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Disclosures

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Correspondence

Ayhan Kanat: ayhankanat@yahoo.com.

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Response

We thank Dr. Kanat for his astute comments regarding our recent manuscript describing a reduction in shunt-dependent hydrocephalus with tandem fenestration of the LT and the MoL at the time of microsurgical treatment of ruptured aneurysms. As he stated, the etiology of hydrocephalus following aSAH is complex and multifactorial, including anatomic obstruction of CSF flow with clotted blood, increases in CSF production by the choroid plexus, decreases in CSF reabsorption by arachnoid granulations, and maybe even alterations in the diameter of the cerebral aqueduct. However, the relative contributions and mechanisms governing each of these factors remain poorly understood, and hydrocephalus remains an important source of morbidity following aSAH. We agree that a better understanding of hydrocephalus after aSAH is badly needed, as are innovations to prevent and treat hydrocephalus without resorting to ventriculoperitoneal shunting.

Recent work identified that posthemorrhagic inflammation may play a role in the development of hydrocephalus through activation of toll-like receptors on the choroid plexus epithelium in experimental models.¹ A reduction in clot burden with tandem fenestration may reduce inflammation and oxidative stress induced by subarachnoid blood products, such as fibrin, thrombin, and hemoglobin.³ As we and others have described,⁴ the MoL often becomes thickened and inflamed following aSAH, leading to loculation of CSF in the prepontine and interpeduncular cisterns, which we informally refer to as the “fifth ventricle.” Restoration of CSF flow through tandem fenestration may

therefore further promote clearance of subarachnoid blood products by reopening the natural circulatory pathway of CSF and improving reabsorption of CSF by the arachnoid granulations along the brain’s convexities.

As noted by Dr. Kanat, tandem fenestration of the LT and MoL requires arachnoid dissection and subarachnoid clot removal that is only possible with open surgery. Clinical experience with surgical techniques to alter CSF hydrodynamics following aSAH has been mixed. Isolated fenestration of the LT is one such technique,² but this alters CSF flow exclusively within the supratentorial compartment and does not re-establish the complete CSF circulation. We view our results as compelling but acknowledge that our analysis was both retrospective and limited to a single surgeon’s experience. We were encouraged by the magnitude of benefit observed and by the low morbidity associated with these extra maneuvers, with no complications in this series. We hope that others will consider validating our findings with future prospective multi-institutional trials.

Ethan A. Winkler, MD, PhD

Jan-Karl Burkhardt, MD

University of California, San Francisco, CA

Michael T. Lawton, MD

Barrow Neurological Institute, Phoenix, AZ

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“Burned out” at work but satisfied with one’s job: anatomy of a false paradox

TO THE EDITOR: In a recent study of 346 US neurosurgery residents, Attenello et al.¹ reported that 67% of their participants suffered from burnout (Attenello FJ, Buchanan IA, Wen T, et al: Factors associated with burnout among US neurosurgery residents: a nationwide survey. *J Neurosurg* [epub ahead of print February 9, 2018. DOI: 10.3171/2017.9.JNS17996]). The authors also found that

81% of surveyed residents were satisfied with their career and that 79% would choose neurosurgery again. Attenello et al.¹ described their results—involving a coexistence of burnout and job satisfaction—as paradoxical. The authors' questioning is understandable. Based on the view that burnout reflects “a crisis in one's relationship with work,”⁹ marked by occupational frustration and vocational disappointment, one could expect burnout to be antithetical to job satisfaction.³ In our estimation, the apparent paradox attached to Attenello and colleagues' findings is accounted for by persistently ignored problems in burnout's conceptualization and measurement.

Attenello et al.¹ used the Maslach Burnout Inventory (MBI)⁹ and the cutoff scores presented in the MBI manual for identifying “cases” of burnout. The MBI, however, is not a diagnostic instrument.^{5,6,10} The cutoff scores presented in the MBI manual have no clinical underpinning; they merely reflect tercile-based splits of convenience samples of workers.^{5,9} The developers of the MBI themselves emphasized that “such arbitrary ‘cut-off’ scores do not have any diagnostic validity” and that “there is no definitive score that ‘proves’ a person is ‘burned out.’”⁹ Attenello et al.¹ thus relied on identification criteria that fail to discriminate clinically relevant symptoms of exhaustion and withdrawal from harmless variations in mood or stress and motivation levels—remarkably, experiencing detachment once per month was sufficient to be classed as “burned out” in the authors' study. This state of affairs sheds a different light on the apparently contradictory findings of the authors. That a vast majority of residents were satisfied with their job may reflect the fact that many (if not most) residents categorized as “burned out” were only experiencing benign, transitory fluctuations in their well-being at work.^{3,4}

Furthermore, although burnout has been defined as a work-induced condition,⁹ the determinants of the symptoms assessed by measures such as the MBI are far from clear.^{3,5,8} In a recent study of 468 Swiss health professionals, only 44% of the individuals presenting with burnout symptoms attributed these symptoms to their job.² Such results are consistent with Attenello and colleagues'¹ finding that stress outside of work is a significant predictor of burnout. Many individuals reporting burnout symptoms may simultaneously be satisfied with their job for the basic reason that their symptoms are not caused by work-related difficulties.

The burnout construct is plagued by definitional problems.⁵ In the current context, relying on “burnout” as an index of occupational ill-health 1) diverts our attention from professionals who truly need help by drowning them in a sea of heterogeneous health conditions and 2) impedes our ability to identify depressogenic workplaces and managerial patterns for the purpose of implementing organizational reforms. The use of loose, catch-all categorization criteria is probably advantageous to individuals attempting to create new consulting and pharmaceutical markets, but it is highly questionable from both a methodological and a public health standpoint.^{5,7} Researchers should stop making claims about burnout's prevalence based on arbitrary criteria.

Eric Laurent, PhD

Bourgogne Franche-Comté University, Besançon, France

Irvin Sam Schonfeld, PhD, MPH

The City College of the City University of New York, New York, NY

Renzo Bianchi, PhD

University of Neuchâtel, Neuchâtel, NE, Switzerland

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Correspondence

Renzo Bianchi: renzo.bianchi@unine.ch.

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Response

We acknowledge and appreciate the observations by Laurent et al., noting that they have published several very similar critiques in recent “Letter to the Editor” commentaries in response to clinicians employing the MBI.^{1–4,6,10} Discourse on surgeon burnout is critical to improving care in the neurosurgical community. We are therefore pleased to address the authors' points that the MBI is not a definitive diagnostic instrument, that non-workplace factors may affect resident well-being, and that burnout should not detract from consideration of psychiatric diagnoses in the workplace.

We agree that the MBI does not provide definitive psychological diagnosis. However, the MBI is widely employed throughout the physician-burnout literature as an easily administered screening tool addressing emotional exhaustion, depersonalization, and lack of accomplishment, each limiting success of health care workers.^{7,8} Our survey was

intended to shed light on a burnout phenomenon and associated modifiable factors, a goal recently espoused by MBI creator Christina Maslach.⁷ Predictably, interventions based on the MBI show positive results in residents.⁹ Though we agree that the MBI cannot provide the definitive clinical diagnosis of a more thorough battery, the 22-item questionnaire was a reasonable length for the 346 neurosurgical residents who complied and provided data.

Laurent et al. further emphasize that burnout is traditionally defined as a workplace-induced phenomenon. We agree, although we must emphasize that neurosurgical residents invariably see a blurring of home and workplace. Residents dedicate 80 hours per week to patient care, often voluntarily allocating their “non-work” time to prepare for cases, follow up on patient imaging and laboratory studies, and pursue academic research. Neurosurgical residents, like many other surgical and medical residents, do not have the traditional exclusive workplace. Not surprisingly, stressors outside of work were associated with burnout in our study.

Finally, regarding “arbitrary criteria” of the MBI diverting attention from workplace depression, we strongly disagree. The MBI is employed in hundreds of studies precisely because it is well-designed, reproducible, and validated, repeatedly showing association with personal, social, and organizational outcomes.^{5,7} Our goal was not to define psychological disease prevalence—our goal was to evaluate modifiable burnout factors, such as mentorship, shown to associate with patient care and outcomes.⁵ The authors, in their title, cite the “false paradox” of 81% resident career satisfaction despite a 67% burnout rate to highlight inadequacy of the MBI. Residents, within survey text (and later, offering response following publication) elaborated upon long-term satisfaction with improving patient outcome, developing surgical skills, and enjoying neuroscience study, although they felt patient-disconnect (depersonalized) and emotionally exhausted on a day-to-day level.

For a neurosurgeon, the Hippocratic oath espouses “do no harm,” and our study aimed to highlight a factor (burnout, via MBI) traditionally associated with poor workplace performance.⁵ Laurent et al. do not offer a practical alternative. Whether the MBI measures transient phenomena or does not truly correlate with depression is not relevant to our goal. MBI-measured burnout shows association with workplace performance, and if there is any factor associated with negative outcome in our residents and critically ill patients, we must make the neurosurgical community aware of it and explore every avenue to improve these outcomes.

Frank J. Attenello, MD

University of Southern California, Keck School of Medicine,
Los Angeles, CA

Paul Klimo Jr., MD, MPH

University of Tennessee Health Science Center, Memphis, TN

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Carotid atherosclerotic plaque instability and cognition: collecting additional data

TO THE EDITOR: We have read with great attention the article by Dempsey and colleagues³ (Dempsey RJ, Varghese T, Jackson DC, et al: Carotid atherosclerotic plaque instability and cognition determined by ultrasound-measured plaque strain in asymptomatic patients with significant stenosis. *J Neurosurg* **128**:111–119, January 2018). The authors presented a stimulating study that describes “the use of ultrasound measurements of physical strain within carotid atherosclerotic plaques as a measure of instability and the potential for vascular cognitive decline, microemboli, and white matter hyperintensities (WMHs).” Subsequently, the same group demonstrated that atherosclerotic vascular cognitive decline may be modified by removal of the unstable plaque.² In that earlier study,³ the authors stated in their results that “the degree of strain instability measured within the atherosclerotic plaque directly predicted vascular cognitive decline in these patients thought previously to be asymptomatic according to classic criteria. Furthermore, 26% of patients showed microemboli, and patients had twice as much white matter hyperintensity as controls.”

As our lead author (A.N.) is involved in studying the natural course of ischemic WMHs and explaining the relationship between brain revascularization and the course of ischemic WMHs,^{5,6} he has uncovered some interesting points that have raised several questions, and we would like to obtain answers from experts³ that may be helpful in our current and future research. Based on a thorough