

## COMMENTS AND RESPONSES

## Implications of Regional Differences in Spending

**TO THE EDITOR:** Fisher and colleagues' study (1) questioned "the notion that additional growth in health care spending is primarily driven by advances in science and technology. . ." in the United States. Shine's accompanying editorial (2) suggested that "poorer access to care may increase costs." I agree and offer further evidence to support their statements.

When the uninsured population and Medicare costs are examined in the 50 states (Table 1), higher uninsured rates (3) correlate with higher per capita Medicare costs (4). In addition, states with the largest percentage of uninsured people have nearly 50% more per capita Medicare expenditures compared with the "low uninsured" states. Some of the extra costs might be explained by cost shifting, ethnic or racial backgrounds, or costs of graduate medical education programs, but these reasons alone cannot explain all of the differences. Of interest, it also appears that more people are living longer in the lower uninsured states with larger percentages of the Medicare population.

Why would the uninsured problem in the United States lead to higher Medicare costs? Lack of insurance and high out-of-pocket medical costs are major reasons for delayed health care (5). Over 40 million Americans are uninsured, and even more are underinsured. Often their illnesses are less likely to be diagnosed or treated in a timely manner compared to those of the "well insured." As a practicing internist and geriatrician, I have seen numerous uninsured and underinsured persons go untreated or undertreated because they could not afford decent health care earlier in their lives. When inadequately insured people eventually reach Medicare enrollment, they are often "sicker" and cost even more to treat than insured Americans (6).

Did Fisher and colleagues look at the Medicare patients' health insurance status before Medicare enrollment or their quality of prescription coverage in Medicare in relationship to Medicare spending? I believe they would find that delayed access to care (lack of previous health insurance or lack of previous or current prescription coverage) in the United States produced some of the regional Medicare spending differences and could further explain much of the exploding health care costs in the United States.

Would per capita Medicare costs fall even further in the United States if all people had decent health insurance? The answer is yes. Poor access to care does increase costs. Medicare costs will not be controlled until quality health insurance and access are provided to all people in this nation, including patients not currently on Medicare.

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**Table 1. U.S. Uninsured Population and Medicare Costs in 1998**

State's Rank in Percentage of Uninsured Persons (High to Low)	Uninsured in the Total Population, %*	Medicare Cost per Enrollee (Total Population), \$ (%)*
1st-3rd (top 3)	23.1	6024 (11.5)
1st-20th (top 20)	19.9	5914 (13.6)
21st-50th (bottom 30)	12.5	4945 (14.5)
46th-50th (bottom 5)	9.4	4100 (15.0)
U.S. average	16.3	5346 (14.4)

\* Weighted averages.

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**TO THE EDITOR:** For the past 7 centuries, an enduring underpinning of western society has been a belief in basic science and its applications as a vehicle of human betterment. The studies of Fisher and colleagues (1, 2) directly challenge the notion that bringing cutting edge biomedical research to the bedside yields value. If one were to extrapolate the conclusions of their presentations and apply them to public policy, several initiatives would seem appropriate.

1) Train fewer or, even better, no future doctors, since physicians are vehicles of cost.

2) In lieu of suggestion number 1, train physicians to a very limited level of sophistication, since those trained for a more prolonged time and in a more intensive fashion are even greater profligate spenders.

3) Eliminate university-based training centers for physicians and return to the pre-Flexnerian halcyon days of medicine when medical costs were an immeasurably small fraction of the gross domestic product.

The troubling results of Fisher and colleagues' study are that many of the very "hot spots" of "wasteful" spending include the great medical education and research institutions of the United States. These very same sites are the source of the few clinical pathways we currently have to guide "cost-effective" medical care and will inevitably be the resource to develop future models of care.

The ultimate truth about Medicare spending is that the availability of dollars has encouraged the use of "high-tech" medicine at its most intensive site: the care of frail seniors (3, 4). New and often minimally tested technologies will inevitably be applied in the setting of academic medicine, thus driving up relative costs. There is evi-

dence (5), subsequent to the data used in Fisher and colleagues' reports, to suggest that those regions identified as low cost are indeed accelerating spending, perhaps in an effort to achieve technologic parity with high-cost health care sites.

To deny great teaching institutions the ability to push the envelope of health care by contracting their financing is to risk ossifying a health care system that is the world's model for teaching and research. Great caution should be taken before the study of only 3 maladies in the vast panoply of human misery—colon cancer, hip fracture, and acute myocardial infarction—serves as a guidepost for future investment in health care, especially when the conclusions reached are so counterintuitive.

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2. Fisher ES, Wennberg DE, Stukel TA, Gottlieb DJ, Lucas FL, Pinder EL. The implications of regional variations in Medicare spending. Part 2: health outcomes and satisfaction with care. *Ann Intern Med.* 2003;138:288-98. [PMID: 12585826]
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**TO THE EDITOR:** The prospect of a 30% decrease in Medicare costs with no degradation in health outcomes is already a powerful incentive for hard-strapped government officials to act. However, as startling as the estimates of saving to the system are, it is possible that this is an underestimate. In the articles by Fisher and colleagues (1, 2), the lowest quintile is assumed to be the benchmark, but this may not be the case. The End-of-Life Expenditure Index includes a highly heterogeneous case mix with many diagnoses and treatment courses. A hospital referral region with overall lower costs might not have low resource consumption for some specific medical problems. Fisher and colleagues did not include the ranges for the quintiles and particularly the range for the lowest quintile. A large range would suggest that the lowest quintile does not represent what could be the benchmark or most efficient practice style and would raise the possibility that savings in excess of 30% are possible. Further studies would be needed to confirm whether outcomes were affected at these lower levels of spending. However, the possibility of even greater savings will increase the pressure to solve the financing problem for Medicare.

On the policy side, the problem is extremely complex, as Dr. Wilensky's examples in her accompanying editorial show (3). The area of discretionary spending often resides outside of those areas covered by evidence-based medicine, such as the cost benefit of an admission to the hospital, a referral, or a diagnostic test. A proposal that would wait until 80% of medical practice was covered by evidence-based medicine, with accompanying guidelines, would take

decades and would be appropriately resisted by physicians trying to manage an individual's unique clinical characteristics. Volume caps are too blunt an instrument and would probably hurt appropriately practicing physicians and their patients. To approach a solution, pilot programs should be undertaken that would use groups of physicians and their referral hospitals. These entities should be given access to benchmarks and paid on the basis of their ability to meet quality and efficiency standards. If we believe that the potential for efficiencies is so great as to save Medicare, we need to invest in programs that would prove these savings and generate a set of management skills to achieve them.

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3. Wilensky GR. The implications of regional variations in Medicare—what does it mean for Medicare? [Editorial]. *Ann Intern Med.* 2003;138:350-1. [PMID: 12585836]

**TO THE EDITOR:** The recent articles by Fisher and colleagues (1, 2) highlighting the implications of regional variations in Medicare spending have added to our understanding of the importance of effective medical spending. The authors should be applauded for challenging the general assumption that additional spending on health services will necessarily provide important health benefits.

It is interesting to note that these findings seem to complement a Canadian study by Zelder (3), which found that regions with higher overall government health spending per capita had no effect on reducing patient waiting times. However, the Zelder study did note that the only direct correlation between increased funds and reduced waiting times comes from money spent to subsidize drugs, leading to fewer hospital visits and less congested waiting rooms. In addition, Michaud and colleagues (4) acknowledged that a challenge to the future of medicine is to allocate resources effectively to reduce the major causes of disease burden globally.

Together the above observations add support to the following conclusion by Fisher and colleagues: "If the United States as a whole could safely achieve spending levels comparable to those of the lowest-spending regions, annual savings of up to 30% of Medicare expenditures could be achieved. Such savings could provide the resources to fund important new benefits, such as prescription drugs or expanded Medicare coverage to younger age groups, or to extend the life of the Medicare Trust Fund to better cover the health care needs of future retirees."

Investments in innovative prescription drugs may be a prudent vehicle to further increase the productivity in health care spending while at the same time improving health outcomes and enhancing patient satisfaction.

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**TO THE EDITOR:** Fisher and colleagues suggest that patients help themselves by seeking out evidence of alternative treatments and asking their provider of care if it might make sense to “watch and wait” before intervening (1, 2). I believe the authors should see a few patients in Philadelphia to get a real reality check. The government first needs to fix the liability crisis. Then, it needs to address reimbursement issues so that a primary care physician doesn’t have to work more hours than would be allowed for an airline pilot or a truck driver to make a decent living. When we address these issues, then you will see physician input and support for reflection on the question of “whether the nation can afford to reduce the intensity of medical treatment without harming health outcomes.”

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**TO THE EDITOR:** With regard to the escalating costs in Medicare spending (1) and efforts to reduce them, the thought to reward physicians who practice high-quality conservative medicine (2) is interesting but begs the question of what should be characterized as “high-quality.” Apart from the outcomes of treatment, in my view, one feature of high-quality medicine is thorough clinical evaluation of patients, a time-honored and rapidly disappearing art that has been relegated to a secondary role with dependence and reliance on new technology, such as imaging and laboratory testing. This reliance on technology for diagnostic work is to some extent institutionally mandated and is also driven by a desire for “objective” hard data to fend off malpractice suits (Brown B. Personal communication). Instead of using noninvasive tests to complement clinical examination, they are often used to supplant it. Neglect of clinical examination leads to declining standards in clinical examination and evaluation.

The lower extremities, for example, which are commonly affected by peripheral arterial disease (PAD), lend themselves to clinical diagnosis with a degree of accuracy unsurpassed in the rest of the body. A good history and physical examination is a cost-effective means to diagnose PAD (3), yet physician awareness of the prevalence and diagnosis of PAD is relatively low (4). A recent study designed to evaluate the performance and interpretation of the examination by residents and medical students demonstrated consider-

able inaccuracy in both data gathering and data interpretation (5). In addition, another report concluded that improvement in medical school education and training of internal medical physicians in the diagnosis of peripheral arterial disease was needed (6).

Deteriorating clinical skills are not confined to peripheral vascular disease. In a recent study of cardiology, a disturbing inaccuracy in cardiac auscultatory skills among generalists in training was noted (7), with a recommendation that family practice and internal medicine certification boards may want to rethink the abandonment of these skills.

In his history of the stethoscope (8), Blaurox opined that the “basis of medical diagnosis established by centuries of practice seems to be eroded. Although the stethoscope has greatly facilitated diagnosis, it is being put aside by devices which obviate the basic examination. Politicians and economists lament the great cost of medicine but do nothing to help students regain these skills.”

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**TO THE EDITOR:** Commenting on the articles by Fisher and colleagues (which suggest that greater medical expenditure does not necessarily correlate with improvements in health outcomes) (1, 2), Dr. Wilensky (3) appropriately concluded that “We need to find ways to encourage better practices . . . and more thought about how to reward physicians who practice high-quality conservative medicine. Today is none too soon to begin.” This statement is right on the mark and needs careful consideration.

We can deal with the issue of encouraging better medical practice by changing the process of medical treatment delivery. That which constitutes necessary treatment is readily available in the form of clinical practice guidelines developed by the experts who have sifted out the best of evidence-based medicine. How to implement such practices constitutes the major challenge. Our group contends that the integration of clinical guidelines into standardized orders

potentially will lead to better medical practice. We have used evidence-based standardized medical orders with much success, including better clinical outcomes and reduced cost. The development of such “tools” requires a committed group of physicians, nurses, and administrators working in concert. Such order sets have the potential to prompt physicians to practice the best of evidence-based medicine. Achieving success with such approaches requires a culture change by physicians and a coordinated effort by physician leaders to gain the “buy-in” of the physician body.

Rewarding physicians who practice high-quality conservative medicine, as suggested by Dr Wilensky, can be done only through the difficult process of measuring and comparing risk-adjusted clinical outcomes by using independent reviewers. Outcome measurement is of great importance and needs far greater attention and investment than it currently receives. Our experience and that of others measuring the impact of clinical practice guidelines on outcomes for procedures such as radical prostatectomy, has been extremely gratifying. Physician practices change (usually for the better) when their outcomes are being measured and compared (4, 5). However, we feel that insufficient efforts are being made in general in this aspect of medical practice. Historically, we physicians have tended to focus on bad outcomes and their prevention (usually reviewed at mortality and morbidity meetings) and have paid little attention to making acceptable outcomes better. The time has come to look at other industries’ approaches to quality improvement and cost efficiency. Dr. Wilensky’s suggestion of rewards is a novel approach that has great potential but will require a dramatic change in attitude by all involved in health care delivery, including physicians, hospitals, payers, and insurers.

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**IN RESPONSE:** Drs. Mueller and Kisloff raise questions about the causes of regional variations in spending and intensity. Regional differences in lack of health insurance and consequent differences in illness burden at age 65 years cannot explain the differences in spending and intensity observed in our study because we grouped regions according to differences in spending that were independent of regional differences in health status. Dr. Kisloff interprets our study as an attack on academic medical centers and technological advances. On the contrary, our study shows that low-spending re-

**Table 2. Differences in Resource Use following Hip Fracture among Patients Treated by Major Teaching Hospitals according to Regional Spending Level\***

Major Teaching Hospitals	Quintile of Regional Spending†					Ratio of Quintile 5 to Quintile 1
	1	2	3	4	5	
Per capita resource use‡	3904	4718	4706	6299	6407	1.64
Inpatient days	13	13	14	15	19	1.46
ICU days	0.7	0.8	0.9	1	1.1	1.57
Inpatient MD visits, <i>n</i>	14.7	17.8	22.1	24	33.2	2.26
Patients seeing ≥ 10 MDs, %	13	18.6	19.3	20.5	33.2	2.55

\* ICU = intensive care unit.

† Quintiles of regional spending were assigned on the basis of the End-of-Life Expenditure Index.

‡ Price-adjusted spending on hospital and physician services per patient-year from 6 months to 5 years after index event.

gions provide just as much of the proven technologically advanced care (such as percutaneous coronary interventions) as high-spending regions. We also showed that regional differences in spending are due to differences in the use of the hospital and the intensive care unit as sites of care and in the frequency of visits, consultations, and associated tests and procedures. We believe that these latter differences are in large part related to differences in the relative availability of beds and specialists for the population served by the specific hospital in which patients receive their care (1). The data in Table 2 show that teaching hospitals in the lowest-spending regions provide more conservative care but, as was shown in our papers, achieve equal quality and outcomes. These findings indicate that conservative practice is perfectly consistent with academic practice and technological innovation.

Dr. Kisloff also questioned whether it was reasonable to generalize results from a study of 3 conditions to the “vast panoply” of other conditions. We chose patients with hip fracture, colorectal cancer, and heart attack because they provide insight into diverse care systems (orthopedics, oncology, and cardiology). These patients, however, had many other chronic conditions, and because we followed patients for up to 5 years, our analyses are likely to reflect the benefit (or harm) from more aggressive care for these other conditions as well.

Dr. Levin and Mr. Fernandes focus on the policy implications of our study. Mr. Fernandes suggests that a prescription drug benefit could lead to improved outcomes for elderly persons. We agree but would argue that such a plan should require the creation of a prescription drug database that would support the kinds of population-based analyses we carried out. We also agree with Dr. Levin that even the lowest-spending quintile may not provide an appropriate benchmark and have argued (2), as he does, for a demonstration project that would engage selected medical centers in efforts to improve both the quality and efficiency of care and provide benchmarks for the rest of the United States.

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## Quadriceps Strength and Osteoarthritis Progression in Malaligned and Lax Knees

**TO THE EDITOR:** The article on quadriceps strength and knee arthritis by Sharma and colleagues (1) has received enormous coverage in the press and on Web sites. The “take-home” message received by patients may be that strength is bad for you. The average patient with knee arthritis is obese, and many have the metabolic syndrome. Sharma and colleagues’ findings may thwart progress in moving our overweight and sedentary population off the couch. For this reason, it is critical to review their methods carefully.

The authors found that participants with quadriceps muscle strength above the median had higher rates of radiologic progression of knee osteoarthritis in the setting of joint laxity, or malalignment. The key issue is the strength of the quadriceps and what this strength should be compared with. The authors tested whether quadriceps strength predicted disease progression and adjusted their results by body mass index (BMI), measured in kg/m<sup>2</sup>. Although BMI is associated with knee arthritis and disease progression, adjusting quadriceps strength using BMI rather than body mass does not make biomechanical sense. For example, to climb stairs or rise from a chair, you are lifting your body mass, not your BMI. For the mean BMI in the sample, 30 kg/m<sup>2</sup>, body mass varies from 67.5 kg for a participant who is 150 cm tall to 97.2 kg for a participant who is 180 cm tall. The participant who is 180 cm tall could be stronger than the participant who is 150 cm tall. It is reasonable to adjust for height, since the moment of inertia for chair rise and stair rise is related to limb length (height) and mass.

I would ask that the authors rerun the analysis and either adjust the knee extensor joint moment for body mass or report the knee moment as Newton-meters per kg<sup>2</sup>, and then adjust for BMI. Also, joint moment should be reported in metric units—Newton-meters, not foot-pounds.

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## Reference

1. Sharma L, Dunlop DD, Cahue S, Song J, Hayes KW. Quadriceps strength and osteoarthritis progression in malaligned and lax knees. *Ann Intern Med*. 2003;138: 613-9. [PMID: 12693882]

**TO THE EDITOR:** To the casual reader, the paper by Sharma and colleagues (1) may convey the wrong message, that is, that quadriceps strengthening is harmful to some osteoarthritic knees. What the authors concluded was that, at 18-month follow-up, progression of tibiofemoral osteoarthritis in malaligned or lax knees was greater in those with high compared with low quadriceps strength at *baseline*. However, since the authors measured baseline quadriceps strength

only and did not collect data during the 18-month interval, they cannot conclude that progression of tibiofemoral osteoarthritis was related to quadriceps strengthening. In fact, they apparently had no idea whether their participants were exposed to therapeutic quadriceps exercise (many were under the care of other physicians, and such exercise is currently recommended practice) or whether quadriceps strength remained constant during the 18-month interval between their observations.

Aware of the unknowns about strength changes in Sharma and colleagues’ sample, Brandt (2) suggested that “patients with the greatest quadriceps strength at baseline may have lost strength during follow-up, perhaps as a result of reduced loading of the arthritic extremity” and it may have been this loss that accounted for the osteoarthritic progression. Another possibility, I would suggest, is that patients with the greatest quadriceps strength at baseline may have performed more weight-bearing physical activity in the past and that it was this lifetime of wear and tear that resulted in the more rapid progression of their osteoarthritis compared with patients who had been sedentary.

Sharma and colleagues have provided an important service in pointing out that not all osteoarthritic knees are similar mechanically. However, a conclusion about the role of quadriceps strengthening in such knees awaits a study in which such strengthening is the experimental variable and we are given more history about factors leading up to the osteoarthritis. In addition, such a study should inform us about any concomitant treatment (for example, use of braces, assistive devices to decrease weight bearing, glucosamine-chondroitin sulfate intake, and intraarticular therapy) to which the patients have been exposed.

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2. Brandt KD. Is a strong quadriceps muscle bad for a patient with knee osteoarthritis? [Editorial]. *Ann Intern Med*. 2003;138:678-9. [PMID: 12693892]

**TO THE EDITOR:** It should come as no surprise that, as Sharma and colleagues report (1), isolated strengthening of the quadriceps is unlikely to help persons with osteoarthritis. An open joint space is contingent on alignment as determined not only by the quadriceps muscle above and in the front but by the hamstrings behind, the abductors and adductors, and the muscles of the lower leg below. In addition, the balance between strength and flexibility is probably more important than strength alone, and integrating alignment with flexibility and strength is essential in patients with osteoarthritis.

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## Reference

1. Sharma L, Dunlop DD, Cahue S, Song J, Hayes KW. Quadriceps strength and osteoarthritis progression in malaligned and lax knees. *Ann Intern Med*. 2003;138: 613-9. [PMID: 12693882]

**IN RESPONSE:** To Dr. Judge, we respond that we too are concerned about errors in the description of our findings and their implications. Our study dealt with the relationship between strength and progression in osteoarthritic knees that were also malaligned or lax. Strength had no deleterious effect in more neutral or stable knees. Also, we stated that these results have no implications for aerobic exercise or physical activity in general. It was important to report our results, regardless of whether they match what has been believed (in the absence of data) about the effect of strength on disease progression in knee osteoarthritis. Descriptive data for quadriceps strength in the sample were provided. Strength cutoffs in healthy persons and persons with osteoarthritis have not been established. Our results were not altered by separately adjusting for body mass (that is, weight) and height rather than BMI. In response to the comments of Dr. Moore, we point out that, as stated in the final paragraph of our Results section, strength was measured at baseline and at 18 months, and minimal change occurred. Further analyses revealed that the mean change in strength (a slight increase) did not differ between stronger and weaker participants; that the proportion of participants who lost strength was similar in stronger and weaker participants (for example, for 10% loss, the proportions were 20% and 19%, respectively); and that among those who declined in strength, the magnitude of decline did not differ between stronger and weaker participants. The results presented in our paper were adjusted for average physical activity. In theory, in some individuals, physical activity in the past may have contributed to disease development; the results presented in the paper are also adjusted for baseline disease severity.

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### Is a Strong Quadriceps Muscle Bad for a Patient with Knee Osteoarthritis?

**TO THE EDITOR:** We thank Dr. Brandt for his editorial (1) regarding our paper (2), and we appreciate very much his comments regarding our contributions in the area of local factors and this work in particular.

Dr. Brandt noted that potentially greater strength loss in the stronger participants could be a confounding factor. However, as we noted in the paper (2), strength was measured at baseline and at 18 months, and minimal change occurred. Further analyses revealed that the mean change in strength (a slight increase) did not differ between stronger and weaker participants; that the proportion of participants experiencing strength loss was similar in stronger and weaker participants (for example, for 10% loss, the proportions were 20% and 19%, respectively); and that among those who declined in strength, the magnitude of decline did not differ between stronger and weaker participants.

As noted in our introduction (2), while the relationship of strength to pain and function in osteoarthritis has been examined in numerous studies, the relationship between strength and structure outcome has received almost no attention. Pain and function have been assessed in the Mechanical Factors in Arthritis of the Knee

(MAK) study; a manuscript reporting the relationship of several factors, including strength, to pain and function has recently been published (3). Did knee pain at the time of testing affect the strength measurement? As we noted in our results (2), knee pain during strength testing was infrequent, and adjustment for pain did not alter the results. Medication use data were collected. Further analyses revealed that psychotropic and muscle relaxant use was extremely rare. Analgesics were used more often, but use did not differ between those with high and those with low strength.

We agree that the Osteoarthritis Initiative (OAI) will provide rich opportunities to address many questions, possibly including the question we examined. However, it is not yet known whether it will be feasible to assess in the multicenter OAI all of the variables that are the focus of our study (Lester G. Personal communication). The amount of change in strength in observational studies such as the OAI may be small; it would be more efficient to examine the structure outcome of strength change in a strengthening intervention trial. One priority of the OAI is to identify surrogate markers of disease progression. Neither our results nor those of others suggest that muscle strength or weakness fulfill criteria to be considered a surrogate marker, and we did not make this claim.

We tested the hypothesis that quadriceps strength is associated with greater probability of tibiofemoral osteoarthritis progression in malaligned knees and high-laxity knees. We found that this indeed was the case (2). We agree with Dr. Brandt that these results generate additional hypotheses about the effect of specific strengthening interventions.

We hope that our results make clear the need for exercise trials in osteoarthritis to include structure outcomes and to address the possibility that the structure outcome of a given exercise regimen may differ between knee subsets. Certain exercise programs may well delay osteoarthritis progression; certain exercises, especially in more vulnerable osteoarthritic knees, may accelerate it.

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### Treatment of Chronic Hepatitis C in a State Correctional Facility

**TO THE EDITOR:** In the past, the treatment of patients with chronic hepatitis C in U.S. correctional facilities has been lacking. Therefore, I commend Allen and colleagues (1) for their recent study on the treatment of 93 individuals with chronic hepatitis C in the Rhode

Island Department of Corrections. Despite the authors' excellent intentions, I take issue with some of their statements.

The authors state that nearly one third of all Americans with chronic hepatitis C pass through correctional facilities annually. Although the problem of hepatitis C in correctional institutions is daunting, with anywhere from 16% to 41% of inmates infected, I do not believe that the one-third statistic is correct (2).

The authors report a sustained viral response rate of 46% (26 of 57 patients treated). They initiated therapy in 90 patients yet analyzed only the 57 patients who completed therapy and who had data available 6 months after stopping treatment. This is not an intention-to-treat analysis, and the results are therefore biased: The final analysis did not include nonresponders and patients who required cessation of therapy because of adverse events or other reasons. In an intention-to-treat analysis, the true sustained viral response achieved in this study was 28.8% (26 of 90 patients who had treatment initiated). This rate falls well below the accepted sustained viral response rates of 38% to 41% achieved with the use of interferon- $\alpha$ 2b, 3 times weekly, plus ribavirin in the landmark studies of McHutchison and Poynard and colleagues (3, 4).

Regardless of this discrepancy, I believe that Allen and colleagues' report is important because it underscores the feasibility and need for treating chronic hepatitis C in correctional institutions.

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**IN RESPONSE:** We published our experience with the treatment of incarcerated persons with hepatitis C in part to highlight the fact that such a large proportion of Americans with hepatitis C pass through a correctional facility each year. Since submission of our report, Hammett and colleagues (1) published a detailed explanation of how they calculated that one third of all Americans with hepatitis C are among the 7.75 million individuals who are released from correctional facilities annually. Others (2) have reaffirmed this estimate. The problems associated with the management of hepatitis C in prisons are daunting, and as a result the Centers for Disease Control and Prevention, the National Institutes of Health, the Society of Correctional Physicians, and the University of Texas Medical Branch recently sponsored a national conference on the topic (3).

We reported a sustained viral response in 26 of 65 patients who completed treatment and had data available 6 months after treatment. Most of those "lost to follow-up" did, in fact, complete the treatment course, but end point data were not available at the time of publication. We included a figure showing the outcomes of all patients found eligible for treatment so that readers could perform their own analyses, such as calculating the response based on the intention-to-treat principle. We agree with Dr. Bernstein that the sustained viral response calculated by intention-to-treat is 29%. It is often difficult to replicate results from pharmaceutical trials in clinical practice. In this retrospective case series, we did not find a response equivalent to that found in enrollees of the initial clinical trials. However, our data, like those from the Virginia Department of Corrections, show that treating hepatitis C in prisoners is feasible (4). The results illustrate that our patients are not biologically different from nonincarcerated patients and that many respond in a setting with enforced sobriety and a highly ordered lifestyle.

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#### CORRECTION

#### Correction: The Continuing Increase in the Incidence of Hepatocellular Carcinoma in the United States

In a recent article on hepatocellular carcinoma (1), some of the figure legends appeared incorrectly. The legend for Figure 2 belongs with Figure 3, the legend for Figure 3 belongs with Figure 4, and the legend for Figure 4 belongs with Figure 2.

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## Evidence for Use of Coronary Stents

**TO THE EDITOR:** Brophy and colleagues nicely outlined the modest, at best, benefits of coronary stenting compared with plain old balloon angioplasty and discussed possible reasons for the widespread adoption of the former (1). I suspect that the allure and false idolatry of “new or high technology” are important (2). Physicians are not immune to using the “bigger is better” heuristic and may believe that more elaborate, more invasive, or more expensive treatments automatically have better outcomes (3). Moreover, technology, such as stents acting like a scaffold, may be easier for patients to comprehend. Previous studies have shown that medical devices have enhanced placebo or expectation effects: Patients tend to believe that a device or surgery is better than a medicine or injection (4, 5).

I am unable to think of a recently approved medication for the treatment of coronary artery disease that is of such limited benefit. Would a newly developed, and potentially expensive, medication be approved if it had no favorable impact on the risk for death or myocardial infarction and modestly reduced the risk for angiographic restenosis but, when restenosis did develop, required that 1 or 2 additional new, and more expensive, industries or technologies (such as coated stents and brachytherapy) be developed to deal with it? I doubt it. Within-stent restenosis is a problem for interventional cardiologists, and the strategy that is likely to have the greatest impact on this problem is to put in far fewer stents.

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