

Stopping clinical trials early for futility: retrospective analysis of several randomised clinical studies

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BACKGROUND: Many clinical trials show no overall benefit. We examined futility analyses applied to trials with different effect sizes. **METHODS:** Ten randomised cancer trials were retrospectively analysed; target sample size reached in all. The hazard ratio indicated no overall benefit ($n = 5$), or moderate ($n = 4$) or large ($n = 1$) treatment effects. Futility analyses were applied after 25, 50 and 75% of events were observed, or patients were recruited. Outcomes were conditional power (CP), and time and cost savings.

RESULTS: Futility analyses could stop some trials with no benefit, but not all. After observing 50% of the target number of events, 3 out of 5 trials with no benefit could be stopped early (low $CP \leq 15\%$). Trial duration for two studies could be reduced by 4–24 months, saving £44 000–231 000, but the third had already stopped recruiting, hence no savings were made. However, of concern was that 2 of the 4 trials with moderate treatment effects could be stopped early at some point, although they eventually showed worthwhile benefits.

CONCLUSIONS: Careful application of futility can lead to future patients in a trial not being given an ineffective treatment, and should therefore be used more often. A secondary consideration is that it could shorten trial duration and reduce costs. However, studies with modest treatment effects could be inappropriately stopped early. Unless there is very good evidence for futility, it is often best to continue to the planned end.

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Randomised phase III trials are usually based on several hundred or thousand subjects. New interventions are often found to be ineffective, or the observed effect is lower than expected and clinically unimportant, despite preliminary evidence that it could be beneficial. If an intervention is ineffective, it is worth considering whether the trial could have been stopped earlier after examining interim data, thus avoiding the recruitment of additional subjects and giving them an ineffective therapy, particularly if there are side effects. Also, the trial treatment could be stopped among those who are already taking it. Stopping for futility has other potential advantages, including savings in staff and financial resources.

Stopping trials early for futility has been discussed as far back as the 1980s (Halperin *et al*, 1982; Lan *et al*, 1982; Lan and Wittes, 1988), and work in this area is ongoing (Whitehead and Matsushita, 2003; Pocock, 2006; Lachin, 2009). There appears to be an increasing number of trials that incorporate futility, either in the protocol or the Independent Data Monitoring Committee (IDMC) requests such analyses during the trial. The Food & Drug Administration, for example, gives some guidance on this (FDA, 2006).

Futility methods involve using earlier results from patients recruited up to a specific point, to make assumptions about future data, so there will be limitations to this. For example with time-to-event outcomes, the assumption of proportional hazards could be

violated during the trial. The two main methods to assess futility are group sequential methods and conditional power (CP) (Whitehead and Matsushita, 2003; Snappin *et al*, 2006; Hughes *et al*, 2009). There are various approaches to futility analysis based on CP (Halperin *et al* 1982, Lan *et al* 1982; Lan and DeMets, 1983; Lan and Wittes, 1988). Other approaches include a Bayesian method to estimate an 'average' CP, called predictive power (Spiegelhalter *et al*, 1986; DeMets, 2006) and the use of a phase II surrogate end point in a phase III trial (Herson *et al*, 2011).

The Cancer Research UK and UCL Cancer Trials Centre has conducted clinical trials for many years, of which several have not shown a worthwhile benefit. We retrospectively examined futility analyses in these trials.

MATERIALS AND METHODS

Ten randomised phase III trials of superiority were included, in which the target sample size was reached in all (none had stopped early); Table 1. These trials were all of those on which the authors had worked that showed either no effect ($n = 5$), a moderate effect ($n = 4$) and one additional study was chosen with a large benefit, for comparison. We aimed to see whether examining futility would stop the five 'negative' trials early (and if so, what the savings could be), but not any of the others. In UKHAN, no effect was shown in patients with prior surgery, whereas those without surgery did benefit, so we regarded them as two separate trials (UKHAN_1 and UKHAN_2, respectively). In ZIPP, results for two endpoints were used to show how different results could arise.

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Table 1 Summary characteristics and final results of the 10 trials examined

Trial name; reference	Cancer	Interventions examined	Trial end point	Target HR	Target sample size (events)	Actual trial size (events)	Observed HR, 95% CI and P-value (at end of trial)
<i>No evidence of an overall benefit</i>							
Study 8; Spiro <i>et al</i> , 2006	Lung (small cell)	Early vs late radiotherapy	Overall survival	0.73	316 (252)	325 (271)	1.16 (0.91–1.47); P = 0.23
Study 12; Lee <i>et al</i> , 2009a	Lung (small cell)	Thalidomide vs placebo	Overall survival	0.78	720 (609)	724 (649)	1.09 (0.93–1.27); P = 0.28
Study 14; Lee <i>et al</i> , 2009b	Lung (non-small cell)	Thalidomide vs placebo	Overall survival	0.78	720 (609)	722 (665)	1.13 (0.97–1.32); P = 0.12
TOPICAL; Lee <i>et al</i> , 2010	Lung (non-small cell)	Erlotinib vs placebo	Overall survival	0.75	664 (550)	670 (644)	0.98 (0.82–1.15); P = 0.77
UKHAN_1 ^a ; Tobias <i>et al</i> , 2010	Head and neck	SIM vs RT alone (prior surgery)	Overall survival	0.76	253 (174) ^b	253 (145)	0.97 (0.70–1.35); P = 0.87
<i>Moderate treatment effect</i>							
ACT I; UKCCCR, 1996	Anal	Chemoradiation vs RT alone	Overall survival	0.72 ^c	577 (236) ^c	577 (236)	0.86 (0.67–1.11); P = 0.25
Over 50s; Hackshaw <i>et al</i> , 2011	Breast	5 vs 2 years of tamoxifen	Event-free survival	0.79	3012 (895)	3449 ^d (1139)	0.85 (0.76–0.96); P = 0.007
UKHAN_2 ^a ; Tobias <i>et al</i> , 2010	Head & neck	SIM vs RT alone (no prior surgery)	Overall survival	0.76	399 (293) ^b	399 (257)	0.81 (0.63–1.04); P = 0.10
ZIPP; Baum <i>et al</i> , 2006	Breast	Goserelin vs no goserelin	Event-free survival	0.81	2700 (742)	2706 (994)	0.80 (0.71–0.91); P = 0.001
ZIPP; Baum <i>et al</i> , 2006			Overall survival	0.81	2700 (742)	2706 (570)	0.78 (0.66–0.92); P = 0.004
<i>Large treatment effect</i>							
ABC02; Valle <i>et al</i> , 2010	Biliary tract	Gemcitabine/cisplatin vs gemcitabine alone	Overall survival	0.73	400 (315)	410 (327)	0.64 (0.52–0.80); P < 0.001

Abbreviations: HR = hazard ratio; RT = radiotherapy; SIM = simultaneous chemoradiotherapy. All HRs are as published, except for three trials whose results were reported after several years of follow-up (Baum *et al*, 2006; Tobias *et al*, 2010; Hackshaw *et al*, 2011). The HRs above were based on events observed 3 years after the last patient was randomised, when the first analyses could have been reported (as were the conditional powers in subsequent tables). However, they are very similar to those published. ^aThe trial compared RT alone with three chemotherapy regimens, but here we only focus on SIM vs RT. ^bThe original sample size was based on six trial groups considered together, so here we use the actual size for the particular patient group and SIM vs RT alone. ^cBased on the expected 5-year survival (65% vs 55%). The original sample size was based on local failure (target 260 patients in total), but 577 were actually recruited, so here we use the actual size and number of events. There was a clear benefit for local failure but we use survival because of the modest treatment effect on this. ^dThe observed number of patients exceeded the initial target because out of the ~4000 patients at registration the number who were eligible (relapse-free and alive) at 2 years was more than the initial estimate of 3012.

The CP is the chance of getting a statistically significant result at the end of the trial given the data so far. At each analysis, the distribution of future data is assumed to be consistent with the target hazard ratio (HR) (Snapinn *et al*, 2006). The CP calculation incorporates the observed and target number of events, and the observed and target HR; see Appendix 1 for the statistical methods, which are described in full elsewhere (Lan and Wittes, 1988; Proschan *et al*, 2006). For time-to-event outcomes, CP is usually based on the expected total number of events. However, one should be cautious in choosing the denominator of the information fraction and it is recommended not to dramatically change it (Proschan *et al*, 2006). Royston *et al* (2003) describe a stopping rule approach based on the expected total number of events in the control group, for multiple experimental arms when each is compared with the control. Generally, the CP should get closer to 100% over time as the observed HR approaches or exceeds the planned HR (i.e., when there is a real treatment effect), and it gets closer to 0% for studies of ineffective treatments. The CP should be low to provide sufficient supporting evidence to stop early, though there is no standard threshold in practice (Snapinn *et al*, 2006). Here, we suggest CP ≤ 15%. Stata v10 (College Station, TX, USA) was used to calculate CP (Appendix 2). We also used the method in which CP is based on the observed treatment effect, rather than the target effect, at the interim analysis (Snappin *et al*, 2006). Although this method can be used with the other one by the Data Monitoring Committee to examine the interim results using various assumptions, it is not often used in practice. For time-to-event outcomes, caution should be used when interpreting CP if the proportional

hazards assumption is violated. This is not so much a concern for calculating the CP, but rather a limitation of the statistic to measure treatment benefit in the presence of non-proportional hazards.

Three interim analyses were specified, after 25, 50 and 75% of events had occurred, or patients had been recruited. Many researchers trigger the interim analyses on events, but using patients recruited is also used. Outcomes were: (i) CP, (ii) the number of patients left to recruit the target sample size and (iii) cost savings if a trial were stopped early.

If analyses are triggered on a specified percentage of recruited patients, we allowed some follow-up so that events could occur in the last patients accrued: 3 months for advanced disease (lung and biliary tract cancer), and 6 months for the others. Further patients would be recruited during this time, but with minimal contribution to the analysis. In addition to this, and also for interim analyses triggered on a specified percentage of events, we allowed two extra months, during which the IDMC would meet, discuss the results, and then make decisions with the trial investigators. Both allowances are expected in practice.

To provide some estimate of uncertainty when interpreting a single observed CP from a trial, we also simulated 1000 bootstrap samples for each trial when 50% patients or 50% events had occurred. Sampling was with replacement. For each trial, patients were randomly selected from the trial, such that they could contribute none or at least once to each of the 1000 bootstrap samples. The patients were sorted by the date of randomisation and the date of events where they occurred. Bootstrapping was

conducted based on the order in which patients were entered into the trial, therefore replicating the interim analyses scenario as they would have occurred prospectively. Each simulation was stratified by treatment arm so that the number of patients in each arm was the same as that observed. For each of the 1000 bootstrap samples we calculated the HR and corresponding CP, in order to assess the proportion of samples that would indicate stopping the trial early (i.e., where $CP \leq 15\%$). Cost savings were examined in the five trials with no overall benefit. The same unit costs were specified for all studies for comparability, without considering inflation and increased expenses over time. The costs were applied to the number of months left to complete the target recruitment at each interim analysis. Investigational drugs were always provided free of charge by the manufacturer or health service provider, as were the costs associated with extra follow-up clinic visits and assessments. Because only the direct costs of conducting the trial were considered, any estimates of savings are conservative.

RESULTS

The 10 trials are summarised in Table 1, of which 6 were relatively large (> 500 patients). The observed HRs at the end of the study in the 5 trials with no overall treatment benefit were either just below or above 1.0, though one (TOPICAL) showed a clear benefit among patients who had first cycle erlotinib rash. Among the four trials with moderate effects, the HRs were no lower than 0.78. The proportional hazards assumption was met in all trials.

Interim analyses triggered after a specified percentage of events are observed

None of the five trials with no overall benefit would be stopped early after observing 25% of events (Table 2). After 50% of events had occurred, Study 12 and UKHAN_1 could have been stopped

(low CP of 2% and 3% respectively), by which point the percentage of patients left to complete accrual would be 12% ($n = 83$) and 14% ($n = 36$), respectively. The proportion of samples in which the CP was expected to be $\leq 15\%$ was 83.6% (Study 12) and 78.2% (UKHAN_1) based on the bootstrap estimates of CP. Therefore, a decision to stop these trials using $CP \leq 15\%$, suggests 16 and 22% probability of the trials continuing (Table 3); that is, the converse percentages. Study 14 also had low CP (15%), but recruitment would already have finished. After observing 75% of events, these same three trials had very low CP, but all had finished recruitment. Study 8 also had low CP, but with only 17% of patients ($n = 54$) left to recruit. The TOPICAL trial would not have been stopped at any point, though at 75% of events the CP (17%) was close to our specified cutoff.

Among trials with a moderate effect, only ACT I had low CP, after 50 and 75% of events had occurred ($CP = 4\%$ and 2%). However, the HR estimates at these times (1.16 and 0.95) are noticeably different from the final estimate of 0.86, so the interim results on overall survival (OS) would be misleading and inconsistent with other trial end points (i.e., local failure, for which a clear benefit was shown), had the study been stopped early. None of the other three trials had low enough CP to be terminated early.

As expected, the trial with the large treatment effect (ABC02) would not be stopped early for futility at any point.

Interim analyses triggered after a specified percentage of patients are observed

Table 4 shows the results at each of the three specified time points. None of the five trials with no benefit had sufficiently low CP ($\leq 15\%$) after either 25 or 50% of patients were recruited. For example, even after half the patients were randomised in Study 12 (26% (156 out of 609) of the target number of events observed), the CP was still 55% and $HR = 1.07$ (final $HR = 1.09$). However,

Table 2 Interim analyses based on a fixed percentage of target events (assumes future data is consistent with the target HR)

Trial	Percentage of the total target events observed											
	25%				50%				75%			
	HR (95% CI); P-value	Conditional power %	No. of patients recruited ^a	No. (%) of patients left	HR (95% CI); P-value	Conditional power %	No. of patients recruited ^a	No. (%) of patients left	HR (95% CI); P-value	Conditional power %	No. of patients recruited ^a	No. (%) of patients left
<i>No evidence of an overall benefit</i>												
Study 8	0.74 (0.45–1.21); 0.23	72	125	191 (60)	0.88 (0.62–1.25); 0.47	39	187	129 (41)	1.08 (0.81–1.43); 0.61	0.02	262	54 (17)
Study 12	1.13 (0.82–1.56); 0.44	48	447	273 (38)	1.17 (0.94–1.47); 0.17	2	637	83 (12)	1.13 (0.94–1.36); 0.18	<0.01	724	0 (0)
Study 14	0.94 (0.69–1.30); 0.72	73	511	209 (29)	1.05 (0.84–1.31); 0.69	15	722	0 (0)	1.09 (0.91–1.31); 0.36	<0.01	722	0 (0)
TOPICAL	0.94 (0.67–1.31); 0.70	81	266	398 (60)	0.87 (0.68–1.10); 0.24	78	425	239 (36)	0.93 (0.77–1.13); 0.49	17	583	81 (12)
UKHAN_1	1.11 (0.61–2.02); 0.73	19	140	113 (45)	1.10 (0.72–1.67); 0.67	3	217	36 (14)	0.98 (0.70–1.39); 0.92	0.3	253	0 (0)
<i>Moderate treatment effect</i>												
ACT I	0.98 (0.59–1.63); 0.93	49	323	254 (44)	1.16 (0.80–1.66); 0.43	4	526	51 (9)	0.95 (0.71–1.27); 0.72	2	577	0 (0)
Over 50s	0.96 (0.74–1.25); 0.75	83	2887	125 (4)	0.83 (0.69–1.00); 0.049	95	3443	0 (0)	0.86 (0.73–1.00); 0.043	89	3449	0 (0)
UKHAN_2	1.05 (0.66–1.66); 0.85	36	194	205 (51)	0.85 (0.61–1.18); 0.33	45	331	68 (17)	0.81 (0.62–1.06); 0.13	49	398	1 (0.3)
ZIPP: EFS	0.76 (0.57–1.01); 0.06	90	1436	1264 (47)	0.85 (0.70–1.04); 0.12	79	2159	541 (20)	0.78 (0.66–0.92); 0.003	99.5	2594	106 (4)
ZIPP: OS	0.98 (0.74–1.31); 0.92	62	2358	342 (13)	0.86 (0.70–1.05); 0.13	76	2691	9 (0.3)	0.78 (0.66–0.93); 0.004	99.5	2706	0 (0)
<i>Large treatment effect</i>												
ABC02	0.59 (0.37–0.94); 0.025	93	109	291 (73)	0.68 (0.50–0.94); 0.018	95	268	132 (33)	0.66 (0.51–0.86); 0.002	99.9	381	19 (5)

Abbreviations: CI = confidence interval; EFS = event-free survival; HR = hazard ratio; OS = overall survival. ^aNo. of patients recruited' plus 'no. of patients left' equals at least the target sample size. ^aIncludes patients recruited while the DMEC meeting would be organised.

Table 3 CP based upon 50% patients recruited or 50% events observed, assuming that future data follows the planned HR distribution, and subsequent 1000 bootstrap replicates

Trial	50% Patients		50% Events	
	CP sample estimate (%) ^a	Percentage of bootstrap samples with CP ≤ 15% (95% CI)	CP sample estimate (%) ^a	Percentage of bootstrap samples with CP ≤ 15% (95% CI)
<i>No evidence of an overall benefit</i>				
Study 8	29	28.9 (26.1–31.7)	39	23.3 (20.7–25.9)
Study 12	55	2.1 (1.2–3.0)	2	83.6 (81.3–85.9)
Study 14	68	0.5 (0.1–0.9)	15	49.3 (46.2–52.4)
TOPICAL	75	3.9 (2.7–5.1)	78	3.4 (2.3–4.5)
UKHAN_1	18	39.6 (36.6–42.6)	3	78.2 (75.6–80.8)
<i>Moderate treatment effect</i>				
ACT I	50	5.8 (4.4–7.2)	4	78.2 (75.6–80.8)
Over 50s	92	0 (0–0)	95	0.8 (0.2–1.3)
UKHAN_2	11	60.1 (57.1–63.1)	45	18.3 (15.9–20.7)
ZIPP: EFS	87	0 (0–0)	79	3.0 (1.9–4.1)
ZIPP: OS	66	0 (0–0)	76	4.0 (2.8–5.2)
<i>Large treatment effect</i>				
ABC02	96	0 (0–0)	95	0.3 (0.0–0.6)

Abbreviations: CI = confidence interval; CP = conditional power; EFS = event-free survival; HR = hazard ratio; OS = overall survival. ^aThe 50% patients CP sample estimate is taken from Table 4 and the 50% events sample estimate from Table 2.

four trials could have been stopped early after 75% of patients had been recruited, where the CP was 0.2%, 3%, 10% and 8%, in Study 8, Study 12, TOPICAL and UKHAN_1, respectively. At this point, there remains 22, 9, 10 and 17% of patients to be recruited to complete the original target for these trials.

Among trials with a modest treatment benefit, there are two instances when recruitment could have terminated early: UKHAN_2 (CP = 11%; 50% of patients), and ACT I (CP = 7%; 75% of patients). Stopping these two trials early would be particularly concerning because the interim data for OS would not indicate any benefit at all (HRs 1.12 and 1.29 for ACT I and UKHAN_2, respectively) – very different from the final estimates (0.86 and 0.81). After 13 years followup of ACT I HR = 0.86, 95% CI 0.70–1.04 (Northover *et al*, 2010) and there was a clear benefit on event-free survival for UKHAN (HR = 0.72, *P* = 0.004; Tobias *et al*, 2010). For ACT I, at 50% events, about 22% of bootstrapped CPs were >15% (Table 3), which shows some uncertainty in any decision to stop early.

Futility assuming future data would be consistent with the observed HR

Table A1 shows the CP when the futility analyses assume that data from future patients follow the same distribution as that observed so far (rather than the original target HR). All of the other results (observed HR, number of patients recruited, and number of patients and events left to accrue) are the same as in Tables 2 and 4.

After 50% recruitment, four trials could be stopped (Study 12, Study 14, Study 8 and UKHAN), but not TOPICAL. All five trials had low CP after 75% recruitment. In TOPICAL, the CP is expected to decrease (given that there was no overall effect), but it was low at first (6%), then higher (26%) and then low again (0.8%). This is because the HR at 50% of patients (0.88) happened by chance to be closer to the target (HR = 0.75). However, all four trials with a moderate benefit could have been stopped early.

Time and cost savings for the trials that showed no evidence of an overall benefit

When interim analyses are based on percentage of events, the number of months left is, as expected, lower than when based on percentages of patients recruited, but there could still be cost savings (Table 5). For example, Study 12 could be terminated early after observing 50% of events (CP = 2%), but there are only 4 more months to complete recruitment and the savings associated with early stopping is £44 000. Overall, after seeing 50% of events, three trials could be stopped early, avoiding 4–24 more months of accrual and saving £44 000–231 000 in two of these (Study 12 and UKHAN_1); in Study 14 no savings are made because recruitment had already finished.

With the futility analysis at 75% of events, only one trial with low CP is still recruiting (Study 8), but the savings would be 15 fewer months of recruitment and £144 000 lower costs.

Table 5 also shows the estimated time and cost savings when the analyses are based on recruited patients. The trials could only be stopped after 75% of patients had been recruited, with 4–28 fewer accrual months and £44 000–270 000 lower costs. For example, in Study 12 only 66 more patients are needed to reach the target sample size, which actually took only 4 months. Had this study been stopped early, the savings would be £44 000. However, the number of months left to complete accrual was 19 for Study 8, 6 for TOPICAL and 28 for UKHAN_1. Even after recruiting 75% of patients there could be significant cost savings by stopping early: £183 000, £58 000 and £270 000, respectively. The observed monthly accrual rates are an important factor when considering whether to stop early or not, which was high in Study 12.

DISCUSSION

To the best of our knowledge, this is the first application of futility analysis to several real phase III oncology trials. Early stopping of those with an ineffective intervention has obvious appeal – primarily not exposing further patients to it, when there is no benefit but there could be side effects. However, we show that the decision to stop recruitment early is not straightforward (unless based on safety concerns and there is clearly more harm in one group than the other). There are trials with no overall benefit that might not be stopped early, but worse still there are studies with modest effects that could. Similar conclusions have been found elsewhere (Barthel *et al*, 2009). Conducting clinical trials is expensive and takes several years, so a secondary consideration is the potential significant savings in accrual time and financial costs, which could be of interest to funding organisations, but should be outweighed by the ethical issues. All of these considerations should be balanced against maximising the sample size to get a more reliable estimate of the treatment effect; examination of secondary end points (DeMets, 2006) and important, pre-specified subgroup analyses; and not missing an intervention with a moderate benefit, which is still clinically worthwhile.

Occasionally, by the time there is sufficient evidence for futility, recruitment is not far from the target, so it is sometimes best to continue to the end, because the savings in time and costs are minimal (e.g., Study 12); but only if there is no unacceptable harm to patients. A further consideration is whether patients are still on treatment. A trial in which all have finished the trial treatments, but subjects are in follow-up, could still continue if there are no concerns over the schedule of clinic assessments. Continuing follow-up in a trial that has been stopped early has the advantages of minimising bias and obtaining more data on adverse events.

The worse situation is for trials where there appears to be no benefit at an interim analysis, but they do in fact have a moderate effect. It would be unsatisfactory to stop such trials early because

Table 4 Interim analyses based on a fixed percentage of recruited patients (assumes future data is consistent with the target HR)

Trial	Percentage of patients recruited of the total target number											
	25%				50%				75%			
	HR (95% CI); P-value	Conditional power % (number of events)	No. of patients recruited ^a	No. (%) of patients left	HR (95% CI); P-value	Conditional power % (number of events)	No. of patients recruited ^a	No. (%) of patients left	HR (95% CI); P-value	Conditional power % (number of events)	No. of patients recruited ^a	No. (%) of patients left
<i>No evidence of an overall benefit</i>												
Study 8	0.61 (0.33–1.15); 0.13	80 (41)	90	226 (72)	0.96 (0.66–1.39); 0.81	29 (111)	166	150 (47)	1.05 (0.78–1.40); 0.77	0.2 (179)	246	70 (22)
Study 12	1.14 (0.66–1.97); 0.64	77 (53)	330	390 (54)	1.07 (0.78–1.47); 0.66	55 (156)	453	267 (37)	1.13 (0.91–1.42); 0.26	3 (315)	654	66 (9)
Study 14	0.93 (0.60–1.44); 0.73	81 (81)	354	366 (51)	0.97 (0.71–1.32); 0.85	68 (161)	543	177 (25)	1.05 (0.83–1.33); 0.69	21 (280)	714	6 (1)
TOPICAL	0.95 (0.67–1.35); 0.78	81 (129)	249	415 (63)	0.88 (0.69–1.12); 0.29	75 (272)	423	241 (36)	0.93 (0.77–1.12); 0.43	10 (434)	600	64 (10)
UKHAN_I	0.80 (0.34–1.87); 0.60	42 (22)	93	160 (63)	1.06 (0.61–1.85); 0.83	18 (50)	157	96 (38)	1.01 (0.65–1.55); 0.98	8 (82)	210	43 (17)
<i>Moderate treatment effect</i>												
ACT I	1.15 (0.51–2.61); 0.74	59 (23)	192	385 (67)	0.93 (0.58–1.50); 0.78	50 (68)	371	206 (36)	1.12 (0.78–1.62); 0.54	7 (114)	510	67 (12)
Over 50s	0.96 (0.46–2.02); 0.92	93 (28)	995	2017 (67)	0.92 (0.63–1.35); 0.68	92 (107)	1970	1042 (35)	1.03 (0.77–1.39); 0.84	81 (173)	2600	412 (14)
UKHAN_2	1.05 (0.55–2.02); 0.87	52 (37)	123	276 (69)	1.29 (0.85–1.94); 0.23	11 (91)	241	158 (40)	0.86 (0.62–1.20); 0.37	42 (148)	333	66 (17)
ZIPP: EFS	0.72 (0.46–1.13); 0.15	87 (79)	926	1774 (66)	0.80 (0.62–1.05); 0.11	87 (223)	1582	1118 (41)	0.81 (0.66–0.98); 0.03	91 (405)	2264	436 (16)
ZIPP: OS	1.14 (0.50–2.59); 0.75	78 (23)	926	1774 (66)	1.16 (0.74–1.81); 0.51	66 (78)	1582	1118 (41)	1.02 (0.75–1.38); 0.91	59 (168)	2264	436 (16)
<i>Large treatment effect</i>												
ABC02	0.62 (0.40–0.97); 0.036	92 (81)	124	276 (69)	0.65 (0.46–0.90); 0.011	96 (141)	247	153 (38)	0.69 (0.53–0.90); 0.006	99 (222)	356	44 (11)

Abbreviations: CI = confidence interval; EFS = event-free survival; HR = hazard ratio; OS = overall survival. ^aNo. of patients recruited' plus 'no. of patients left' equals the target sample size. ^bIncludes patients recruited while the first 25, 50 or 75% are being followed up, and also the time for the DMEC to meet.

Table 5 Potential savings (time and costs) associated with the interim analyses shown in Tables 2 and 4 for the five trials in which there was no overall treatment effect

	25% Target				50% Target				75% Target			
	CP (%)	Information fraction	No. of months left to complete recruitment	Costs saved (£'000)	CP (%)	Information fraction	No. of months left to complete recruitment	Costs saved (£'000)	CP (%)	Information fraction	No. of months left to complete recruitment	Costs saved (£'000)
<i>Events observed</i>												
Study 8	72	0.25	67	645	39	0.50	40	385	0.02	0.75	15	144
Study 12	48	0.25	12	133	2	0.50	4	44	<0.01	0.75	0	0
Study 14	73	0.25	7	78	15	0.50	0	0	<0.01	0.75	0	0
TOPICAL	81	0.25	23	221	78	0.50	14	135	17	0.75	7	67
UKHAN_I	19	0.25	51	491	3	0.50	24	231	0.3	0.75	0	0
<i>Patients recruited</i>												
Study 8	80	0.16	78	751	29	0.44	50	481	0.2	0.71	19	183
Study 12	77	0.09	18	200	55	0.26	12	133	3	0.52	4	44
Study 14	81	0.13	11	122	68	0.26	6	67	21	0.46	1	11
TOPICAL	81	0.23	24	231	75	0.49	15	144	10	0.79	6	58
UKHAN_I	42	0.13	65	626	18	0.29	47	452	8	0.47	28	270

Abbreviations: CP = conditional power. Calendar years of recruitment were: Study 8 (Dec 1992–Oct 2001), Study 12 (May 2003–Feb 2006), Study 14 (Jun 2003–Sep 2005), TOPICAL (April 2005–April 2009) and UKHAN (Jan 1990–Jun 2000). Annual costs used here were: full-time co-ordinator (£45 000), full-time data manager (£35 000), half-time administrator (0.5 of £27 000), regulatory support (£10 000), IT support (£5000) and running expenses of £7000. TOPICAL, Study 12 and Study 14 were large, so we allowed for 1.5 data managers. Costs saved, if the trial is stopped early, are rounded to the nearest £1000.

of insufficient patients or events. We give examples (ACT I and UKHAN_2) where interim HRs are close to or exceed 1.0, with low CP, but the final HR indicated a clinically important effect.

The results and conclusions of three of the trials with no overall effect provided useful information after reaching the target sample size, especially when examining important subgroup analyses. Study 8, whose results were unexpectedly inconsistent with a

preceding Canadian trial (despite having the same protocol), led to a systematic review showing that early radiotherapy only improved survival if patients completed chemotherapy (Spiro *et al*, 2006). A *post-hoc* subgroup analysis in Study 14 (Lee *et al*, 2009b) indicated that patients with squamous histology who had at least stable disease by chemotherapy cycle 3 had an OS HR of 0.71, and this has led to a randomised phase II trial using another

Box 1 Considerations for stopping a clinical trial early for futility

- Futility might not be useful for early-stage cancers that have a good prognosis, where events (e.g., recurrences or deaths) take several years to be seen. By the time lack of benefit is determined to be reliable, recruitment (and probably treatment) is probably close to finishing.
- There should be a low conditional power (e.g., $\leq 15\%$), based on the target effect size. The research team and IDMC should agree what they consider to be low. It is also worth considering estimates of uncertainty (e.g., bootstrapping CPs).
- Effect size should be very close to or above the no effect value (e.g., HR > 1 , possibly with lower 95% CI limit ≥ 0.90 or 0.95).
- The IDMC and trial team should agree that enough patients and, importantly, events have been observed so far to produce a reliable effect (remembering that the trial investigators are likely to want to continue); interim data will be influenced by chance, especially in early analyses.
- There are many more patients left to recruit, or to finish accrual is likely to take many more months (with financial cost considerations).
- Other clinically important end points do not show evidence of a benefit.
- There is no evidence of an effect in important pre-specified subgroups. However, if there is evidence within a subgroup (but not overall), an early effect could be spurious, especially if not based on many events or patients. Continuing to the end should confirm whether there really is an effect in the subgroup, and if there is, obtain a better estimate of it.
- The adverse events profile is acceptable (if there are no safety concerns, one might wish to continue to ensure that a modest effect is not missed).

antiangiogenic agent in these particular patients. In a prespecified subgroup analyses in TOPICAL (Lee *et al*, 2010), OS and PFS were significantly improved only among those who developed first-cycle erlotinib rash, but the reliability of these results would have been less clear if based on fewer patients and events. Continuing to the planned end in order to have reliable subgroup analyses has sometimes been used as justification for not conducting futility analyses, especially if there is unlikely to be an overall effect. However, there must be clear justification for these subgroup analyses, acknowledging the problems with data dredging. Also, if there is a positive treatment effect in one subgroup, when no overall effect is found, there may be a negative effect in another subgroup.

Our analysis has several key strengths. First, it is based only on trials that reached the original target sample size. Second, we use real clinical trial data, not just statistical simulations. Third, we took a practical approach to the interim analyses by allowing time for follow-up and for the IDMC to meet and make decisions with the trial team. Fourth, the trials had a range of effect sizes and sample sizes. Fifth, we undertook bootstrap simulation to provide estimates of measuring uncertainty for any decision to stop early, in order to support the analyses based on a single CP estimate from each trial. We are not aware of any previously published report that has examined the application of futility with all these considerations in mind.

Stopping a trial early is a crucial decision to be made between the IDMC and trial team. The evidence should be robust and based on several pieces of information, not just one statistic, be it the CP or otherwise. On the basis of our findings, a list of considerations for stopping for futility is shown in Box 1, so that only truly 'negative' trials are likely to be stopped early. It is worthwhile having two successive interim analyses to see if the data are consistent, hence strengthening the justification to terminate. Herson *et al* (2011) suggest that stopping trials early might miss late treatment effects and so futility methods should be used with caution. Freidlin *et al* (2010) comment on the need to strike a balance between aggressive and conservative stopping rules, suggesting a repeated monitoring approach. Overly aggressive stopping rules in the second half of a study may result in trials with moderate effects being stopped early. For example, in ACT I (after 50% events) the HR = 1.16 and CP = 4%, but the bootstrapping analysis indicates that there is still 22% chance of reaching the target HR. Conversely, conservative stopping rules may allow trials to continue past the point of when sufficient evidence to stop early has been attained.

Assumptions about the distribution of future data and timing of the interim looks are important. The CP method we used is based on the target HR (Snapinn *et al*, 2006). There is another method in which CP is estimated using the observed HR as the new target. The problem with this is that the observed HR is likely to be unreliable early on in the trial. However, CP based on the target

effect size is relatively insensitive to the early results of a trial. Deciding whether to trigger the interim analysis on proportion of patients recruited or events observed is also important. The observed effect size early on in a trial may fluctuate too much and so be unreliable, especially if there is treatment imbalance (Herson *et al*, 2011), and regardless of the method or assumptions used. Many researchers use percentage of events to trigger the interim analysis, a reasonable approach given that the statistical analyses are often influenced most by the number of events, and hence might be more reliable than percentage of patients. In the set of trials we examined, futility analyses triggered on events (after 50 or 75%) could stop four out of the five trials with no overall benefit, and only one trial with a moderate effect. Whereas analyses triggered on patients could also stop four out of five studies with no benefit, but 2 trials with a moderate effect. An important consideration is that analyses triggered on events are more likely to be based on longer follow-up, so the potential savings are generally less than analyses triggered on number of patients (Table 5).

Further research using modelling and simulations could examine an appropriate frequency of interim analyses, specifying situations when futility may or may not be appropriate, and which method(s) are appropriate, including whether to trigger the early looks on percentage of events or patients observed. Terminology from medical screening could be useful: detection rate (DR – the proportion of truly negative trials that are stopped early) and false-positive rate (FPR – the proportion of trials with modest treatment effects that are stopped early). A good method will have high DR and low FPR, and these parameters could be examined in relation to trial size, the timing of interim analyses, and different statistical methods. Other authors have discussed futility in relation to falsely stopping studies (Hughes *et al*, 2009). Methods examining two or more end points could also be developed.

In summary, careful application of futility methods can lead to ineffective treatments not being given to future trial patients, and this could also lead to shorter trial duration and reduced financial costs. However, there are situations when the end of the trial is not far off, so the research team may as well complete it. A major concern is that there are studies with modest treatment effects that could be inappropriately stopped early, and a clinically important effect missed. Therefore, unless there is very clear and sufficient evidence for futility, it is often best to continue to the planned end.

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APPENDIX 1

Equation for calculating conditional power (CP) (Proschan *et al*, 2006) used in (Table 2).

The CP at a specific time = $1 - \varphi[(Z_{\alpha/2} - E[B_{(t)}] | B_{(t)}) / \sqrt{1 - t}]$, where φ is the area under the standard normal distribution associated with what is in the brackets.

- $Z_{\alpha/2}$ is the Z-value cutoff associated with the target level of statistical significance (we use a P-value of 0.05, so $Z_{\alpha/2}$ is 1.96).
- $B_{(t)}$ is the transformed Z-statistic (based on a Brownian motion applied to sequential analyses), i.e., $B_{(t)} = Z(t) \times \sqrt{t}$.
- $E[B_{(t)} | B_{(t)}]$ is the expected value of $B_{(t)}$ at the end of the trial (when $t = 1$), given the data observed until point t .
- The information fraction, t , is the number of observed events so far, expressed as a proportion of the planned number of events.

General form	Example (TOPICAL trial, after 25% of patients have been recruited)
n = Number of events observed so far	$n = 129$
N = Target number of events	$N = 550$
$t = n/N$ (information fraction)	$t = 129/550 = 0.234$
HR_O = observed hazard ratio	HR_O = 0.95
HR_E = target (expected) hazard ratio	HR_E = 0.75
ln = natural logarithms	
$E[B_{(t)} B_{(t)}] = \left[\sqrt{\frac{n}{4}} \times \ln\left(\frac{1}{HR_O}\right) \times \sqrt{t} \right] + \left[\sqrt{\frac{N}{4}} \times \ln\left(\frac{1}{HR_E}\right) \times (1 - t) \right]$	$E[B_{(t)} B_{(t)}] = \left[\sqrt{\frac{129}{4}} \times \ln\left(\frac{1}{0.95}\right) \times \sqrt{0.234} \right] + \left[\sqrt{\frac{550}{4}} \times \ln\left(\frac{1}{0.75}\right) \times 0.766 \right]$
$CP = 1 - \varphi[(Z_{\alpha/2} - E[B_{(t)} B_{(t)}]) / \sqrt{(1 - t)}]$	$E[B_{(t)} B_{(t)}] = 0.141 + 2.582 = 2.723$ $CP = 1 - \varphi[(1.96 - 2.723) / \sqrt{(0.766)}]$ $= 1 - \varphi[-0.872]$ $= 0.81$ (i.e., 81%)

APPENDIX 2

Stata code for calculating conditional power and also for generating 1000 bootstrap samples for a trial

Conditional power

The following variables need to be present in the data set for each interim analysis:

- n*: the number of events observed up until the interim analysis.
- N*: the number of planned events at the end of the trial.
- t*: the information fraction = $n \div N$.
- HR_O: the observed hazard ratio at the interim analysis.
- HR_E: the planned hazard ratio.

*Conditional Power, based on planned data (the following represents two lines of code):

```
generate con_power_plan = (1 - normal((1.96 - ((sqrt(n/4)
× ln(1/HR_O) × sqrt(t)) + (sqrt(N/4) × ln(1/HR_E) × (1 - t))))/sqrt
(1 - t))) × 100
label variable con_power_plan Conditional power (%) - planned.
```

Bootstrap sampling

The bootstrap sampling is based upon data at a particular time point. In our analysis this relates to 25, 50 or 75% events or patients. That is, this restricts the data set to include only those patients who have been entered into the study by the specified time point (see Materials and Methods for further details).

*Bootstrap samples: 1000 replicates, based on generic data (one line of code):

```
bootstrap _b N_fail = e(N_fail), rep(1000) strata(treat)
saving(trial_bootstrap, replace): stcox treat
```

Table A1 Conditional power based on a fixed percentage of recruited patients or events (assumes future data is consistent with the observed hazard ratio so far

Trial	Percentage of patients recruited of the total target number			Percentage of the total target events observed		
	25%	50%	75%	25%	50%	75%
<i>No evidence of a benefit</i>						
Study 8	98	1	<0.01	69	9	<0.01
Study 12	<0.01	0.1	<0.01	<0.01	<0.01	<0.01
Study 14	13	3	0.02	8	0.01	<0.01
TOPICAL	6	26	0.8	8	32	1
UKHAN_1	30	0.3	0.3	0.1	0.01	0.01
<i>Moderate treatment effect</i>						
ACT I	0.07	5	<0.01	2	<0.01	0.09
Over 50s	9	22	0.4	6	88	72
UKHAN_2	0.5	<0.01	17	0.3	21	38
ZIPP: EFS	99.6	90	91	98	64	99.8
ZIPP: OS	<0.01	<0.01	0.6	3	55	99.8
<i>Large treatment effect</i>						
ABC02	99.6	99.4	99.3	99.9	98	100

General form	Example (TOPICAL trial, after 25% of patients have been recruited)
<i>n</i> = Number of events observed so far	<i>n</i> = 129
<i>N</i> = Target number of events	<i>N</i> = 550
<i>t</i> = <i>n</i> / <i>N</i> (information fraction)	<i>t</i> = 129/550 = 0.234
HR_O = observed hazard ratio	HR_O = 0.95
$E[B_{(t)} B_{(t)}] = \sqrt{\frac{t}{4}} \times \ln\left(\frac{1}{HR_O}\right) / \sqrt{t}$	$E[B_{(t)} B_{(t)}] = \sqrt{\frac{129}{4}} \times \ln\left(\frac{1}{0.95}\right) / \sqrt{\frac{129}{550}} = 0.601$
$CP = 1 - \phi\left(\frac{z_{0.95} - E[B_{(t)} B_{(t)}]}{\sqrt{1-t}}\right)$	$CP = 1 - \phi\left(\frac{1.96 - 0.601}{\sqrt{0.766}}\right) = 1 - \phi(1.553) = 0.06$ (i.e. 6%)