# Introduction to Clinical Trials - Day 2

## Session 6 - Group Sequential Monitoring

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Elements of Trial Monitoring

Group Sequential Designs

Statistical framework for trial monitoring

Types of group sequential designs

Example: Sepsis trial

# Trial monitoring

## Elements and motivation for trial monitoring

- Motivation: Many trials have been stopped early:
  - Physician health study showed that aspirin reduces the risk of cardiovascular death.
  - A phase III study of tamoxifen for prevention of breast cancer among women at risk for breast cancer showed a reduction in breast cancer incidence.
  - A phase III study of anti-arrhythmia drugs for prevention of death in people with cardiac arrhythmia stopped due to excess deaths with the anti-arrhythmia drugs.
  - A phase III study of folic acid supplements for prevention of neural tube defects.
  - Women's Health Initiative: Hormones cause heart disease.

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# Trial monitoring



## Elements and motivation for trial monitoring

- What is trial monitoring?
  - Monitoring for quality control; for example,
    - Patient accrual.
    - Data quality/completeness.
    - Unanticipated adverse events.
  - Monitoring study endpoints(s); for example,
    - Treatment benefits.
    - Toxicity differences.
  - Good quality control should be part of every study to ensure that the study achieves its goals.
  - Monitoring study endpoints is not applicable in every study, and requires special statistical methods to avoid increased statistical errors.

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# Trial monitoring

## Elements and motivation for trial monitoring

- Reasons to monitor study endpoints:
  - To maintain the validity of the informed consent for:
    - Subjects currently enrolled in the study.
    - New subjects entering the study.
  - To ensure the ethics of randomization.
    - Randomization is only ethical under equipoise.
    - If there is not equipoise, then the trial should stop.
  - To identify the best treatment as quickly as possible:
    - For the benefit of all patients (i.e., so that the best treatment becomes standard practice).
    - For the benefit of study participants (i.e., so that participants are not given inferior therapies for any longer than necessary).

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# Trial monitoring

## Elements and motivation for trial monitoring

- If not done properly, monitoring of endpoints can lead to biased results:
  - Data driven analyses cause bias:
    - Analyzing study results because they look good leads to an overestimate of treatment benefits.
  - Publication or presentation of 'preliminary results' can affect:
    - Ability to accrue subjects.
    - Type of subjects that are referred and accrued.
    - Treatment of patients not in the study.
  - Failure to design for interim analyses can lead to hasty decisions. Decisions made 'in the heat of the moment' are subject to:
    - Inadequate consideration of trade-offs between competing endpoints (toxicity versus benefit).
    - External pressures from study investigators or sponsors.
    - Lack of objectivity by study monitors.

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## Elements and motivation for trial monitoring

- Thus,
  - Monitoring of study endpoints is often required for ethical reasons.
  - Monitoring of study endpoints must carefully planned as part of study design to:
    - Avoid bias
    - Assure careful decisions
    - Maintain desired statistical properties

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# Elements and motivation for trial monitoring

## Key elements of monitoring

- How are trials monitored?
  - Investigator knowledge of interim results can lead to biased results:
    - Negative results may lead to loss of enthusiasm.
    - Positive interim results may lead to inappropriate early publication.
    - Either result may cause changes in the types of subjects who are recruited into the trial.
  - “Data Safety and Monitoring Boards (DSMB)" are used to avoid biased decisions:
    - DSMB members are *independent* of the study investigators
    - The DSMB reviews unblinded data in the midst of a trial to:

      1. Assure the trial is safe to continue.
      2. Make decisions about early termination based on the statistical monitoring plan (“group-sequential clinical trial design").

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# Elements and motivation for trial monitoring



## Key elements of monitoring

The trial monitoring plan is typically pre-specified in two documents:

- DSMB charter:
  - Defines scope of trial monitoring
  - Defines DSMB responsibilities
  - Defines sponsor responsibilities
  - Pre-specifies monitoring plans and decisions (reasons for stopping)
- Interim Statistical Analysis Plan (ISAP):
  - Defines monitoring endpoint(s)
  - Pre-specifies analysis timing, decision criteria, and rationale
  - Pre-specifies methods for implementation (changes to analysis timing)
  - Pre-specifies adjustments to statistical inference about treatment effects

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# Elements and motivation for trial monitoring



## Key elements of monitoring

- Typical content for DSMB charter:
  - Trial synopsis; for example:
    - Summary of design
    - Eligibility/exclusions
    - Statistical design and sample size
  - DSMB organization
    - Composition and selection of members
  - Responsibilities of DSMB
    - What will be monitored (accrual, QC, safety, endpoints?)
  - Responsibilities of sponsor
    - Providing open/closed reports; data summaries
  - Committee meetings:
    - Open session; closed session; executive session
  - Communication
    - Open report; closed report to be provided to DSMB
    - Responsibility for meeting minutes (open and closed minutes)
    - Process for DSMB recommendations

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# Elements and motivation for trial monitoring



## Key elements of monitoring

- Typical content for ISAP:
  - Safety monitoring plan (if there are formal safety interim analyses)
    - Decision rules for formal safety analyses
    - Evaluation of decision rules (power, expected sample size, stopping probability)
    - Methods for modifying rules (changes in timing of analyses)
    - Methods for inference (bias adjusted inference)
  - Monitoring plan for primary endpoint(s)
    - Decision rules and reasons for early termination (e.g., efficacy, futility, equivalence, harm)
    - Evaluation of decision rules (power, expected sample size, stopping probability)
    - Methods for modifying rules (changes in timing of analyses)
    - Methods for inference (bias adjusted inference)
  - Data handling and responsibilities for analysis

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## Statistical framework for trial monitoring: Statistical design of the fixed-sample trial

- The interim statistical analysis plan is based on the fixed sample design
  - Primary endpoint
  - Probability model
  - Functional
  - Contrast
  - Statistical hypotheses
  - Statistical standards for decisions (interval estimate)

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## Statistical framework for trial monitoring: Statistical design of the fixed-sample trial

- The statistical decision criteria are referenced to the trial's design hypotheses. For example:
  - One-sided superiority test (assume small $\theta$ favors new treatment):

$$\begin{aligned} \text{Null:} \quad & \theta \geq \theta_{\emptyset} \\ \text{Alternative:} \quad & \theta \leq \theta_{+} \end{aligned}$$

with $\theta_{+} < \theta_{\emptyset}$, and $\theta_{+}$ is chosen to represent the smallest difference that is clinically important.

  - Two-sided (equivalence) test:

$$\begin{aligned} \text{Null:} \quad & \theta = \theta_{\emptyset} \\ \text{Lower Alternative:} \quad & \theta \leq \theta_{-} \\ \text{Upper Alternative:} \quad & \theta \geq \theta_{+} \end{aligned}$$

with $\theta_{-} < \theta_{\emptyset} < \theta_{+}$. $\theta_{-}$ and $\theta_{+}$ denote the smallest important differences.



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## Statistical framework for trial monitoring: Selecting decision criteria

- A decision to stop needs to consider what has or has not been ruled out. For example
  - One-sided superiority test (assume small $\theta$ favors new treatment):
    - Stop for superiority when any harm ($\theta \geq \theta_{\emptyset}$) has been ruled out.
    - Stop for futility when important benefits ($\theta \leq \theta_{+}$) have been ruled out.
  - Two-sided (equivalence) test:
    - Stop for treatment *A* better than treatment *B* when inferiority of *A* ($\theta \leq \theta_{\emptyset}$) has been ruled out.
    - Stop for treatment *B* better than treatment *A* when inferiority of *B* ($\theta \geq \theta_{\emptyset}$) has been ruled out.
    - Stop for equivalence when important differences (either $\theta \geq \theta_{+}$ or $\theta \leq \theta_{-}$ ) have been ruled out.
- The hypotheses that have been ruled in/out are given by the interval estimate.



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# Overview of group sequential designs

## Statistical framework for trial monitoring: Group sequential designs (superiority trial)

- Suppose that the trial is planned for $j = 1, ..., J$ interim analyses.
- Let $\hat{\theta}_j$ denote the estimated treatment effect at the $j$th analysis.
- Consider stopping criteria $a_j < d_j$ with:

$$\begin{aligned} \hat{\theta}_j \leq a_j &\Rightarrow \text{Decide new treatment is superior} \\ \hat{\theta}_j \geq d_j &\Rightarrow \text{Decide new treatment is not superior} \\ a_j < \hat{\theta}_j < d_j &\Rightarrow \text{Continue trial} \end{aligned}$$

  Set $a_J = d_J$ so that the trial stops by the $J$th analysis.

- How should we choose these critical values?

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# Statistical framework for trial monitoring

## Inadequacy of Fixed Sample Methods

- Suppose we simply ignore the fact that we are repeatedly testing our hypothesis
- We can quickly see the impact of this via simulation
  - Let $X_i \sim_{\text{iid}} \mathcal{N}(\theta, \sigma^2)$
  - $j = 1, ..., 4$ equally spaced analyses at 25, 50, 75, and 100 observations
  - Test statistic after $n_j$ observations have been accrued

$$\bar{X}_{n_j} = \frac{1}{n_j} \sum_{i=1}^{n_j} X_i$$

  - Test $H_0 : \theta = 0$ with level $\alpha = .05$
- Fixed sample methods (2-sided test): Reject $H_0$ first time

$$|\bar{X}_{n_j}| > z_{1-\alpha/2} \frac{\sigma}{\sqrt{n_j}}, \qquad j = 1, 2, 3, 4$$

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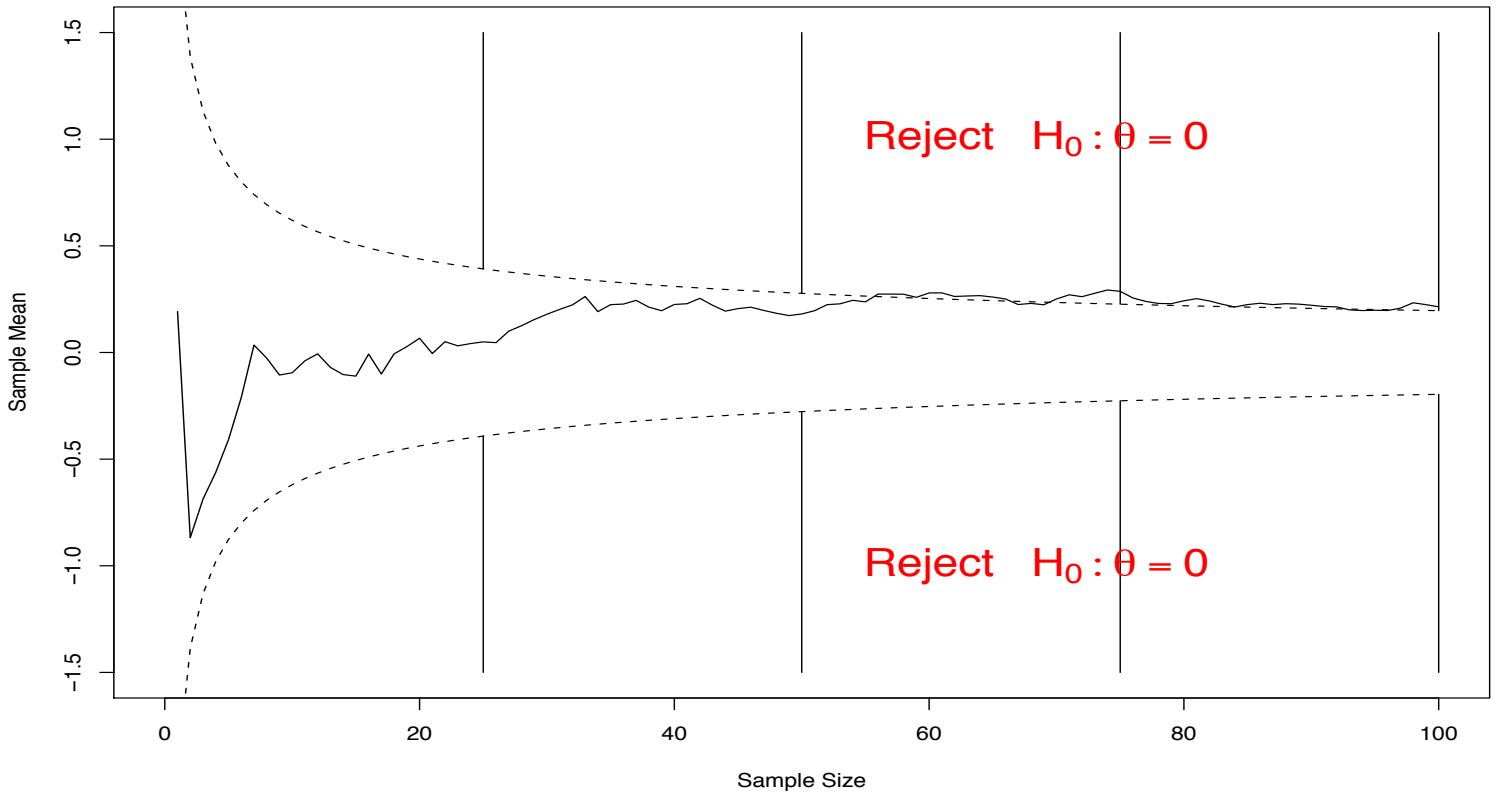
# Statistical framework for trial monitoring

## Inadequacy of Fixed Sample Methods : Simulation

- Consider the sample path of the statistic for a single simulated trial

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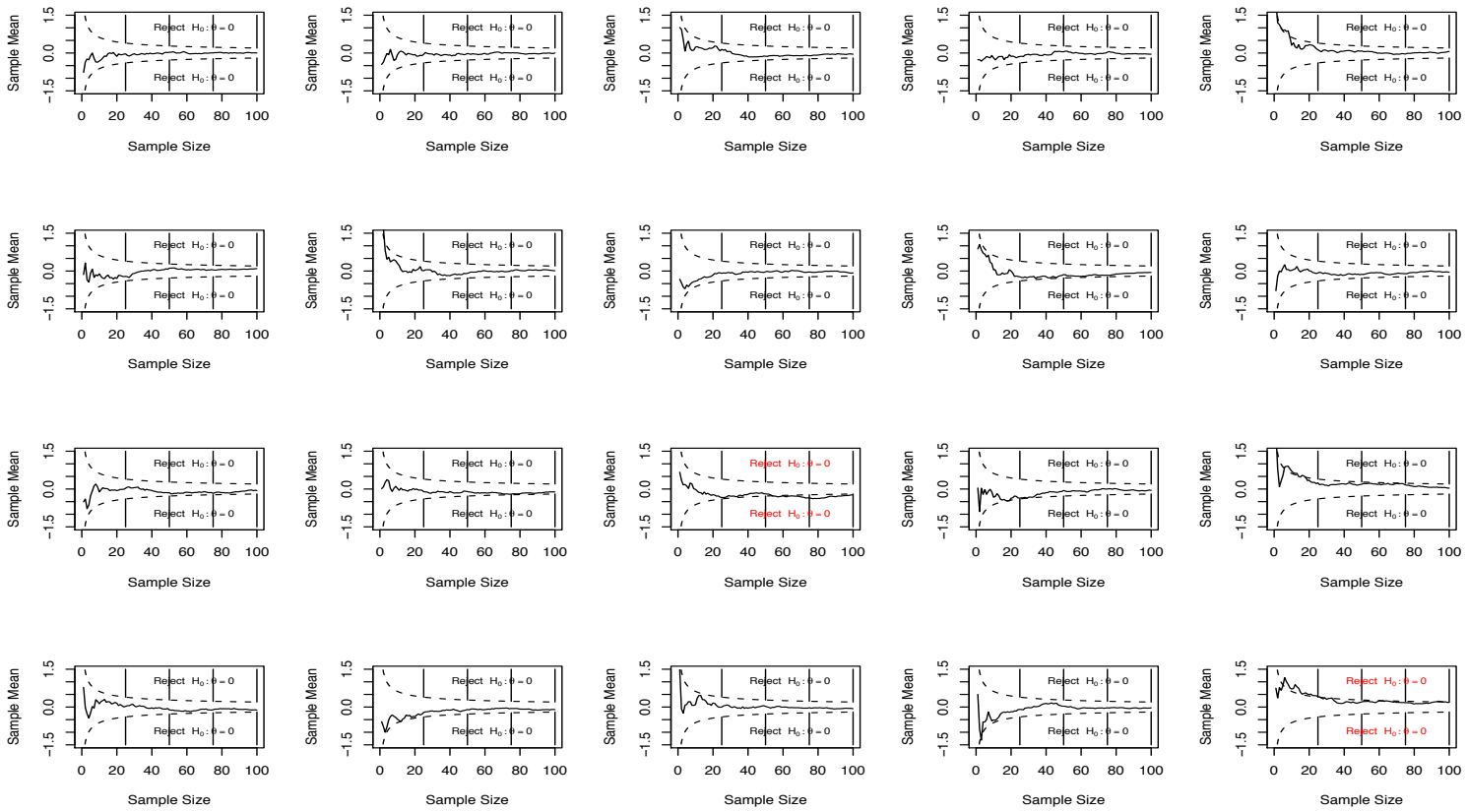
# Statistical framework for trial monitoring

## Inadequacy of Fixed Sample Methods : Simulation

- Consider the sample path of the statistic for 20 randomly sampled trials



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# Statistical framework for trial monitoring

## Inadequacy of Fixed Sample Methods : Simulation

- Simulated type I error rate using fixed sample methods
- Based on 100,000 simulations

| Significant at | Proportion Significant | Number Significant | Proportion Significant |
|---|---|---|---|
| Analysis 1 | 0.05075 | Exactly 1 | 0.07753 |
| Analysis 2 | 0.04978 | Exactly 2 | 0.02975 |
| Analysis 3 | 0.05029 | Exactly 3 | 0.01439 |
| Analysis 4 | 0.05154 | All 4 | 0.00554 |
| | | Any | 0.12721 |

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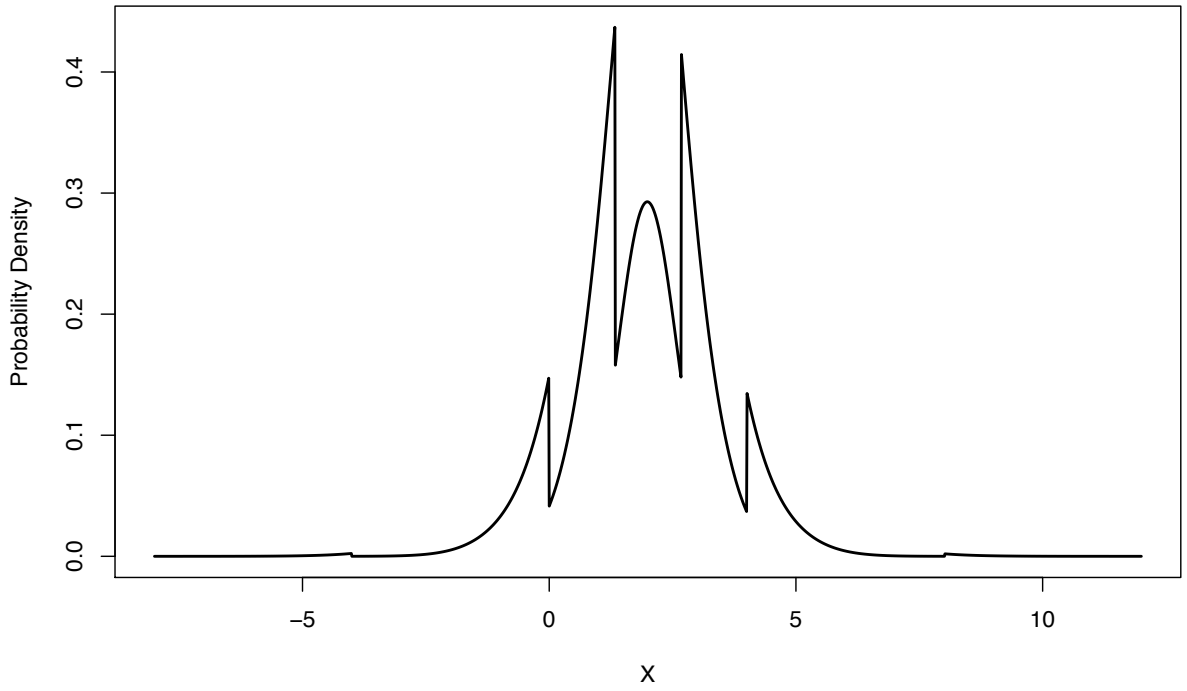
# Interim analyses require special methods

## Sampling density for sequentially-monitored test statistic

- The filtering due to interim analyses creates non-standard sampling densities as the basis for inference.
- Sampling density depends on the stopping rule.
- In order to correct the type 1 error rate, we must be able to compute the density of the statistic that accounts for the possibility of stopping at interim analyses



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# Sampling density for sequentially sampled test statistic

- Let $C_j$ denote the continuation set at the $j$th interim analysis.
- Let $(M, S)$ denote the bivariate statistic where $M$ denotes the stopping time ($1 \leq M \leq J$) and $S = S_M$ denotes the value of the partial sum statistic at the stopping time.
- The sampling density for the observation $(M = m, S = s)$ is:

$$p(m, s; \theta) = \begin{cases} f(m, s; \theta) & s \notin C_m \\ 0 & else \end{cases}$$

where the (sub)density function $f(j, s; \theta)$ is recursively defined as

$$\begin{aligned} f(1, s; \theta) &= \frac{1}{\sqrt{n_1 V}} \phi\left(\frac{s - n_1\theta}{\sqrt{n_1 V}}\right) \\ f(j, s; \theta) &= \int_{C_{(j-1)}} \frac{1}{\sqrt{n_j V}} \phi\left(\frac{s - u - n_j\theta}{\sqrt{n_j V}}\right) f(j-1, u; \theta)\, du, \\ & \quad j = 2, \ldots, m \end{aligned}$$

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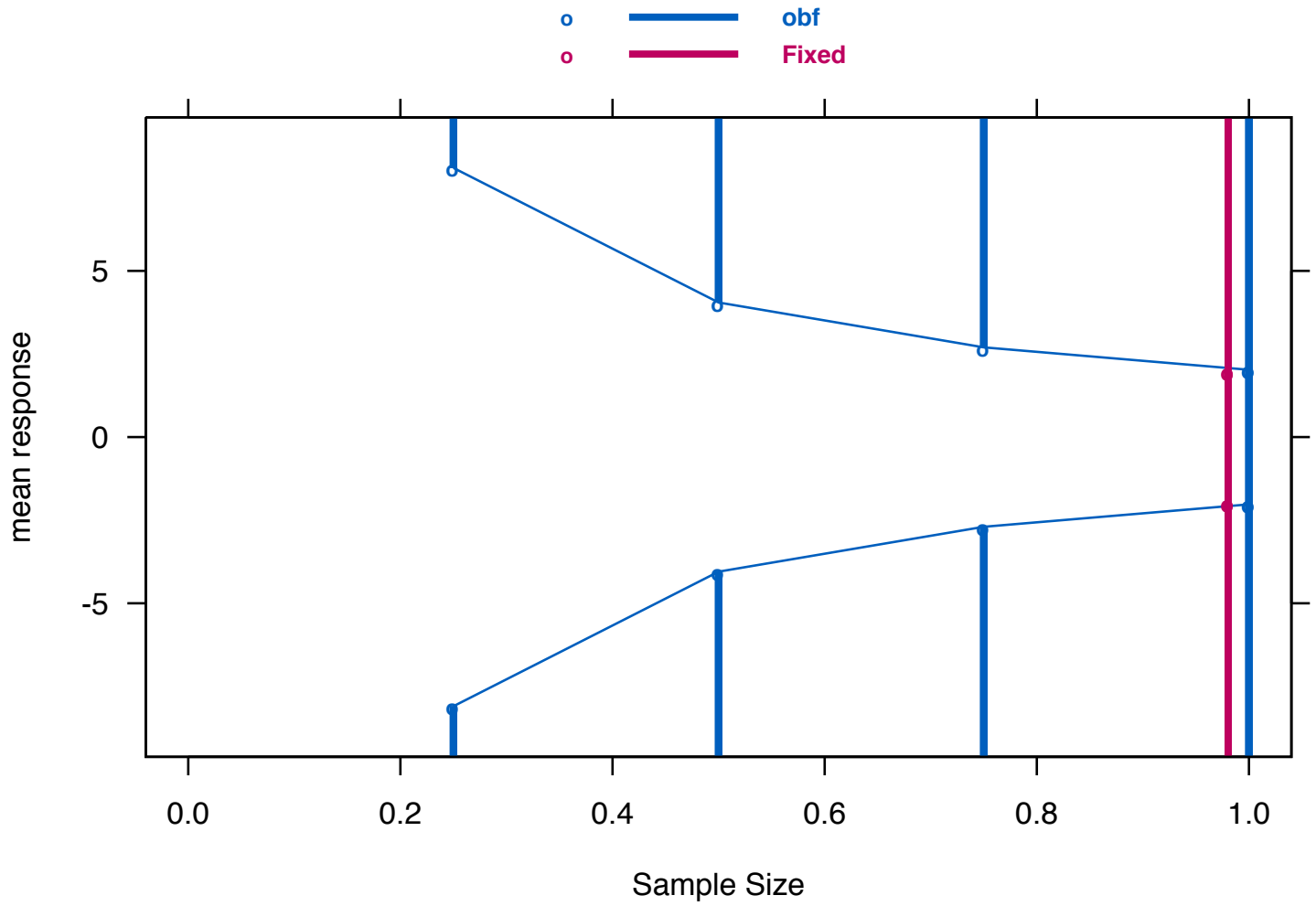
# Types of group sequential designs

## Example: O'Brien-Fleming (OBF) 2-sided design

- Using the correct sampling density, we can choose boundary values that maintain experiment wise Type I error



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# Types of group sequential designs



## Example: O'Brien-Fleming (OBF) 2-sided design

- Simulated type I error rate using fixed sample methods
- Based on 100,000 simulations

| Significant at | Proportion Significant | Number Significant | Proportion Significant |
|---|---|---|---|
| Analysis 1 | 0.00006 | Exactly 1 | 0.03610 |
| Analysis 2 | 0.00409 | Exactly 2 | 0.01198 |
| Analysis 3 | 0.01910 | Exactly 3 | 0.00210 |
| Analysis 4 | 0.04315 | All 4 | 0.00001 |
| | | Any | 0.05019 |

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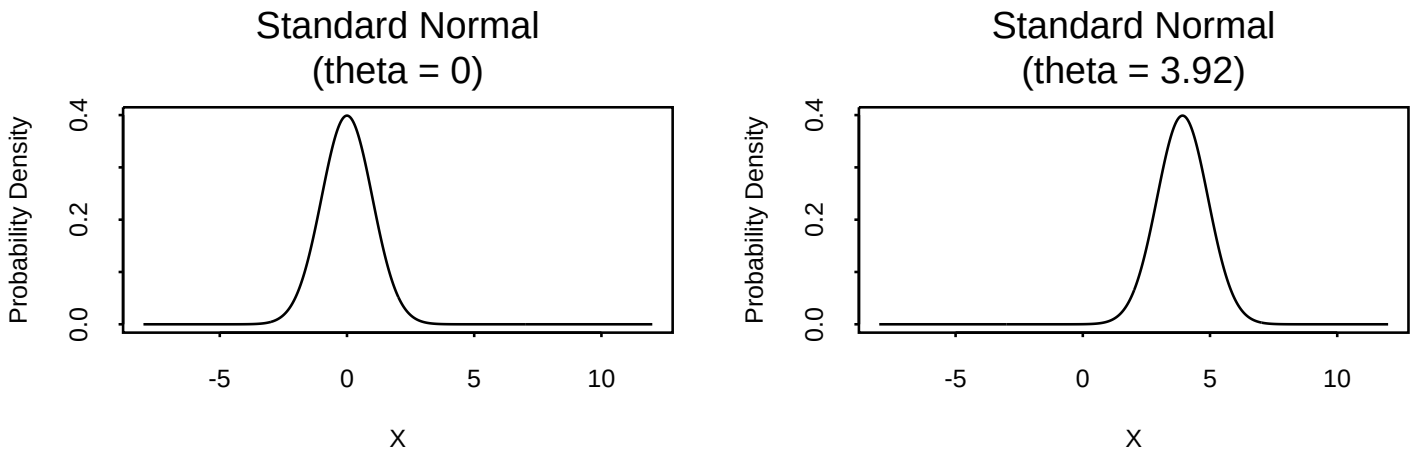
# Types of group sequential designs

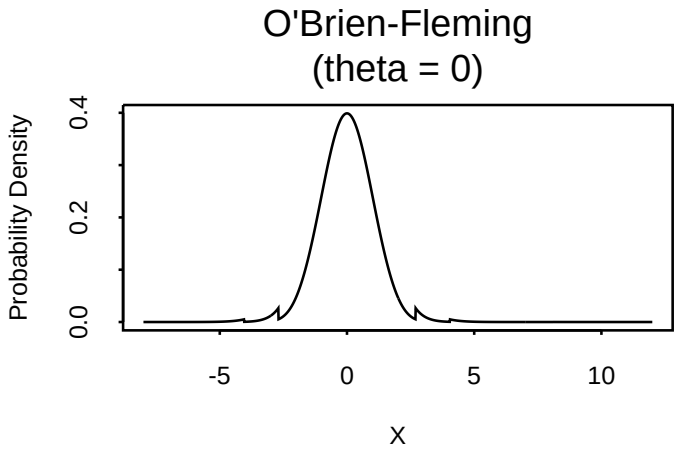
## Example: O'Brien-Fleming (OBF) 2-sided design

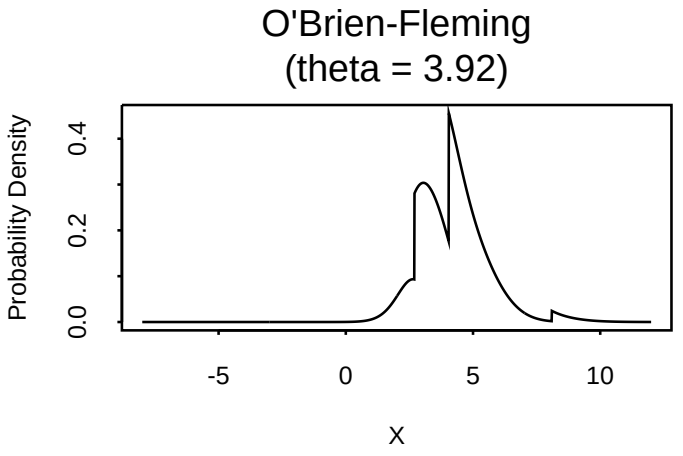
- Sampling density for OBF boundaries with $\theta = 0$ and $\theta = 3.92$ (corresponding Normal sampling density for comparison):









# Types of group sequential designs

## Boundary shape functions

- There are an infinite number of stopping boundaries to choose from that will maintain a given family-wise error
  - They will differ in required sample size and power
- Kittelson and Emerson (1999) described a “unified family" of designs that are parameterized by three parameters ($A$, $R$, and $P$)
- Parameterization of boundary shape function includes many previously described approaches
  - Wang & Tsiatis boundary shape functions:
    - $A = 0$, $R = 0$, and $P > 0$
    - $P = 0.5$ : Pocock (1977)
    - $P = 1.0$ : O’Brien-Fleming (1979)
  - Triangular Test boundary shape functions (Whitehead):
    - $A = 1$, $R = 0$, and $P = 1$
  - Sequential Conditional Probability Ratio Test (Xiong):
    - $R = 0.5$, and $P = 0.5$

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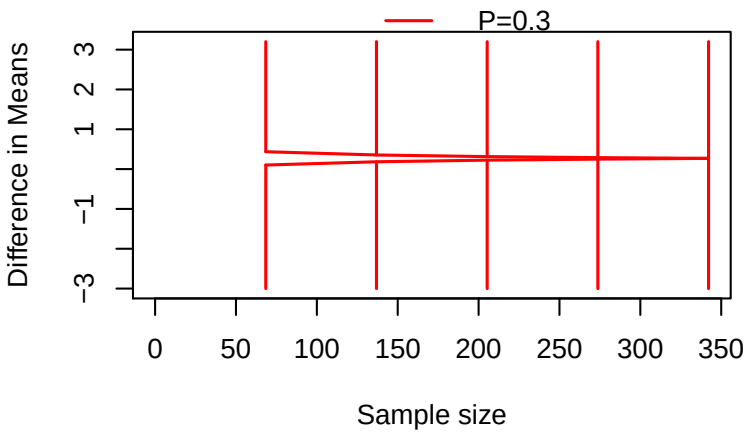
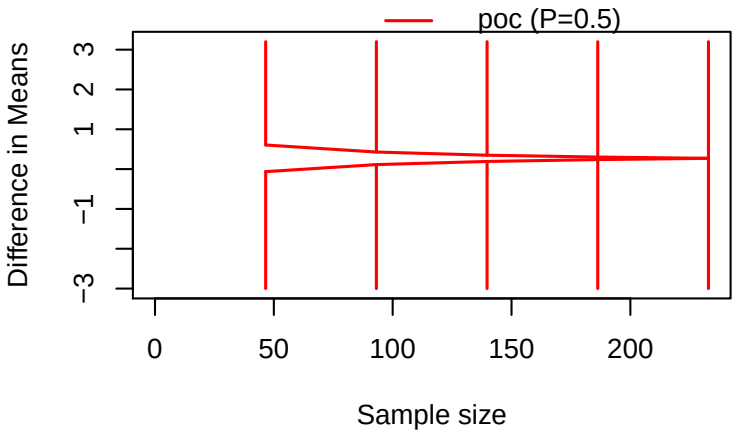
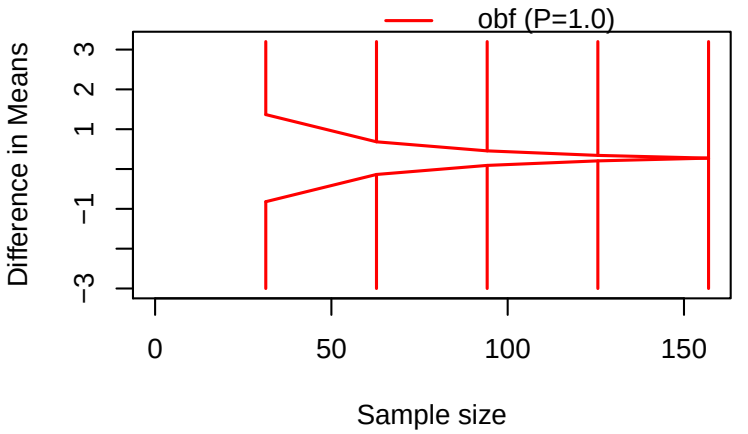
# Types of group sequential designs

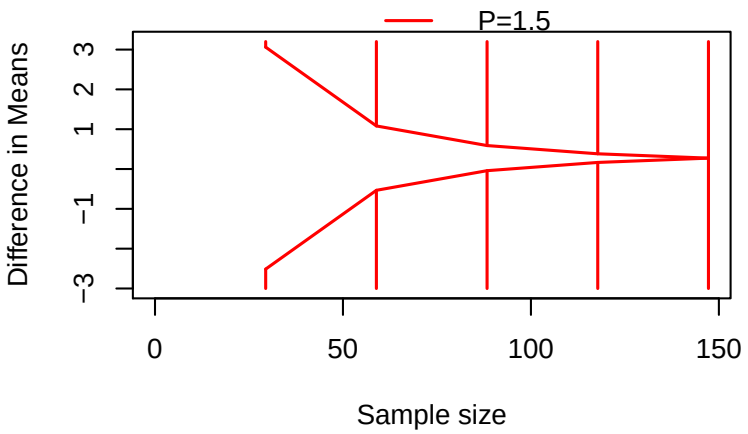
## Boundary shape functions

- Consider differing choices of $P$



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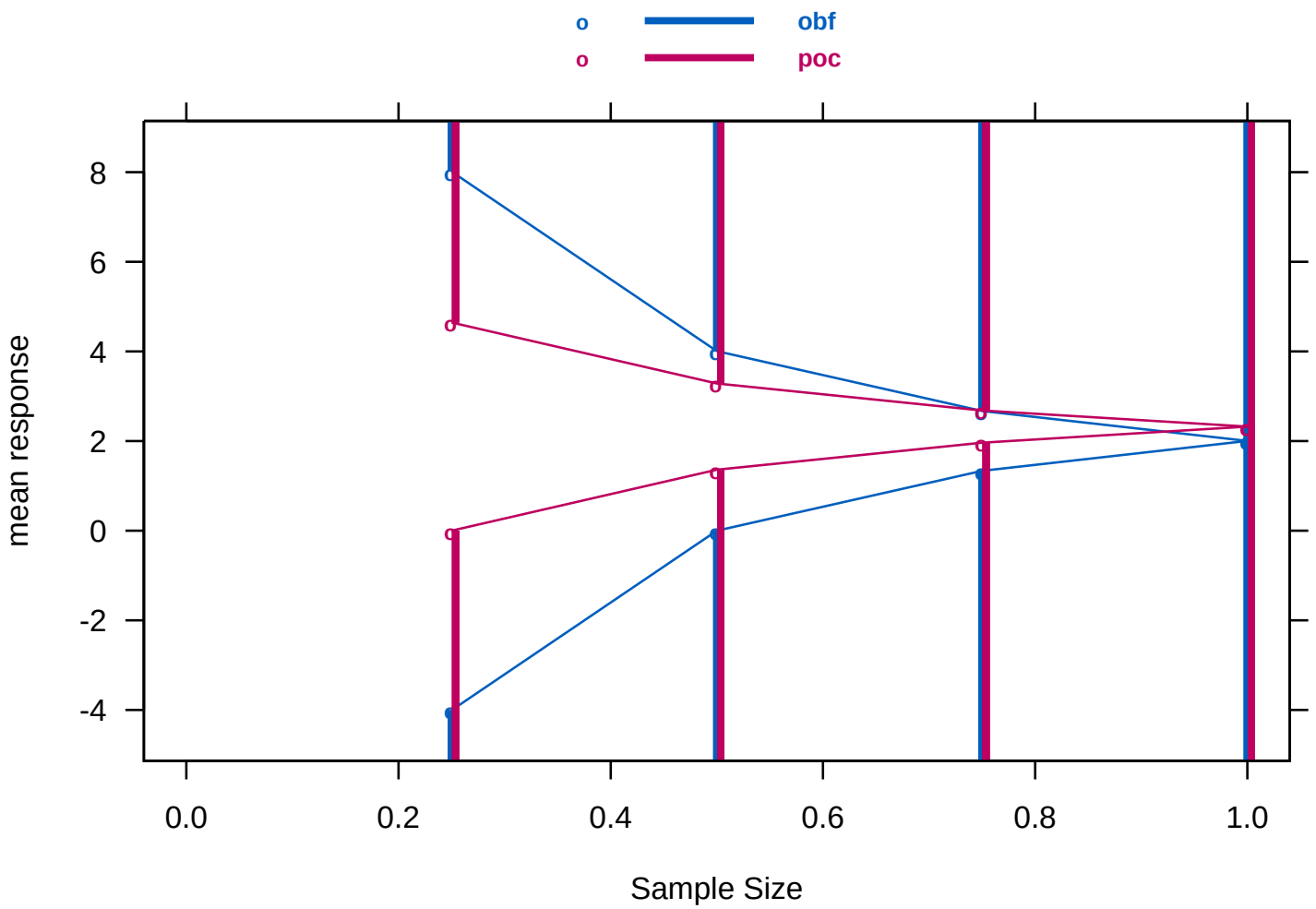
# Example: OBF (P=1) versus Pocock (P=0.5) 1-sided designs



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# Types of group sequential designs



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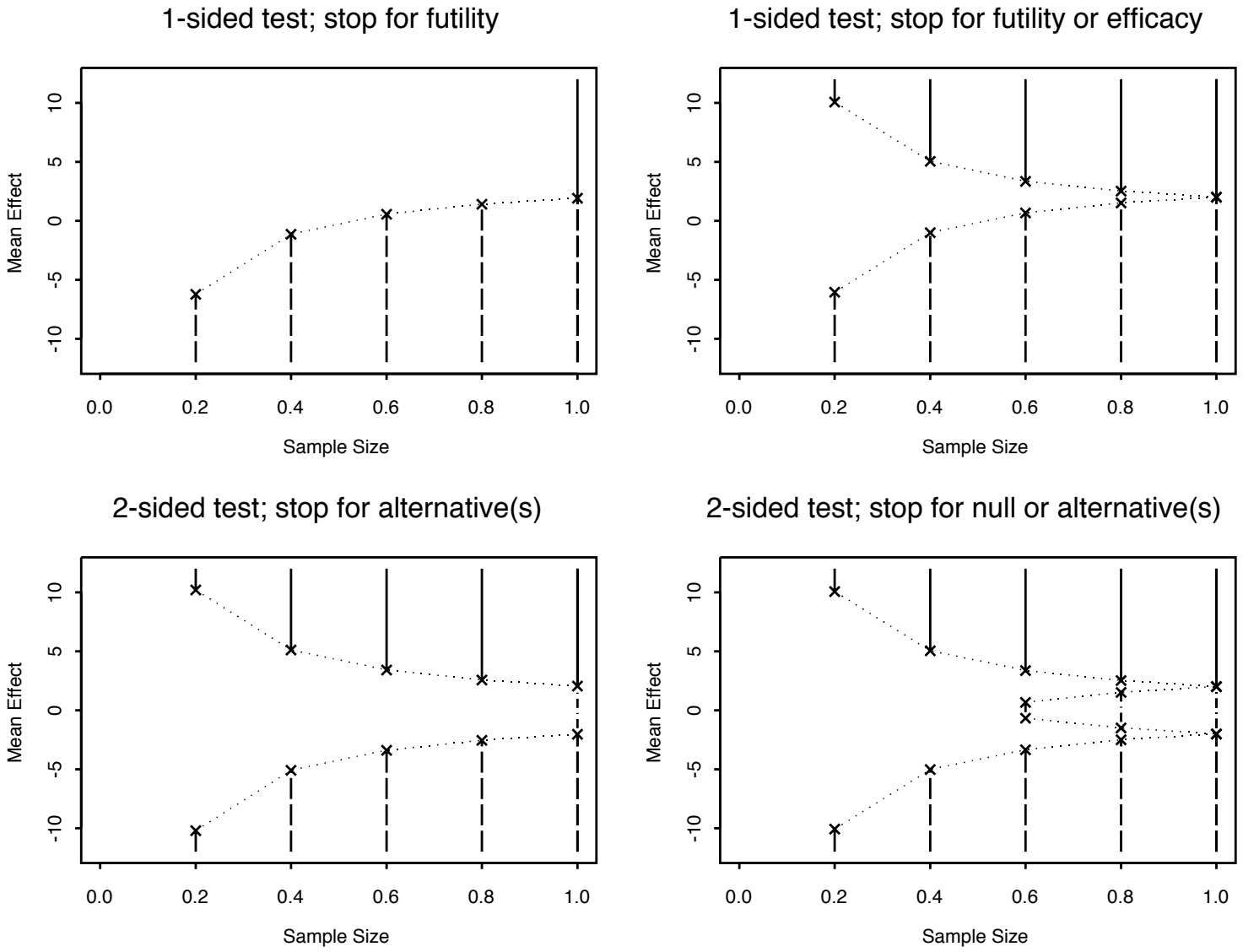
## Group sequential designs can be formulated for various hypotheses

- Four design categories:
  - One-sided test; One-sided stopping
    (allow stopping for efficacy *or* futility, but not both)
  - One-sided test; Two-sided stopping
    (allow stopping for either efficacy or futility)
  - Two-sided test; One-sided stopping
    (allow stopping only for the alternative(s))
  - Two-sided test; Two-sided stopping
    (allow stopping for either the null or the alternative)

# Four general design categories



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# Types of group sequential designs



## So how should we choose a stoping rule?

- Consider appropriate type of hypothesis to test
- Maintain statistical design criteria of the fixed sample trial:
  - Type I error rate of $\alpha = 0.025$ (one-sided test) or $\alpha = 0.05$ (two-sided test).
  - Maintain maximal sample size (with potential loss of power)
  - Maintain power (with larger maximal sample size)
- Other considerations when selecting critical values:
  - Number of interim analyses
  - Timing of interim analyses
  - Degree of early conservatism
  - Characteristics of the sample size distribution:
    - Expected sample size (Average Sample Number; ASN)
    - Quantiles of the sample size distribution
    - Maximal sample size
    - Stopping probabilities at each of the interim analyses

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# Interim analyses require special methods



## Characteristics of the group sequential sampling density

- Density is not shift invariant
- Jump discontinuities
- Requires numerical integration
- Sequential testing introduces bias:

| | $E(\hat{\theta})$ | |
|---|---|---|
| $\theta$ | OBF | Pocock |
| 0.00 | -0.29 | -0.48 |
| 1.96 | 1.95 | 1.82 |
| 3.92 | 4.21 | 4.38 |

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# Case Study : Sepsis Trial



## Background

- Critically ill patients often get overwhelming bacterial infection (sepsis), after which mortality is high
- Gram negative sepsis is often characterized by production of endotoxin, which is thought to be the cause of much of the ill effects of gram negative sepsis
- Hypothesis: Administering antibody to endotoxin may decrease morbidity and mortality
- Two previous randomized clinical trials showed a slight benefit
- There were no safety concerns at the inception of the trial

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# Case Study : Sepsis Trial



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## Definition of Treatment

- Single administration of antibody to endotoxin within 24 hours of diagnosis of sepsis
- Reductions in dose not applicable
- Ancillary treatments unrestricted

# Case Study : Sepsis Trial



## Defining the target population

- Patients in ICU with newly diagnosed sepsis
- Infected with gram negative organisms
  - culture proven
  - gram stain

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# Case Study : Sepsis Trial



## Defining the Comparison Group

- Need to ensure scientific credibility for regulatory approval
- Crossover designs impossible
- Ultimate decision:
  - Single comparison group treated with placebo
    - Not interested in studying dose response
    - No similar current therapy (still ethical to use placebo)
  - Randomized
    - Allow for causal inference
    - No blocking

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# Case Study : Sepsis Trial



## Defining the Outcomes of Interest

- **Goals**:
  - Primary: Increase survival
    - Long term (always best)
    - Short term (many other processes may intervene)
  - Secondary: Decrease morbidity
- **Refinement of the primary endpoint**
  - Possible primary endpoints
    - Time to death
    - Mortality rate at a fixed point in time
    - Time alive out of ICU during fixed period of time

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# Case Study : Sepsis Trial



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## Refinement of the primary endpoint

Option 1: Time to death (censored continuous data)

- Trial is likely to have early censoring due to logistical constraints of the trauma centers
- Such early censoring might place emphasis on clinically meaningless improvements in very short term survival
  - eg. We may be detecting differences in 1 day survival even though there is no difference in survival at 10 days

# Case Study : Sepsis Trial



## Refinement of the primary endpoint

Option 2: Mortality rate at a fixed point in time (binary data)

- Allows for choice of a *scientifically* relevant time frame
  - Treatment is a single administration; short half-life
- Allows for choice of a *clinically* relevant time frame
  - Avoids sensitivity to improvements lasting only short periods of time

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## Refinement of the primary endpoint

Option 3: Time alive out of the ICU during a fixed period of time (continuous data)

- Incorporates morbidity endpoints
- Addresses patient quality of life
- May be sensitive to clinically meaningless improvements depending upon the time frame chosen

# Case Study : Sepsis Trial



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## Refinement of the primary endpoint

Final Choice: Mortality rate at a fixed point in time (binary data)

- Sponsor proposed 14 day mortality
- FDA countered with a suggestion of 28 day mortality

# Case Study : Sepsis Trial



## Method of analysis

- Test for differences in binomial proportions
  - Ease of interpretation
  - 28 day mortality not a rare event
  - 1:1 correspondence with tests of odds ratio (for known baseline event rates)
- No adjustment for covariates
- Statistical information dictated by mean variance relationship of Bernoulli random variables:
  - Let $Y_{ki}$ denote binary response (mortality at 28 days) for $i$-th subject in group $k$, $k = 0, 1$
  - $Y_{ki} \sim \mathcal{B}(1, \theta_k)$
  - $\mathrm{E}[Y_{ki}] = \theta_k$ and $\mathrm{Var}[Y_{ki}] = \theta_k(1 - \theta_k)$

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# Case Study : Sepsis Trial



## Definition of statistical hypotheses

### Null hypothesis

- No difference in mortality between groups
- Estimated baseline rate
  - 28 day mortality: 30%
  - (needed in this case to estimate variability)

### Alternative hypothesis

- One-sided test for decreased mortality
- Targeted 28 day mortality rate in antibody arm: 25%
  - 5% absolute difference in mortality

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## Criteria for statistical evidence

- Type I error: Probability of falsely rejecting the null hypothesis Standards:
  - Two-sided hypothesis tests: 0.050
  - One-sided hypothesis test: 0.025
- Power: Probability of correctly rejecting the null hypothesis (1-type II error)
- Popular choice: 80% power

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## Determination of sample size

- Sample size chosen to provide desired operating characteristics
  - Type I error : 0.025 when no difference in mortality
  - Power : 0.80 when 5% absolute difference in mortality
  - Statistical variability based on mortality rate of 30% in placebo arm

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## Determination of sample size

- General sample size formula:
  - $\delta$ = standardized alternative
  - $\Delta$ = difference between null and alternative treatment effects
  - $V$ = variability of a single sampling unit
  - $n$ = number of sampling units

$$n = \frac{\delta^2 V}{\Delta^2}$$

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## Determination of sample size

- Parameter values in the present case:
  - $\delta = (z_{1-\alpha} + z_{\beta})$ with $\alpha = 0.025$ and $\beta = 0.80$
  - $\Delta = \theta_{1,H_1} - \theta_{0,H_1} = -0.05$
  - $V = \theta_{1,H_1}(1 - \theta_{1,H_1}) + \theta_{0,H_1}(1 - \theta_{0,H_1}) =$ $.25 \times .75 + .3 \times .7 = .3975$
  - $n$ = sample size per arm

$$n = \frac{\delta^2 V}{\Delta^2} = \frac{(1.96 + .841)^2 \times .3975}{(-.05)^2} = 1247.97 \rightarrow 1248$$

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## Resulting Fixed sample design

- Problem: Sponsor was concerned that 2496 ($2 \times 1248$) patients would be logistically infeasible and wanted to consider a design with 1700 patients
- Operating characteristics with $N$=1700:
  - Critical value : -0.0424
  - 64% power for alternative of 5% absolute difference; 90% power for alternative of 7% absolute difference; Corresponding p-value : 0.025
  - 95% confidence interval : (-0.085, 0)
  - Interpretation: Smallest magnitude of (observed) effect which would result in a significant result is a 4.24% decrease in mortality on the treatment arm with corresponding CI ( -0.085, 0).



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# Example: Sepsis Trial



## Addition of interim analyses

- FDA requires an interim safety analysis
- DSMB considers 4 interim analyses to stop for harm or futility using an O'Brien-Fleming stopping rule

```
PROBABILITY MODEL and HYPOTHESES:
   Theta is difference in probabilities (Treatment - Comparison)
   One-sided hypothesis test of a lesser alternative:
          Null hypothesis : Theta >=  0.00    (size  = 0.0250)
    Alternative hypothesis : Theta <= -0.07    (power = 0.9021)

STOPPING BOUNDARIES: Sample Mean scale
                     Efficacy Futility
    Time 1 (N=  425)     -Inf   0.0883
    Time 2 (N=  850)     -Inf   0.0019
    Time 3 (N= 1275)     -Inf  -0.0269
    Time 4 (N= 1700)  -0.0413  -0.0413
```

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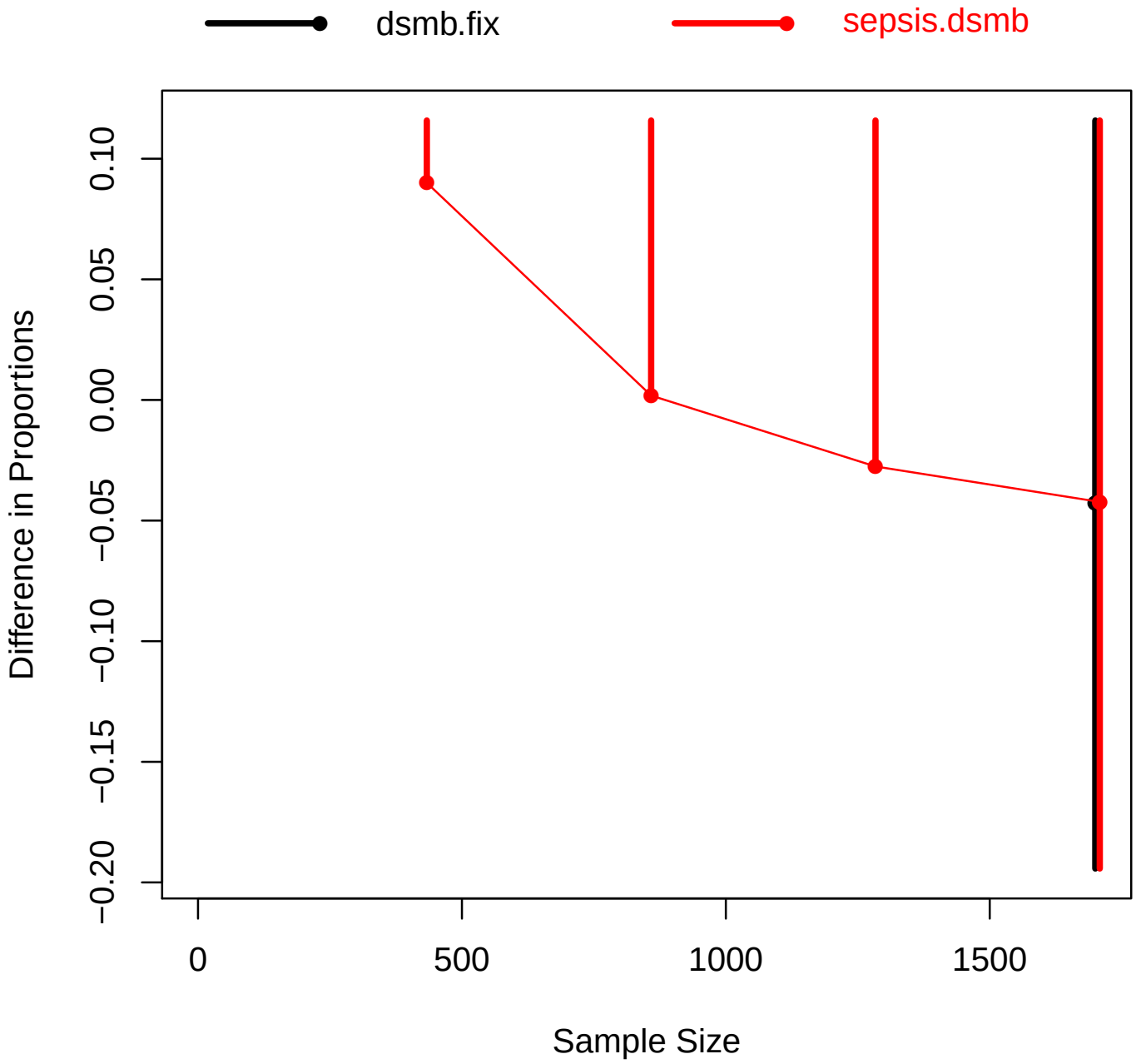
# Example: Sepsis Trial

- Stopping boundaries



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# Example: Sepsis Trial



## Addition of interim analyses

- Sponsor and DSMB would also like to consider stopping for efficacy
- Consider an O'Brien-Fleming boundary for both efficacy and futility

```
PROBABILITY MODEL and HYPOTHESES:
   Theta is difference in probabilities (Treatment - Comparison)
   One-sided hypothesis test of a lesser alternative:
           Null hypothesis : Theta >=  0.00    (size  = 0.0250)
    Alternative hypothesis : Theta <= -0.07    (power = 0.8947)
   (Emerson & Fleming (1989) symmetric test)

STOPPING BOUNDARIES: Sample Mean scale
                     Efficacy Futility
    Time 1 (N=  425)  -0.1710   0.0855
    Time 2 (N=  850)  -0.0855   0.0000
    Time 3 (N= 1275)  -0.0570  -0.0285
    Time 4 (N= 1700)  -0.0427  -0.0427
```

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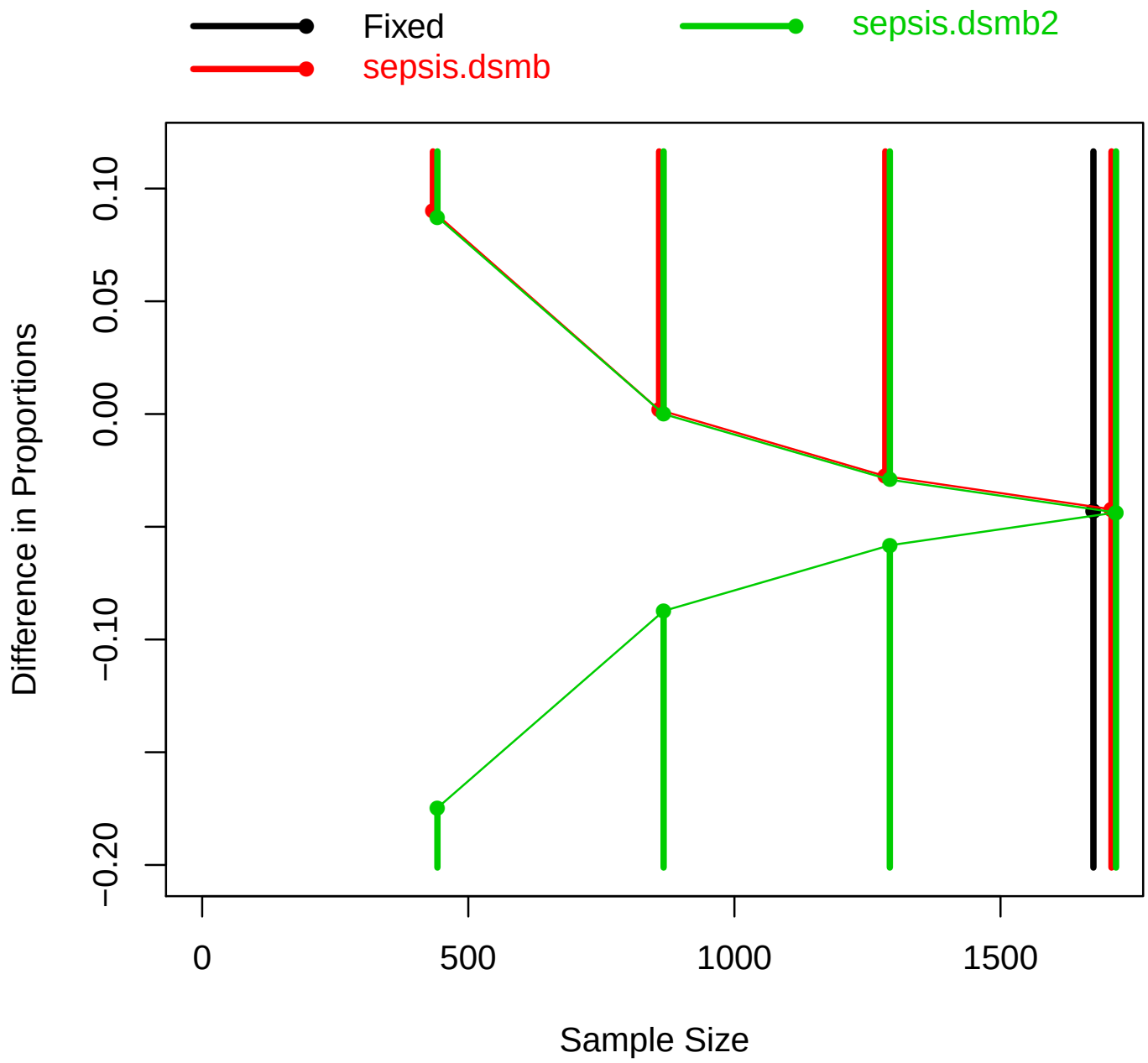
- Stopping boundaries



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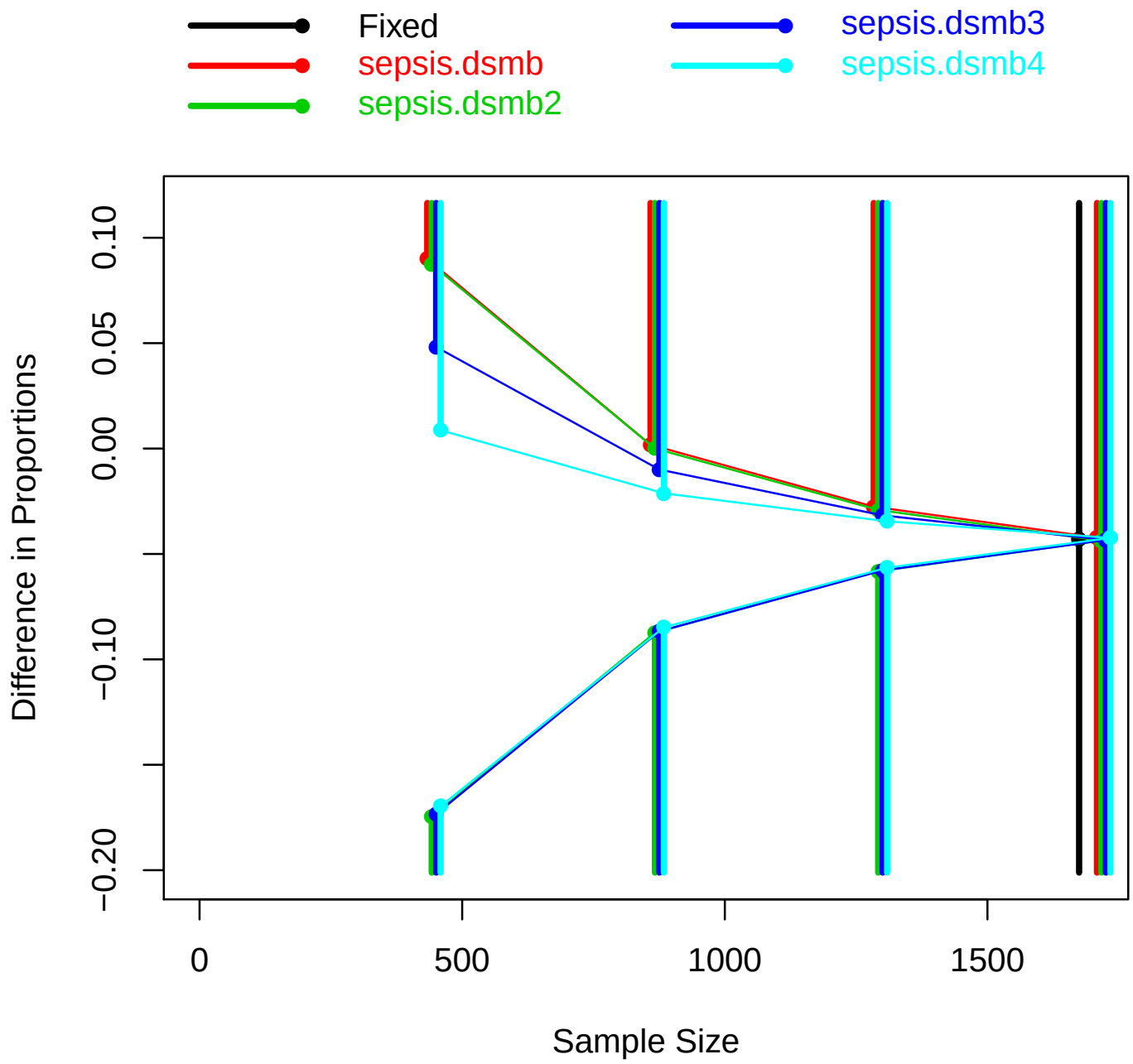
## Addition of interim analyses

- DSMB sought a design with less early conservatism for futility
- Sponsor considered a Pocock futility bound and something between an O'Brien-Fleming and Pocock design

# Example: Sepsis Trial

- Stopping boundaries



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# Example: Sepsis Trial



## Choosing a boundary

- In order to choose between the considered designs, need to consider operating characteristics
  - Point estimates of treatment effect at boundary decisions
  - Confidence intervals resulting from decisions on the boundary
  - Statistical power as a function of treatment effect
  - Sample size distribution as a function of treatment effect

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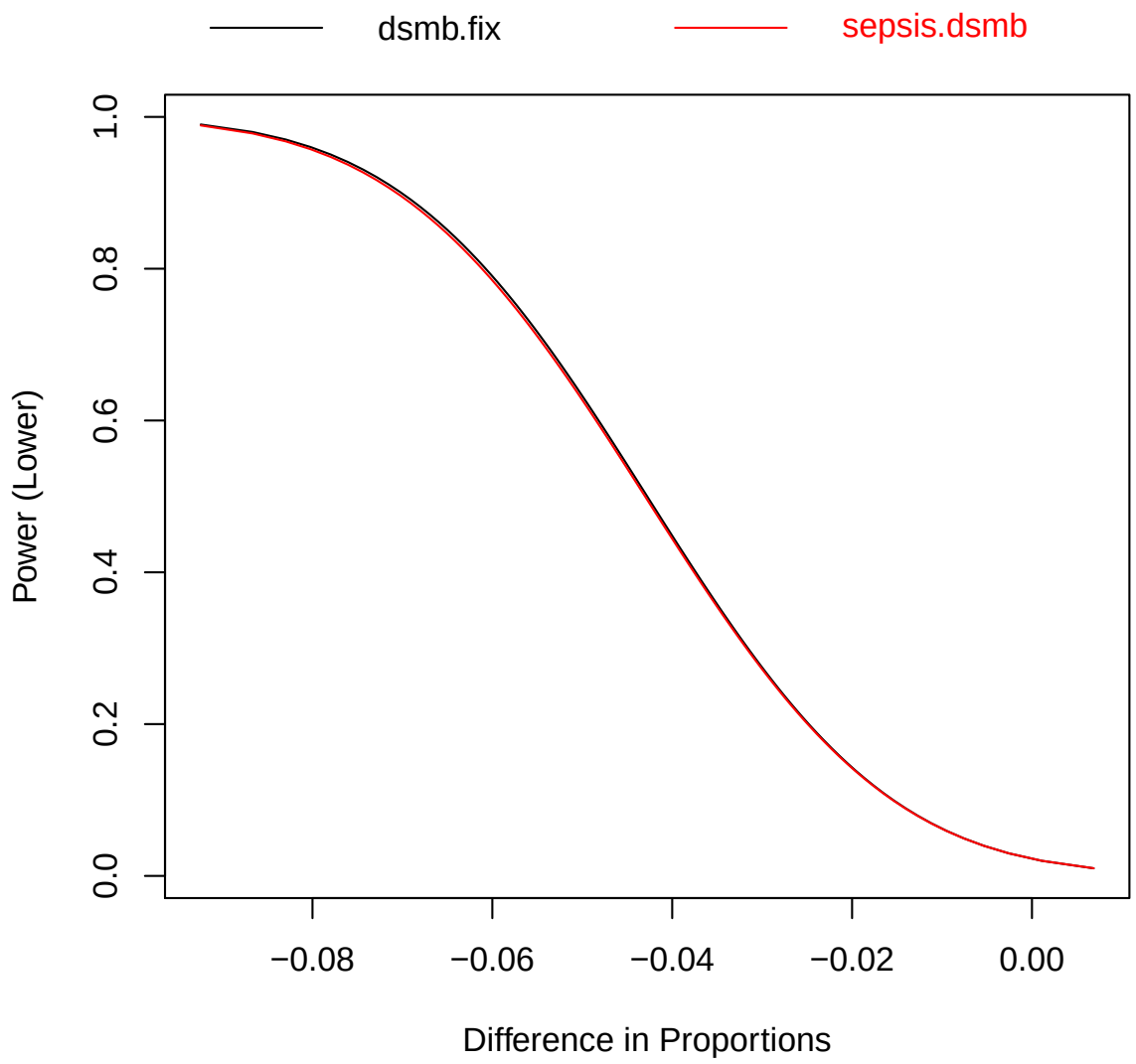
# Example: Sepsis Trial

- Comparing power (adding futility-only stopping):



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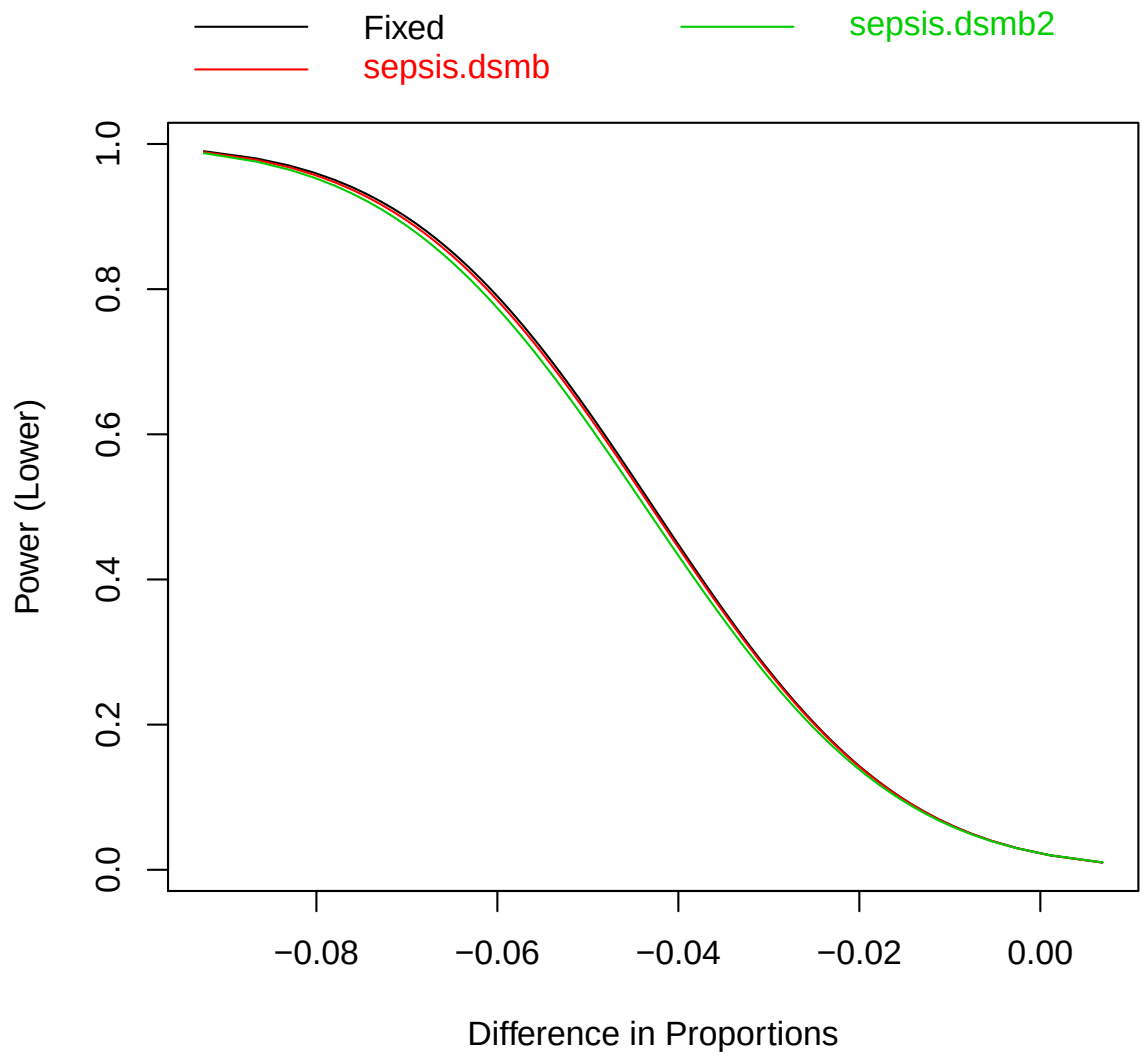
- Comparing power (adding futility and efficacy stopping):



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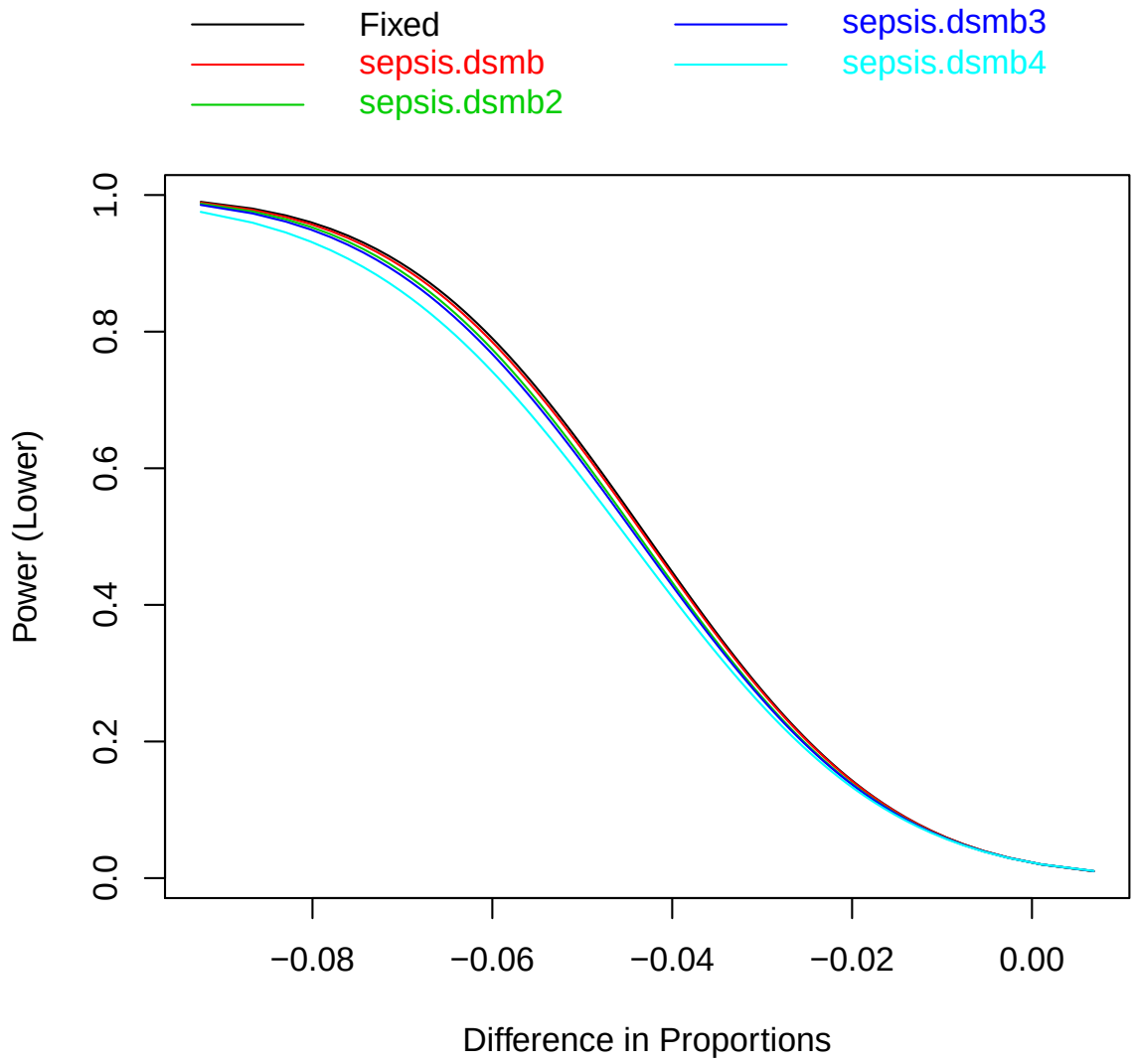
- Comparing power (effect of conservatism):



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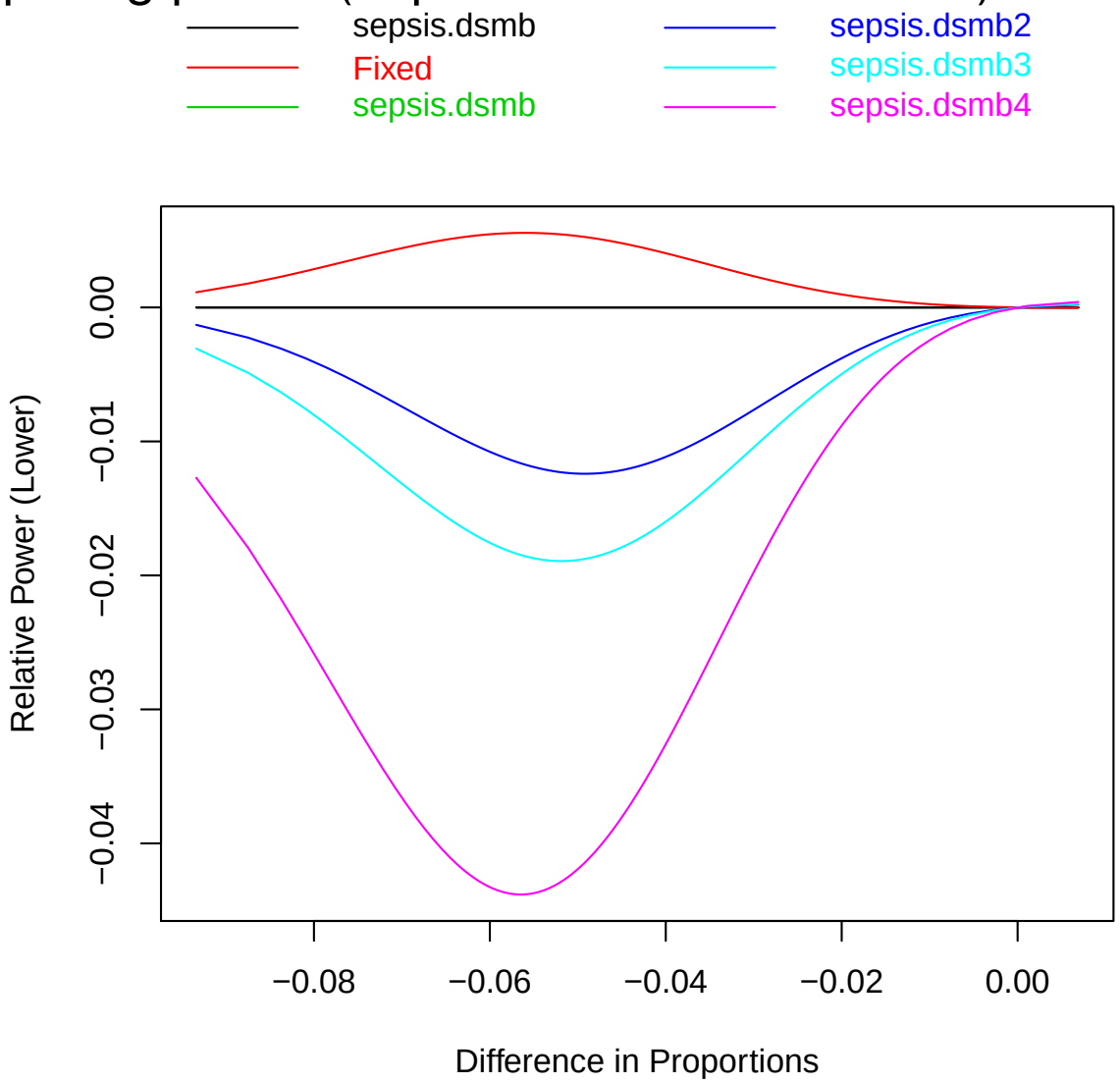
- Comparing power (sepsis.dsmb as reference):



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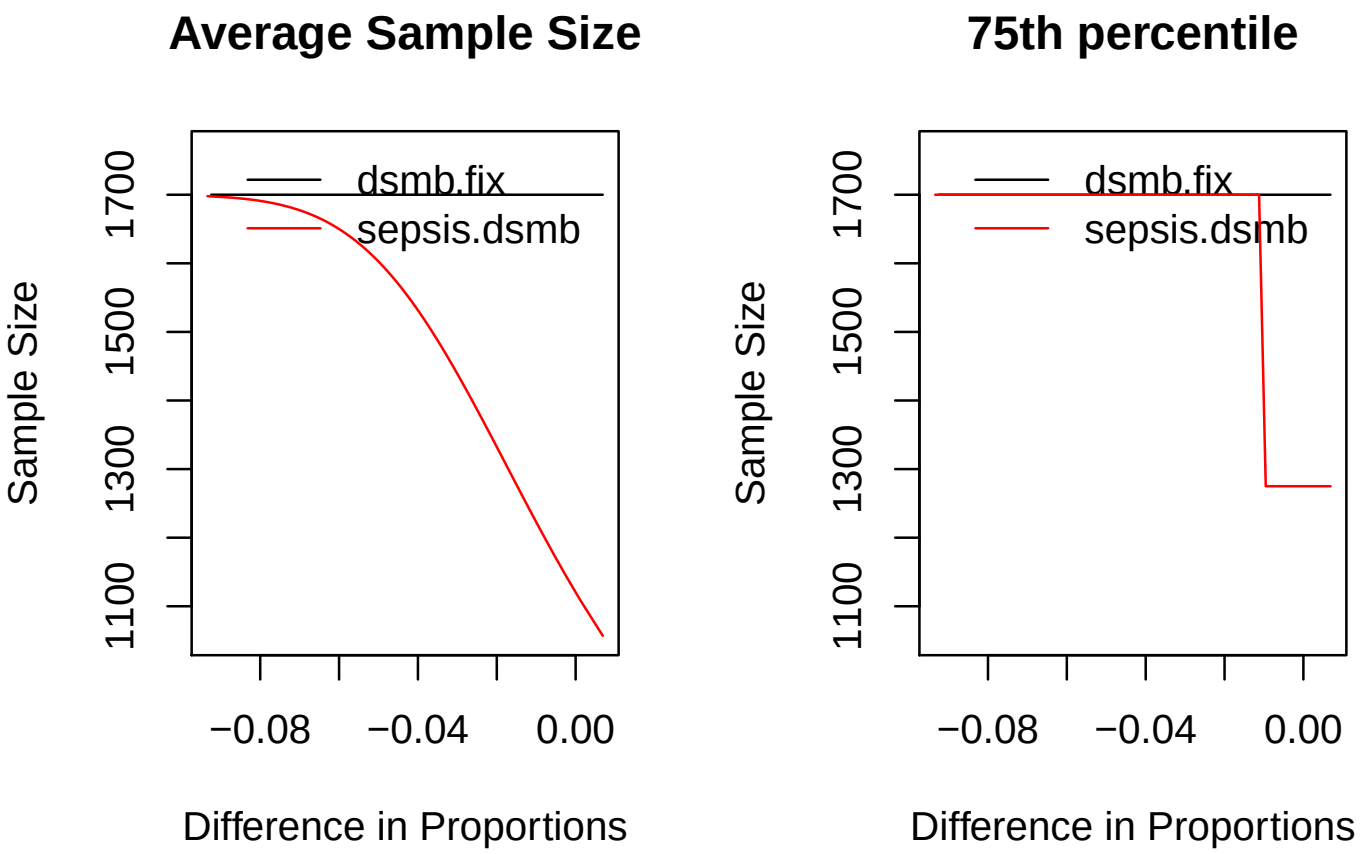
- Comparing expected sample size (ASN): adding futility-only stopping:

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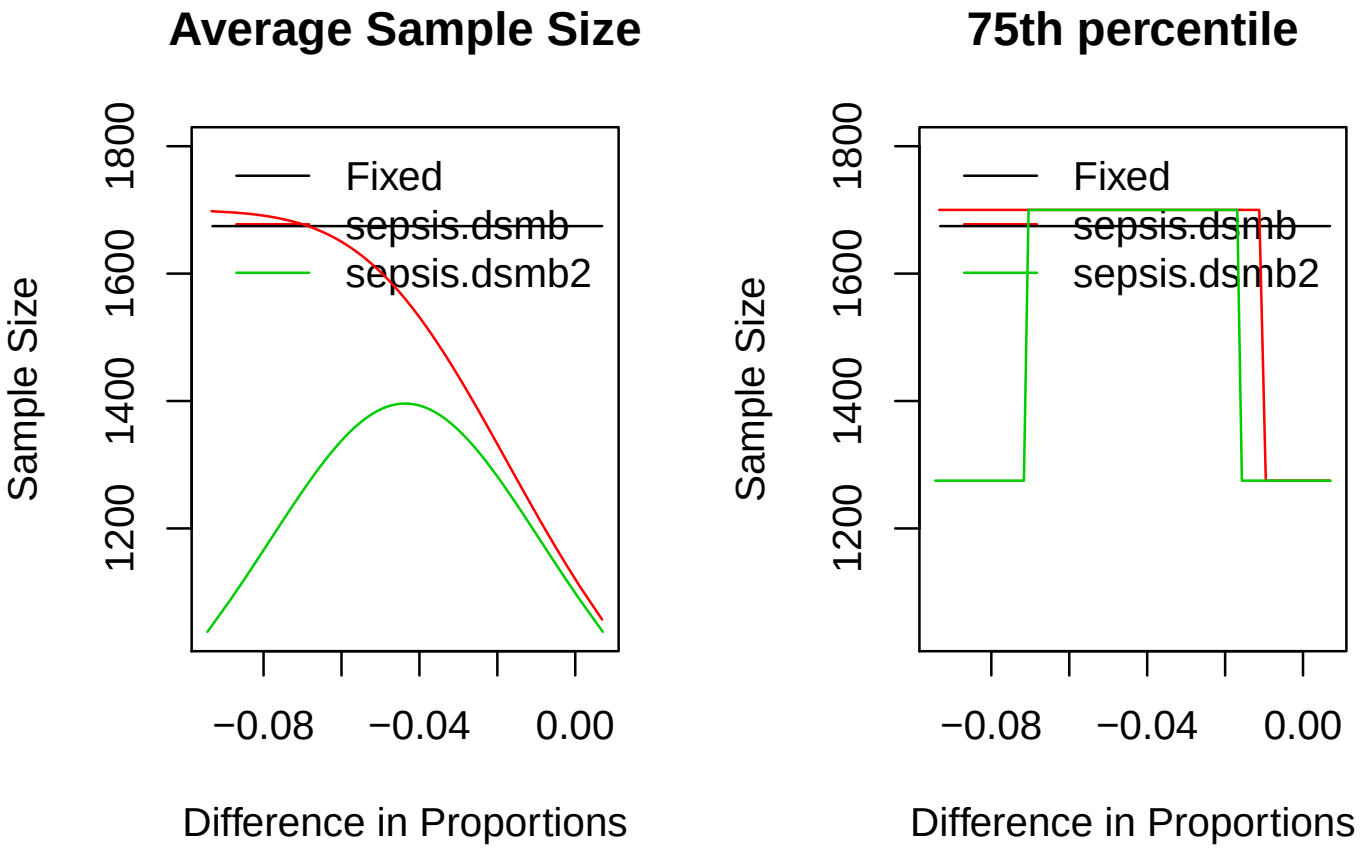
- Comparing expected sample size (ASN): futility and efficacy stopping:

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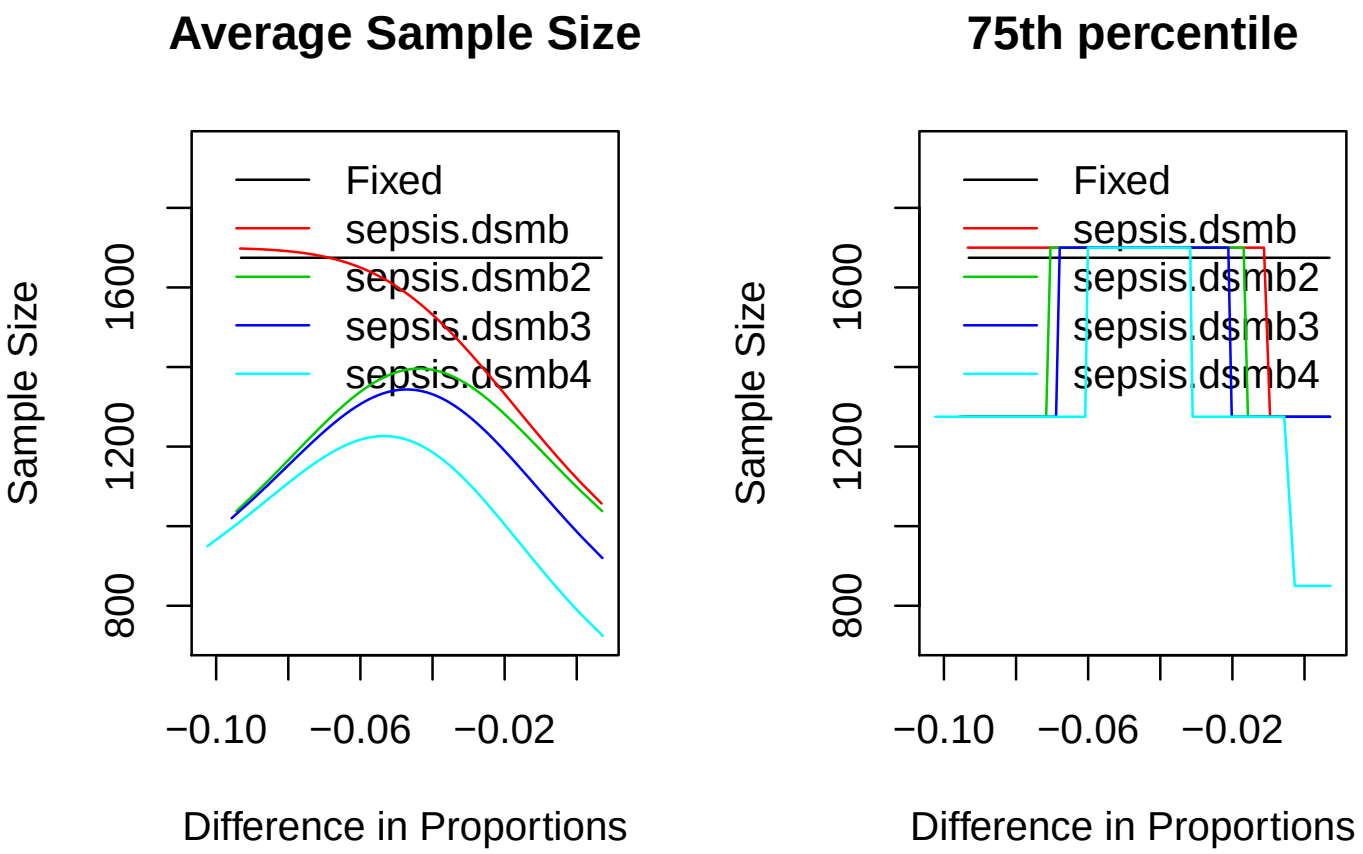
- Comparing expected sample size (ASN): early conservatism:

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## General behavior of interim analyses

- Decreasing early conservatism gave smaller ASN for unimportant benefits.
- Decreasing early conservatism also reduces power for efficacy.

# Example: Sepsis Trial



## General behavior of interim analyses

- For any given sample size, adding interim analyses reduces power.
- For any given power, adding interim analyses increases the sample size.
- Having fewer interim analyses:
  - Leads to properties (maximal sample size, power, etc) that are closer to those of a fixed sample study.
  - However, ASN may be larger and stopping probabilities lower.
- Having more early conservatism:
  - Leads to properties (maximal sample size, power, etc) that are closer to those of a fixed sample study.
  - However, ASN may be larger and stopping probabilities lower.

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