

Intensive care medicine in 2050: clinical trials designs

Original

Intensive care medicine in 2050: clinical trials designs / Gasparini, M.; Chevret, S.. - In: INTENSIVE CARE MEDICINE. - ISSN 0342-4642. - 45:(2019), pp. 668-670. [10.1007/s00134-018-5364-9]

Availability:

This version is available at: 11583/2724788 since: 2019-02-15T12:53:03Z

Publisher:

Springer Verlag

Published

DOI:10.1007/s00134-018-5364-9

Terms of use:

This article is made available under terms and conditions as specified in the corresponding bibliographic description in the repository

Publisher copyright

Springer postprint/Author's Accepted Manuscript

This version of the article has been accepted for publication, after peer review (when applicable) and is subject to Springer Nature's AM terms of use, but is not the Version of Record and does not reflect post-acceptance improvements, or any corrections. The Version of Record is available online at: <http://dx.doi.org/10.1007/s00134-018-5364-9>

(Article begins on next page)

1 Intensive Care Medicine in 2050: Clinical trials designs

2

3 M Gasparini and S Chevret

4

5

6 *1. Department of Mathematical Sciences, Politecnico di Torino, Torino, Italy*

7 *2. AP-HP, Saint Louis Hospital, Paris, France; Paris 7 University; Inserm ECSTRA*
8 *Team, UMR1153, Paris, France*

9

10

11 Corresponding author

12 *Prof Sylvie Chevret*

13 *AP-HP, Saint Louis Hospital, Medical Intensive Care Unit,*

14 *Paris, France;*

15 *Paris 7 University;*

16 *Inserm ECSTRA Team, UMR1153, Paris, France*

17

18 Competing interests: none

19 Word count: 1,000

20 References: 15

21 1 table

22

1 For a long time, clinical trials have been designed in a fairly standard way. In
2 particular, in confirmatory randomized clinical trials (RCTs), widely considered the
3 top of the evidence pyramid, each patient typically has a 1:1 chance of being
4 allocated to the experimental or the control treatment. Such scheme involves a large
5 number of patients, due to the often modest expected benefits (“effect size”), while
6 the statistical requirements to control misconclusions are quite rigid: the type I
7 error rate of false positive findings is consensually fixed at 5% and that of type II
8 error rate of false negative findings fixed at most at 20%. Indeed, the effect size is
9 the factor of greatest impact on both sample size and power computations, which
10 explain the failure of most RCTs in critical care medicine to demonstrate the desired
11 effect size (10.1% on average), often largely above the observed one (1.4% on
12 average, [1]).

13 If RCT must be characterized ethically by the principle of equipoise, that is, of some
14 genuine uncertainty over whether a treatment will be beneficial, then it has been
15 argued that such negative and likely underpowered trials are unethical [1]:
16 participants may be called to sacrifice their own best interests for the benefit of
17 future patients. Lastly, RCTs are also faced with feasibility issues, when dealing with
18 interventions that could not be easily controlled and quantified such as ICU
19 admission and mechanical ventilatory support.

20 For all these reasons, it has been claimed that effectiveness of clinical trials should
21 be improved by adopting a more integrated model that increases flexibility and
22 maximizes the use of accumulated knowledge. Novel tools include the smart use of
23 supplementary evidence, adaptive designs, and Bayesian designs.

24 **Using supplementary evidence for precision medicine**

25 First, merging the strength of randomized clinical trials on homogeneous
26 populations (carefully selected through inclusion/exclusion criteria) and
27 observational studies could be promising. In this regard, mixed randomized trials
28 that allocate patients first to trial arm and then to treatment group have been
29 proposed [2]. This solution seems to be mostly applicable to population-based
30 screening or interventions that appear far from the ICU setting.

31 In the ICU, the complexity of critical illness syndromes is a fundamental justification
32 for the adoption of a personalized approach to research [3]. Thus, identifying more
33 effectively the patients who will benefit from treatment, by refining critical illness

1 (pheno)types of patients, has been the motivation for innovative proposals of the
2 so-called “precision medicine”. The change of paradigm has been mostly beneficial
3 in the oncology setting, where widespread changes in clinical practice for diagnosis
4 and treatment have been increasingly based on genomic features [4].

5 To increase our knowledge on the population that should be targeted when
6 designing a particular trial, Bioinformatics and Machine Learning have provided
7 useful tools for the exploration of the huge amount of data derived from new
8 genomic platforms, physiologic waveforms, RCTs and electronic medical records.
9 This was exemplified since the early 2000s with the development of the MIMIC II
10 (Multiparameter Intelligent Monitoring in Intensive Care) databases that contain
11 physiologic signals and vital signs time series captured from ICU patients [5]. To
12 take full advantage of these big data, prediction models should be validated
13 rigorously given their potential to influence decision making [6].

14 In the light of what has been done in oncology, providing precise information about
15 ICU phenotypes should lead to targeted treatments or interventions in pre-specified
16 subpopulations. Pivotal clinical trials of such therapies will then naturally be based
17 on innovative adaptive and/or Bayesian designs.

18 **Adaptive designs**

19 Adaptive designs can make clinical trials more flexible by utilising results
20 accumulating in the trial to modify the trial course in accordance with pre-specified
21 rules, aiming at improving the study power and reducing sample size and trial cost.
22 First proposed in oncology to assess many treatments and biomarkers, they have
23 raised many controversial discussions from the beginning [7], and are still
24 underused [8] and surrounded by misconceptions [9]. Nevertheless, they appear to
25 provide a possible blueprint for therapeutic development in the ICU.

26 Many innovative adaptive designs have been proposed, including enrichment
27 designs, marker-stratified designs, and marker strategy designs (umbrella trials,
28 basket trials) (Table). Most of these designs aim at treating more patients with more
29 effective treatments, or identifying efficacious drugs for specific subgroups of
30 patients. Such “enrichment” adaptive designs give investigators the ability to study
31 treatment approaches in multiple patient phenotypes within a single trial, while
32 maintaining a reasonable overall sample size, based on their biomarker profiles
33 including omics [10], and shortening the time for drug development. Conversely,

1 treatments found to be ineffective can be dropped from a study for selected patient
2 subpopulations.

3 **Bayesian designs**

4 Bayesian statistics and adaptive designs go often hand in hand. For instance, taking
5 multiple looks at the data is (statistically) not a problem, since in a Bayesian
6 framework such operation does not have to be adjusted for in any special way.
7 Thus, many adaptive designs have been proposed in this framework. They include
8 Bayesian adaptive biomarker/enrichment designs or randomization–adaptive
9 designs that update random allocation probabilities, so that more patients are
10 allocated to the most promising strategy as evidence accumulate [11].

11 Bayesian designs can compare multiple active treatment strategies in real–world
12 settings by allowing for the evaluation of more than one new agent at the same time
13 and by dropping/adding arms when a sufficient level of evidence is reached [12].
14 Such a Multi–Arm Multi–Stage (MAMS) design has been proposed in sepsis–like
15 patients [13].

16 **Conclusions**

17 Adaptive and Bayesian designs are a methodologically sound way to improve clinical
18 trials in critical care but they add significant complexity [14]. First, outcomes should
19 be available soon enough to permit adaptation of the trial design. Furthermore,
20 design is impacted by the accumulated data. This requires statisticians to be
21 engaged both in the planning phase and in the conduct phase of the trial, which
22 may delay its large use in ICU as observed in other settings [15]. However,
23 multidisciplinary collaborations and team science including experts from Genetics,
24 Bioinformatics and Statistics appear a key to the success of these new design
25 strategies in ICU.

26 **References**

- 27 1. Aberegg SK, Richards DR, O'Brien JM (2010). Delta inflation: a bias in the
28 design of randomized controlled trials in critical care medicine 14:R77. doi:
29 10.1186/cc8990.
- 30 2. Alsop J, Scott M, Archey W (2016). The mixed randomized trial: combining
31 randomized, pragmatic and observational clinical trial designs. J Comp Eff
32 Res. 5(6):569–579. doi:10.2217/ce-2016-0034

- 1 3. Maslove DM, Lamontagne F, Marshall JC, Heyland DK (2017). A path to
2 precision in the ICU. *Critical Care* 21:79. doi: 10.1186/s13054-017-1653-x.
- 3 4. Renfro LA, An MW, Mandrekar SJ. Precision Oncology: A New Era of Cancer
4 Clinical Trials (2017). *Cancer Lett.* 387: 121–126. doi:
5 10.1016/j.canlet.2016.03.015.
- 6 5. Scott DJ, Lee J, Silva I, Park S, Moody GB, Celi LA, Mark RG (2013). Accessing
7 the public MIMIC-II intensive care relational database for clinical research.
8 *BMC Med Inform Decis Mak.* doi: 10.1186/1472-6947-13-9.
- 9 6. Shah ND, Steyerberg EW, Kent DM (2018). Big data and predictive analytics.
10 Recalibrating expectations. *JAMA.* doi: 10.1001/jama.2018.5602
- 11 7. Bauer P, Bretz F, Dragalin V, König F, Wassmer G (2016). Twenty-five years of
12 confirmatory adaptive designs: opportunities and pitfalls. *Stat Med.*
13 35(3):325–47. doi: 10.1002/sim.6472.
- 14 8. Hatfield I, Allison A, Flight L, Julious SA, Dimairo M (2016). Adaptive designs
15 undertaken in clinical research: a review of registered clinical trials. *Trials*
16 17(1):150. doi: 10.1186/s13063-016-1273-9.
- 17 9. Pallmann P, Bedding AW, Choodari-Oskooei B, Dimairo M, Flight L, Hampson
18 LV, Holmes J, Mander AP, Odondi L, Sydes MR, Villar SS, Wason JMS, Weir CJ1,
19 Wheeler GM, Yap C1, Jaki T (2018). Adaptive designs in clinical trials: why use
20 them, and how to run and report them. *BMC Med.* 16(1):29. doi:
21 10.1186/s12916-018-1017-7.
- 22 10. Simon R. Genomic Alteration-Driven Clinical Trial Designs in Oncology
23 (2016). *Ann Intern Med* 165:270–278. doi:10.7326/M15-2413
- 24 11. Ondra T, Dmitrienko A, Friede T, Graf A, Miller F, Stallard N, et al (2016).
25 Methods for identification and confirmation of targeted subgroups in clinical
26 trials: a systematic review. *Journal of Biopharmaceutical Statistics* 26(1):99–
27 119. doi: 10.1080/10543406.2015.1092034.
- 28 12. Sydes MR, Parmar MK, Mason MD, Clarke NW, Amos C, Anderson J, et al.
29 (2012). Flexible trial design in practice – stopping arms for lack-of-benefit
30 and adding research arms mid-trial in STAMPEDE: a multi-arm multi-stage
31 randomized controlled trial. *Trials* 13(1):168.

- 1 13. Magaret A, Angus DC, Adhikari NJK, Banura P, Kisson N, Lawler JV, Jacob
2 ST (2016). Design of a multi-arm randomized clinical trial with no control
3 arm. *Contemporary Clinical Trials* 46: 12–17. doi: 10.1016/j.cct.2015.11.003.
- 4 14. Bhatt DL, Mehta C. Adaptive designs for clinical trials (2016). *N Engl J Med*.
5 375(1):65–74. doi: 10.1056/NEJMra1510061.
- 6 15. Thorlund K, Haggstrom J, Park JJH, Mills EJ (2018). Key design considerations
7 for adaptive clinical trials: a primer for clinicians. *BMJ* 360:k698. doi:
8 10.1136/bmj.k698.

1 **Table:** Schematization of proposed biomarker-based adaptive designs

2

Enrichment or targeted designs				
Denomination	Basket trial	Umbrella trial	Platform trials	Biomarker-based Bayesian adaptive trial
Main setting	Single treatment, single biomarker, different subsets of patients	One subset of patients, different biomarkers, Different drugs	Multiple biomarkers and multiple drugs	Response-adaptive randomization
Enrollment	All the subsets are enrolled	One drug for one biomarker (separate enrichment design for each biomarker)	Randomization between strata (allocation probabilities modified to favor assignment of drugs with higher within-stratum response rate)	Modified allocation probabilities within each of biomarker-based treatment
Advantages	Access to targeted agents for patients in various subsets	Conclusions specific to the patient subset	More patients allocated to the best treatment	Incorporate external information, and report based on probabilities on effect size
Drawbacks	Rely on the assumption that profiling based on biomarker is enough	Feasibility, notably for rare diseases (poor accrual and slow trial progress)	Increased samples and heterogeneity	Larger complexity and the involvement of statisticians

3