

their personal experience. We respectfully disagree with their comments that “the presence of an IABP during ECMO may potentially exacerbate the risk of complications.” In our study and the previous study by Tay et al (2), complication rates were not significantly higher after treatment with a combination of IABP and VA-ECMO when compared with VA-ECMO alone.

Tay et al (1) point out that we excluded patients who had undergone out-of-hospital cardiopulmonary resuscitation. It is well recognized that these patients have a poor prognosis and are less likely to receive IABP. Epidemiologically speaking, exclusion of this group of patients is an appropriate means of reducing the influence of potentially confounding factors and allowed us to establish whether IABP confers any additional therapeutic benefits with greater precision.

The database used in our study included the start dates of IABP and VA-ECMO for each patient. We defined the IABP plus VA-ECMO group as those who received VA-ECMO at admission and IABP within 1 day of admission. As Tay et al (1) point out, there may have been some patients who had IABP first but subsequently received VA-ECMO. Our study design ensured that the interval between establishing IABP and VA-ECMO was no more than 1 day, and consequently, we judge that the sequence of initiation of IABP and VA-ECMO would have little or no impact on our results. We, therefore, respectfully disagree with their comment that “the VA-ECMO-only group may, therefore, have a higher disease severity that could not have been adjusted by propensity matching.” We undertook propensity score adjustment for disease severity-related factors including etiology; use of catecholamines, antiarrhythmic agents, and sodium bicarbonate; percutaneous coronary intervention; coronary artery bypass grafting; and continuous renal replacement therapy. We have acknowledged that several unmeasured confounders may have existed and that randomized controlled studies are required to confirm the true effect of IABP combined with VA-ECMO.

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The “Burnout” Construct: An Inhibitor of Public Health Action?

To the Editor:

In a recent issue of *Critical Care Medicine*, Moss et al (1) called for action regarding the prevention and treatment of the “burnout syndrome” within the critical care community. An

important objective of the authors was to raise awareness about burnout in this occupational area, based on the idea that burnout is especially common in individuals who care for critically ill patients. We think that the authors’ observations and recommendations are diminished by the fact that studies of burnout’s prevalence are methodologically problematic. More globally, the current definition and use of the burnout construct may in fact be detrimental to public health decision making.

In most prevalence studies reviewed by the authors, the Maslach Burnout Inventory (MBI) and the cutoff values presented in the MBI manual (2) were used to identify cases of burnout. It is noteworthy, however, that these criteria have not been established for diagnostic purposes and are actually devoid of any clinical or theoretical underpinning. Here is what the developers of the MBI state in the MBI manual (page 9):

...neither the coding nor the original numerical scores should be used for diagnostic purposes; there is insufficient research on the pattern(s) of scores as indicators of individual dysfunctions or the need for intervention.

The cutoff scores provided in the MBI manual have been defined *arbitrarily* on a tercile-split basis. In other words, they reflect mere descriptive statistics. Relying on such criteria to estimate burnout’s prevalence is therefore unwarranted. Had a median-, a quartile-, or a quintile-split been chosen, then different cutpoints would have been found, leading to dramatically different estimations of burnout’s prevalence. Ultimately, because they are clinically obscure and theoretically groundless, the estimations of burnout’s prevalence derived from these criteria are unsuited for informing public health decision making (3).

Interestingly, the authors indicated that burnout may overlap with other conditions, “including moral distress, perceived delivery of inappropriate care, and compassion fatigue...” Problematically, the authors did not mention the crucial issue of burnout-depression overlap. Burnout and depressive symptoms have been shown to be inextricably linked (4). Moreover, unresolvable stress, which is assumed to be central in burnout’s etiology, plays a key causal role in the development of depressive syndromes (4, 5). It is well-established that unresolvable stress induces depressive responses in individuals, starting with the feeling of helplessness, a hallmark of depressive processes. The long-studied, nosologically well-characterized construct of depression is manifestly better suited than the diagnostically undefined construct of burnout for providing an accurate picture of critical care (or other) professionals’ health.

Historically, the burnout construct probably helped underscore the importance of work life for people’s health. Today, however, its limitations make it a source of noise, rather than of information, for occupational medicine practitioners, work psychologists, and public health decision makers. In our view, it is high time to face the fundamental methodological problems that undermine burnout research and to put an end to the current runaway movement toward confusion. We recommend, as a key step toward clarification, that burnout be defined as a (job-related) depressive syndrome and assessed as such.

The authors have disclosed that they do not have any potential conflicts of interest.

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The authors reply:

We thank Bianchi et al (1) for their comments on our article (2) and their interest in the problem of burnout in healthcare. In their letter (1), they are critical of the studies that address the prevalence of burnout that we cite, arguing that most of these studies use the Maslach Burnout Inventory (MBI) to establish estimates of the prevalence of burnout. They note that the MBI is not intended to be used for diagnostic purposes, and they are critical of the arbitrary tercile-split basis used by Maslach to establish cutoff scores, arguing that the use of a quartile-split or median-split basis, for example, might lead to different cutpoints and thus different estimates of prevalence. In previous letters, they contend that the actual prevalence of burnout is likely lower than when estimated by MBI (3).

The MBI is a 22-item questionnaire with three domains or subscales (emotional exhaustion, depersonalization, and personal accomplishment) (4). Many investigators set MBI score ranges for low, moderate, and high levels and defined burnout as a high-MBI score. In our article (2), we acknowledge limitations of the MBI stating, “...accurate cutoff values for critical care healthcare providers have not been determined. As a result, the diagnostic criteria for BOS vary across studies, making comparisons difficult from one study to another.” Nevertheless, although imperfect, the MBI represents the industry standard, has been demonstrated to be valid and reproducible, and has been used in the vast majority of studies focused on burnout in healthcare settings (2, 5). Interestingly, studies using other less robust survey methods generally estimate even higher prevalence rates than those defining burnout as

high-level MBI. Our call to action embraces the critical need for additional research into burnout and the development and validation of additional tools to assess burnout.

Another concern raised by Bianchi et al (1) of the burnout construct is that of significant overlap between burnout syndrome and depression. We agree that the entities are indeed closely related as each of them is characterized by emotional exhaustion and depersonalization and reflect the consequences of chronic stress. They conclude their letter with the recommendation that burnout be defined as a (job-related) depressive syndrome and assessed as such. Although we respectfully disagree with such recommendation, we concur with the concluding statement from one of their prior publications, “whether burnout and depressive symptoms are distinct or overlapping features should be further elucidated” (6). In fact, the considerable attention focused on burnout syndrome among healthcare workers is likely to help shine a welcome light on workplace-related mental health disorders. Importantly, such work, as emphasized in our call to action (2) may be highly important at identifying core underlying and precipitating factors, promoting early identification, and validating interventions to mitigate these disorders. In contrast to being an inhibitor of public health action as argued by Bianchi et al (1), we believe the growing interest in burnout as an increasingly common major threat to healthcare (7) is likely to accelerate public health awareness and implementation.

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