the intervention or a second comparison group? Are outcomes really improved? One outcome is mortality, and a quick glance at the table suggests that in-hospital mortality is increasing with time. Logistic regression analysis is reported in the Results section, though no mention is made in the Methods of why or how this was done. No denominator for the number of heart failure patients in the practice is reported. Although the authors suggest that a reduced number of admissions resulted from the process, the use of angiotensin-converting enzyme inhibitors in the outpatient settings did not appear to change, as evidenced by its constant rate of use (or nonuse) among those admitted with heart failure. Additionally, data sets such as those used by insurance companies do not classify heart failure based on left ventricular ejection fraction measurement.[2,3] As written, the article serves as an excellent guide to implementing an excellent quality improvement intervention. The lack of a comparison group and the insufficient data available to examine rates of hospital admissions for heart failure prevent us from accepting the conclusions of reduced hospitalizations at this time. Even though the authors' assertions might ultimately prove to be valid, we would encourage more caution in the stating of conclusions.

Paul A. James, MD
Laurene Tumiel, MA
State University of New York
Buffalo

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3. Croft JB, Giles WH, Roegner RH, Anda RF, Casper ML, Livengood JR. Pharmacologic management of heart failure among older adults by office-based physicians in the United States. J Fam Pract 1997;44:382–90.

The above letter was referred to the authors of the article in question, who offer the following reply.

To the Editor: As discussed in the Study Design and Practice Guideline sections of the article, the guideline was introduced at the outset of the study period and revisited each month at our regularly scheduled continuing medical education meetings. Also, as mentioned, the physicians were apprised of their performance data at quarterly quality improvement meetings; therefore, the intervention indeed occurred throughout the study period. As mentioned in the Conclusions section of the article, we believed this was paramount to our success.

We did not measure our performance at any time before the intervention. There was no control population in our study. Our intent was to measure whether the guideline would improve our care for congestive heart failure. It was not our intent to compare our performance to that of another medical group. We believed it would be impossible or unethical to develop a control population of patients within our medical group.

As stated in the Conclusions section of our paper, reducing hospital admissions for systolic congestive heart failure has been a valid outcome measure in a previously published landmark trial. We therefore conclude that outcomes improved throughout our study. Statistical regression was the simplest modeling tool to support our findings. The study was not powered to develop any statistical significance in regard to mortality; therefore, we would reserve judgment relating to any mortality statistics presented.

Because this population was not a closed population, there is no fixed denominator. The statistical relevance of the data, however, lies in the five consecutive quarters that we experienced progressively lower numbers of admissions for systolic dysfunction while recording remarkably steady numbers of admissions for diastolic dysfunction. The only way in which these data could be considered faulty would be if only our systolic congestive heart failure patients somehow self-directed their care to other hospitals. We consider that extremely unlikely.

Selecting only those patients who required admission to the hospital for congestive heart failure as a fair representation of angiotensin-converting enzyme (ACE) inhibitor use within our entire outpatient congestive heart failure practice is in error. In fact, one could intuitively expect that the subset of patients requiring admission would likely have the lowest rates of ACE inhibitor use.

Finally, as stated in the conclusion, we would have preferred to have completed our own measurement of ACE inhibitor use by our physicians in the outpatient setting. The group believed, however, that the additional demands required to complete the audit exceeded our financial and human resources. As a best alternative, Aetna US Healthcare data were used as surrogate data. Though we agree that it is possible, we consider it highly unlikely that the rise in ACE inhibitor use as measured by Aetna US Healthcare was the result of increased use primarily in patients with diastolic dysfunction.

Louis A. Civitarese, DO
Nicholas DeGregorio, MD
Preferred Primary Care Physicians
Pittsburgh

Prenatal Testing and Counseling for Down Syndrome

To the Editor: This letter is in response to the article entitled "Multiple Marker Screening for Down Syndrome—Whom Should We Screen?" by Dr. Sara Cate and Susie Ball (*J Am Board Fam Pract* 1999;12:367–74). An otherwise clear and concise review of prenatal genetic screening was marred by some muddled statements that, I suppose, were meant to reflect ethical issues.

The authors noted that family physicians and internists were more likely than other specialists to interject their own opinions regarding abortion. Male physicians were noted to be more likely than female physicians to

"be directive and deviate from the tradition of nondirectiveness." Physicians were advised to recognize bias in their own opinions about abortion, Down syndrome, and so on, and if such bias existed, to refer to a genetics counselor for pretest counseling.

Now, I suppose it is possible that, somehow, research has shown a tendency toward improved maternal and fetal health among pregnant women who are not subjected to bias for or against abortion, Down syndrome, or other related subjects, when counseled regarding prenatal testing. It is possible but clearly very unlikely, and in any event, no such claim was make by the authors. It is not illegal to express such bias (yet). One is therefore left with the assumption that somehow the authors consider it inappropriate, unethical, or immoral to "be directive and deviate from the tradition of nondirectivenesss" when counseling regarding prenatal testing.

Several questions immediately arise. Whose tradition would be violated? The genetics counselors'? Is this tradition supported by any research into outcomes? Is it supported by any ethical reasoning? Let's be clear: the authors are recommending that, under certain circumstances, a referral is warranted. The referral is not being recommended on the grounds of lack of clinical competency or technical skills, or because a certain specialty is likely to have better clinical outcomes when handling a particular problem. So why is a referral being recommended? Why is being directive bad during genetics counseling when it is routine in other clinical circumstances? No physician thinks twice about being directive when recommending an appendectomy or a coronary artery bypass surgery. Why is counseling concerning prenatal testing any different? Another question concerns directiveness itself. Is there something wrong with being directive? If so, why are the authors being directive by directing us not to be directive? I don't want to be disingenuous. I strongly suspect the real point the authors are trying to make is that it is somehow wrong or unethical for a physician to recommend for or against prenatal testing or for or against an abortion based on the results of such testing. Such a concern might, I suppose, be based on an abortion rights basis or perhaps on a more general patient autonomy principle.

The abortion rights' position, while popular among certain segments of the population (including some physicians), does have certain philosophical problems. For instance, either it must deny the personhood of the unborn baby, or it must make that unborn baby's right to life subordinate to the mother's right to happiness. Other objections could be voiced as well. One must at the very least conclude that the issue is contentious; therefore, being directive based on such a position in a general clinical publication, without acknowledging one's own bias on the issue, is inappropriate. Because not all physicians share this particular bias, it would be more appropriate to acknowledge this bias is why a particular recommendation is being made rather than dogmatically assert (in this particular instance) that referral should be made.

The patient autonomy principle, while more widely acknowledged, is also philosophically questionable. Even if one accepts it at face value, however, many clinicians will acknowledge that there are two persons involved in a pregnancy, not one. My personal autonomy does not give me the right to do anything I want with my own body if, in so doing, I injure someone else. If the unborn is a human being, then it is immoral to kill him, since that would violate his personal autonomy. One can take the position that the unborn is not human. This position is illogical. Even those who reject the logic supporting the personhood of the unborn, however, must at least acknowledge the contentiousness of this issue. It is, again, inappropriate to make clinical recommendations that are based on a bias concerning such a contentious issue without acknowledging that bias.

Jeremy Klein, MD
Louisa, Ky

The above letter was referred to the authors of the article in question, who offer the following reply.

To the Editor: Nondirectiveness does have a long tradition for genetic service providers (both genetic counselors and clinical geneticists) in the Untied States. Nondirectiveness requires sharing all the relevant facts with the patient, but not telling the patient what to do. The issue is not that directiveness per se is bad, but that it is inappropriate in certain situations, including most genetic and prenatal diagnosis situations. Usually in prenatal diagnosis, the physician's knowledge, experience, and wisdom are not the most important factors in deciding a course of action; rather, it is the patient's beliefs and values. Thus, if a provider cannot assist the patient in recognizing his or her core beliefs and determining what course of action is most comfortable for that patient, the provider should refer to someone else who can provide that service.

Sarah Cate, MD, MPH
Susie Ball, MS
Central Washington Family Medicine
Yakima

Delivery of Preventive Services

To the Editor: I would like to comment on the interesting study of predictors of screening for breast, cervical, colorectal, and prostatic cancer by Ruffin and colleagues.[1] In an audit of community-based primary care practices, they made three key observations and one key interpretation. They observed that screening rates were suboptimal, that the strongest predictor of screening was scheduling a health maintenance visit, and that less than one third of patients have annual health maintenance examinations. These observations are in agreement with another community-based primary care audit of about 75,000 adults.[2] Ruffin et al interpreted their findings to suggest that "the promotion of an annual visit to a health care provider to focus [on] preventive services is likely to increase the screening recommendations provided to patients and subsequent delivery of preventive services."