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## Buprenorphine Maintenance Therapy in Opioid-Addicted Health Care Professionals Returning to Clinical Practice

**To the Editor:** Hamza and Bryson<sup>1</sup> argue against health care professionals returning to clinical practice while taking buprenorphine, based on purported neurocognitive effects. Their argument is based on weak science and flawed assumptions. Studies examining neurocognitive effects associated with buprenorphine are mostly based on small, selected samples and frequently fail to account for preexisting neurocognitive function or to distinguish between short- and long-term effects (after development of full tolerance) of the drug. Most studies use weak, ie, nonrandomized study designs. None of the studies was based on health care professionals. These limitations preclude firm conclusions regarding the presence or absence of neurocognitive effects associated with buprenorphine.

More important, the impact of purported neurocognitive effects on job performance is not clear. Laboratory tests that show subtle effects cannot be extrapolated to real work performance. This would require direct measures of job task performance after long-term use of the drug—ideally using randomized study designs.

Furthermore, many factors affect neurocognitive performance. Examples include baseline ability, age, previous head injury, impaired sleep, chronic illness, viral infection, and many commonly prescribed medications (including those that

are not controlled). Thus, even if buprenorphine is shown through scientifically valid studies to have meaningful effects on neurocognitive performance after long-term use, it would be wrong to single out health care professionals taking this medication. Rather, the same standards for evaluation of neurocognitive performance would have to be uniformly applied to all health care professionals regardless of the cause for any decrement in performance. It is doubtful that most health care organizations are prepared to undertake such mass neurocognitive screening given its high costs and uncertain benefit.

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**In reply:** We read with great interest the Letters to the Editor written in response to our article discussing the use of buprenorphine maintenance therapy in opioid-addicted health care professionals, and we are encouraged by the discussion that continues to evolve around this important issue. We are pleased that our review has generated so much conversation from those on the front lines of addiction medicine and welcome the opportunity to reply to the letters from Drs Earley, Newman, Selzer and Stancliff, and Fiscella.

Regarding the quality of research reviewed, Dr Fiscella asserts that our conclusions are based on “weak science and flawed assumptions,” citing small sample size, nonrandomized study design, and failure to account for the possibility of preexisting neurocognitive deficits among other limitations as reasons to support his position that buprenorphine use should not preclude one from a return to clinical practice after treatment for substance abuse. Dr Fiscella also asserts that, because none of these studies were performed with

actual health care professionals, any conclusions regarding the presence or absence of neurocognitive effects cannot be extrapolated to this group. Although we agree that the available studies have limitations, they hardly qualify as “weak science,” and the concerns that he raises are not based in fact. Dr Fiscella claims that the studies were not randomized, yet the studies that we cited performed by Soyka et al in 2005<sup>1</sup> and 2008<sup>2</sup> and by Mintzer et al in 2004<sup>3</sup> did, in fact, use a randomized, double-blind design. Dr Fiscella claims that the studies fail to distinguish between long- and short-term maintenance therapy, but it is unclear what he means by this. The 2008 study by Jensen et al<sup>4</sup> evaluated single-dose effects, whereas the 2004 study by Mintzer et al<sup>3</sup> looked at dosage differences with study participants taking a particular dose for 7 to 10 days with performance assessment at 3 different time points, and the 2009 study by Messinis et al<sup>5</sup> evaluated participants who had been taking buprenorphine for 18 to 28 weeks. The results of these and the other studies included in our review, regardless of the design, are very consistent. Each of the peer-reviewed and published studies cited in our article reported similar disadvantageous effects on neurocognitive performance when patients were under the influence of buprenorphine. Whether undergoing short- or long-term therapy, healthy volunteers and recovering addicts alike demonstrated evidence of impairment. This in and of itself is troubling and, as was clearly stated in our article,<sup>6</sup> we believe that further studies need to be conducted that specifically examine the influence of buprenorphine on the ability of health care professionals to perform tasks directly related to their roles as clinicians.

Drs Selzer and Stancliff seem to suggest that we chose to include only poorly designed studies to support our conclusion that caution should be used when prescribing buprenorphine in this population. In fact, we reviewed all of the published literature on the topic and came to the same conclusion as Drs Selzer and Stancliff did: the literature on the topic of the cognitive effects of buprenorphine is limited and more research is needed. This view is shared by Dr Earley and others, who echo the need for more definitive research. Weakness in the existing literature