TO THE EDITOR: I want to thank Stillman (1) for his thoughtful article on concierge medicine. However, I believe that his perspective is colored by assumptions and biases that may not be valid. The first assumption is that his role as a physician is to give patients what he or some third party decides they need rather than what the patients want. Although these 2 goals are ideally the same, more often than not, they diverge. It is unfortunate that the present payment system has created bizarre incentives that foster overconsumption of frequently useless and even dangerous medical interventions and actions. The present payment system can be used to justify inserting physicians as gatekeepers to prevent patients from getting what they want. However, we should not forget that we are basically in a service industry and that ideally our focus should be to help patients reach their goals, not ours.

Stillman may not understand that concierge medicine is growing because it delivers a service not covered by insurance: access to physicians whenever patients want to see them, rather than when they need to see them. Who are we to say that patients with financial resources should not be able to contract with physicians to purchase what insurance will not pay for?

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Potential Conflicts of Interest: None disclosed.

Reference

TO THE EDITOR: As a general internist who worked in a “regular” group practice for 15 years and in an independently designed retainer practice since 2003, I feel both qualified and compelled to respond to Stillman’s essay (1).

I do not collect or accept money from any insurance company or from the government. This simple decision removes ethical or contractual concerns about “double-dipping,” eliminates the immense complexities and headaches of billing, and greatly reduces paperwork. In exchange for surrendering that insurance money, I am freed of the rules and restrictions of the insurance companies, which simplifies my life immeasurably.

By taking an hour or more with most of my patient visits and by accepting their telephone calls and e-mails, I am confident that I actually order fewer of the expensive tests Stillman refers to than do physicians who spend just a few minutes with each patient. Also, each patient I see in his or her home (about 100 per year) instead of in a costly and overburdened emergency department keeps patients and their families from waiting hours to see a doctor they do not know and who may order costly tests they do not need.

Contrary to the most common criticism, retainer practices need not only select for wealthy patients. I have patients from every walk of life who have chosen my practice, and 99% of them renew each year. The cost is $29 per week, which means my patients value our relationship and their medical care as much as they value their cable TV service and their cell phone contract. I reserve 15% of my spaces for patients who pay a reduced fee or none at all, depending on their ability to pay. I also have the time now to volunteer at a community clinic several times a month, which I could not do before.

General internal medicine is critically ill, but I believe that lower salaries aren’t the only reason. It is time-consuming and demoralizing to fight insurance companies every day for our patients’ welfare. And, from an ethical standpoint, it should be noted that money is not purified by first passing through an insurance company.

Stillman suggests that doctors who leave the current “system” are abandoning their patients. But unhappy internists will leave clinical medicine or retire early, and they are doing so in record numbers. The future of internal medicine does not lie in an ever-dwindling number of increasingly dissatisfied doctors. Only by transforming our field to one that medical students see as enjoyable will we attract sufficient quantities of the best students to keep our specialty viable and, in that way, continue to serve our patients.

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Potential Conflicts of Interest: None disclosed.

Reference
TO THE EDITOR: In his Perspective essay, Stillman (1) misses an important point. I sense that today’s primary care physicians, especially those in direct employment with a hospital or health group, have a “shift mentality”—that is, they work their 8-to-5 shift and go home. Concierge doctors fill the need for customers seeking time and access. Also, instead of only coming in to see a doctor when a patient has a “complaint,” a concierge practice fosters wellness through prevention. I admit I do a better job when I get to spend time with each patient, rather than getting rushed from patient to patient every 15 minutes. Finally, insurance reimbursement is a travesty. How is it that I bill $75 for a visit and get only $14 back? Insurance reimbursement needs to change. Doctors need direct reimbursement, not the obtuse structure that exists today.

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Potential Conflicts of Interest: None disclosed.

Reference

Finally, Stillman asserts that the patients in such practices are rich. In fact, the tax-adjusted cost of membership is about the same as maintaining a cell phone, about $85 per month. In my practice, few are rich. Some are retired school teachers, others are office clerks, and some are bus drivers. When the facts on concierge medicine are well reviewed, the conclusions drawn by Stillman must change.

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Potential Conflicts of Interest: None disclosed.

Reference

TO THE EDITOR: I read Stillman’s Perspective essay (1) with concern. He asserted that very little is known about such “luxury” practices and cites “well-written” articles that question the ethics of those who provide such services. An investigation of the subject reveals data that demand a very different opinion.

A statistical analysis by Applied Quantitative Sciences (Goldman E. MDVIP Hospital Utilization Data, Medicare and Commercial, 2004–2006. In preparation.) has shown that Medicare beneficiaries in such practices have only 56.4% as many hospitalizations as those of matched nonparticipating Medicare beneficiaries, and that commercially insured patients aged 25 to 65 years had 61.5% fewer hospitalizations. The total savings to the country if all 36 million Medicare patients were in such a program would be about $54 billion. Add to this the savings from the population aged 25 to 65 years, and the savings would be vast and would repay the public moneys spent on training internists many times over.

In addition, Stillman confuses practices that provide for prepaid preventive care evaluations with those that use retainers. Personalized care contracts provide for preventive care screening and health maintenance planning, which is not covered by Medicare or most commercial contracts. The improved physician availability found in personalized care practices is a result of contracting physicians choosing to keep their practices small so that they can do their job well.

Stillman is correct in asserting that “[p]atients deserve and desire our fullest attention and consideration.” Sir William Osler declared that physicians could only see 4 patients a day if their work was to be done well. “Regular” physicians currently allocate 10 to 12 minutes per visit. This is certainly not what Osler had in mind.

Potential Conflicts of Interest: None disclosed.

Reference
patients and doctors could be what this country needs to climb out of the doldrums and distrust we have found ourselves in over the past decade.

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Potential Conflicts of Interest: None disclosed.

References

Population Strategies to Decrease Sodium Intake

TO THE EDITOR: Smith-Spangler and colleagues (1) strategize how to reduce dietary sodium in entire populations and countries for the benefits they assume will result. Unfortunately, their assumptions are incorrect. They state that “sodium consumption raises blood pressure, increasing the risk for heart attack and stroke.” This is true only in a small percentage of the population. Most persons (especially white persons) are not salt-sensitive, as shown in a Cochrane review (2), and salt restriction does not achieve better survival, as shown in NHANES I (First National Health and Nutrition Examination Survey) (3), although it may reduce blood pressure somewhat. Furthermore, the Japanese consume the largest amount of salt (>20 g/d) in the world, and yet they have the longest life expectancy.

The best way to treat hypertension is to elucidate its underlying cause in each patient and treat that, rather than assuming that all hypertension is the same and should be treated the same way (4).

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Potential Conflicts of Interest: None disclosed.

References

TO THE EDITOR: If we expect to reduce salt consumption to improve the cardiovascular health of the nation, we have to involve patients and the general public in making informed decisions through education instead of the one-sided approach proposed by Smith-Spangler and colleagues (1). The media have covered high-

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References
IN RESPONSE: As Dr. Tapolyai notes, the blood pressure response to reduced sodium intake varies. Our model accounts for observable causes of such variation by adjusting for age, sex, and hypertension. However, overall sodium reduction is clearly an effective strategy for controlling blood pressure. Many studies have shown that sodium reduction leads to lower blood pressure in white persons as well as black persons. The Cochrane review (1) cited by Dr. Tapolyai found that in 58 trials comprising mostly white persons with hypertension, low-sodium diets reduced systolic blood pressure by an average of 4.18 mm Hg when compared with a high-sodium diet. In addition, in the control group of the DASH (Dietary Approaches to Stop Hypertension) study (2), lower levels of sodium consumption led to lower blood pressure in many subgroups, including white persons, black persons, persons younger and older than 45 years, and those with and without hypertension.

Readers may be interested in the letters (3) accompanying the analysis of the NHANES study cited by Dr. Tapolyai. They point out that sodium and calorie intake measurements in NHANES may have been inexact, because the survey relied on dietary recall and did not measure urinary sodium levels, which is the gold standard in assessing sodium intake (3). A meta-analysis of prospective studies of salt intake in 19 independent cohorts (4) found higher sodium intake to be associated with a significantly higher risk for stroke.

We agree with Drs. Narayan and Kumar that consumers could benefit from education about the sodium content of foods. We are unaware of any evaluation of the effectiveness of a “black box” warning label on salty foods. However, if few low-sodium alternatives are available, it is difficult for even the most informed consumer to reduce sodium intake. For consumers who eat out often, the total sodium consumption from restaurant meals can be substantial; some contain more than 5000 mg of sodium (5), more than double the total daily recommended maximum of 2300 mg.

Finally, we did not evaluate the effects of laws mandating reduced salt in restaurants, nor did we evaluate bans on salty foods. Instead, we evaluated a strategy in which industry voluntarily works with the government to reduce the amount of sodium in processed and prepared foods, as has been done in the United Kingdom.

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Potential Conflicts of Interest: None disclosed.

References

Errors in Meta-analysis Regarding the 3CPO Trial

TO THE EDITOR: As co-investigators on the 3CPO (Three Interventions in Cardiogenic Pulmonary Oedema) trial (1), we were interested in the meta-analysis of noninvasive ventilation by Weng and colleagues (2). We found that the authors of the meta-analysis misrepresented 2 quality criteria and the prevalence of myocardial infarction in relation to the 3CPO trial. First, the authors incorrectly stated that the 3CPO trial did not have allocation concealment. Although 3CPO was an open trial, participants were allocated to a treatment group by a telephone randomization system after they had been irreversibly entered into the trial. Second, the withdrawal rate for 3CPO participants was quoted in the text as 19.4% and in Appendix Table 2, which provided quality assessments based on the PEDro (Physiotherapy Evidence Database) scale, as more than 15%. Presumably, the authors made this calculation by using the 205 participants who did not complete their allocated treatment. In reality, the primary outcome for 3CPO (mortality status assessed at 7 days) was recorded for 1062 of 1069 participants (99%). Third, the authors state that only about 20% of 3CPO participants had myocardial ischemia or infarction, whereas according to the universal definition, about 50% of the 3CPO population had myocardial infarction. We have not checked whether similar errors have been made in relation to other studies in their review, but we request that the subgroup analysis on study quality and baseline myocardial infarction or ischemia be corrected for our study.

The meta-analysis raises an interesting point: Why did the results of the 3CPO trial differ from those of previous studies? We believe that the trials included in the meta-analysis are markedly heterogeneous (for example, clinical setting, study population, and entry criteria) and that the 3CPO trial asked a different type of research question. It was a pragmatic trial aimed at determining the effectiveness of noninvasive ventilation as a first-line treatment of severe acute cardiogenic pulmonary edema in patients presenting to the emergency department. Selection of patients and treatment protocols was designed to replicate routine practice and, as noted, patients who did not respond to standard therapy were allowed to cross over to noninvasive ventilation. The 3CPO trial showed that mortality outcomes of patients receiving standard oxygen therapy followed by noninvasive ventilation if they do not respond to initial treatment is similar to those of patients receiving noninvasive ventilation as first-line treatment. It does not exclude a role for noninvasive ventilation in certain patients or negate previous conclusions that noninvasive ventilation can improve symptoms and clinical outcome.

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Potential Conflicts of Interest: All authors are co-investigators on the 3CPO trial.

References


IN RESPONSE: We thank Dr. Goodacre and colleagues for their careful review and comments. We unfortunately judged allocation concealment incorrectly for 3CPO and 2 other studies (1–3). Subgroup meta-analysis of trials with allocation concealment properly classified for these trials still shows an advantage of continuous positive airway pressure over standard therapy for mortality (relative risk, 0.58 [95% CI, 0.37 to 0.90]) and need for mechanical intubation (relative risk, 0.43 [CI, 0.28 to 0.66]).

We also thank Dr. Goodacre and colleagues for pointing out that the primary outcome in 3CPO was assessed in almost all participants. We did indeed mistakenly count crossovers as withdrawals. Results of cumulative meta-analysis by trial quality did not substantively change when we properly classified all trials with allocation concealment or when we counted the 3CPO trial as having a withdrawal rate less than 15% (data not shown).

We based assessments for myocardial infarction on traditional criteria from the World Health Organization rather than the universal definition (4), because most other trials that reported myocardial infarction or ischemia data used criteria from the World Health Organization. Most trials of noninvasive ventilation included in our analysis were published before consensus reporting about the universal definition. It would be difficult to classify cases from those reports by the universal definition and perform reliable analyses using the new definition. The legend to Figure 4 specifies the regression equation that readers can use to estimate the risk ratio for mortality with continuous positive airway pressure compared with standard therapy using different proportions of trial participants with acute myocardial infarction or ischemia. Interested readers can contact us for regression equations to assess changes in risk estimates with changes in proportion for the review’s other comparisons (bilevel ventilation vs. standard therapy, continuous positive airway pressure vs. bilevel ventilation) and outcome (need for intubation).

We agree that patient variability and differences in trial design, inclusion and exclusion criteria, and target populations are important in summarizing this body of evidence. We stand by our overall conclusion that available evidence supports the use of noninvasive ventilation for patients with acute cardiogenic pulmonary edema.

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Potential Conflicts of Interest: None disclosed.

References


CLINICAL OBSERVATIONS

Delayed Complications From Exposure to Improvised Explosive Devices

Background: Immediate injuries from improvised explosive devices (IEDs) have been described elsewhere (1), and an awareness of delayed injuries, such as traumatic brain injury, is emerging.

Objective: To describe a case of delayed injury from an IED, which has not been reported before.

Case Report: A U.S. Army soldier aged 31 years was exposed to IEDs on 3 occasions over 9 months in Iraq. The explosions occurred while he was in a vehicle and caused him to be displaced within the vehicle. During each exposure, he was wearing protective body armor that covered his torso and he did not have penetrating trauma. He reported bruising to his chest wall associated with myalgia for a few days. On 1 occasion, he had a concussion. He did not seek formal medical attention immediately after these exposures.

A few months before leaving Iraq to return to the United States, he began to have new-onset, lower-extremity edema and exertional fatigue. After his return, he began to have progressive symptoms of worsening lower-extremity edema, abdominal fullness, fatigue, exertional dyspnea, and early satiety. His quality of life declined significantly as his active lifestyle was progressively curtailed.

Magnetic resonance imaging showed a thickened pericardium (Figure), diastolic ventricular septal motion suspicious for exaggerated ventricular interdependence, a plethoric inferior vena cava, large left pleural effusion, ascites, and hepatosplenomegaly. Transthoracic echocardiography showed severely elevated right atrial pressure but was indeterminate as to whether dissociation of intrathoracic and intracardiac pressures was present. At catheterization, discordance of right and left ventricular pressures with respiration was shown. The collection of evidence favored a diagnosis of pericardial constriction. Before pericardiectomy, his right atrial pressure was 34 mm Hg and decreased to 18 mm Hg after the procedure. Pathology examination showed fibrous thickening without granulomas. The patient feels much better, and his symptoms have resolved completely.

Discussion: We believe that blunt chest trauma from a tertiary blast injury was a delayed-onset cause of this soldier’s pericardial constriction (2). Others (3) have postulated that pericardial injury and hemopericardium, which in less severe, acute cases may be clinically asymptomatic at the time of exposure, may play a role in pathogenesis.
Conclusion: As more U.S. soldiers return from duty in Iraq and Afghanistan, where exposure to blast injuries is not uncommon, clinicians should be aware of the delayed signs and symptoms that can be attributed to these experiences.

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Potential Conflicts of Interest: None disclosed.

References

The Ice Diet

Calorie-deficient weight loss diets are a cornerstone of obesity treatment. There is a systemic error in calorie determinations in commonly available foods. Recognition and clinical use of the consequences of this error may improve the outcomes of dietary management of obesity.

Most calorie determinations are made by identifying the protein, carbohydrate, and fat contents of foods and by using their known contribution to energy: protein, 4 kcal/g; carbohydrate, 4 kcal/g; and fat, 9 kcal/g (1). Complex carbohydrates, having less available energy due to incomplete digestion, are estimated to provide 2 kcal/g (2).

Water in food has not been considered to contribute to energy metabolism, because it has no calories that can be used for basic metabolism. However, water has energy content. In the average home freezer, the temperature is −20 °C. Under those circumstances, water freezes into ice. If one were to ingest 1 L of ice, it would first have to be warmed from −20 °C to 0 °C, which would require 20 calories of energy [20 kcal or 20 dietary Calories]. When ice at 0 °C melts to water at 0 °C, approximately 80 Calories of additional energy are required. This energy is referred to as the water’s heat of enthalpy. Water at 0 °C must then be warmed to 37 °C (normal body temperature). Therefore, a patient ingesting 1 L of ice would burn approximately 140 Calories of energy to melt the ice and warm the resulting water to body temperature. Because the body’s metabolism is not completely efficient, the actual stored energy used to melt 1 L of ice would be greater than 140 Calories. This inefficiency has been estimated to be between 10% and 20%, which increases the energy deficit to 154 to 168 Calories (estimated 160 calories). This is the same amount of energy as the calorie expenditure in running 1 mile.

Just as we correct the calories for complex carbohydrates, we also should correct the calories for foods that are intended to be eaten when frozen, like confectionary ices, whose calories should be decreased by 160 kcal/L. The resulting calories would be actual ingested net calories, which could be called “ice calories” or “icals.” These observations suggest that ingestion of large quantities of ice can lead to a clinically relevant calorie deficit and can be used as a dietary technique to help induce weight loss.

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References
Correction: Noninvasive Ventilation in Acute Cardiogenic Pulmonary Edema

In their meta-analysis (1), Weng and colleagues incorrectly classified 3 trials as having no allocation concealment. The “Concealed Allocation” cells in Appendix Table 2 for Bersten et al (1991), Nava et al (2003), and Gray et al (2008) should be “yes” rather than “no.” The Table shows relative risk estimates from subgroup analyses by allocation concealment using these correct classifications. The meta-analysis and Appendix Table 2 incorrectly gave the withdrawal rate for the 3CPO trial as 19.4% and more than 15%, respectively, although the primary outcome was assessed in 99% of trial participants. Cumulative meta-analysis results based on quality scores used the incorrect assessments (in the text and in Appendix Figures 4 and 6). Corrected cumulative analyses by trial quality were qualitatively unchanged.

This has been corrected in the online version.

Reference

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CPAP = continuous positive airway pressure; MI = myocardial infarction; NA = not available.

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