training pathways and subsequent adjustment (rather than investment) is urgently needed to enable these trainees to still contribute alongside their well-funded academic colleagues.

Some may argue, as Pandit 4 does, that any explanation for the decline in publishing numbers is superfluous, for it is the quality (impact factors, citation scores, h-scores, g-scores) that really matters. Unfortunately, all these measures are prone to manipulation and while one cannot argue that quality is paramount, it is difficult to disregard such a dramatic decline in actual publishing numbers. To countenance this, Pandit 4 suggests that publishing numbers may not be an accurate marker of academic output, preferring number of academic departments, academic staff, research trainees, and magnitude of any grants.1 However, Feneck and colleagues suggest these are merely surrogate measures of academic activity2 and that one cannot ignore a direct measure such as the number of published articles, particularly when the trend is so striking.

Inevitably by scrutinizing BJA publications alone, the generalizability of the results is reduced (while the trend remains arresting).

The process of discerning article origin is prone to occasional error particularly with multi-centre trials, but is unlikely to have impacted upon such a significant trend.

As a premier anaesthetic journal and the only one which all UK trainees receive monthly, the BJA’s decline in UK publications should not be ignored. The growing number of trainee assessments and constant rotation is surely contributory and must be addressed to enable continued participation in research by trainees that remain in busy clinical posts. Pandit acknowledges that much high-quality research comes from non-academic departments, and it is these clinical anaesthetists that must be facilitated alongside those pursuing full-time academia. Moppett and Hardman 6 distinguish between the ‘anaesthesia research community’ and the ‘UK anaesthetic community as a whole’ and it is the latter which the current structure of training and assessment is at risk of alienating. Ultimately, the RCoA and NIAA should be tasked with creating training pathways that ensure research remains accessible to all trainees.

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### Thrombus in left ventricle discovered by transoesophageal echocardiography (TOE) in a patient with acute abdomen: how TOE can be crucial for decision-making in non-cardiac surgery

Editor—A 58-year-old male presented to the emergency room with an acute abdomen requiring emergency laparotomy; the diagnosis was unclear. Preoperative work-up showed air-fluid levels in abdominal X-ray and elevated Troponin I. Since the induction of general anaesthesia was associated with extremely vulnerable haemodynamics, we decided to perform transoesophageal echocardiography (TOE). The TOE at the ME four-chamber view and the ME LAX view discovered a loose and mobile thrombus at size of 1 × 4 mm (Fig. 1A) at the bottom of left ventricle (LV). Additionally, we noticed that heart
contraction was very sluggish and the estimated left heart ejection fraction (LVEF) was \( \approx 20\% \), which was subsequently confirmed using the modified Simpson’s method. The TOE findings, along with the patient’s symptoms, highly implied the diagnosis of mesenteric artery embolism. After that the surgery started and the ischaemic and necrotic intestines were discovered (Fig. 1B), which again confirmed mesenteric vascular occlusion. The ultrasonographer was then consulted and the subsequent ultrasonic examination confirmed the thrombus (Fig. 1C) in superior mesenteric artery, which was then extracted by a vascular surgeon (Fig. 1D). In the meantime, from the anaesthesia point of view, elevated Troponin I, LV thrombus and severely impaired LVEF indicated myocardial infarction, which helped to establish the intraoperative haemodynamic management to enhance the heart performance, and subsequent management and recovery.

In this case, TOE was utilized because of both severe haemodynamic instability and unclear diagnosis. The indication for TOE was recommended by the practice guidelines for perioperative transoesophageal echocardiography in 2010\(^1\) and then stressed again in 2013.\(^2\) Due to the information generated by TOE, the cardiac contraction capability was disclosed to the anaesthesiologists and haemodynamic management strategies were modified accordingly. In addition, the thrombus discovered by the TOE, convincingly helped the surgeon to get a more accurate diagnosis and make an appropriate surgical decision in time.

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Counter statement to open letter to the Executive Director of the European Medicines Agency concerning the licensing of hydroxyethyl starch solutions for fluid resuscitation

Editor—We were surprised to read the letter of Bellomo and colleagues1 criticizing the Co-ordination Group for Mutual Recognition and Decentralised Procedures-human (CMDh) position related to the benefit/risk evaluation of hydroxyethyl starch (HES)-containing solutions. Since the conclusion of these EU Article 31 and 107i procedures is based on a review of all available safety and efficacy data, including recent data from clinical studies, meta-analyses, post-marketing experience, and stakeholders’ opinions, it should be respected.

Notably, the safety signals reported in the three investigator-initiated trials VISEP, 6S, and CHEST2–4 have all been reported in the setting of critically ill patients in general and mostly in patients with sepsis. These facts have been acknowledged and will be included in the product information, as proposed by the PRAC and endorsed by the CMDh by majority vote.

On the contrary, in surgical and trauma patients, the benefit/risk ratio has been evaluated as positive. This is in line with the results of many clinical trials and the recent review article by Van der Linden and colleagues,5 showing, for example, a decreased requirement of blood transfusion and no difference in mortality and need for renal replacement therapy (RRT). These results confirm that the use of modern HES solutions is safe in the perioperative setting and are congruent with other reports.6 The judgement of a positive benefit/risk ratio is also in agreement with the majority of stakeholders, who have already expressed their opinion during the EU Article 107i procedure.

However, the PRAC has recommended conducting additional clinical studies in the surgical and the trauma setting. In the letter by Bellomo and associates, it is important to note that many articles are misquoted like the CRISTAL study.7 In fact, this clinical trial showed that colloids—when given in patients with hypovolaemic shock—are life-saving (significantly reduced 90 day mortality). In this study, ~70% of the patients have been treated with HES. The subgroup analysis confirmed a significantly reduced 90 day mortality in HES-treated patients when compared with patients treated with 0.9% saline. Withdrawing HES would therefore not decrease but increase the risk for patients.

Another example of a misquotation is linked to the reference James and colleagues,8 which is misleadingly cited to suggest that HES ‘…increases the risk of bleeding and need for blood products in patients…following blunt trauma’. Notably, the study results do not support the statement of Bellomo and colleagues. In fact, organ function was better in penetrating trauma patients treated with 6% HES 130/0.4 compared with 0.9% saline. Owing to baseline imbalances among groups, no firm conclusion on the treatment effects in patients with blunt trauma was possible.

In general, Bellomo and colleagues do not differentiate between HES types with different molecular substitutions and physicochemical properties. The references cited to reflect negative effects of HES in part used outdated solutions, for example, Cittanova and colleagues9 (6% HES 200/0.62), Brunkhorst and colleagues10 (VISEP-study, 10% HES 200/0.5), and the meta-analyses including starch solutions of older generations. On the contrary, there is increasing evidence showing that there are relevant differences between the effects of the different products, with the best profile for the latest generation of starches. This is supported by recent data of the RaFTinG registry10 that have been evaluated by PRAC in the Article 107i procedure.

In their letter, Bellomo and colleagues did not discuss the major limitations of the three investigator-initiated studies VISEP, 6S, and CHEST.2–4 In this context, it is important to note that many patients were already treated before randomization and were not hypovolaemic at the time of study inclusion. Accordingly, there was no need of volume therapy in at least this subset of patients. It is also important to consider that many patients with contra-indications to HES have been included in the studies. In addition, dose limitations have not been respected in the VISEP trial. Overdosing and use outside the indication of hypovolaemia were associated with increased mortality. These criticisms have been expressed by the scientific community.11 Most importantly, data from the CHEST trial are used incorrectly, although the letter was written and signed by a number of CHEST investigators: ‘In CHEST, increased use of renal replacement therapy in intensive care patients occurred after a total cumulative dose of 5 mL/kg, one tenth of the maximal daily dose of 50 mL/kg’. This cannot be correct, since on the first treatment day, a mean dose of ~980 mL was administered, which amounts to 12 mL kg−1. Moreover, the cumulative HES dose within the first 4 days of treatment was 26.5 mL kg−1. Thus, the cumulative HES dose was greater than five times more than acknowledged by Bellomo and colleagues. It is also important to consider that the difference in the use of RRT was only of borderline significance between groups and that no rules for initiating and stopping RRT were defined.

There are also major concerns about study designs and data analyses11 in VISEP, 6S, and CHEST. Analyses by independent third parties are needed to clarify the open issues.

We would also like to express that although some physicians signed the open letter, it is a minority not taking the current status of knowledge of the risk–benefit assessment of HES into account.

In addition, we would like to emphasize that the conduct of further clinical studies is of high value to gain