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Primary research

Experience collecting interim data on mortality: an example from the RALES study

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Abstract

Introduction: The Randomized Aldactone Evaluation Study (RALES) randomized 822 patients to receive 25 mg spironolactone daily and 841 to receive placebo. The primary endpoint was death from all causes. Randomization began on March 24, 1995; recruitment was completed on December 31, 1996; follow-up was scheduled to continue through December 31, 1999. Evidence of a sizeable benefit on mortality emerged early in the RALES. The RALES data safety monitoring board (DSMB), which met semiannually throughout the trial, used a prespecified statistical guideline to recommend stopping for efficacy. At the DSMB’s request, its meetings were preceded by an ‘endpoint sweep’, that is, a census of all participants to confirm their vital status.

Methods: We used computer simulation to evaluate the effect of the sweeps.

Results: The sweeps led to an estimated 5 to 8% increase in the number of reported deaths at the fourth and fifth interim analyses. The data crossed the statistical boundary at the fifth interim analysis. If investigators had reported all deaths within the protocol-required 24-h window, the DSMB might have recommended stopping after the fourth interim analysis.

Discussion: Although endpoint sweeps can cause practical problems at the clinical centers, sweeps are very useful if the intervals between patient visits or contact are long or if endpoints require adjudication by committee, reading center, or central laboratory.

Conclusion: We recommend that trials with interim analyses institute active reporting of the primary endpoints and endpoint sweeps.

Keywords: data safety monitoring boards, data sweeps, interim analysis, randomized clinical trials

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DSMB = data safety monitoring board; NYHA = New York Heart Association; RALES = Randomized Aldactone Evaluation Study.
Introduction
In randomized multicenter clinical trials monitored by an independent Data and Safety Monitoring Board (DSMB) charged with periodic evaluation of safety and efficacy, the data available for review typically include fewer events than have actually occurred when the database is closed. The discrepancies arise from either a failure of the investigators to report events soon after they occur or a failure of the adjudication committees to classify events quickly. This paper deals with an example of delays in reporting.

Methods
The RALES investigators randomized 1663 people with class III or IV heart failure on the scale of the New York Heart Association (NYHA) to receive spironolactone or placebo. Randomization began on 24 March 1995. Each participant was followed from randomization until the study’s common closing date, which was planned to be December 31, 1999, but actually occurred on August 24, 1998. The study was designed with 80% power to detect a 17% reduction in mortality, assuming some noncompliance with study medication and complete ascertainment of vital status. The secondary endpoints were cardiac mortality, hospitalization for cardiac causes, the composite of cardiac death or cardiac hospitalization, and change in class of heart failure on the NYHA scale. Pitt et al [4] describe the protocol, the dosing scheme, and the results; here we focus on the planning and execution of the interim analyses.

Protocol-defined visits occurred at weeks 0, 4, 8, and 12; at months 3, 6, 9, and 12; and then every 6 months until the common close of the study. Investigators were to report deaths within 24 h of discovery. Originally, the trial was to continue until 540 deaths had occurred in the placebo group. The sponsor amended the protocol in March 1996 to continue randomizing until reaching a sample size of 2500 patients or until 31 December 1996, whichever occurred sooner. This change implied that the last patient randomized would be followed for 36 months unless the trial stopped early.

The DSMB, which was composed of three cardiologists (DJ, chair; J-P B, HK), a cardiovascular epidemiologist, (CF), and a statistician (SP), met semiannually throughout the trial to review data relating to safety and efficacy. Spironolactone, which has been in use since 1960, has a well-known adverse event profile. The most common adverse events are gynecomastia and other feminizing symptoms in males. The most serious expected adverse event associated with the use of spironolactone is hyperkalemia, because the drug acts as a potassium-sparing diuretic.

The board monitored mortality with a statistical guideline for stopping based on a Lan–DeMets [5] use function and an O’Brien–Fleming boundary [6] for efficacy at a two-sided $\alpha$ level of 0.05. It did not specify a formal boundary for safety, relying instead on its collective judgment to recommend early termination if spironolactone showed a net adverse effect. The board unblinded itself regarding the treatment code on 24 August 1996, its first planned interim analysis.

At the board’s third interim analysis meeting, on 25 August 1997, the prespecified critical z-value for declaring efficacy of the drug was 3.67 (two-sided $P=0.00024$); the observed relative risk was 0.80 with a z-value of 2.55 (two-sided $P=0.011$), insufficient evidence to stop the trial. The board members were concerned that some...
Deaths might remain unreported, especially among the patients who were lost to follow-up and those who had not come to their 12-month visit. Given the strong trends in the data and the consistent patterns observed across subgroups of interest, the board suspected that the data would cross the prespecified stopping boundary before the planned end of the study. To avoid crossing the statistical boundary with uncertainty remaining about the number of unreported deaths, the board requested that each investigator provide a census of vital status as of 31 December 1997. In order not to alert the sponsor or investigators that the data were showing a strong trend, the board worded its request in terms of the need for a ‘standard two-year’ accounting of data. At the DSMB meeting in March 1998, after the ‘sweep’, the Board announced it found the process so valuable that it requested sweeps prior to each subsequent meeting. Because the board recommended stopping early, the study had only one additional sweep of deaths.

We used computer simulation to evaluate the effect of the sweeps. We assumed that the patterns observed up to August 1998 would have persisted throughout the study and simulated 1000 replications of a RALES trial under these assumptions.

### Results

The RALES showed fewer deaths on spironolactone than on placebo throughout the entire course of the study. The relative risk of mortality remained close to 0.80 (see Table 1).

At the fifth interim analysis, in August 1998, 620 deaths were reported. Assuming the protocol-projected mortality rates, 1080 deaths would be expected at the planned end of the study, yielding an estimated ‘information time’ of 620/1080 or 0.57. The actual number of deaths that would have occurred had the study continued to its planned end was of course unknowable; therefore, the statistical center (JP, JW) provided a range of projected number of deaths to examine the results’ sensitivity to the assumptions. The range, from 518 to 648, corresponded to a range of critical z-values from 2.70 to 3.08. The observed z-value of 3.75 was greater than the z-value for any of the projected number of deaths. Therefore, even if the projection of 1080 were incorrect, the data would have crossed the formal statistical boundary for efficacy.

The DSMB recommended termination of the study on 24 August 1998, when a total of 620 deaths had been reported: 351 in the placebo group and 269 in the spironolactone group. The chair of the study and the
sponsor concurred with this recommendation. After RALES ended, the investigators and sponsor reported an additional 46 deaths that had occurred prior to midnight, 2 August 1998. If all deaths had been reported within the 24-h window required by the protocol, the DSMB might have recommended stopping after the fourth interim analysis, when the critical and observed z-values would have been 2.95 and 3.56, respectively (see Table 1, Look no. 4b).

To assess how the sweep affected the interim analyses, we assumed that the patterns observed up to August 1998 would have persisted throughout the study; that is, 80% of those identified in the sweep would have been identified at the subsequent interim analysis, another 10% two meetings later, and the final 10% at the end of the study. We simulated 1000 replications of a RALES trial under these assumptions. Table 1 (Look nos 4a and 4b) shows the data that would have been reported. Even without the sweeps, the data were highly likely to have crossed the monitoring boundary at the fifth interim analysis. In terms of decision-making, therefore, the sweeps probably did not change the behavior of the board. The sweeps did, however, lead to a much more complete set of data to form the basis of the decision and much more security, on the part of the DSMB, that its decision was correct. In the RALES, the data showed no evidence of differential delay according to treatment or of a differential rate of events in the deaths reported with a delay and those reported on time. Thus, the final results showed essentially the same reduction in mortality as the DSMB observed, but because of the larger number of deaths, the final P-value was lower than that observed at the interim analysis.

In the RALES, the sweeps led to sizeable increases in the number of reported deaths, and hence in the statistical power, at both the fourth and fifth interim analyses. At the fourth interim analysis, the sweep led to an 8% increase in the number of reported deaths (from an estimated 503 to an observed 545). If the sweep had identified all deaths, the increase would have been 16% (503 to 584). At the fifth interim analysis, the increase in number of deaths observed was about 5% (589 to 620), with a potential increase of 13% (589 to 666) if the sweeps had identified all deaths.

Discussion

Our experience in the RALES emphasizes three aspects of data collection for interim analysis – the need to project the total number of events in a trial of fixed duration, the importance of timely collection of data regarding the primary endpoint, and the practical difficulties of reporting endpoints in trials with long-term follow-up. To project the total number of deaths that would have occurred in the RALES if the trial had continued to its planned end, we augmented the assumptions in the protocol with ranges of reasonable other assumptions. If crossing the statistical boundary is sensitive to the estimated number of deaths that would occur, then a DSMB should be uncomfortable recommending stopping.

The DSMB for the RALES, recognizing the importance of timely data, requested endpoint sweeps. The board was initially concerned that its request for endpoint sweeps would raise suspicion that it was positioning itself for early stopping. After the study ended, the DSMB learned that the process had in fact alerted neither the sponsor nor the investigators to such a possibility. Indeed, those carrying out the study felt burdened by the request for sweeps. The sponsor and the sites would have found them easier to perform if the operations manual had originally incorporated them. In the RALES, a monitor reviewed charts at each clinical site. This procedure, though consistent with the Good Clinical Practice guidelines of the International Conference on Harmonisation (see http://www.ifpma.org/ich5.htm), is not consistent with rapid reporting of endpoints. We recommend that trials with interim analyses institute active reporting of the primary endpoints and endpoint sweeps, along with DSMB meetings.

Continuous identification of endpoints that require independent adjudication poses more problems for interim analyses than does mortality. If the primary endpoint for the RALES had included, say, hospitalization due to worsening heart failure, endpoint sweeps would have been much more difficult. The investigators reported many of the hospitalizations many months after the study ended. Trials with an adjudication committee charged with assessing the primary endpoint and a DSMB charged with interim analysis must incorporate methods for timely adjudication of endpoints in order for the DSMB to discharge its responsibilities. The institution of formal interim analysis must be accompanied by resources that allow rapid collection of relevant data. While we strongly recommend sweeps, we also recommend that the procedures of a trial specifically describe the sweeps to help alleviate the practical problems of identifying the endpoint status of people at times other than at their planned clinic visits.

References