ment process and its therapeutic objective. The authors' notion that there is an inherent contradiction between continued prescribing of medication and a patient's "recovery" and the suggestion that reducing harm and controlling damage are not part and parcel of any practice of medicine are extraordinary. The authors accept a role for medications such as buprenorphine and methadone when "used to help retain people in the detoxification phase of treatment" but postulate that "maintenance is another matter and indicates severe difficulty with maintaining recovery." In fact, the primary challenge faced by health care professionals and recipients of addiction treatment of all kinds is precisely this "difficulty." In other words, the problem is not the achievement of abstinence but how to maintain it.[2]

No empirical evidence is presented to support the recommended exclusion from practice, across the board, of health care professionals who are being prescribed buprenorphine. None of the studies cited relied on employment data, malpractice experience, or other measures or proxies of practice competence, and several reported results of buprenorphine administration (some by intravenous injection) among nontolerant individuals. Furthermore, to the extent that there is a basis for concern over individuals receiving *maintenance* treatment with opioid agonists, it would presumably be vastly greater for those receiving opioids for pain management (acute or chronic) and probably extend to those taking benzodiazepines for insomnia, antidepressants, and a wide variety of other medications.

The efficacy of maintenance treatment of addiction has been confirmed consistently in reports from throughout the world for almost half a century. This treatment has been strongly endorsed by the highest governmental, academic, and clinical authorities in the United States and internationally. It is ironic that health care professionals, of all people, should argue that it should be rejected when it comes to colleagues who want and need the help that it can provide.

Robert G. Newman, MD, MPH
Baron Edmond de Rothschild Chemical Dependency Institute
Beth Israel Medical Center
New York, NY

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Buprenorphine Maintenance Therapy in Opioid-Addicted Health Care Professionals Returning to Clinical Practice: A Hidden Controversy

To the Editor: The article by Hamza and Bryson[1] cites several studies to support their opinion that health care professionals should not be returned to practice if their treatment includes opioid agonist therapy. The quality of the evidence cited is poor. Three of the studies[2-4] evaluate the effects of buprenorphine in "healthy volunteers" rather than in patients after careful dose titration. Other studies are small and poorly controlled for the duration of therapy and other drug use. The most relevant study[5] compares patients taking buprenorphine with those taking naltrexone. The findings were not striking; although the buprenorphine patients differed significantly from the controls on several measures, they did not differ from the naltrexone group. In fact, the authors of this study state, "Furthermore, the nondiffering percentage of abnormal cases between the two patient groups led us to infer that treatment with either BPM [buprenorphine] or FHAN [naltrexone] is not accompanied by qualitative differences in the cognitive profiles of these patients."

The poor response rate of the physician health programs they surveyed may have more to do with the skill of the authors in engaging their study participants than with secretive practices by these programs. The survey protocol is vague, and there is no statement of institutional review board approval for the study. Furthermore, the methods in the survey may have resulted in invalid findings. For example, we find the comment describing the New York program as "no policy, left to treating psychiatrist" extremely misleading. In fact, treatment decisions are made in collaboration with the physician health program and subject to its approval. Although it is not uncommon for a participant to require agonist therapy initially, continued use is carefully reevaluated, including the use of neuropsychiatric evaluation and clinical skills assessment before return to work if indicated. The same approach is used for participants prescribed other psychoactive medications with potential cognitive untoward effects.

Much in this article is informed by bias rather than science. The authors characterize opioid-addicted health care professionals as "masters of drug diversion." This view perpetuates stigma by stereotyping health care professionals with substance use disorders. Although the authors note that physicians in physician health programs tend to do better in treatment than other patients with substance use disorders, without good evidence they promote naltrexone because "it undeniably strengthens the safety net." The pervasive bias is further reflected in value judgments about "the improved quality of life for the professional" with the use of the abstinence model and by citing an oral communication describing opioid agonist therapy as "psychotoxic" and "a potential predictor of increased risk for relapse." Hamza and Bryson are correct in their conclusion that more study would contribute to a fuller understanding of the role of opioid agonist therapy in the treatment of health care professionals. It is unfortunate that their review and survey results are so unilluminating.

Jeffrey Selzer, MD
Committee for Physician Health
Albany, New York

Sharon Stancliff, MD
Harm Reduction Coalition
New York, New York

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

To the Editor: Hamza and Bryson[1] 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.

Kevin Fiscella, MD, MPH
Department of Family Medicine, Community and Preventive Medicine, and Oncology
University of Rochester
School of Medicine and Dentistry and Wilmot Cancer Center
Rochester, NY

1. Hamza H, Bryson EO. Buprenorphine maintenance therapy in opioid-addicted health care professionals returning to clinical practice: a hidden controversy. *Mayo Clin Proc*. 2012;87(3):260-267.

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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[1] and 2008[2] and by Mintzer et al in 2004[3] 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[4] evaluated single-dose effects, whereas the 2004 study by Mintzer et al[3] 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[5] 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,[6] 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