)		26 (15)	13 (18)		35 (15)	4 (36)	
Sample size									
Mean (SD)	935 (2562)	1800 (6750)	0.14	1470 (4833)	414 (574)	0.066	842 (2005)	8145 (16364)	<0.001
Median (IQR)	269 (140, 560)	256 (150, 861)	0.43	290 (150, 697)	203 (132, 373)	0.024	264 (140, 599)	600 (250, 11880)	0.016
Masking used			0.22			0.082			0.062
No	62 (34)	28 (42)		58 (33)	32 (44)		83 (35)	7 (64)	
Yes	121 (66)	38 (58)		119 (67)	40 (56)		155 (65)	4 (36)	
Primary length of follow-up (days)									
Mean (SD)	330 (465)	295 (304)	0.56	342 (482)	270 (243)	0.23	307 (377)	616 (1032)	0.019
Median (IQR)	210 (91, 365)	182 (90, 365)	0.62	182 (91, 365)	182 (91, 365)	0.57	182 (91, 365)	365 (84, 548)	0.26

Intervention efficacy			0.55		0.082		0.043
Not statistically significant	84 (46)	35 (53)	90 (51)	29 (40)	115 (48)	4 (36)	
Statistically significant	98 (54)	31 (47)	87 (49)	42 (58)	123 (52)	6 (55)	
Not evaluated	1 (1)	0 (0)	0 (0)	1 (1)	0 (0)	1 (9)	

Abbreviations: SD, standard deviation; IQR, interquartile range.

^a P values are derived from Student's t tests for means, Wilcoxon rank-sum tests for medians, Pearson's Chi-squared tests for frequencies if all cell counts exceed 5 or Fisher's Exact tests if at least one cell count is less than 5. Bold text indicates P<.05.

eFigure 2. Scatterplot of percentage difference in retention probability versus pilot trial sample size



Appendix C: Supplementary Materials for Paper 3

eTable 1. Search Strategy

#1	"Pilot Projects"[Mesh] OR "Feasibility Studies"[Mesh]
#2	(Feasib*[Title/Abstract] OR pilot[Title/Abstract]) AND (study[Title/Abstract] OR trial[Title/Abstract])
#3	#1 OR #2
#4	retention[Title/Abstract] OR attrition[Title/Abstract] OR recruitment[Title/Abstract] OR randomization[Title/Abstract] OR participation[Title/Abstract] OR adherence[Title/Abstract] OR compliance[Title/Abstract] OR acceptability[Title/Abstract] OR completion[Title/Abstract] OR attendance[Title/Abstract]
#5	randomized controlled trial[pt] OR controlled clinical trial[pt] OR randomized[tiab] OR placebo[tiab] OR drug therapy[sh] OR randomly[tiab] OR trial[tiab] OR groups[tiab] NOT (animals [mh] NOT humans [mh])
#6	random*[Title/Abstract]
#7	#5 AND #6
#8	#3 AND # 4 AND #7

eTable 2. Study selection criteria

Meta-analysis ID	# all	# eligible	# included	Criteria used to define similar trials	Further exclusion reasons
Ma 2022	6	6	2	all	3 pilot, 1 nonrct
Liu 2021	7	2	2	Figure 4. BPI pain-related interference. 1.1.1 TA vs SA, Body acupuncture and auricular acupuncture	
Dalla Via 2018	12	4	4	Fig3 bone mineral density between the exercise and control groups at a lumbar spine, breast cancer	
Azarpazhooh 2016	5	3	3	Analysis 4.1. Comparison 4 Xylitol syrup versus control for younger children unable to chew, Outcome 1 Final diagnosis of at least one episode of AOM.	
Ramamoorthi 2019	14	4	2	outcome: glucose	1 nonrct, 1 same pub
Jefferson 2020	78	6	6	Analysis 1.1. Comparison 1: Randomised trials: medical/surgical masks versus no masks, Outcome 1: Viral illness, 1.1.2 Laboratory-confirmed influenza	
Petrucci 2021	16	3	2	Figure 6. Disability: MBSR versus control.	1 pilot
Wayne 2018	15	4	4	Table 1 breast, RCT, FACT, quality of life, figure 2E	
Naqvi 2020	15	14	9	Analysis 1.1.	3 pilot, 2 no fulltext
Grant 2017	9	9	2	all	6 pilot, 1 no fulltext
Whittaker 2016	12	12	6	Analysis 1.1.	3 pilot, 2 ongoing, 1 with a pilot study
Walker 2020	26	6	4	Analysis 1.4. Comparison 1: UDCA versus placebo, Outcome 4: Stillbirth	1 pilot, 1 no fulltext
Laurenzi 2021	30	12	5	Viral load (n = 12)	7 pilot
Mead 2017	70	15	9	Analysis 1.15. Comparison 1 Behaviour-changing interventions versus no treatment/usual care, Outcome 15 Change in BMI z score - type of control. 1.15.1 No treatment	3 pilot, 1 nonrct, 1 no fulltext, 1 with a pilot study
Grimmett 2019	27	4	3	Table 1 Measure of physical activity: Actirgraph	1 pilot

Lewis 2022	77	5	4	eFigure 7. Proportion of Time at Target Sedation Forest Plot (%)	1 pilot
Smith 2018	64	5	3	Analysis 1.1. Comparison 1 Acupuncture versus no treatment/waitlist/TAU, Outcome 1 Severity of depression at the end of treatment. 1.1.1 Manual acupuncture	1 pilot, 1 no fulltext
Storebø 2020	75	3	3	Analysis 8.1. Comparison 8: Systems training for emotional predictability and problem solving (STEPPS) vs TAU, Outcome 1: Primary: BPD symptom severity (continuous), at end of treatment, 8.1.1 End of treatment	
Sherrington 2019	108	8	6	1.5.2 Not group exercise, 1.3.2 Age 75+, 1.4.1 Health professional delivering intervention, 12.2.1 Balance and functional exercises vs control, outcome: rate of fall	2 pilot
Huang 2013	8	3	2	outcome: Figure 6a Adherence to antidepressant medication and oral hypoglycemic agent, primary care setting	1 pilot
Légaré 2018	87	3	2	Analysis 4.19 Comparison 4 Group 4: Interventions targeting patients compared to other interventions targeting patients, Outcome 19 Adherence (categorical).	1 nonrct
Coxeter 2015	9	8	6	Analysis 1.1. Comparison 1 Shared decision making versus usual care (control), Outcome 1 Antibiotics prescribed, dispensed or decision to use (short-term, index consultation to \leq 6 weeks).	1 pilot, 1 no fulltext
Shi 2020	26	2	2	kidney, immunosuppressive medication adherence rate	
Turrini 2019	26	12	8	fig2, appendix, PTSD, post-treatment, adult, formal diagnosis	4 pilot
Williams 2020	75	15	11	Analysis 2.4. Comparison 2: Cognitive behavioural vs treatment as usual, Outcome 4: Pain follow-up	1 no fulltext, 4 with a pilot study
Carandini 2018	6	4	2	fig 2, outcome: mortality or disability	3 pilot
Danon 2022	40	3	2	intervention: relaxation, pain was studied in a cluster syndrome and	1 pilot

				measured by visual analog scale or numerical pain rating scale	
Moullaali 2022	16	9	8	primary outcome: mRS, population: Mixed stroke & prehospital	1 pilot
Jackson 2022	21	5	4	Analysis 2.1. Comparison 2: Acceptance and commitment therapy (ACT), Outcome 1: ACTvs matched-intensity smoking cessation treatment	1 pilot
Ma 2017	54	2	2	White, male	
Abbott 2019	7	2	2	rct, fig3 agitation	
Wu 2022	23	5	5	Diet+PA vs clt,obese	
Lou 2017	12	4	3	figure3a 1.6.1 operative management, Tibia	1 pilot
Rankin 2018	32	11	9	Analysis 1.5. Comparison 1 Postintervention analysis, Outcome 5The proportion of patients with one or more potentially inappropriate medications	2 pilot
Smith 2017	7	2	2	Figure 4 Forest plot of exercises into pain versus pain-free exercises—short term, Physiotherapy and home setting	
Aemaz 2022	10	6	2	figure 2 headache frequency	4 pilot
Fraguas 2021	69	23	20	universal intervention, primary school, outcome: overall bullying	1 pilot, 2 no fulltext
Blackburn 2020	62	3	3	Behavioural intervention, inactive control, sedentary outcome: sedentary behavior, setting: school	
van Agteren 2021	39 3	8	6	outcome: Warwick–Edinburgh Mental Wellbeing Scale (WEMWBS), "Multi-theoretical interventions"	2 pilot
Connolly 2021	19	3	3	depression	
Ye 2021	14	5	3	figure 5 3.1.1 methicillin-resistant S. aureus bacteraemia (MRSAB)	2 pilot
Fan 2021	4	4	2	all	2 pilot
Lau 2021	25	8	5	figure 4, postnatal, community setting, anxiety symptoms for digital psychotherapeutic intervention and comparators, CBT	2 pilot, 1 no fulltext
Smith 2021	22	2	2	All outpatients (S3 Table)	

Marcum 2021	40	7	5	intervention: Pharmaceutical care, outcome: Pharmacy refills	1 pilot, 1 no fulltext
Zhu 2020	18	8	6	CLBP, Intermediate-term effects (6 to 7 months)	2 pilot
Neil-Sztramko 2021	89	6	3	Analysis 1.3. 1.3.1 Before and after school programmes	3 pilot

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