Author's response to reviews

Title: Goal-directed fluid therapy- A survey of anaesthetists in the UK, USA, Australia and New Zealand

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Author's response to reviews: see over
Editorial Board
BMC Anesthesiology

Re: Goal-directed fluid therapy- A survey of anaesthetists in the UK, USA, Australia and New Zealand

Dear Sir/ Madam,

Thank you for reviewing this manuscript and for giving us the opportunity to respond to the queries raised. The reviewers’ points have been reproduced below verbatim along with our response and any consequent changes to the manuscript have been explicitly stated.

Reviewer 1: William McGee

Limitations should be more emphatically stated in discussion

The discussion has now been edited to include these in more detail.
Reviewer 2: Bala Subramaniam

Major compulsory revisions:

1) The authors primary aim has not been well defined. They state that they wanted to see the uptake of GDFT. Does it mean they simply wanted to know what is the prevalence of GDFT use amongst anesthetists?

The aim of the study was to investigate various characteristics regarding GDFT. Expanding on these in the aim was thought to be too verbose but this has now been edited within the manuscript.

2) The authors go on to state that they wanted to understand the barriers for using GDFT. 3) For example, the question that was asked .....if never used GDFT why? This question does not address the primary issue...are you aware of it? Would you like to more about this technique? This question assumes that all the people that are interviewed are aware of this technique well.

This was done as the survey was administered with a covering letter which explained the concept of GDFT.

4) How was the survey questionnaire developed ? Was there an involvement of a focus group? How was the validity for each question assessed? For example in a survey like this, you have to have alternative questions that kind of tests the same content but framed in a different way?

The survey was designed on the basis of questions of clinical importance but was not developed with a focus group nor with redundant questions. This has been identified as a weakness in the discussion.

5) The methodology that was used to develop and validate the survey questionnaire needs to be mentioned in the methods?

As per question 4.

6) The take away point from this questionnaire is GDFT is primarily a UK technique and this is well known. It would be interesting if this questionnaire was developed to find out exactly what the barriers would be? Unfortunately just one question was dedicated to this and that I think is the major weakness of this study.

Whilst this is an interesting question in itself, it formed a very small part of the study as the aim was to characterise the use and uptake of GDFT. We agree that further work could focus on niche topics identified by this study.
Reviewer’s report:

Dear Authors,

thank you very much for this very interesting manuscript that addresses a highly important question!

Major compulsory revisions:

My major concerns with your work are two-fold:
1. This survey has only a very small number of respondents and is not representative for the anesthesiological community in UK or AUS/NZ and, therefore, does not allow to generalize the results.

The survey has aimed to get a sample of individuals to represent the population. The number of individuals to be surveyed was limited by the respective professional associations. This has been mentioned as a limitation of the study. The colleges facilitated selection of individuals with no author input allowed regarding this. The only selection criterion was to prevent administration of the survey to individuals who had previously replied other college-led surveys to minimise responder fatigue. Thus it is unknown whether representative selection was ensured. This has been noted as a weakness in the discussion.

2. The authors state the anesthesiologists were randomly chosen with the help of the national societies. But does the random selection secure a representative selection? Please comment on this.

As per question 1.

Minor Essential Revisions

1. It is unclear why the use of pressors were analyzed. Does this really add to the purpose of this survey on goal directed fluid therapy to know which pressors are used in the UK oder NZ/AUS?

Intraoperative fluid amounts and indices of cardiovascular function are influenced by the use of pressors. Previous trials have not accounted for this fact (Srinivasa et al Acta Anaesthesiol Scand 2010) and hence this information was sought to accurately characterise the environment within which GDFT was employed.

2. It might be interesting to analyze the choice of colloids used in different parts of the world, but is this relevant to the implementation of GDFT? Please comment on this.

Since colloids are the recommended fluid for boluses in GDFT (Morris et al Anaesthesia 2011), the preferences in colloids were relevant since starch based
fluids have an upper limit for volume of administration beyond which renal function may be adversely affected.

3. Apart from the fluids used, this survey lacks one of the most important questions in the field of goal directed fluid therapy that is if a hemodynamic protocol is applied when using tools for hemodynamic monitoring. The early Connors study from 1997 has shown that the use of hemodynamic monitoring (PAC in this case) without a hemodynamic treatment protocol does not benefit the patients at all (this was in contrast to the even earlier work of William Shoemaker et al. 1988). Please comment on this.

We agree with the reviewer. This question was not specifically asked whether the tools for GDFT were used alongside other monitoring instruments or whether a specific protocol was used. The questions asked of respondents were comparatively basic, which is why further details were not enquired about. This has been further mentioned in the discussion.

4. Despite the small number of respondents from the US, these results are presented in the figures but not in the tables. Why is this? If you decide to not comment the US results due to 9% prespondent rate, than these results should be omitted completely.

The survey was also administered in the USA. Unfortunately, the response rate was especially poor. Thus it was felt that although the results need to be reported, they could not be used for any comparative analysis since any conclusions would be severely limited by the response rate. Hence, the results are presented separately.

5. Reference 20 should state that the revised GIFTASUP recommendation from March 2011 are meant.

This has now been edited.

6. The discussion section mainly discusses the ODM and neglects the other hemodynamic monitoring tools as PPV, pulse contour analysis etc.

The survey is primarily about the theory of GDFT rather than specific instruments. The ODM has been discussed since many users opted for it as their instrument of choice and because it is supported by the greatest body of evidence.

7. On page 10 is stated: „A proportion of people from all the regions surveyed remain sceptical regarding the proposed benefits of GDFT. To an extent, this is justified as important questions remain unanswered, such as efficacy in settings where fluid restriction has been shown to be beneficial.[21-23]“. This reviewer doubts that the terms „restrictive“ or „liberal“ reflect the essential issues of goal directed fluid therapy. Although it is important to avoid hypovolemia, goal directed fluid therapy aims at improving stroke volume, cardiac output, oxygen delivery or other physiological indices. Please comment on this.
The aim of GDFT is to optimise fluid management based on cardiovascular measures of fluid responsiveness. However, this may not be necessary for some patients where the avoidance of fluid overload (termed fluid restriction) may be equally efficacious (Brandstrup et al BJA 2012). It has, however, been demonstrated that both GDFT and fluid restriction are superior to liberal fluid management (Rahbari et al Br J Surg 2009)
Review of “Goal-directed fluid therapy-a survey of anaesthetists in the UK, USA, Australia and New Zealand”

To the Authors:
Some important questions that may help clarify some of the reasons for the responses seen in this survey might easily be answered by generally considering the health systems in the various countries:

Question #1: what are the particular cost constraints and who ultimately bears the cost for providing this additional monitoring equipment, i.e., do the hospitals receive capitated payment for cases based on a DRG model or are they allowed to bill and collect based on their actual costs?

Question #2: what cost accounting systems are used within the various countries surveyed, more specifically in the different locales where there seems to be wide variability in uptake. Specifically, are we looking at hospital-specific or regional cost containment or is cost accounting done at the departmental level? Are the operating room and the Department of Anesthesiology responsible for the costs of monitoring equipment which would have a negative impact to their budget? Or is more global accounting done at either the hospital level or for the disease process itself (DRG model)?

Question #3: what is the reimbursement to individual physicians for using the different technologies across the geographic areas that have been surveyed? Generally I think this is an interesting report in terms of the state of affairs among those who answered the survey, but these data are severely limited by the number of respondents begging the question: does this accurately represent what is truly going on across the spectrum of high-risk surgical cases? The additional questions that I have posed may shed some light on whether or not this is clinically or economically driven. Some discussion regarding the individual practitioner’s decision to use a particular technology and their ability within the different health systems to actually influence that is also warranted. It is also not clear from this report what the expected use of these technologies is within the individual institutions beyond the operating room.

Finally, from an anesthesiologist’s perspective, where the overwhelming majority of patients will leave the operating room with a pulse, blood pressure, and adequate urine output, the benefits to goal-directed therapy are likely to be obscure and only realized further down the road in terms of decreased complications and organ dysfunction. Some discussion of the fragmentation of medical care around ongoing responsibility for outcomes is essential in explaining these results. Furthermore, until cost accounting in medicine truly addresses the cost of the outcome of production, which is a healthy patient at the end of their episode of medical care, these discrepancies will persist. Additional influences to physicians in terms of economic rewards strongly influence the use of technology. This may be a particular area where it would benefit payors to pay providers for the use of these technologies.
The statistical information seems valid, and the tables presented are reasonable. Tables 1 and 2 could be presented with more detail.

New Zealand and the UK have their costs as per a predictive model of hospital departmental costs. In the USA, within a privatised system, patients (or their insurance companies), are billed for costs. As a result, the government ultimately pays in the UK or NZ, whilst this is not the case in the USA unless the Medicare or Medicaid systems are involved.

Since billing is at the departmental level, the costs of GDFT are often borne by the department of Anaesthesia with the economic and clinical benefits of relevance to surgeons (decreased LOS, decreased complications) (Kuper et al BMJ 2011). This is the same situation as in NZ.

Physicians are not reimbursed for the use of this technology in either NZ or the UK since health care is administered via the government. The extra costs involved may be passed on to the patient in the USA.

The response rate is a concern in this study and it is difficult to speculate about whether a representative sample has been obtained as described previously. In essence, the more widespread use in the UK is a reflection of national endorsement, whilst GDFT is an area of interest in both NZ and USA.

As per previously, I would like to assure you that this work has not been published in whole or in part elsewhere and is not currently submitted to any other journal. I attest to the fact that all authors listed on the title page have read the manuscript, attest to the validity and legitimacy of the data and its interpretation, and agree to its submission. All individuals who have contributed to this work have been listed as authors.

Thank you for considering this manuscript for publication.

Yours Sincerely,

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