Redesign of Internal-Medicine Teaching

TO THE EDITOR: McMahon et al. (April 8 issue) report “higher trainee satisfaction” of interns and residents (trainees) in an Integrated Teaching Unit (ITU), as compared with trainees in a general medical service (GMS) team. The ITU interns had a lower clinical workload (3.5 vs. 6.6 average patient census), decreased call frequency, and attending physicians “who had received superior ratings for their teaching ability,” among other favorable features. Because of the way these conditions were structured in the study, the results were preordained. Overall trainee satisfaction was 78% in the ITU team and 55% in the GMS team. Patient satisfaction was the same but cost was higher in the ITU team (five trainees per ITU team vs. three trainees per GMS team). Whether the ITU trainees had greater medical knowledge or would be better able to care for a variety of patients was not assessed. In an editorial in the same issue, Ludmerer comments that “there is no reason to believe that such a model cannot be adopted by other hospitals.” No reason, except that current health care costs are unsustainable and no benefit was shown in training.

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TO THE EDITOR: I am concerned about the volume of patients seen by the ITU team in the study by McMahon et al. Although the division of trainees’ time over fewer encounters might result in greater thoughtfulness, a schedule with fewer encounters negatively affects the competence of the trainee. Malcolm Gladwell has recently made the compelling case that, if simplified case for a direct correlation between the time spent performing a skill and subsequent competence and expertise.

I am also concerned about the reliance on the measure of learner satisfaction. Regrettably, competence is difficult to measure and therefore is generally scientifically neglected, but the unaddressed question of the study is whether residents in the experimental system, though happier, will be as capable of caring for patients and educating future generations of doctors. Kruger and Dunning demonstrated that individuals fail to recognize their own lack of competence. As educators, we cannot afford to neglect the competence of our trainees simply because it is simpler and more agreeable to prioritize their satisfaction.

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TO THE EDITOR: In their report on redesigning an inpatient-medicine teaching service, McMahon et al. indicate that reducing trainees’ workload resulted in greater direct costs that were offset by funding from the Department of Medicine and the hospital. In the Aliki Initiative at Johns Hopkins, the workload of residents is reduced by shifting admissions from house officers to hospitalists on a nonteaching service. Although funds for hospitalists that were needed to initiate this program were provided by a donor, Aliki Perroti, whose name our program bears, we have observed that the greater efficiency of care by hospitalists appears to make this training model economically self-sustaining. At many hospitals, including ours, care by hospitalists is associated with a shorter hospital stay. This results in increased bed-days and admissions that more than offset the cost of staffing additional hospitalists. Ludmerer and Johns noted that 70 years ago there was already a call for hospitals to hire salaried physicians in order to ensure adequate educational opportunities for house officers; this may be not only appropriate but also economically feasible.

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THE AUTHORS REPLY: Graduate medical education is in need of reform. In our study, we sought to clarify and quantify the impact of a package of educationally driven changes on the quality and efficiency of care and trainee satisfaction in comparison to a more typical model. As suggested by Weir, we anticipated that there would be differences between the models but sought to quantify the magnitude of the differences that could be generated through thoughtful investment in education and supervision of our trainees.

Our trainees are professional consumers of education, and we value their satisfaction. However, we did not rely on satisfaction alone to measure the effectiveness of the intervention, as suggested by both Weir and Kalus, but rather used a multidisciplinary approach. For such an intervention to be worthwhile, it should improve patient care. We chose to determine the effect of the new service on metrics of quality of inpatient care rather than on more intermediate end points, such as resident knowledge. In addition, our design emphasized individualized learning plans, which are not easily measured on standardized testing. Our findings provide evidence that investment in trainees can improve education without a negative impact on care quality and operational efficiency.

We agree with Kalus that providing some higher-volume experiences is critical to ensure sufficient exposure to a broad variety of patients and to promote professional development. However, time spent reflecting comprehensively on fewer patients may be at least as important as more frequent, but typically more rapid, encounters with a greater number of patients. Our data suggest that a more comprehensive approach did not have a negative impact on the quality of care. Effective supervision and appropriate amounts of rest are likely to be especially critical to the provision of high-quality care during higher-volume rotations.

As Weir observes, this was a costly program to initiate. However, as Ziegelstein et al. found for their model, the operational efficiencies gained in the new model offset our extra staff costs. We have since incorporated the new team model into our general medicine teams, with similar early results.

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Since publication of their article, the authors report no further potential conflict of interest.

THE EDITORIALIST REPLIES: In response to Weir: the educational hope is that better preparation of residents in decision making and the management of uncertainty will pay off down the road by pro-
TO THE EDITOR: In McColl’s Clinical Practice article about Helicobacter pylori infection (April 29 issue),1 the primary treatment recommendations are in keeping with guidelines from the American College of Gastroenterology, but it should be noted that bismuth subsalicylate and bismuth subcitrate potassium are available in the United States and have been shown to be effective in combination therapies for H. pylori.2 In a U.S. study, the eradication rate with quadruple therapy (consisting of a proton-pump inhibitor, bismuth, amoxicillin, metronidazole, and tetracycline) was 88% (95% confidence interval [CI], 82 to 93).3

The Maastricht consensus report recommends starting with clarithromycin triple therapy, followed by quadruple therapy in the case of failure, and using levofloxacin triple therapy (consisting of a proton-pump inhibitor, amoxicillin, and levofloxacin) if the first two strategies fail.4 With this approach, a 90% overall eradication rate can be achieved.5 In contrast, the triple-therapy regimen suggested by McColl (consisting of a proton-pump inhibitor, amoxicillin, and metronidazole) had a poor eradication rate, 76% (95% CI, 68 to 84), and is a less satisfactory alternative.6

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Dr. Vakil reports receiving consulting fees from AstraZeneca, Takeda, Ascan, and Salix and research funds from AstraZeneca and Abbott and being the author of the American College of Gastroenterology guideline on the management of dyspepsia; Dr. Vakil and Dr. Malfertheiner report being coauthors of the Maastricht consensus conference report on the treatment of H. pylori; Dr. Malfertheiner reports receiving research funds from Axcan Pharma; and Dr. Chey reports receiving consulting fees from Albireo, AstraZeneca, Ascan, Ironwood, Prometheus, Procter & Gamble, Salix, SmartPill, and Takeda and being the author of the American College of Gastroenterology guideline on the treatment of H. pylori infection. No other potential conflict of interest relevant to this letter was reported.


TO THE EDITOR: McColl reviews the topic of H. pylori infection and recommends as a first choice of treatment triple therapy consisting of a proton-pump inhibitor, amoxicillin, and clarithromycin or metronidazole. With few exceptions, this triple therapy currently provides unacceptably low treatment success and is not consistently effective in any population.1,2 Regardless of the overwhelming evidence showing unacceptably low treatment success, McColl’s recommendations continue to provide clinicians with the message to use triple therapies containing clarithromycin for first-line treatment. Our recent review shows that the treatment success rate failed to reach even 80% in the majority of studies worldwide (including a recent large U.S. trial) and that the treatment was uniformly unsuccessful (eradication rate, <80%) in France, Italy, Spain, and Turkey.1 We recommend that