Redesigning After-Hours Primary Care

TO THE EDITOR: The editorial by Margolius and Bodenheimer (1) in response to the successful Dutch after-hours primary care model reviewed by Giesen and colleagues (2) does not mention several essential differences between the U.S. and Dutch health care systems that would make the process of adopting such a model in the United States difficult.

The Netherlands and most of Western Europe have invested in more developed primary care systems than the United States and seem to value primary care physicians more—there is a notably smaller salary gap between specialists and primary care providers in those countries. Specialty societies in the United States are adamant about not reducing their members’ income to enhance that of the primary care sector. Health insurance is mandatory in the Netherlands, enhancing access and limiting out-of-pocket expense. Taking time off from work to see a physician is not as anxiety-provoking. Here, at least in Boston, extending office hours seems only to shift the time of regular patient visits from weekdays to evenings and weekends. Recently, "patient-directed" (that is, higher deductibles and copayment) insurance plans have led to more telephone calls during off hours to manage patient problems without an office visit, and its fee.

Primary care providers are disproportionately burdened in the United States. Too many hours, administrative hassles, less compensation, and the unmistakable feeling of an increasingly salary-threatening schizophrenic demand to boost productivity while being tasked with constraining health care system costs does not make for a very attractive career choice. The creeping "conditional" nature of the mechanism for enhancing one’s salary is most marked in primary care. Surgeons and specialty physicians do not conditionally generate additional income by not operating or by performing fewer procedures. Asking primary care organizations to now assume a disproportionately large share of financial risk for the provision of care to a population of patients only worsens the environment. Medical student internship choices show that idealism and altruism—often accompanied by substantial debt—will significantly limit those same choices, more often away from primary care.

Accountable care organizations will certainly promote less emergency department use to reduce cost or financial risk, but the elephant in the boardroom will be the one who gains control of the organization. It’s not only hospital administration versus physicians—there could be “blood on the floor” as specialists and primary care physicians vie for the power of the purse. Experience unfortunately holds out scant hope for increasing the value of primary care cognitive services in the accountable care organization as opposed to the procedure-oriented services of the specialties.

My longitudinal view of 30 years in primary care—from friendly to managed competition; to oppressive managed care HMO gatekeepers; to expensive preferred and exclusive provider organizations; to urgent, large-scale institutional consolidation; to cut-throat competition; to high deductible, cost-shifting, patient-directed plans, and currently accountable care organizations—parallels the clear decline in its practice environment, prestige, relative compensation, and numbers of newly minted primary care physicians.

Until our country’s health care system radically evolves to a value-driven (that is, primary care–driven) institution that covers all Americans, we can only admire how far ahead of us—at least in certain system-related aspects—are the Dutch and the other European primary care structures. All the while, our patients experience even longer waits and incur even greater expense in our state-of-the-art emergency rooms.

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Potential Conflicts of Interest: None disclosed.

References
A Clinical Practice Guideline Update on the Diagnosis and Management of Stable Chronic Obstructive Pulmonary Disease

TO THE EDITOR: We read with disappointment the first recommendation of recently published guidelines (1) on the diagnosis and management of stable chronic obstructive pulmonary disease (COPD). This recommendation states, “Spirometry should not be used to screen for airflow obstruction in individuals without respiratory symptoms” (1). In our opinion, this would be like recommending that fasting blood glucose not be measured in obese patients until angina develops. Even if spirometry does not in itself modify the underlying risk (smoking cessation) or warrant immediate treatment (use of bronchodilators), airflow limitation is a marker of premature death from all causes—in particular, heart attack and lung cancer (2, 3).

We believe this recommendation reflects an unduly nihilistic attitude toward the wider use of spirometry, primarily owing to its poor implementation to date rather than poor clinical utility. By not screening high-risk individuals, such as chronic asymptomatic smokers, we are losing a teachable moment that demonstrates to smokers their inherent susceptibility and irreversible end-organ damage.

That smokers do not quit smoking in greater numbers in response to poor spirometry results is possibly because physicians do not sufficiently emphasize the substantial increased risk for heart attack and lung cancer conferred by reduced FEV1 (2, 3). Moreover, if we wait for symptoms before spirometry is offered, many smokers will have irreversibly lost as much as 50% of lung function (2). Evidence shows that the greatest potential to optimize lung health (preservation of lung function and reduced lung cancer risk) comes from quitting smoking before significant airflow limitation is established.

Another lost opportunity would be in identifying smokers most at risk for lung cancer (3) at a time when computed tomography (CT)—based screening for lung cancer seems to show a significant survival benefit (4). For lung cancer screening to be widely and cost-effectively adopted, it is necessary to identify current and former smokers at greatest risk, over and above that conferred by age and exposure (3).

If we abandon spirometric screening of asymptomatic smokers, we will lose this opportunity, as we and others have shown reduced FEV1 confers a 6-fold greater risk for lung cancer compared with smokers with normal lung function (5). We believe that the utility of spirometry as a diagnostic test is far outweighed by its ability to establish end-organ lung damage, increased all-cause mortality, and targeted risk-mitigating interventions. In our view, to abandon spirometry in asymptomatic smokers will certainly promote (if not worsen) the continued underdiagnosis and underresourcing of COPD.

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Potential Conflicts of Interest: Dr. Young and the funding of his research have been supported by grants from the University of Auckland, Health Research Council of New Zealand, and Synergene BioSciences Ltd.

References
2. Young RP, Hopkins R, Eaton TE. Forced expiratory volume in one second: not just...
a lung function test but a marker of premature death from all causes. Eur Respir J. 2007;30:616-22. [PMID: 17906084]

IN RESPONSE: We thank Dr. Young and Ms. Hopkins for their comments regarding the recent joint clinical guideline from the American College of Physicians, American College of Chest Physicians, American Thoracic Society, and European Respiratory Society on the diagnosis and management of stable COPD. However, we respectfully disagree with their argument that the recommendation against spirometry for screening asymptomatic patients is nihilistic.

There are agreed-upon criteria that define when to consider screening (1): There should be an accepted treatment available, and if treatment is started at an early stage, it must be beneficial and change clinical outcomes compared with waiting until patients develop signs or symptoms of disease. The current evidence indicates that identification and treatment of individuals with asymptomatic airflow obstruction does not improve clinical outcomes and that spirometry does not act as a motivator to help patients stop smoking (1–4).

In addition, we found no evidence to support the use of routine periodic spirometry after initiation of therapy to monitor disease status or to guide therapy modification. On the contrary, spirometry in asymptomatic patients may be associated with such harms as “labeling,” follow-up visits, repeated office spirometry, full pulmonary function tests with bronchodilator testing, lung imaging, and use of unnecessary and ineffective treatments (5). With this evidence in mind, there is no net benefit to obtaining spirometry in asymptomatic individuals.

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Potential Conflicts of Interest: Disclosures can also be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=M11-0925.

References

NICE Guideline for Management of Chronic Heart Failure in Adults

TO THE EDITOR: The National Institute for Health and Clinical Excellence (NICE) guidelines presented by Mant and colleagues (1) for management of chronic heart failure in adults describe the problem as “a complex clinical syndrome.” The diagnostic algorithm begins with a detailed history and a clinical examination.

However, the authors then say, “Clinical signs and symptoms are of limited use in the diagnosis of heart failure” (1). This must surely be a misprint.

The next node down in the algorithm is either “Specialist assessment and Doppler echocardiography” or “Measure serum natriuretic peptide.” Are these the real tools to diagnose of heart failure? It seems to be another example of gizmo idolatry (2).

Clinical evaluation is central to nearly all complex clinical syndromes; history is almost everything in diagnosing ischemic heart disease, stroke, dementia, and more. Downplaying history and physical examination is harmful to physicians and their patients in many ways. And it seems to belittle the diagnosis and management of heart failure by physicians around the world who lack access to these technologies and must rely solely on clinical skills.

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References

TO THE EDITOR: I accept the evidence-based suggestion in Mant and colleagues’ article (1) that specialists and coordinated care in heart failure clinics can create better outcomes than “usual” care. But I cannot find the evidence base that supports the need for specialist assessment to diagnose chronic heart failure in patients with prior myocardial infarction.

Generalists are capable of reviewing echocardiogram reports (generated by appropriately trained echocardiographers) that tell them whether a patient has preserved ejection fraction, systolic dys-
function, or other cardiac abnormality, which can help to further classify patients with heart failure. If these physicians are able to diagnose chronic heart failure in patients without myocardial infarction, they ought to be able to do so in patients with prior myocardial infarction.

I have no problem with referral to cardiologists or chronic heart failure management clinics at this point, but I do not see the evidence for referral to a “specialist assessment” to make the diagnosis in the first place.

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Potential Conflicts of Interest: None disclosed.

Reference

IN RESPONSE: We are grateful to Dr. Finucane for his comments and agree with him regarding the central role of history and physical examination in the evaluation of heart failure (and other complex clinical syndromes). It is through history and examination that a clinician will suspect heart failure as a diagnosis and consider potential aggravating factors and alternative diagnoses (1). However, the limitations of symptoms and signs alone need to be acknowledged.

A systematic review of the evidence found that individual symptoms and signs are of limited use in the diagnosis of heart failure (2). Furthermore, diagnoses of heart failure by generalists may be inaccurate: A review of 103 patients with a diagnostic label of heart failure in primary care in the United Kingdom confirmed this diagnosis in 35 patients (34%) after echocardiography and examination (3).

Echocardiography is required in persons with suspected heart failure not only to help establish whether the syndrome is present but also to help identify the underlying cardiac abnormality, such as left ventricular systolic dysfunction or valve disease. This is vital, because subsequent management is underpinned by such knowledge (1). There is a strong evidence base that measurement of serum natriuretic peptides is useful to rule out heart failure and thus can reduce the use of echocardiography in people with normal heart function (1, 2). The central role of natriuretic peptides and echocardiography in the NICE guideline does not reflect “gizmo idolatry,” rather a considered application of the principles of evidence-based medicine.

We are also grateful to Dr. Simel for his comments, but we believe that he may have misinterpreted the algorithm. The guidelines recommend that all patients with heart failure should have specialist assessment and echocardiography. People with a history of myocardial infarction who present with symptoms and signs suggestive of heart failure have a higher likelihood of a positive diagnosis than someone without such a history (2). Cost-effectiveness analysis suggests that it is more cost-effective to refer such people immediately for echocardiography and specialist assessment without the prior use of natriuretic peptide testing as a “rule out” test (2). In the case of patients with symptoms and signs of heart failure but no history of myocardial infarction, the diagnostic algorithm recommends that the generalist refer them for echocardiography and specialist assessment if serum natriuretic peptide levels are increased.

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Potential Conflicts of Interest: Disclosures can also be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=M11-0261.

References

Hip Fracture: A Complex Illness Among Complex Patients

TO THE EDITOR: Hung and Morrison’s editorial (1) on management of hip fracture does not address the patient in whom hip fracture will not be surgically repaired. I have had only a couple such patients who made the decision not to have surgery and were more than a few days from death. Both were competent to make their own choices, and despite encouragement to reconsider, these elderly patients stood firm and chose to die in their beds.

In caring for these patients, I could not convince anesthesiologists to consider regional approaches that might mitigate the need to use substantial narcotic analgesia and that might make it easier to maintain hygiene and to keep skin intact. PIER (http://pier.acponline.org) does mention the need to provide “adequate” doses of narcotic analgesia in persons who will not have surgical repair, thus at least noticing the problem. However, regional anesthesia could be continuous (for example, epidural) and could allow for a much more comfortable course. We should test this approach and report on the results so that patients and their physicians who choose this course have the benefit of prior experience.

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Potential Conflicts of Interest: None disclosed.
TO THE EDITOR: Hung and Morrison’s editorial (1) states that high-quality care requires optimization of 5 core elements. Most hip fractures are secondary to osteoporosis. I was, therefore, surprised to see that there was no mention in the editorial of screening and treating osteoporosis as part of the treatment plan.

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Potential Conflicts of Interest: None disclosed.

Reference

IN RESPONSE: Although the vast majority of hip fracture patients are treated surgically, approximately 60% do not receive surgery (1). There is a paucity of data to guide treatment decisions in this population, and well-designed prospective studies are needed. In the absence of such data, extrapolation from studies that have used regional techniques (for example, femoral nerve blocks) before and after surgical repair suggests that they are viable and effective options for reducing systemic opioid requirements and improving the management of hip fracture pain (2). A more definitive study on efficacy is currently under way. Nevertheless, pain control in patients undergoing nonsurgical care may require a combination of regional block and a systemic analgesic program.

Dr. Dixit makes an excellent point that osteoporosis care is an important aspect of post–hip fracture care. Treatment with bisphosphonates—namely zoledronic acid, a mainstay of osteoporosis treatment—initiated within 90 days after fracture may reduce clinical fractures and mortality at a median follow-up of 1.9 years (3). However, the mechanism of the mortality benefit seems to be mostly related to reduction in cardiovascular events and pneumonia rather than in subsequent fracture prevention (4). The role of osteoporosis screening and treatment in the immediate perioperative period is less clear and as such was not addressed in our editorial.

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Potential Conflicts of Interest: Disclosures can be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=M11-1559.

References

Correction: Therapy-Related Acute Myelogenous Leukemia

Four authors were inadvertently omitted from a recent letter (1). These authors and their affiliations are as follows: Deepika S. Darari, Irina Maric, Zachariah McIver, and Diane C. Arthur.

This has been corrected in the online version.

Reference

Correction: Diagnostic Imaging for Low Back Pain

In the guideline by Chou and colleagues (1), the third statement in the section “Defer imagining after a trial of therapy” in Table 4 should read as follows: “Risk factors for vertebral compression fracture (history of osteoporosis, use of corticosteroids, significant trauma, or older age [≥65 y in women and >75 y in men]). This has been corrected in the online version.

Reference

Correction: Oseltamivir Compared With Chinese Traditional Therapy

The Patients section in the abstract of a recent article (1) should read as follows: 410 persons aged 15 to 69 years with laboratory-confirmed H1N1 influenza.

This has been corrected in the online version.

Reference

Correction: Liquid-Based Cytology and Human Papillomavirus Screening for Cervical Cancer

The first full paragraph on page 693 (1) of a recent article should read as follows:

Reference
Four large, fair-quality RCTs compared cotesting with cytology screening alone in European women aged 30 to 64 years (Table 3) (46–49). These 4 studies included NTCC phase 1, POBASCAM, Swedscreen, and ARTISTIC, and comprised 82,390 participants. In contrast to HPV screening alone, cotesting did not detect more CIN3+ after 2 screening rounds than cytology alone (Table 3). Round-specific screening results were not completely consistent. Generally, cotesting detected relatively more CIN2+ (and sometimes CIN3+) after 1 screening round, compared with cytology alone. Cotesting identified less CIN3+ (and, where reported, cancers) after the second round, tending toward fewer cancers cumulatively. Interpreting these mixed results is complicated by uncertainty about the completeness of outcome ascertainment for cancers and high-grade precancers due to between-trial differences in duration and completeness of follow-up for the entire screened population and screening episode (15); lack of consensus on the appropriate surrogate outcome (15), with possible asymmetry bias due to limited long-term follow-up (1); and screening protocol differences (Table 3 and Appendix Table) between trials and with U.S. practice.

Also, on the right-hand column on page 695, in the first full paragraph, the third sentence should cite reference 67; the fifth sentence of that paragraph should cite reference 1, not reference 67. This has been corrected in the Online version.

Reference