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The premature closure of ROMPA Clinical Trial. Presentation of the results concerning the 49 randomized patients before the study closure.

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TITLE PAGE

Title: The premature closure of ROMPA Clinical Trial. Presentation of the results concerning the 49 randomized patients before the study closure.

Short title: The closure of ROMPA Clinical Trial.

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24 Units; Clinical Trials.
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ABSTRACT

Objectives: Coupled Plasma Filtration and Adsorption (CPFA) use in septic shock remains controversial. The objective is to clarify whether the application of high dosis of CPFA in addition to the current clinical practice could reduce hospital mortality in septic shock patients in Intensive Care Units at 28 days and 90 days follow up.

Design: We designed a prospective randomized clinical trial, ROMPA (Reducción de la Mortalidad Plasma-Adsorción), to demonstrate an absolute mortality reduction of 20% [$\alpha=0.05$; $1-\beta=0.8$; $n=190(95 \times 2)$].

Setting: Being aware of the pitfalls associated with previous medical device trials, we developed a training program to improve CPFA use (especially clotting problems). The protocol was approved by the ethics committees of all participating centers. Circumstances beyond our control produced a change in recruitment conditions unacceptable to ROMPA researchers and the trial was discontinued.

Participants: By closure, 5 centres from an initial 10 fulfilled the necessary trial criteria, with 49 patients included, 30 control group (CG) and 19 intervention group (IG).

Intervention: CPFA.

Main outcome measures: Hospital mortality at 28 days and 90 days follow up

Results: After 28 days, 14 patients died (46.7%) from the CG and 11 (57.9%) from the IG, not reaching statistical significance ($p=0.444$). At 90 days 19 patients had died (63.3%) from the CG and 11 patients (57.9%) from the IG, ($p=0.878$). The adjustment by propensity score or the use of the Kaplan Meier technique failed to achieve statistical difference, neither in the Intention to Treat Approach nor by the Actual Intervention Received.

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3 Conclusion: We herewith present the results gained from the prematurely-closed trial. The
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5 results are inconclusive due to low statistical power but we consider that this data is of
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7 interest for the scientific community and potentially necessary for any ensuing debate.
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11 Register: NCT02357433 in clinicaltrials.gov
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STRENGTHS AND LIMITATIONS OF THIS STUDY

- Randomised control clinical trial testing the efficacy of CPFA in septic shock.
- Premature closure of the trial by circumstances beyond the trial.
- Scarce sample size: underpowered trial.

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INTRODUCTION

Sepsis is still a leading cause of mortality in Intensive Care Units (ICU) patients, with a 20-50% mortality rate of sepsis and septic shock [1]. There exists a feeling of frustration generated by the large series of negative randomized clinical trials (RCTs) in septic shock treatment (especially in targeting mortality) during the past 30 years [2]. Patients included in these trials have a wide variability in their probability of death translating into differences in benefits to be derived from specific therapy application, producing a handicap to sample size calculation. This can lead to the trial having less power than initially planned increasing risk of a type 2 error and undoubtedly this is the origin of unexpected results.[3] It questions the use of subgroups in an attempt to extract some kind of useful information in negative RCTs.[4]

The COMPACT 1, a multicentre RCT study, failed to show benefit by using Coupled Plasma Filtration and Adsorption (CPFA) therapy in a population with septic shock. In a per protocol analysis, patients treated with CPFA at treated plasma volume superior to 0.20 l/kg/day showed a reduction in mortality rate [5]. Although an interesting finding, our group considered it was necessary to carry out a RCT to confirm this hypothesis.

The response to this question was the ROMPA (Reducción de la Mortalidad Mediante Plasma-Adsorción en Shock séptico), a multicenter RCT carried out in ICUs of southeastern Spain. The ROMPA Study (NCT02357433 in clinicaltrials.gov) tried to clarify whether the application of high doses CPFA in addition to the current clinical practice was able to reduce hospital mortality in septic shock patients in ICUs. The protocol of ROMPA has been published in a free access support and the details of the protocol could be consulted without restrictions.[6]

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3 In October 2017, COMPACT 2 (NCT01639664 in clinical trials.gov) trial investigators, a
4 similar study to ROMPA conducted in Italian ICUs,[7] reported the premature closure of the
5 study for having detected an increase in early mortality (3 first days) in the intervention
6 branch, 6/42 (12.5%) vs 19/58 (32.8%) $p=0.020$, not having reached the sample size prefixed
7 in the protocol (350 patients). The adjusted odds ratio (OR) of the treatment yielded by
8 logistic regression is 2.1 (95% CI: 0.7-6.6, $p=0.19$) and the adjusted hazard ratio (HR)
9 yielded by the Cox model is 2.5 (95% CI: 1.4- 4.4, $p=0.002$). This information was
10 immediately reported to our Ethical Committee and these results were published on the
11 research group website in Italian.[8] Subsequent events, including a provisional warning by
12 the product supplier, motivated us to take the final decision of closing ROMPA. At that time,
13 of the 10 initial hospitals only 5 had exceeded the technical capacity requirements and
14 availability of resources required to access the randomization portal. In this paper and as a
15 result of events of such severity, our group shows the data collected to date and the results
16 from the 49 enrolled patients (30 control and 19 intervention groups).

39 **METHODS**

42 **Protocol**

45 The full study protocol was previously published.[6] A synthesis of it is made in this section
46 (Methods) .

51 **Setting and participants**

54 The study was performed in 5 ICUs, in the southeast of Spain, that follow the same protocol
55 in the treatment of septic shock, based on the recommendations of the Surviving Sepsis
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3 Campaign with the participation of the following centers: Vega Baja Hospital of Orihuela,
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5 General University Santa Lucía Hospital of Cartagena, University Hospital of San Juan de
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7 Alicante, Lluís Alcanyís Hospital of Xàtiva and Francesc de Borja Hospital of Gandía.
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11 The ROMPA study is a multi-centric, randomized, prospective, open clinical trial with 28-
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13 and 90-day follow-up and allocation ratio 1:1, assessing the mortality reduction by CPFA in
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15 patients with septic shock. Furthermore, we analyzed 3-day mortality to compare our results
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17 with the Italian group.
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21 Each center obtained technical proficiency with the machine and CPFA treatment before they
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23 could become “activated” for enrolment by the investigator monitoring team. This was done
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25 to avoid similar problems as those reported for the first COMPACT study,[4] and also
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27 because CPFA is not routinely done in Spain and a new machine with improved
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29 anticoagulation support was subsequently developed and used for this trial.
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32 33 **Participants**

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36 Patients ≥ 18 years old admitted to the ICU of the participant hospitals, with a diagnosis of
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38 septic shock can be included in the study. The inclusion and exclusion criteria are detailed in
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40 the published protocol.
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43 44 **Interventions**

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47 The patient is considered registered once the informed consent form has been obtained by the
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49 patient or legal representative. The recruitment process ends with the patient randomization.
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51 Patients were divided randomly into two arms (control and intervention). ROMPA has a
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53 stratified randomization based on gender, age (≤ 65 or >65 years) and SAPS III score (<50 or
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55 ≥ 51). On the one hand, in the control group we followed the suggestions provided by the
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3 recent surviving sepsis guidelines, as well as standard care guidelines typically followed in
4 Spain. On the other hand, in the CPFA group, we applied the same protocol plus high doses
5 of CPFA in the first 3 days after randomization.
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10 **Variables and measurements**

11 **Primary and Secondary Outcomes**

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17 The primary outcome variable is all-cause of mortality assessed at 3, 28 and 90 days from the
18 recruitment of the patient. Moreover, at the descriptive level and in order to check
19 homogeneity of both groups, the following variables will be collected at the time of
20 recruitment: birth year, gender, height, dry weight, body temperature, heart rate, blood
21 pressure, blood cell count, coagulation values, glucose level, plasma creatinine,
22 bilirubinemia, plasma C reactive protein, procalcitonin level, blood gas analysis, lactate,
23 urinary output (ml/kg/h), Pa O₂/FiO₂ ratio, APACHE II, SOFA and SAPSIII scores.
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34 **Sample Size**

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37 Originally, a sample size of 190 patients was calculated to determine differences in mortality
38 rates in both groups with a power-of-contrast of 80%. A partial analysis with the first 49
39 patients has been carried out as described in this paper. Using the data from the initial sample
40 size calculation, these patients represent an approximate power-of-contrast of 30%.
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47 **Statistical analysis**

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50 Initially, the calculation of the indicators of clinical relevance (relative risk, RR, absolute risk
51 reduction, ARR, relative risk reduction, RRR, number needed to treat, NNT, by intent to
52 treat, ITT) was planned. Without having the sample size calculated for the study
53 (intermediate analysis) and having made the allocation based on a set of variables, the
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3 homogeneity of the groups was not able to be established. To minimize this problem, the
4 propensity scores as a population overlap weight technique was applied with the objective of
5 overcoming the problem caused by the lack of homogeneity between the two groups.[9] The
6 adjustment variables were APACHE II, previous lactate levels and the presence of urinary
7 sepsis. Finally, although it was not established in the study protocol, Kaplan-Meier survival
8 curves were analyzed to determine differences in mortality in the analyzed groups (log-rank
9 test). Since a significant number of patients died in the first three days and were unable to
10 receive the technique (n = 3) we decided to perform the analysis by actual intervention
11 received (AIR).
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23 24 25 **Ethical issues**

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28 The study was originally approved by all Ethics Committees of the different Hospitals
29 participating in the study. There was a general agreement that the trial closure was the best
30 option, since the decision adopted by the Italian group to close its trial had been made public
31 through its website and the supplier consequently marked their product used for the test with
32 a warning.
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43 44 **RESULTS**

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46 A total of 49 patients were included in the final analysis (30 in the control group and 19 in the
47 intervention group) (Figure 1). The randomization tables are displayed in Table 1. Parametric
48 statistics did not allow us to establish significant differences between the analyzed factors due
49 to the small sample size. However, we can see a mean difference between the two groups of
50 1.9 on an APACHE II score, 0.6 mmol/l of lactate levels and 10.9% in the prevalence of
51 urinary sepsis. All these factors have been used in the propensity score test.
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3 With regard to mortality (without adjusting by propensity score), 7 patients (23.3%) died in
4 the first three days from the control group and 8 patients (40.6%) died from the intervention
5 group (p=0.146). After 28 days, 14 patients died (46.7%) from the control group and 11
6 patients died (57.9%) from the intervention group, not reaching statistical significance
7 (p=0.444). At 90 days 19 patients had died (63.3%) from the control group and 11 patients
8 had died (57.9%) from the intervention group, which is to say no patient died from the
9 intervention group between 28 and 90 days (p=0.878). Adjusting by propensity score and
10 using the Kaplan-Meier technique (Figure 2), statistical significant difference was not
11 reached, neither in the ITT (Table 2) approach nor by the AIR approach (Table 3).
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25 In patients who died in the first three days, we found that the base-line levels of lactate were
26 higher compared with the rest (in mmol/L): 7.96 ± 4.79 vs 4.43 ± 2.41 , $p=0.015$. This situation
27 was similar in the APACHE score: 29.7 ± 5.1 vs 27.5 ± 5.5 , $p=0.194$, although this variable was
28 not significant.
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38 **DISCUSSION**

41 **Summary**

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44 Our results seem to indicate that the patient who received CPFA had less chance of mortality
45 in the long term (90 days), whether by ITT analysis or AIR analysis. However, in the short
46 and medium term during ITT analysis, CPFA had a detrimental effect and when using AIR
47 analysis the effect was protected. In any case, the statistical power to obtain conclusions from
48 these results was low.
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57 **Limitations of the study**

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3 This RCT was designed to determine medium and long-term differences between the CPFA
4 and the standard care. For this purpose, a sample size of 190 patients was pre-determined. In
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6 these partial results, the sample size of 190 was not reached and therefore the statistical
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8 power of the comparison contrast is very low (~ 30%). In addition, as the randomization
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10 process was done based on the baseline characteristics of the patient, this can produce
11
12 differences between the groups. Moreover, we can observe that the sample sizes of the two
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14 groups are not similar (the control group has approximately 50% more patients). All this has
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16 led to the use of propensity score adjustment in order to obtain results similar to an RCT
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18 (totally homogeneous groups, except in the intervention received). [9] However, even if we
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20 apply this technique we still have a low power of contrast. Despite this limitation, we want to
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22 communicate our partial results following the premature closure of the RCT COMPACT 2.
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29 **Comparison with the existing literature**

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32 We agreed with the Ethics Committee to review the incidence of early mortality in our trial
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34 on account of the findings communicated to us by the COMPACT 2 team. It should be
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36 emphasised that the ROMPA investigators were not given any impression of these
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38 COMPACT 2 findings during their own clinical practice. In any case, the analysis of what
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40 happened in the ROMPA sample collected up to that moment was carried out having a
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42 statistical power of only 30%, as expressed previously. Therefore, from a methodological
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44 point of view, it cannot have more value than the purely descriptive one. The results reported
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46 by the group of researchers of COMPACT 2 here deserve a special mention [8]. In these
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48 results, as occurred in our group, a preliminary analysis was developed that is far from the
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50 sample size initially calculated and therefore with low statistical power of contrast (not
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52 indicated by them in their report). In addition, as in our study, the COMPACT 2 group used a
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54 randomization system based on prognostic scores, [6] which means that the groups will not
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3 be similar until the end of recruitment (which is reason to introduce the propensity score in
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5 our results).
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9 In this situation, subgroup analysis has the problem of introducing analytic challenges and
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11 can lead to overstated and misleading results, [10] and as such we have to consider the
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13 remarkably low mortality of the control group, together with a remarkably high mortality of
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15 the intervention group. These results seem to be far removed from those that are obtained in
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17 usual clinical practice. This situation was not observed in the first COMPACT trial [5] and
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19 we have not heard of a retrospective analysis to explain these results.
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24 In our group, patients who died in the first 72 h had significantly higher initial lactate values
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26 than the rest of the patients included in the trial (7.96 ± 4.79 vs 4.43 ± 2.41 , $p=0.015$). Increased
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28 blood lactate in sepsis or trauma reflects anaerobic glycolysis due to hypoperfusion, and / or
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30 increased aerobic glycolysis.[11] In septic shock it mainly reflects hypoperfusion. At the
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32 present time, there is solid evidence about the predictive role of high lactate levels with
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34 respect to mortality in septic shock patients and our results are reflecting this.[12-18]
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39 Lastly, we would like to comment on the margin of time chosen by the Italian group to carry
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41 out its partial results. We think it is important to assess the patient's mortality, but this
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43 mortality should be assessed with a global calculation. In other words, for a technique to be
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45 effective, it must decrease the patient's mortality in a reasonable period of time in order to
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47 allow the healing of sepsis and its possible subsequent consequences. For this reason, the
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49 period of 28 and 90 days was fixed by our protocol. Consequently, for the sake of conducting
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51 an effective clinical trial, it is not of relevance that the patient unfortunately dies early, but
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53 whether the patient dies in a period of time where he has a high mortality risk due to sepsis.
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57 In addition, in the calculation of the sample size of the Italian group, this was not
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3 contemplated and could be a result of random error or heterogeneous groups in the treatment
4 allocation. At this point, it must be stated that the ROMPA investigators are not at any
5 moment criticising the COMPACT 2 decision to halt their trial. ROMPA's researchers
6 remain aware of the complexity of such a decision and that it involves multiple factors, the
7 most important being the security of the patients.
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15 **Implications to research**

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18 Our study was halted prematurely for the reasons we have previously explained when only 49
19 patients had been randomized (out of a target 190 patients). In the intervention arm, 19
20 patients were randomized and 30 patients randomized in the control arm. In both approaches
21 (ITT and real intervention), we have not found evidence of either benefit or harmful effect in
22 the tested treatment and, of course, this comes as no surprise due to the premature
23 termination.
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33 At this level of recruitment and with a power of 30% our sample is absolutely exposed to the
34 random effect, resulting in a lack of homogeneity in the levels of basal risk. This lack of
35 homogeneity pre-determines that the technique can be presented as either beneficial or
36 harmful. In fact the technique appears less beneficial in the subgroup (not predefined) of
37 patients who died in 72 hours and that, logically, were patients with an elevated basal risk,
38 primarily expressed through lactate levels and APACHE 2 score .
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48 We would like to comment on a controversial issue. Three patients who had been randomized
49 to the intervention group died in the first 72 hrs and did not receive the CPFA treatment. The
50 rapid hemodynamic deterioration of the patients did not allow the connection to the
51 extracorporeal circuit. It is obvious that in the ITT analysis these patients are considered to all
52 effects as belonging to the intervention group, assuming the great negative impact they will
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3 have on the efficacy analysis. It should always be borne in mind that these 3 patients,
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5 representing 20% of total intervention group who died early, did not actually receive
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7 treatment. Undoubtedly, adequate sample size management would minimize the problem. But
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9 if what we are considering is the possible harmful effect of the technique in a non pre-
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11 specified subgroup of an underpowered sample we cannot ignore this situation. It seems
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13 reasonable to think that if we talk about the possible harmful action of a device, the technique
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15 in question should have actually been applied.
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23 **CONCLUSIONS**

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26 In this paper we have presented the results of the 49 patients randomized in our trial up until
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28 the moment of closure. As a consequence of the procedure being underpowered, it was not
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30 possible to do an analysis of contrast of hypothesis and under this inconvenience, we present
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32 the results obtained for the interest of all concerned in knowing what has happened in our
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34 trial. When all is taken into consideration, we have not found a difference in mortality
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36 between the two groups.
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AUTHORS' CONTRIBUTIONS

CG drafted the paper of the protocol, CP helped draft the paper, and the rest of the authors critically reviewed the paper before sending it to *BMJ Open*.

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COMPETING INTERESTS STATEMENT

The authors declare no conflict of interest.

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3 **FIGURE LEGENDS:**
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6 Figure 1: Flow chart of the clinical trial (partial results).
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12 Figure 2: Survival analysis using the Kaplan-Meier estimator comparing both groups.
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15 Red, intervention; Blue, control.
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18 A, intention to treat; B, real intervention.
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TABLES:

Table 1: Comparison between the intervention and the control group.

Variable	Control group n=30 n(%) / x±s	CPFA n=19 n(%) / x±s	p-value
Gender male	18(60.0)	11(57.9)	0.884
Abdominal sepsis	12(40.0)	8(42.1)	0.884
Cancer	11(36.7)	7(36.8)	0.990
Community-acquired pneumonia	5(16.7)	3(15.8)	>0.999
Nosocomial pneumonia	3(10.0)	3(15.8)	0.665
Diabetes	9(30.0)	5(26.3)	0.781
Urinary sepsis	8(26.7)	3(15.8)	0.492
APACHE II	28.9±5.6	27.0±5.1	0.244
SOFA	12.8±3.3	12.2±4.4	0.541
SAPS II	74.5±20.9	70.7±21.0	0.587
Lactate (mmol/l)	5.3±3.4	5.9±4.1	0.580
Age (years)	70.0±13.6	71.0±14.5	0.812

Abbreviations: APACHE, Acute Physiology And Chronic Health Evaluation; CPFA, Coupled Plasma Filtration Adsorption; n(%), absolute frequency (relative frequency); SAPS, Simplified Acute Physiology Score; SOFA, Sequential Organ Failure Assessment; x±s, mean ± standard deviation.

Table 2: Clinical relevance of the intervention (intention-to-treat) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT/NNH*	p-value
3-day mortality	1.67(0.51-5.46)	-0.67(-4.46 to 0.49)	-0.17(-0.54 to 0.20)	6 (H)	0.667
28-day mortality	1.28(0.57-2.87)	-0.28(-1.87 to 0.43)	-0.13(-0.53 to 0.28)	8 (H)	0.537
90-day mortality	0.92(0.48-1.76)	0.08(-0.76 to 0.52)	0.05(-0.35 to 0.45)	19 (T)	>0.999

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; H, Harm; NNH, Number Needed to Harm; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction; T, Treat.

*, not possible to compute the confidence interval (division by zero).

Table 3: Clinical relevance of the intervention (real group) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT*	p-value
3-day mortality	0.84(0.26-2.73)	0.16(-1.73 to 0.74)	0.06(-0.32 to 0.44)	18	>0.999
28-day mortality	0.93(0.42-2.06)	0.07(-1.06 to 0.58)	0.04(-0.37 to 0.45)	26	>0.999
90-day mortality	0.72(0.35-1.48)	0.28(-0.48 to 0.65)	0.19(-0.21 to 0.59)	6	0.417

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction.

*, not possible to compute the confidence interval (division by zero).

Enrollment

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Assessed for eligibility (n=72)

Excluded (n=32)
◆ Not meeting inclusion criteria (n=13)
◆ Declined to participate (n=1)
◆ Other reasons (n=8)

Randomized (n=49)

Allocation

Allocated to intervention (n=19)
◆ Received allocated intervention (n=16)
◆ Did not receive allocated intervention (premature death) (n=3)

Allocated to intervention (n=30)
◆ Received allocated intervention (n=30)
◆ Did not receive allocated intervention (n=0)

Follow-Up

Lost to follow-up (give reasons) (n=0)
Discontinued intervention (give reasons) (n=0)

Lost to follow-up (give reasons) (n=0)
Discontinued intervention (give reasons) (n=0)

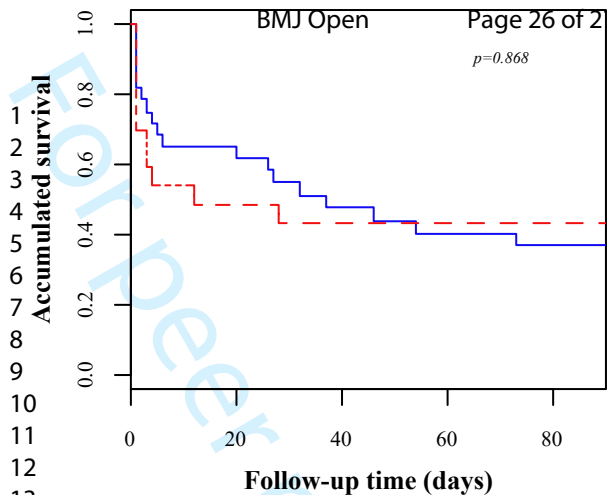
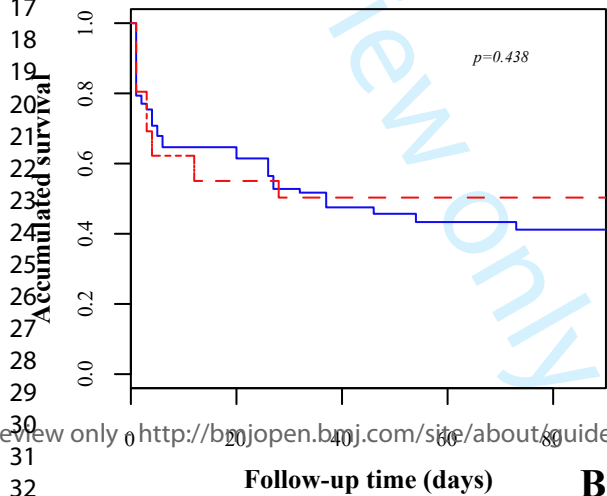
Analysis

Analysed (n=19)
◆ Excluded from analysis (n=0)

Analysed (n=30)
◆ Excluded from analysis (n=0)

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$p=0.868$  $p=0.438$ 



CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	3
Introduction			
Background and objectives	2a	Scientific background and explanation of rationale	6-7
	2b	Specific objectives or hypotheses	7
Methods			
Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	7-8
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	7-10
Participants	4a	Eligibility criteria for participants	7-8
	4b	Settings and locations where the data were collected	7-8
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	8-9
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	9
	6b	Any changes to trial outcomes after the trial commenced, with reasons	9
Sample size	7a	How sample size was determined	9
	7b	When applicable, explanation of any interim analyses and stopping guidelines	
Randomisation:			
Sequence generation	8a	Method used to generate the random allocation sequence	Protocol
	8b	Type of randomisation; details of any restriction (such as blocking and block size)	Protocol
Allocation concealment mechanism	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	Protocol
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	Protocol
Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those	Protocol

		assessing outcomes) and how	
	11b	If relevant, description of the similarity of interventions	N/A
Statistical methods	12a	Statistical methods used to compare groups for primary and secondary outcomes	9-10
	12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	9-10
Results			
Participant flow (a diagram is strongly recommended)	13a	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	10
	13b	For each group, losses and exclusions after randomisation, together with reasons	10
Recruitment	14a	Dates defining the periods of recruitment and follow-up	7-8
	14b	Why the trial ended or was stopped	6-7
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group	21
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	11
Outcomes and estimation	17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)	11
	17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	11
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	11
Harms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	11
Discussion			
Limitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	12
Generalisability	21	Generalisability (external validity, applicability) of the trial findings	14-15
Interpretation	22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	14-15
Other information			
Registration	23	Registration number and name of trial registry	4
Protocol	24	Where the full trial protocol can be accessed, if available	7
Funding	25	Sources of funding and other support (such as supply of drugs), role of funders	16

*We strongly recommend reading this statement in conjunction with the CONSORT 2010 Explanation and Elaboration for important clarifications on all the items. If relevant, we also recommend reading CONSORT extensions for cluster randomised trials, non-inferiority and equivalence trials, non-pharmacological treatments, herbal interventions, and pragmatic trials. Additional extensions are forthcoming: for those and for up to date references relevant to this checklist, see www.consort-statement.org.

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The premature closure of ROMPA Clinical Trial. Presentation of the results concerning the 49 randomized patients before the study closure.

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Primary Subject	Intensive care

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Heading:	
Secondary Subject Heading:	Intensive care, Infectious diseases
Keywords:	INFECTIOUS DISEASES, Adult intensive & critical care < INTENSIVE & CRITICAL CARE, Clinical trials < THERAPEUTICS



TITLE PAGE

Title: The premature closure of ROMPA Clinical Trial. Presentation of the results concerning the 49 randomized patients before the study closure.

Short title: The closure of ROMPA Clinical Trial.

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23 Keywords: Shock, Septic; Coupled Plasma Filtration Adsorption; Mortality; Intensive Care
24 Units; Clinical Trials.
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28 Word count: 2846.
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ABSTRACT

Objectives: Coupled Plasma Filtration and Adsorption (CPFA) use in septic shock remains controversial. The objective is to clarify whether the application of high dosis of CPFA in addition to the current clinical practice could reduce hospital mortality in septic shock patients in Intensive Care Units at 28 days and 90 days follow up.

Design: We designed a prospective randomized clinical trial, ROMPA (Reducción de la Mortalidad Plasma-Adsorción), to demonstrate an absolute mortality reduction of 20% [$\alpha=0.05$; $1-\beta=0.8$; $n=190(95 \times 2)$].

Setting: Being aware of the pitfalls associated with previous medical device trials, we developed a training program to improve CPFA use (especially clotting problems). The protocol was approved by the ethics committees of all participating centers. Circumstances beyond our control produced a change in recruitment conditions unacceptable to ROMPA researchers and the trial was discontinued.

Participants: By closure, 5 centres from an initial 10 fulfilled the necessary trial criteria, with 49 patients included, 30 control group (CG) and 19 intervention group (IG).

Intervention: CPFA.

Main outcome measures: Hospital mortality at 28 days and 90 days follow up

Results: After 28 days, 14 patients died (46.7%) from the CG and 11 (57.9%) from the IG, not reaching statistical significance ($p=0.444$). At 90 days 19 patients had died (63.3%) from the CG and 11 patients (57.9%) from the IG, ($p=0.878$). The adjustment by propensity score or the use of the Kaplan Meier technique failed to achieve statistical difference, neither in the Intention to Treat Approach nor by the Actual Intervention Received.

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3 Conclusion: We herewith present the results gained from the prematurely-closed trial. The
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5 results are inconclusive due to low statistical power but we consider that this data is of
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7 interest for the scientific community and potentially necessary for any ensuing debate.
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11 Register: NCT02357433 in clinicaltrials.gov
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STRENGTHS AND LIMITATIONS OF THIS STUDY

- Randomised control clinical trial testing the efficacy of CPFA in septic shock.
- Premature closure of the trial by circumstances beyond the trial.
- Scarce sample size: underpowered trial.

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INTRODUCTION

Sepsis is still a leading cause of mortality in Intensive Care Units (ICU) patients, with a 20-50% mortality rate of sepsis and septic shock [1]. There exists a feeling of frustration generated by the large series of negative randomized clinical trials (RCTs) in septic shock treatment (especially in targeting mortality) during the past 30 years [2]. Patients included in these trials have a wide variability in their probability of death translating into differences in benefits to be derived from specific therapy application, producing a handicap to sample size calculation. This can lead to the trial having less power than initially planned increasing risk of a type 2 error and undoubtedly this is the origin of unexpected results.[3] It questions the use of subgroups in an attempt to extract some kind of useful information in negative RCTs.[4]

The COMPACT 1, a multicentre RCT study, failed to show benefit by using Coupled Plasma Filtration and Adsorption (CPFA) therapy in a population with septic shock. In a per protocol analysis, patients treated with CPFA at treated plasma volume superior to 0.20 l/kg/day showed a reduction in mortality rate [5]. Although an interesting finding, our group considered it was necessary to carry out a RCT to confirm this hypothesis.

The response to this question was the ROMPA (Reducción de la Mortalidad Mediante Plasma-Adsorción en Shock séptico), a multicenter RCT carried out in ICUs of southeastern Spain. The ROMPA Study (NCT02357433 in clinicaltrials.gov) tried to clarify whether the application of high doses CPFA in addition to the current clinical practice was able to reduce hospital mortality in septic shock patients in ICUs. The protocol of ROMPA has been published in a free access support and the details of the protocol could be consulted without restrictions.[6]

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3 In October 2017, COMPACT 2 (NCT01639664 in clinical trials.gov) trial investigators, a
4 similar study to ROMPA conducted in Italian ICUs,[7] reported the premature closure of the
5 study for having detected an increase in early mortality (3 first days) in the intervention
6 branch, 6/42 (12.5%) vs 19/58 (32.8%) $p=0.020$, not having reached the sample size prefixed
7 in the protocol (350 patients). The adjusted odds ratio (OR) of the treatment yielded by
8 logistic regression is 2.1 (95% CI: 0.7-6.6, $p=0.19$) and the adjusted hazard ratio (HR)
9 yielded by the Cox model is 2.5 (95% CI: 1.4- 4.4, $p=0.002$). This information was
10 immediately reported to our Ethical Committee and these results were published on the
11 research group website in Italian.[8] Subsequent events, including a provisional warning by
12 the product supplier, motivated us to take the final decision of closing ROMPA. At that time,
13 of the 10 initial hospitals only 5 had exceeded the technical capacity requirements and
14 availability of resources required to access the randomization portal. In this paper and as a
15 result of events of such severity, our group shows the data collected to date and the results
16 from the 49 enrolled patients (30 control and 19 intervention groups).

39 **METHODS**

42 **Protocol**

45 The full study protocol was previously published.[6] A synthesis of it is made in this section
46 (Methods) .

51 **Setting and participants**

54 The study was performed in 5 ICUs, in the southeast of Spain, that follow the same protocol
55 in the treatment of septic shock, based on the recommendations of the Surviving Sepsis
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3 Campaign with the participation of the following centers: Vega Baja Hospital of Orihuela,
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5 General University Santa Lucía Hospital of Cartagena, University Hospital of San Juan de
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7 Alicante, Lluís Alcanyís Hospital of Xàtiva and Francesc de Borja Hospital of Gandía.
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11 The ROMPA study is a multi-centric, randomized, prospective, open clinical trial with 28-
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13 and 90-day follow-up and allocation ratio 1:1, assessing the mortality reduction by CPFA in
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15 patients with septic shock. Furthermore, we analyzed 3-day mortality to compare our results
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17 with the Italian group.
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21 Each center obtained technical proficiency with the machine and CPFA treatment before they
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23 could become “activated” for enrolment by the investigator monitoring team. This was done
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25 to avoid similar problems as those reported for the first COMPACT study (coagulation of the
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27 extracorporeal circuit, technical problems intimately linked to the management of a complex
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29 extracorporeal circuit, problems related to necessary logistic that require an extracorporeal
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31 technique such as CPFA as necessary and problems related to the need for specialized
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33 personnel),[4] and also because CPFA is not routinely done in Spain and a new machine with
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35 improved anticoagulation support was subsequently developed and used for this trial.
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40 **Participants**

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43 Patients ≥ 18 years old admitted to the ICU of the participant hospitals, with a diagnosis of
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45 septic shock can be included in the study. This was defined as documented or suspected
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47 infection with systemic manifestations of infection accompanied by signs of organ failure, or
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49 tissue hypoperfusion with persistent hypotension despite administration of adequate fluid
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51 resuscitation (at least 30ml/kg crystalloides) and in the absence of other causes of
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53 hypotension. The inclusion and exclusion criteria are detailed in the published protocol.[6]
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58 **Interventions**

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3 The patient is considered registered once the informed consent form has been obtained by the
4 patient or legal representative. The recruitment process ends with the patient randomization.
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7 The time between septic shock diagnosis and randomization was established in 12 hours,
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10 because this window adjusts much more to the reality of the clinical scenario, at least that of
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12 the hospitals that participated in the ROMPA study. The researchers of the COMPACT 2
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14 study reached the same conclusion.[7]
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18 Patients were divided randomly into two arms (control and intervention). ROMPA has a
19
20 stratified randomization based on gender, age (≤ 65 or >65 years) and SAPS III score (<50 or
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22 ≥ 51). On the one hand, in the control group we followed the suggestions provided by the
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24 recent surviving sepsis guidelines, as well as standard care guidelines typically followed in
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26 Spain. On the other hand, in the CPFA group, we applied the same protocol plus high doses
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28 of CPFA in the first 3 days after randomization.
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32 **Variables and measurements**

33 **Primary and Secondary Outcomes**

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36 The primary outcome variable is all-cause of mortality assessed at 3, 28 and 90 days from the
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38 recruitment of the patient. The analysis of 3-day mortality, although it was not initially pre-
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40 specified in the protocol, it was a recommendation of our Ethical Committee after knowing
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42 the data of the Italian group.[8]
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49 Moreover, at the descriptive level and in order to check homogeneity of both groups, the
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51 following variables will be collected at the time of recruitment: birth year, gender, height, dry
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53 weight, body temperature, heart rate, blood pressure, blood cell count, coagulation values,
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55 glucose level, plasma creatinine, bilirubinemia, plasma C reactive protein, procalcitonin
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3 level, blood gas analysis, lactate, urinary output (ml/kg/h), Pa O₂/FiO₂ ratio, APACHE II,
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5 SOFA and SAPSIII scores.
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8 **Sample Size**

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11 Originally, a sample size of 190 patients was calculated to determine differences in mortality
12 rates in both groups with a power-of-contrast of 80%. The assumed control mortality rate was
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14 50% and we tried to demonstrate a reduction in mortality of 20% with the intervention
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16 (similar to the COMPACT I results).[4] A partial analysis with the first 49 patients has been
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18 carried out as described in this paper. Using the data from the initial sample size calculation,
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20 these patients represent an approximate power-of-contrast of 30%.
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26 **Statistical analysis**

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29 Initially, the calculation of the indicators of clinical relevance (relative risk, RR, absolute risk
30 reduction, ARR, relative risk reduction, RRR, number needed to treat, NNT, by intent to
31 treat, ITT) was planned. Without having the sample size calculated for the study
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33 (intermediate analysis) and having made the allocation based on a set of variables, the
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35 homogeneity of the groups was not able to be established. To minimize this problem, the
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37 propensity scores as a population overlap weight technique was applied with the objective of
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39 overcoming the problem caused by the lack of homogeneity between the two groups.[9] The
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41 adjustment variables were APACHE II, previous lactate levels and the presence of urinary
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43 sepsis. Finally, although it was not established in the study protocol, Kaplan-Meier survival
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45 curves were analyzed to determine differences in mortality in the analyzed groups (log-rank
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47 test). Since a significant number of patients died in the first three days and were unable to
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49 receive the technique (n = 3, 15.8%) we decided to perform the analysis by actual
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51 intervention received (AIR). Although initially we did not plan this analysis
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3 (clinicaltrials.gov), the fact that one out of six patients did not receive the intervention could
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5 produce results completely different from its real effect.
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8 **Ethical issues**

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11 The study was originally approved by all Ethics Committees of the different Hospitals
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13 participating in the study. There was a general agreement that the trial closure was the best
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15 option, since the decision adopted by the Italian group to close its trial had been made public
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17 through its website and the supplier consequently marked their product used for the test with
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19 a warning. A Data and Safety Monitoring Board periodically review and evaluate the study
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21 data for the safety of the patients. It was formed by the Principal Investigator, the Senior
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23 Investigator and the Biostatistician of the project.
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28 **Patient and Public Involvement**

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31 Patients were not involved.
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38 **RESULTS**

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41 A total of 49 patients were included in the final analysis (30 in the control group and 19 in the
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43 intervention group) (Figure 1). The randomization tables are displayed in Table 1. Parametric
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45 statistics did not allow us to establish significant differences between the analyzed factors due
46
47 to the small sample size. However, we can see a mean difference between the two groups of
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49 1.9 on an APACHE II score, 0.6 mmol/l of lactate levels and 10.9% in the prevalence of
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51 urinary sepsis. All these factors have been used in the propensity score test.
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3 With regard to mortality (without adjusting by propensity score), 7 patients (23.3%) died in
4 the first three days from the control group and 8 patients (40.6%) died from the intervention
5 group (p=0.146). After 28 days, 14 patients died (46.7%) from the control group and 11
6 patients died (57.9%) from the intervention group, not reaching statistical significance
7 (p=0.444). At 90 days 19 patients had died (63.3%) from the control group and 11 patients
8 had died (57.9%) from the intervention group, which is to say no patient died from the
9 intervention group between 28 and 90 days (p=0.878). Adjusting by propensity score and
10 using the Kaplan-Meier technique (Figure 2), statistical significant difference was not
11 reached, neither in the ITT (Table 2) approach nor by the AIR approach (Table 3).
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25 In patients who died in the first three days, we found that the base-line levels of lactate were
26 higher compared with the rest (in mmol/L): 7.96 ± 4.79 vs 4.43 ± 2.41 , $p=0.015$. This situation
27 was similar in the APACHE score: 29.7 ± 5.1 vs 27.5 ± 5.5 , $p=0.194$, although this variable was
28 not significant.
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38 DISCUSSION

41 Summary

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44 Our results seem to indicate that the patient who received CPFA had less chance of mortality
45 in the long term (90 days), whether by ITT analysis or AIR analysis. However, in the short
46 and medium term during ITT analysis, CPFA had a detrimental effect and when using AIR
47 analysis the effect was protected. In any case, the statistical power to obtain conclusions from
48 these results was low and these were non-significant, consequently we are only describing the
49 estimation of the analyzed parameters (HR and proportions).
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Limitations of the study

This RCT was designed to determine medium and long-term differences between the CPFA and the standard care. For this purpose, a sample size of 190 patients was pre-determined. In these partial results, the sample size of 190 was not reached due to the cessation of the trial and therefore the statistical power of the comparison contrast is very low (~ 30%). As a consequence, the ARR of 20% is much too high and overly ambitious. This combined with the low sample size yields an extraordinarily low power. In addition, as the randomization process was done based on the baseline characteristics of the patient, this can produce differences between the groups. Moreover, we can observe that the sample sizes of the two groups are not similar (the control group has approximately 50% more patients). All this has led to the use of propensity score adjustment in order to obtain results similar to an RCT (totally homogeneous groups, except in the intervention received). [9] However, even if we apply this technique we still have a low power of contrast. Despite this limitation, we are obliged to communicate our partial results following the premature closure of the RCT COMPACT 2.

Comparison with the existing literature

We agreed with the Ethics Committee to review the incidence of early mortality in our trial on account of the findings communicated to us by the COMPACT 2 team. It should be emphasised that the ROMPA investigators were not given any impression of these COMPACT 2 findings during the ROMPA clinical trial. The results reported by the group of researchers of COMPACT 2 here deserve a special mention [8]. In these results, as occurred in our group, a preliminary analysis was developed that is far from the sample size initially calculated and therefore with low statistical power of contrast (not indicated by them in their report). In addition, as in our study, the COMPACT 2 group used a randomization system

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3 based on prognostic scores, [6] which means that the groups will not be similar until the end
4 of recruitment (which is reason to introduce the propensity score in our results).
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8 In this situation, subgroup analysis has the problem of introducing analytic challenges and
9 can lead to overstated and misleading results, [10] and as such we have to consider the
10 remarkably low mortality of the control group, together with a remarkably high mortality of
11 the intervention group. These results seem to be far removed from those that are obtained in
12 usual clinical practice. This situation was not observed in the first COMPACT trial [5] and
13 we have not heard of a retrospective analysis to explain these results.
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23 Lastly, we would like to comment on the margin of time chosen by the Italian group to carry
24 out its partial results. We think it is important to assess the patient's mortality, but this
25 mortality should be assessed with a global calculation. For a technique to be effective, it must
26 decrease the patient's mortality in a reasonable period of time in order to allow the healing of
27 sepsis and its possible subsequent consequences. For this reason, the period of 28 and 90 days
28 was fixed by our protocol. Consequently, for the sake of conducting an effective clinical trial,
29 it is not of relevance that the patient unfortunately dies early, but whether the patient dies in a
30 period of time where he has a high mortality risk due to sepsis. In addition, in the calculation
31 of the sample size of the Italian group, this was not contemplated and could be a result of
32 random error or heterogeneous groups in the treatment allocation. At this point, it must be
33 stated that the ROMPA investigators are not at any moment criticising the COMPACT 2
34 decision to halt their trial. ROMPA's researchers remain aware of the complexity of such a
35 decision and that it involves multiple factors, the most important being the security of the
36 patients.
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56 **Implications to research**

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3 Our study was halted prematurely for the reasons we have previously explained when only 49
4 patients had been randomized (out of a target 190 patients). In the intervention arm, 19
5 patients were randomized and 30 patients randomized in the control arm. In both approaches
6 (ITT and real intervention), we have not found evidence of either benefit or harmful effect in
7 the tested treatment and, of course, this comes as no surprise due to the premature
8 termination.

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18 At this level of recruitment and with a power of 30% our sample is absolutely exposed to the
19 random effect, resulting in a lack of homogeneity in the levels of basal risk. This lack of
20 homogeneity pre-determines that the technique can be presented as either beneficial or
21 harmful. In fact the technique appears less beneficial in the subgroup (not predefined) of
22 patients who died in 72 hours and that, logically, were patients with an elevated basal risk,
23 primarily expressed through lactate levels and APACHE 2 score.

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32 We would like to comment on a controversial issue. Three patients who had been randomized
33 to the intervention group (20%) died in the first 72 hrs and did not receive the CPFA
34 treatment. The rapid hemodynamic deterioration of the patients did not allow the connection
35 to the extracorporeal circuit. Undoubtedly adequate sample size management minimized this
36 problem, but if what we are considering is the possible harmful of the technique in a non-pre-
37 specified subgroup of an underpowered sample we cannot ignore this situation of the
38 technique not being applied.

39 40 41 42 43 44 45 46 47 48 49 50 51 52 **CONCLUSIONS**

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55 In this paper we have presented the results of the 49 patients randomized in our trial up until
56 the moment of closure. As a consequence of the procedure being underpowered, it was not

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3 possible to do an analysis of contrast of hypothesis and under this inconvenience, we present
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5 the results obtained for the interest of all concerned in knowing what has happened in our
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7 trial. When all is taken into consideration, we have not found a difference in mortality
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10 between the two groups.
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For peer review only

AUTHORS' CONTRIBUTIONS

CG designed the study and drafted the paper of the protocol; CP participated in the study design and helped draft the paper; FC, JMA, MG, CM, José LAP, EM, CD, MR, JLM, PJA, EMB, JC, EG, FS, MS, JT and VFG participated in the study design and reviewed critically the manuscript; AP participated in the study design, performed the statistical analysis and reviewed critically the manuscript. All the authors approved the final version of the text to be submitted for publication.

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COMPETING INTERESTS STATEMENT

The authors declare no conflict of interest.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from Francisco Colomina-Climent, but restrictions apply to the availability of these data, which were used under license for the current study, and so are not publicly available. Data are however available from the authors upon reasonable request and with permission of all the Ethics Committee which approved the study.

For peer review only

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3 **FIGURE LEGENDS:**
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6 Figure 1: Flow chart of the clinical trial (partial results).
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12 Figure 2: Survival analysis using the Kaplan-Meier estimator comparing both groups.
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15 Red, intervention; Blue, control.
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18 A, intention to treat; B, real intervention.
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TABLES:

Table 1: Comparison between the intervention and the control group.

Variable	Control group n=30 n(%) / x±s	CPFA n=19 n(%) / x±s	p-value
Gender male	18(60.0)	11(57.9)	0.884
Abdominal sepsis	12(40.0)	8(42.1)	0.884
Cancer	11(36.7)	7(36.8)	0.990
Community-acquired pneumonia	5(16.7)	3(15.8)	>0.999
Nosocomial pneumonia	3(10.0)	3(15.8)	0.665
Diabetes	9(30.0)	5(26.3)	0.781
Urinary sepsis	8(26.7)	3(15.8)	0.492
APACHE II	28.9±5.6	27.0±5.1	0.244
SOFA	12.8±3.3	12.2±4.4	0.541
SAPS II	74.5±20.9	70.7±21.0	0.587
Lactate (mmol/l)	5.3±3.4	5.9±4.1	0.580
Age (years)	70.0±13.6	71.0±14.5	0.812

Abbreviations: APACHE, Acute Physiology And Chronic Health Evaluation; CPFA, Coupled Plasma Filtration Adsorption; n(%), absolute frequency (relative frequency); SAPS, Simplified Acute Physiology Score; SOFA, Sequential Organ Failure Assessment; x±s, mean ± standard deviation.

Table 2: Clinical relevance of the intervention (intention-to-treat) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT/NNH*	p-value
3-day mortality	1.67(0.51-5.46)	-0.67(-4.46 to 0.49)	-0.17(-0.54 to 0.20)	6 (H)	0.667
28-day mortality	1.28(0.57-2.87)	-0.28(-1.87 to 0.43)	-0.13(-0.53 to 0.28)	8 (H)	0.537
90-day mortality	0.92(0.48-1.76)	0.08(-0.76 to 0.52)	0.05(-0.35 to 0.45)	19 (T)	>0.999

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; H, Harm; NNH, Number Needed to Harm; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction; T, Treat.

*, not possible to compute the confidence interval (division by zero).

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Table 3: Clinical relevance of the intervention (real group) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT*	p-value
3-day mortality	0.84(0.26-2.73)	0.16(-1.73 to 0.74)	0.06(-0.32 to 0.44)	18	>0.999
28-day mortality	0.93(0.42-2.06)	0.07(-1.06 to 0.58)	0.04(-0.37 to 0.45)	26	>0.999
90-day mortality	0.72(0.35-1.48)	0.28(-0.48 to 0.65)	0.19(-0.21 to 0.59)	6	0.417

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction.

*, not possible to compute the confidence interval (division by zero).

Enrollment

BMJ Open
Assessed for eligibility (n=72)

Excluded (n=23)

- ◆ Not meeting inclusion criteria (n=13)
- ◆ Declined to participate (n=1)
- ◆ Other reasons (n=9)

Randomized (n=49)

Allocation

15 Allocated to intervention (n=19)

16 Received allocated intervention (n=16)

17 Did not receive allocated intervention
18 (premature death) (n=3)

Allocated to intervention (n=30)

◆ Received allocated intervention (n=30)

◆ Did not receive allocated intervention (n=0)

Follow-Up

24 Lost to follow-up (give reasons) (n=0)

25 Discontinued intervention (give reasons) (n=0)

Lost to follow-up (give reasons) (n=0)

Discontinued intervention (give reasons) (n=0)

Analysis

32 Analysed (n=19)

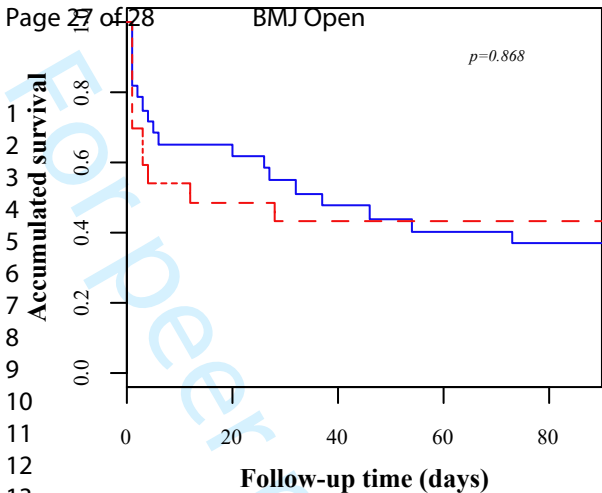
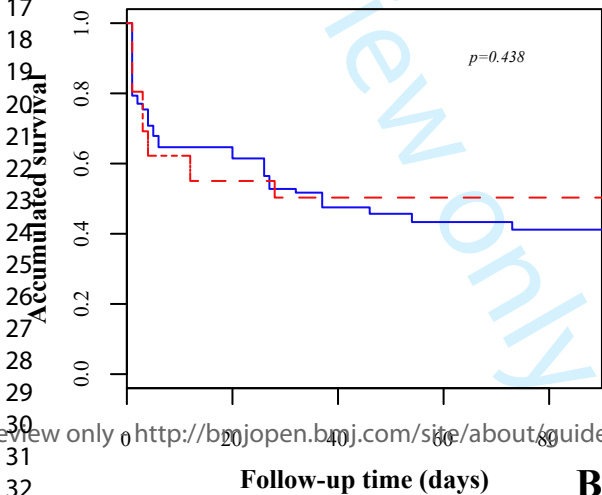
33 Excluded from analysis (n=0)

Analysed (n=30)

◆ Excluded from analysis (n=0)

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**A****B**



CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	3
Introduction			
Background and objectives	2a	Scientific background and explanation of rationale	6-7
	2b	Specific objectives or hypotheses	7
Methods			
Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	7-8
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	7-10
Participants	4a	Eligibility criteria for participants	7-8
	4b	Settings and locations where the data were collected	7-8
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	8-9
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	9-10
	6b	Any changes to trial outcomes after the trial commenced, with reasons	9-10
Sample size	7a	How sample size was determined	9-10
	7b	When applicable, explanation of any interim analyses and stopping guidelines	
Randomisation:			
Sequence generation	8a	Method used to generate the random allocation sequence	Protocol
	8b	Type of randomisation; details of any restriction (such as blocking and block size)	Protocol
Allocation concealment mechanism	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	Protocol
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	Protocol
Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those	Protocol

1		assessing outcomes) and how	
2		11b If relevant, description of the similarity of interventions	N/A
3	Statistical methods	12a Statistical methods used to compare groups for primary and secondary outcomes	10-11
4		12b Methods for additional analyses, such as subgroup analyses and adjusted analyses	10-11
5			
6	Results		
7	Participant flow (a	13a For each group, the numbers of participants who were randomly assigned, received intended treatment, and	11
8	diagram is strongly	were analysed for the primary outcome	
9	recommended)	13b For each group, losses and exclusions after randomisation, together with reasons	11
10	Recruitment	14a Dates defining the periods of recruitment and follow-up	7-8
11		14b Why the trial ended or was stopped	6-7
12	Baseline data	15 A table showing baseline demographic and clinical characteristics for each group	21
13	Numbers analysed	16 For each group, number of participants (denominator) included in each analysis and whether the analysis was	11
14		by original assigned groups	
15	Outcomes and	17a For each primary and secondary outcome, results for each group, and the estimated effect size and its	12
16	estimation	precision (such as 95% confidence interval)	
17		17b For binary outcomes, presentation of both absolute and relative effect sizes is recommended	12
18	Ancillary analyses	18 Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing	12
19		pre-specified from exploratory	
20	Harms	19 All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	12
21			
22	Discussion		
23	Limitations	20 Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	13
24	Generalisability	21 Generalisability (external validity, applicability) of the trial findings	13-16
25	Interpretation	22 Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	13-16
26			
27	Other information		
28	Registration	23 Registration number and name of trial registry	4
29	Protocol	24 Where the full trial protocol can be accessed, if available	7
30	Funding	25 Sources of funding and other support (such as supply of drugs), role of funders	17

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37 *We strongly recommend reading this statement in conjunction with the CONSORT 2010 Explanation and Elaboration for important clarifications on all the items. If relevant, we also
38 recommend reading CONSORT extensions for cluster randomised trials, non-inferiority and equivalence trials, non-pharmacological treatments, herbal interventions, and pragmatic trials.
39 Additional extensions are forthcoming: for those and for up to date references relevant to this checklist, see www.consort-statement.org.
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BMJ Open

The premature closure of ROMPA Clinical Trial: Mortality Reduction in Septic Shock by Plasma Adsorption.

Journal:	<i>BMJ Open</i>
Manuscript ID	bmjopen-2019-030139.R2
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Date Submitted by the Author:	17-Oct-2019
Complete List of Authors:	Giménez-Esparza, Carola; Vega Baja Hospital of Orihuela, Intensive Care Unit Portillo-Requena, Cristina; Vega Baja Hospital of Orihuela, Intensive Care Unit Colomina-Climent, Francisco; Miguel Hernández University, Clinical Medicine Allegue-Gallego, José; General University Santa Lucía Hospital of Cartagena, Intensive Care Unit Galindo-Martínez, María; General University Santa Lucía Hospital of Cartagena, Intensive Care Unit Mollà-Jiménez, Cristina; University Hospital of San Juan de Alicante, Intensive Care Unit Antón-Pascual, José; University Hospital of San Juan de Alicante, Intensive Care Unit Mármol-Peis, Enrique; University Hospital of San Juan de Alicante, Intensive Care Unit Dólera-Moreno, Cristina; University Hospital of San Juan de Alicante, Intensive Care Unit Rodríguez-Serra, Manuel; Lluís Alcanyís Hospital of Xàtiva, Intensive Care Unit Martín-Ruíz, José; Lluís Alcanyís Hospital of Xàtiva, Intensive Care Unit; Fernández-Arroyo, Pablo; Marina Baixa Hospital of Villajoyosa, Intensive Care Unit Blasco-Císcar, Eugenia; Marina Baixa Hospital of Villajoyosa, Intensive Care Unit Cánovas-Robles, José; General University Hospital of Alicante, Intensive Care Unit González-Hernández, Enrique; La Plana Hospital of Villarreal, Intensive Care Unit Sánchez-Morán, Fernando; La Plana Hospital of Villarreal, Intensive Care Unit Solera-Suárez, Manuel; Francesc de Borja Hospital of Gandía, Intensive Care Unit Torres-Tortajada, Jesús; Francesc de Borja Hospital of Gandía, Intensive Care Unit Palazón-Bru, Antonio; Miguel Hernández University, Clinical Medicine Gil-Guillen, Vicente F.; Miguel Hernández University, Clinical Medicine
Primary Subject Heading:	Intensive care

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TITLE PAGE

Title: The premature closure of ROMPA Clinical Trial: Mortality Reduction in Septic Shock by Plasma Adsorption.

Short title: The closure of ROMPA Clinical Trial.

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24 Units; Clinical Trials.
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ABSTRACT

Objectives: Coupled Plasma Filtration and Adsorption (CPFA) use in septic shock remains controversial. The objective is to clarify whether the application of high doses of CPFA in addition to the current clinical practice could reduce hospital mortality in septic shock patients in Intensive Care Units at 28 days and at 90 days follow-up.

Design: We designed a prospective randomized clinical trial, ROMPA (Reducción de la Mortalidad Plasma-Adsorción), to demonstrate an absolute mortality reduction of 20% [$\alpha=0.05$; $1-\beta=0.8$; $n=190(95 \times 2)$].

Setting: Being aware of the pitfalls associated with previous medical device trials, we developed a training program to improve CPFA use (especially clotting problems). The protocol was approved by the ethics committees of all participating centers. Circumstances beyond our control produced a change in recruitment conditions unacceptable to ROMPA researchers and the trial was discontinued.

Participants: By closure, 5 centers from an initial 10 fulfilled the necessary trial criteria, with 49 patients included, 30 in the control group (CG) and 19 in the intervention group (IG).

Intervention: CPFA.

Main outcome measures: Hospital mortality at 28 days and 90 days follow-up.

Results: After 28 days, 14 patients died (46.7%) from the CG and 11 (57.9%) from the IG, not reaching statistical significance ($p=0.444$). At 90 days, 19 patients had died (63.3%) from the CG and 11 patients (57.9%) from the IG, ($p=0.878$). The adjustment by propensity score or the use of the Kaplan Meier technique failed to achieve statistical difference, neither by Intention to Treat nor by the Actual Intervention Received.

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3 Conclusion: We herewith present the results gained from the prematurely closed trial. The
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5 results are inconclusive due to low statistical power but we consider that this data is of
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7 interest for the scientific community and potentially necessary for any ensuing debate.
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11 Register: NCT02357433 in clinicaltrials.gov
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STRENGTHS AND LIMITATIONS OF THIS STUDY

- Randomized control clinical trial testing the efficacy of CPFA in septic shock.
- Premature closure due to circumstances beyond the control of the trial.
- Scarce sample size: underpowered trial.

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INTRODUCTION

Sepsis is still a leading cause of mortality in Intensive Care Unit (ICU) patients, with a 20-50% mortality rate from sepsis and septic shock [1]. There has been a feeling of frustration generated by the large amount of negative randomized clinical trials (RCTs) in septic shock treatment (especially in those targeting mortality) over the past 30 years [2]. Patients included in these trials vary widely in their probability of death. This translates into differences in the benefits derived from specific therapy application, which in turn handicaps sample size calculation. This can lead to the trial having less statistical power than initially planned, increasing the risk of a type 2 error and this is the origin of unexpected results. [3] It questions the use of subgroups in an attempt to extract some kind of useful information in negative RCTs. [4]

The COMPACT 1, a multicenter RCT study, failed to show any benefits when using Coupled Plasma Filtration and Adsorption (CPFA) therapy in a population with septic shock. In a per-protocol analysis, patients treated with CPFA using a volume of treated plasma superior to 0.20 l/kg/day demonstrated a reduction in mortality rate [5]. Although an interesting result, our group considered it was necessary to carry out a RCT to confirm this hypothesis.

The response to this question was ROMPA (Reducción de la Mortalidad Mediante Plasma-Adsorción en Shock séptico), a multicenter RCT carried out in ICUs of southeastern Spain. The ROMPA Study (NCT02357433 in clinicaltrials.gov) tried to clarify whether the application of high doses of CPFA in addition to the current clinical practice was able to reduce hospital mortality in septic shock patients in ICUs. The protocol of ROMPA has been published in a free access journal and the details of the protocol can be consulted without restrictions. [6]

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3 In October 2017, the trial investigators of COMPACT 2 (NCT01639664 in clinicaltrials.gov),
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5 a similar study to ROMPA conducted in Italian ICUs [7], reported the premature closure of
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7 the study for having detected an increase in early mortality (3 first days) in the intervention
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9 branch (6/42 (12.5%) vs 19/58 (32.8%) $p=0.020$, not reaching the 350 patient sample size
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11 prefixed in the protocol). The adjusted odds ratio (OR) of the treatment yielded by logistic
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13 regression is 2.1 (95% CI: 0.7-6.6, $p=0.19$) and the adjusted hazard ratio (HR) yielded by the
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15 Cox model is 2.5 (95% CI: 1.4- 4.4, $p=0.002$). This information was immediately reported to
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17 our Ethical Committee and these results were published on the research group website in
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19 Italian [8]. Subsequent events, including a provisional warning by the product supplier, led us
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21 to close ROMPA. At that time, of the 10 initial hospitals, only 5 had exceeded the technical
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23 capacity requirements and availability of resources required to access the randomization
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25 portal. Due to the severity of the events, our group wishes to show the data collected so far
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27 and the results from the 49 enrolled patients (30 control and 19 intervention groups).
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37 **METHODS**

38 **Protocol**

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43 The full study protocol has been published previously. [6] In this section, a synthesis of the
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45 protocol is given (Methods).
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48 **Setting and participants**

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52 The study was performed in 5 ICUs in the southeast of Spain, all following the same protocol
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54 in the treatment of septic shock, which is based on the recommendations of the Surviving
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56 Sepsis Campaign. The following centers participated: Vega Baja Hospital of Orihuela,
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3 General University Santa Lucía Hospital of Cartagena, University Hospital of San Juan de
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5 Alicante, Lluís Alcanyís Hospital of Xàtiva and Francesc de Borja Hospital of Gandía.
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8 The ROMPA study is a multi-center, randomized, prospective, open clinical trial with 28-
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10 and 90-day follow-up and allocation ratio 1:1, assessing the mortality reduction by CPFA in
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12 patients with septic shock. Furthermore, we analyzed 3-day mortality to compare our results
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14 with the Italian group.
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18 Each center obtained technical proficiency with the machine and CPFA treatment before they
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20 could become “activated” for enrolment by the investigator-monitoring team. This was done,
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22 firstly, in order to avoid similar problems as those reported in the first COMPACT study
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24 (coagulation of the extracorporeal circuit; technical problems intimately linked to the
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26 management of a complex extracorporeal circuit; logistical problems which required an
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28 extracorporeal technique such as CPFA; problems related to the need for specialized
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30 personnel). [4] Secondly, as CPFA is not routinely administered in Spain, a new machine
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32 with improved anticoagulation support was developed and used for this trial.
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38 **Participants**

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40 Patients ≥ 18 years-old with a diagnosis of septic shock and admitted to the ICU of the
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42 participant hospitals are eligible to be included in the study. Diagnosis of septic shock was
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44 defined as documented or suspected infection with systemic manifestations of infection
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46 accompanied by signs of organ failure, or tissue hypoperfusion with persistent hypotension
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48 despite administration of adequate fluid resuscitation (at least 30ml/kg crystalloides) and in
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50 the absence of other causes of hypotension. The inclusion and exclusion criteria are detailed
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52 in the published protocol. [6]
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58 **Interventions**

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3 The patient is registered once the informed consent form has been obtained by the patient or
4 legal representative. The recruitment process ends with the patient randomization. The time
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6 between septic shock diagnosis and randomization was established in 12 hours, because this
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8 window adjusts much more to the reality of the clinical situation, at least that of the hospitals
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10 that participated in the ROMPA study. The researchers of the COMPACT 2 study reached
11
12 the same conclusion. [7]
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17 Patients were divided randomly into two arms (control and intervention). ROMPA has a
18 stratified randomization based on gender, age (≤ 65 or >65 years) and SAPS III score (<50 or
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20 ≥ 51). On the one hand, in the control group we followed the suggestions provided by the
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22 recent surviving sepsis guidelines, as well as standard care guidelines typically followed in
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24 Spain. On the other hand, in the CPFA group, we applied the same protocol plus high doses
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26 of CPFA in the first 3 days after randomization.
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32 **Variables and measurements**

33 **Primary and Secondary Outcomes**

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36 The primary outcome variable is all-cause mortality assessed at 28 and 90 days after the
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38 recruitment of the patient. The analysis of 3-day mortality, although not initially specified in
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40 the protocol, was an added recommendation by our Ethical Committee once the data of the
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42 Italian group had become known. [8]
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49 Moreover, at the descriptive level and in order to check homogeneity of both groups, the
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51 following variables will be collected at the time of recruitment: birth year, gender, height, dry
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53 weight, body temperature, heart rate, blood pressure, blood cell count, coagulation values,
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55 glucose level, plasma creatinine, bilirubinemia, plasma C reactive protein, procalcitonin
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3 level, blood gas analysis, lactate, urinary output (ml/kg/h), Pa O₂/FiO₂ ratio, APACHE II,
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5 SOFA and SAPSIII scores.
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8 **Sample Size**

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11 Originally, a sample size of 190 patients was calculated to determine differences in mortality
12 rates in both groups with a power-of-contrast of 80%. The assumed control group mortality
13 rate was 50% and we tried to demonstrate a reduction in mortality of 20% in the intervention
14 group (similar to the COMPACT I results). [4] A partial analysis with the first 49 patients has
15 been carried out as described in this paper. Using the data from the initial sample size
16 calculation, these patients represent an approximate power-of-contrast of 30%.
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26 **Statistical analysis**

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29 Initially, the calculation of the indicators of clinical relevance (relative risk, RR, absolute risk
30 reduction, ARR, relative risk reduction, RRR, number needed to treat, NNT, by intent to
31 treat, ITT) was planned. Without having the sample size calculated for the study
32 (intermediate analysis) and having made the allocation based on a set of variables, the
33 homogeneity of the groups could not be established. The propensity scores as a population
34 overlap weight technique was applied with the objective of overcoming the problem caused
35 by the lack of homogeneity between the two groups.[9] The adjustment variables were
36 APACHE II, previous lactate levels and the presence of urinary sepsis. Finally, although not
37 established in the study protocol, Kaplan-Meier survival curves were analyzed to determine
38 differences in mortality in the analyzed groups (log-rank test). Since a significant number of
39 patients died in the first three days and were unable to receive the technique (n = 3, 15.8%)
40 we decided to perform the analysis by actual intervention received (AIR). Although **this**
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3 analysis was not initially planned (clinicaltrials.gov), the fact that one out of six patients did
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5 not receive the intervention necessitated it.
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8 **Ethical issues**

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11 The study was originally approved by all Ethics Committees of the Hospitals participating in
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13 the study. There was a general agreement that the trial closure was the best option, since the
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15 decision adopted by the Italian group to close its trial had been made public through its
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17 website resulting in the device supplier marking their product used for the test with a
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19 warning. A Data and Safety Monitoring Board was set up to periodically review and evaluate
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21 the study data for the safety of the patients. It was formed by the Principal Investigator, the
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23 Senior Investigator and the Biostatistician of the project.
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28 **Patient and Public Involvement**

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31 This research was done without patient involvement. Patients were not invited to comment
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33 on the study design and were not consulted to develop patient relevant outcomes or interpret
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35 the results. Patients were not invited to contribute to the writing or editing of this document
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37 for readability or accuracy.
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45 **RESULTS**

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48 A total of 49 patients were included in the final analysis (30 in the control group and 19 in the
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50 intervention group) (Figure 1). The randomization tables are displayed in Table 1. Parametric
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52 statistics did not allow us to establish significant differences between the analyzed factors due
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54 to the small sample size. However, we can see a mean difference between the two groups
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56 based on three variables: 1.9 on an APACHE II score, 0.6 mmol/l of lactate levels and 10.9%
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3 in the prevalence of urinary sepsis. All these factors have been used in the propensity score
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5 test.
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3 With regard to mortality (without adjusting by propensity score), 7 patients (23.3%) had died
4 in the first three days from the control group and 8 patients (40.6%) had died from the
5 intervention group ($p=0.146$). After 28 days, 14 patients had died (46.7%) from the control
6 group and 11 patients had died (57.9%) from the intervention group, not reaching statistical
7 significance ($p=0.444$). At 90 days 19 patients had died (63.3%) from the control group and
8 11 patients had died (57.9%) from the intervention group, which is to say no patient died
9 from the intervention group between 28 and 90 days ($p=0.878$). Adjusting by propensity
10 score and using the Kaplan-Meier technique (Figure 2), statistical significant difference was
11 not reached, neither by ITT (Table 2) nor by the AIR. (Table 3).
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25 In patients who died in the first three days, we found that the base-line levels of lactate were
26 higher compared with the rest of the patients (in mmol/L): 7.96 ± 4.79 vs 4.43 ± 2.41 , $p=0.015$.
27 A similar situation was revealed in the APACHE score: 29.7 ± 5.1 vs 27.5 ± 5.5 , $p=0.194$,
28 although this variable was not significant.
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38 DISCUSSION

41 Summary

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44 Our results seem to indicate that the patients who received CPFA had less chance of mortality
45 in the long term (90 days), whether by ITT analysis or AIR analysis. However, in the short
46 and medium term during ITT analysis, CPFA had a detrimental effect and when using AIR
47 analysis the effect was protective. In any case, the statistical power to obtain conclusions
48 from these results was low and therefore non-significant. As a consequence, we are only
49 describing the estimation of the analyzed parameters (HR and proportions).
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Limitations of the study

This RCT was designed to determine medium and long-term differences between CPFA and standard care. For this purpose, a sample size of 190 patients was pre-determined. In these partial results, the sample size of 190 was not reached due to the cessation of the trial and therefore the statistical power of the comparison contrast is very low (~ 30%). As a consequence, the ARR of 20% is much too high and unrealistic. Combined with the low sample size, this yields a very low statistical power. In addition, as the randomization process was undertaken based on the baseline characteristics of the patient, this can produce differences between the groups. Moreover, we can observe that the sample sizes of the two groups are not similar (the control group has approximately 50% more patients). All this has led to the use of propensity score adjustment in order to obtain results similar to an RCT (homogeneous groups, except in the intervention received). [9] However, even if we apply this technique we still have a low power of contrast. Despite this limitation, we are obliged to communicate our partial results following the premature closure of the RCT COMPACT 2.

Comparison with the existing literature

We agreed with the Ethics Committee to review the incidence of early mortality in our trial after the findings communicated to us by the COMPACT 2 team. It should be emphasized that the ROMPA investigators were not given any impression of these COMPACT 2 findings during the ROMPA clinical trial. The results reported by the group of researchers of COMPACT 2 here deserve a special mention [8]. In these results, as occurred in our group, a preliminary analysis was developed that is far from the sample size initially calculated and therefore with low statistical power of contrast (not indicated by them in their report). In addition, as in our study, the COMPACT 2 group used a randomization system based on

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3 prognostic scores, [6] which means that the groups will not be similar until the end of
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5 recruitment (which is reason to introduce the propensity score in our results).
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9 In this situation, subgroup analysis has the problem of introducing analytic challenges and
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11 can lead to overstated and misleading results, [10] and, as such, we have to consider the
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13 remarkably low mortality of the control group, together with a remarkably high mortality of
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15 the intervention group. These results seem to be far removed from those that are obtained in
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17 usual clinical practice. This situation was not observed in the first COMPACT trial [5] and
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19 we have not heard of a retrospective analysis to explain these results.
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24 Lastly, we would like to comment on the margin of time chosen by the Italian group to carry
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26 out its partial results. We think it is important to assess the patient's mortality, but this
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28 mortality should be assessed with a global calculation. For a technique to be effective, it must
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30 decrease the patient's mortality in a reasonable period of time in order to allow the healing of
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32 sepsis and its possible consequences. For this reason, the periods of 28 days and 90 days were
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34 fixed by our protocol. Consequently, for the sake of conducting an effective clinical trial, it is
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36 not of relevance that the patient dies early, but whether the patient dies in a period of time
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38 where he has a high mortality risk due to sepsis. Additionally, in the calculation of the sample
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40 size of the Italian group, mortality at three days was not contemplated and could be a result of
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42 either random error or heterogeneous groups in the treatment allocation. At this point, it must
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44 be stated that the ROMPA investigators are not at any moment criticizing the COMPACT 2
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46 decision to halt their trial. ROMPA's researchers remain aware of the complexity of such a
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48 decision and that it involves multiple factors, the most important being the security of the
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50 patients.
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55 56 **Implications to research** 57 58 59 60

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3 Our study was halted prematurely for the reasons we have previously explained when only 49
4 patients had been randomized (out of a target 190 patients). In the intervention arm, 19
5 patients were randomized and 30 patients in the control arm. In both approaches (ITT and
6 real intervention), we have not found evidence of either benefit or harmful effect in the tested
7 treatment and, of course, this comes as no surprise due to the premature termination.
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12 At this level of recruitment and with a statistical power of 30%, our sample is exposed to the
13 random effect, resulting in a lack of homogeneity in the levels of basal risk. This lack of
14 homogeneity pre-determines that the technique can be presented as either beneficial or
15 harmful. In fact the technique appears less beneficial in the subgroup (not predefined) of
16 patients who died in 72 hours and these were patients with an elevated basal risk, primarily
17 expressed through lactate levels and APACHE 2 score.
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22 We would like to comment on a controversial issue. Three patients who had been randomized
23 to the intervention group (20%) died in the first 72 hours and did not receive the CPFA
24 treatment. The rapid hemodynamic deterioration of the patients did not allow the connection
25 to the extracorporeal circuit. Undoubtedly, adequate sample size management minimized this
26 problem, but if what we are considering is the potential harmfulness of the technique in a
27 non-pre-specified subgroup of an underpowered sample size, we cannot ignore the fact that
28 the technique was not applied.
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50 **CONCLUSIONS**

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53 In this paper we have presented the results of the 49 patients randomized in our trial up until
54 the moment of closure. As a consequence of the procedure being underpowered, it was not
55 possible to do an analysis of contrast of hypothesis and under this inconvenience, we present
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3 the results obtained for the interest of all who are concerned about what happened in our trial.
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5 When all is taken into consideration, we have not found a difference in mortality between the
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8 two groups.
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AUTHORS' CONTRIBUTIONS

CG designed the study and drafted the paper of the protocol; CP participated in the study design and helped draft the paper; FC, JMA, MG, CM, José LAP, EM, CD, MR, JLM, PJA, EMB, JC, EG, FS, MS, JT and VFG participated in the study design and reviewed critically the manuscript; AP participated in the study design, performed the statistical analysis and reviewed critically the manuscript. All the authors approved the final version of the text to be submitted for publication.

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COMPETING INTERESTS STATEMENT

The authors declare no conflict of interest.

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DATA AVAILABILITY STATEMENT

The complete data that supports the findings of this study is available from Francisco Colomina-Climent. Upon reasonable request and with the permission of all the Ethics Committees who approved the study, this data may be freely accessed by the authors.

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3 **FIGURE LEGENDS:**
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6 Figure 1: Flow chart of the clinical trial (partial results).
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12 Figure 2: Survival analysis using the Kaplan-Meier estimator comparing both groups.
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15 Red, intervention; Blue, control.
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18 A, intention to treat; B, real intervention.
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TABLES:

Table 1: Comparison between the intervention and the control group.

Variable	Control group n=30 n(%) / x±s	CPFA n=19 n(%) / x±s	p-value
Gender male	18(60.0)	11(57.9)	0.884
Abdominal sepsis	12(40.0)	8(42.1)	0.884
Cancer	11(36.7)	7(36.8)	0.990
Community-acquired pneumonia	5(16.7)	3(15.8)	>0.999
Nosocomial pneumonia	3(10.0)	3(15.8)	0.665
Diabetes	9(30.0)	5(26.3)	0.781
Urinary sepsis	8(26.7)	3(15.8)	0.492
APACHE II	28.9±5.6	27.0±5.1	0.244
SOFA	12.8±3.3	12.2±4.4	0.541
SAPS II	74.5±20.9	70.7±21.0	0.587
Lactate (mmol/l)	5.3±3.4	5.9±4.1	0.580
Age (years)	70.0±13.6	71.0±14.5	0.812

Abbreviations: APACHE, Acute Physiology And Chronic Health Evaluation; CPFA, Coupled Plasma Filtration Adsorption; n(%), absolute frequency (relative frequency); SAPS, Simplified Acute Physiology Score; SOFA, Sequential Organ Failure Assessment; x±s, mean ± standard deviation.

Table 2: Clinical relevance of the intervention (intention-to-treat) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT/NNH*	p-value
3-day mortality	1.67(0.51-5.46)	-0.67(-4.46 to 0.49)	-0.17(-0.54 to 0.20)	6 (H)	0.667
28-day mortality	1.28(0.57-2.87)	-0.28(-1.87 to 0.43)	-0.13(-0.53 to 0.28)	8 (H)	0.537
90-day mortality	0.92(0.48-1.76)	0.08(-0.76 to 0.52)	0.05(-0.35 to 0.45)	19 (T)	>0.999

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; H, Harm; NNH, Number Needed to Harm; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction; T, Treat.

*, not possible to compute the confidence interval (division by zero).

Table 3: Clinical relevance of the intervention (real group) in the patients with septic shock (adjusted by propensity scores as a population overlap weight).

Outcome	RR (95% CI)	RRR (95% CI)	ARR (95% CI)	NNT*	p-value
3-day mortality	0.84(0.26-2.73)	0.16(-1.73 to 0.74)	0.06(-0.32 to 0.44)	18	>0.999
28-day mortality	0.93(0.42-2.06)	0.07(-1.06 to 0.58)	0.04(-0.37 to 0.45)	26	>0.999
90-day mortality	0.72(0.35-1.48)	0.28(-0.48 to 0.65)	0.19(-0.21 to 0.59)	6	0.417

Abbreviations: ARR, Absolute Risk Reduction; CI, confidence interval; NNT, Number Needed to Treat; RR, relative risk; RRR, Relative Risk Reduction.

*, not possible to compute the confidence interval (division by zero).

Enrollment

BMJ Open
Assessed for eligibility (n=72)

Excluded (n=23)
◆ Not meeting inclusion criteria (n=13)
◆ Declined to participate (n=1)
◆ Other reasons (n=9)

Randomized (n=49)

Allocation

Allocated to intervention (n=19)
◆ Received allocated intervention (n=16)
◆ Did not receive allocated intervention (premature death) (n=3)

Allocated to intervention (n=30)
◆ Received allocated intervention (n=30)
◆ Did not receive allocated intervention (n=0)

Follow-Up

Lost to follow-up (give reasons) (n=0)
Discontinued intervention (give reasons) (n=0)

Lost to follow-up (give reasons) (n=0)
Discontinued intervention (give reasons) (n=0)

Analysis

Analysed (n=19)
◆ Excluded from analysis (n=0)

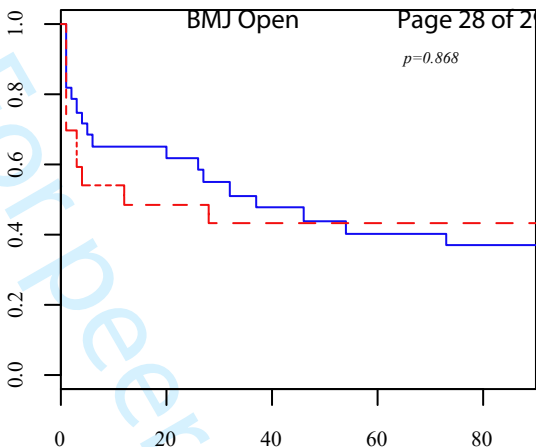
Analysed (n=30)
◆ Excluded from analysis (n=0)

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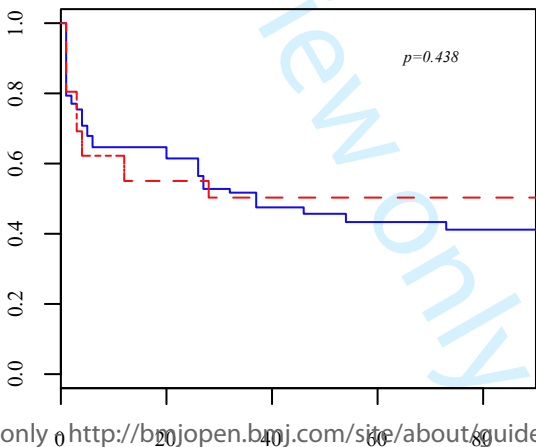
Accumulated survival

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Follow-up time (days)

A

Accumulated survival

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Follow-up time (days)

B



CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	3
Introduction			
Background and objectives	2a	Scientific background and explanation of rationale	6-7
	2b	Specific objectives or hypotheses	7
Methods			
Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	7-8
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	7-10
Participants	4a	Eligibility criteria for participants	7-8
	4b	Settings and locations where the data were collected	7-8
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	8-9
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	9-10
	6b	Any changes to trial outcomes after the trial commenced, with reasons	9-10
Sample size	7a	How sample size was determined	9-10
	7b	When applicable, explanation of any interim analyses and stopping guidelines	
Randomisation:			
Sequence generation	8a	Method used to generate the random allocation sequence	Protocol
	8b	Type of randomisation; details of any restriction (such as blocking and block size)	Protocol
Allocation concealment mechanism	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	Protocol
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	Protocol
Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those	Protocol

		assessing outcomes) and how	
	11b	If relevant, description of the similarity of interventions	N/A
Statistical methods	12a	Statistical methods used to compare groups for primary and secondary outcomes	10-11
	12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	10-11
Results			
Participant flow (a diagram is strongly recommended)	13a	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	11
	13b	For each group, losses and exclusions after randomisation, together with reasons	11
Recruitment	14a	Dates defining the periods of recruitment and follow-up	7-8
	14b	Why the trial ended or was stopped	6-7
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group	21
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	11
Outcomes and estimation	17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)	12
	17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	12
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	12
Harms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	12
Discussion			
Limitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	13
Generalisability	21	Generalisability (external validity, applicability) of the trial findings	13-16
Interpretation	22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	13-16
Other information			
Registration	23	Registration number and name of trial registry	4
Protocol	24	Where the full trial protocol can be accessed, if available	7
Funding	25	Sources of funding and other support (such as supply of drugs), role of funders	17

*We strongly recommend reading this statement in conjunction with the CONSORT 2010 Explanation and Elaboration for important clarifications on all the items. If relevant, we also recommend reading CONSORT extensions for cluster randomised trials, non-inferiority and equivalence trials, non-pharmacological treatments, herbal interventions, and pragmatic trials. Additional extensions are forthcoming: for those and for up to date references relevant to this checklist, see www.consort-statement.org.