Should We Take the C-TraIn to Reduce Hospital Readmissions?

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Innovators around the world are trying to improve health outcomes and the experience of care in a manner that provides good value for money.¹ Trying to achieve all three of these aims simultaneously is never easy, and trying to achieve all three aims when focusing on complex and socio-economically disadvantaged patients in the period immediately after hospital discharge is proving to be especially difficult.

In this issue of JGIM, Englander et al. report the results of a single-center, randomized controlled trial of a multi-component intervention designed to improve post-hospital care and reduce readmissions.² The intervention, called C-TraIn, consisted of transitional nurse coaching, home visits for many patients, the involvement of a pharmacist, free medications for the uninsured and increased efforts to link patients to primary care. Englander et al. did not find a difference in 30-day readmissions or in emergency department visits between the C-TraIn group and the control group. However, patients in the C-TraIn group were more likely to report that their care plan was patient-centered and that they understood it. The investigators also found a small but statistically significant difference in mortality favoring C-TraIn, but they appropriately caution that this finding “warrants further study and confirmation in larger trials.” Given the small number of deaths in the study, and the negative result for the primary outcomes, the mortality difference may be a spurious finding.

Englander et al. deserve much credit for subjecting the C-TraIn intervention to the evaluative method least prone to bias—the randomized controlled trial.³ In many health care systems, an unfortunate pattern can be seen. A complex intervention is developed by a group of enthusiastic innovators. These enthusiasts are often local opinion leaders who are able to convince decision-makers to provide additional resources to implement the new intervention. A substantial amount of resources are spent designing and implementing the new intervention, but neither the originators of the innovation nor the decision-makers who are providing support insist on a high-quality prospective

evaluation. Sometimes, a randomized trial is even deemed to be unethical because of an unwillingness to expose patients to “usual care.” The intervention is then declared to be a success, sometimes on the basis of anecdotal evidence, but more often based on the results of a biased evaluation, such as a “before-after” study without concurrent controls. The intervention then persists, often for many years and perhaps indefinitely, until one of three things happens. One possibility is that a high-quality evaluation is eventually completed and the evidence supports continuing the intervention. A second possibility is that a high-quality evaluation may suggest that the intervention is at best modestly effective or perhaps even ineffective. In these circumstances, the intervention could be discontinued, and scarce resources directed elsewhere. In practice, a debate about the quality of the research and the importance of local context usually ensues, with many individuals finding it difficult to change their views. A third possibility is that the intervention is discontinued not because of damning evidence, but because of budgetary issues, because the champions of the initiative have moved on, or because of a change in leadership.

Rigorously evaluating complex interventions when they are first implemented is a much better approach.⁴ However, high-quality evaluations of new interventions almost always demonstrate that the intervention was not as effective as its developers had hoped. Such is clearly the case with the C-TraIn trial.

Englander et al. designed their study hoping that C-TraIn would reduce the readmission rate from 25 to 13 %.² The actual readmission rates were 16.1 % in the control group and 14.4 % in the C-TraIn group. (Of note, the findings of an uncontrolled “before-after” study could have been used to argue that C-TraIn cut readmissions by nearly half.) Unsurprisingly, the ~2 % difference in readmissions was not statistically significant.

Although there are many issues raised by the C-TraIn trial that could be explored here, we will focus on two related but distinct questions. First, is a 50 % relative risk reduction in the 30-day readmission rate realistic? Second, is a 10 % relative risk reduction in the 30-day readmission rate important?

Some readmissions are not preventable. Imagine, for example, a patient who is shot by a psychopath in a mass shooting the day after she is discharged. In more common scenarios,

however, whether or not one views a readmission as being preventable depends to a large extent on what one expects from the health care system. Most physicians would agree that a patient who returns in acute decompensated heart failure two weeks after having her furosemide dose cut in half has suffered a preventable readmission. What about a patient with ongoing alcohol use and recurrent admissions for complications of decompensated cirrhosis such as hepatic encephalopathy and tense ascites? Such readmissions might not be easily prevented, but is it accurate to say that they are not preventable at all? Would it not be possible to design a health care system that cared for such a patient in his or her own home or in a clinic setting, or perhaps a combination of the two? Perhaps because of cases like this one, experts disagree profoundly about the proportion of readmissions that may be preventable. At one extreme, the Medicare Payment Advisory Commission has stated that as many as 76 % of readmissions are potentially preventable.⁵ Others have argued that probably fewer than 20 % of readmissions are avoidable.⁶ What proportion of today's readmissions will we be able to avoid in tomorrow's health care system? To borrow from Yogi Berra, it is tough to make predictions, especially about the future.

What if only a small proportion of readmissions are preventable—say 10 %? This would translate very closely to the point estimate observed by Englander et al. Clearly, the C-TraIn trial was not designed to test the hypothesis that the intervention would reduce 30-day readmissions by a such a small amount; such a trial would have needed to enroll thousands of patients. Some might argue that such trials are impractical, or that they are simply not worth doing at all if the absolute benefit is so small. Yet, in cardiovascular medicine such trials have been common for decades now, and the incremental benefits of each new intervention—coronary care units, thrombolysis, aspirin, statins, etc.—coupled with public health interventions, smoking cessation, and better control of hypertension, have accumulated slowly to produce a 70–80 % decrease in cardiovascular mortality over the last half-century.⁷ No single intervention has reduced cardiovascular mortality in a broad population of patients by 50 %, and it is hard to understand why our expectations for quality improvement interventions would be so much higher than they are for drugs and devices.

Reflecting on cardiology's triumphs over the last 60 years, we would propose the following two arguments for those trying to achieve the triple aim for complex patients. First, it is unlikely there will be a single intervention—even one with multiple components—that reduces hospital use by 50 % while also improving the patient experience and lowering cost. Second, there are probably a reasonably large number of interventions that could reduce readmissions by a smaller amount—5 to 15 %—and also improve the patient experience. Not all of these interventions will save money. Some of these interventions will, like C-TraIn, be directed at individual

patients. Others may be “system level” interventions—for example, reforms that promote collaborative care, standards for electronic health records that promote information continuity, or quality improvement collaboratives.⁸

The key implication of these arguments is that we should continue to pursue larger studies that will usually involve multiple sites.⁸ We do not pretend that designing and implementing such studies is easy. Not only are larger studies more expensive, the contextual factors associated with complex interventions raise many issues that need to be considered carefully. For example, should the intervention be implemented in exactly the same manner across all study sites? Not necessarily.³ Given that large studies often require extensive collaboration and planning, innovators such as Englander et al. might also consider focusing on experience measures or process measures for smaller studies conducted at their home institution. Even though we might not take the C-TraIn to prevent readmissions, we would happily hop on board to improve patient experience.

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