

## Cardiovascular Procedures in Patients With Mental Disorders

**To the Editor:** In their study of the association of mental disorders with use of cardiovascular procedures after myocardial infarction, Dr Druss and colleagues<sup>1</sup> conclude that elderly patients (aged 65 years and older) with mental illness are less likely to undergo coronary artery catheterization or revascularization than those without mental illness. We conducted a study to determine whether these findings can be generalized to patients of all ages.

We used information from the Healthcare Investment Analysts (HCIA)-Sachs 1998 projected inpatient database,<sup>2</sup> an all-payer database that contains data from more than 40% of US inpatient discharges. Our cohort included 354 195 patients with a principal diagnosis of acute myocardial infarction, of whom 143 421 (40.5%) were younger than 65 years. Using definitions similar to those used by Druss et al, we identified 25 237 patients (7.1%) with mental illness.

Using methods similar to Druss et al, we modeled the likelihood of undergoing cardiac catheterization, percutaneous transluminal coronary angioplasty (PTCA), and coronary artery bypass graft (CABG) surgery as a function of mental illness for 2 age groups: 65 years and older and younger than 65 years. However, we were unable to adjust for admission characteristics or left ventricular function.

We found a significantly reduced likelihood of undergoing cardiac catheterization and revascularization procedures for those with mental illness in both age groups (TABLE). Schizophrenia was associated with the lowest likelihood of undergoing catheterization; those younger than 65 years were about two thirds as likely to undergo catheterization as the reference group, while those aged 65 years and older were about half as likely. Patients with schizophrenia also had the lowest likelihood of undergoing PTCA, with about a 70% reduction for those aged 65 years and older and a 45% reduction for those younger than 65 years.

We also measured the likelihood of death during hospitalization for those with and without mental disease. Mental illness appears to have a slightly protective effect against inpatient mortality in the group aged 65 years and older, with a 21% lower risk-adjusted likelihood of death compared with the reference group ( $P < .001$ ). However, in the group younger than 65 years, there is a significantly increased likelihood of inpatient mortality for those with schizophrenia and for patients with substance abuse; there is an 86% increased risk-adjusted likelihood of death among those with schizophrenia ( $P < .001$ ) and a 71% increase in this risk for those with substance abuse ( $P < .001$ ).

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1. Druss BG, Bradford DW, Rosenheck RA, Radford MJ, Krumholz HM. Mental disorders and use of cardiovascular procedures after myocardial infarction. *JAMA*. 2000;283:506-511.

2. Sullivan KM, Belay ED, Durbin RE, Foster DA, Nordenberg DF. Epidemiology of Reye syndrome, United States, 1991-1994: comparison of CDC surveillance and hospital admissions data. *Neuroepidemiology*. In press.

**To the Editor:** Dr Druss and colleagues<sup>1</sup> allude to the fact that patients with mental disorders may have difficulty making decisions about the appropriateness of diagnostic tests and interventional procedures. They failed to mention that the major difficulty in treating patients who have serious mental disorders is obtaining informed consent. Patients with the most severe mental disorders often have court-appointed guardians or family members who are responsible for their care. In this litigious society, the performance of invasive procedures without carefully defined and informed consent is potentially hazardous both to the physician and the patient. There can be little doubt that one of the major reasons for a decreased incidence of the performance of these procedures is the simple inability to obtain a properly executed informed consent for the procedure.

Furthermore, patients with severe mental disorders may be poorly educated and may not understand the reasons for the procedure. They may be unable or unwilling to comply with postoperative care or may be unable to remain still enough to prevent complications such as bleeding and infection. I think that physician prejudice against patients with mental disorders is the least convincing of all of the possible explanations for the reduced use of cardiovascular procedures in this population.

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1. Druss BG, Bradford DW, Rosenheck RA, Radford MJ, Krumholz HM. Mental disorders and use of cardiovascular procedures after myocardial infarction. *JAMA*. 2000;283:506-511.

**In Reply:** The data of Drs Young and Foster demonstrate that patients with mental disorders who are younger than 65 years have low rates of cardiovascular procedures similar to that of those aged 65 years and older. The increased in-hospital mortality among

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**Letters Section Editors:** Phil B. Fontanarosa, MD, Deputy Editor; Stephen J. Lurie, MD, PhD, Contributing Editor.

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**Table.** Use of Cardiac Catheterization and Revascularization Procedures in Patients With and Without Mental Disorders by Age Group\*

	Cardiac Catheterization			PTCA			CABG		
	Unadjusted %	RR	95% CI	Unadjusted %	RR	95% CI	Unadjusted %	RR	95% CI
<b>Age 65 Years and Older</b>									
Mental disorder (n = 12 366)	29.8	0.77	0.75-0.80	12.2	0.68	0.65-0.72	6.6	0.67	0.62-0.72
Schizophrenia (n = 496)	16.9	0.51	0.42-0.62	4.8	0.32	0.21-0.47	5.7	0.67	0.46-0.95
Affective disorder (n = 846)	30.5	0.80	0.71-0.88	13.7	0.78	0.65-0.92	6.0	0.61	0.47-0.80
Substance abuse (n = 1943)	40.6	0.90	0.84-0.95	16.3	0.72	0.64-0.80	10.4	0.78	0.67-0.89
Other (n = 9418)	28.1	0.77	0.74-0.79	11.6	0.69	0.66-0.74	5.9	0.66	0.60-0.71
No mental disorder (n = 198 408)	39.2			18.4			10.2		
<b>Younger Than Age 65 Years</b>									
Mental disorder (n = 12 871)	54.0	0.88	0.86-0.90	27.4	0.70	0.68-0.73	9.3	0.79	0.75-0.84
Schizophrenia (n = 619)	41.5	0.70	0.63-0.77	18.7	0.55	0.46-0.65	7.9	0.71	0.54-0.92
Affective disorder (n = 851)	54.2	0.93	0.87-0.99	26.0	0.79	0.70-0.88	8.6	0.78	0.62-0.97
Substance abuse (n = 5962)	53.4	0.83	0.81-0.86	26.4	0.62	0.59-0.65	8.8	0.70	0.65-0.77
Other (n = 6263)	55.1	0.96	0.94-0.98	28.9	0.86	0.82-0.90	9.6	0.91	0.84-0.98
No mental disorder (n = 130 550)	60.0			36.2			11.8		

\*See Table 3 of Druss et al<sup>1</sup> for description of derivation and models. PTCA indicates percutaneous transluminal coronary angioplasty; CABG, coronary artery bypass graft surgery; and RR, relative risk. Source: HCIA-Sachs Projected Inpatient Database, 1998.

younger patients with mental disorders is consistent with the literature demonstrating excess cardiovascular mortality in this population.<sup>1</sup> The juxtaposition of these 2 findings raises the important question of whether, or to what degree, differences in these patients' cardiac care might mediate that excess mortality.

Dr Shander reports that, in his experience as a clinician, difficulties in obtaining informed consent and the potential risks for postoperative complications are important factors leading to decreased use of cardiovascular procedures in patients with mental disorders. Each of these useful observations serves as a starting point for further exploration and potential intervention. First, more standardized mechanisms may be needed to assess competency and to ensure appropriate use of proxy decision makers after myocardial infarction. Many patients with serious mental disorders are capable of making medical decisions; for those who cannot, it is essential that proxy decision makers have a full understanding of risks and benefits of treatments and are able to act in patients' best interests. Second, the risk-benefit ratio of revascularization vs medical management is still not known for the general population, much less for vulnerable populations such as patients with mental disorders. More data are needed to guide these treatment decisions.

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1. Frasure-Smith N, Lesperance F, Talajic M. Depression following myocardial infarction: impact on 6-month survival. *JAMA*. 1993;270:1819-1825.

### Handling Conflict in End-of-Life Care

**To the Editor:** In their article on conflicts regarding decisions to limit treatment, Dr Goold and colleagues<sup>1</sup> do not mention a scenario that I have faced a number of times in caring for patients with chronic neurological diseases. This might be called

the *proud caregiver syndrome*. The caregiver's entire life is centered around the patient. The caregiver gains respect, pride, a sense of noble self-sacrifice, service, and accomplishment from his or her caregiving. Life would become meaningless without the individual to whom he or she can administer care. The decision to continue all-out efforts is based on the need to continue the caregiver role, rather than on the patient's wishes or needs.

We are not told much about the son and daughter-in-law, the individuals who are pushing aggressive care in the patient example given in the article. This might be a possible reason for their behavior.

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1. Goold SD, Williams B, Arnold RM. Conflicts regarding decisions to limit treatment: a differential diagnosis. *JAMA*. 2000;283:909-914.

**To the Editor:** Dr Goold and colleagues<sup>1</sup> have presented an appealing approach to family-physician conflict, which is likely to be an improvement on the unschooled but well-intentioned efforts that are generally used.

However, the case they present is so shocking in its inattentive pattern of routinized inadequate care that it should not pass without comment. The patient had Alzheimer dementia and was hospitalized with recurrent aspiration pneumonia. His son and daughter-in-law provided daily care and wanted him to live out his days at home. A consulting gastroenterologist and a resident who treated the patient on "several prior hospitalizations" for pneumonia agreed against inserting a feeding tube. However, the family wanted "everything done," including a feeding tube and resuscitation if necessary.

This case tells the story of a family trying to live a decent, virtuous life in the face of a dreadful illness and getting no help. Any patient with a predictably disabling and fatal illness should have

a physician, nurse, and social worker who serve him or her and his or her family across time and who understand the family's history and values. Any reasonable care system would help all such patients and families plan for a good end of life, including how to handle predictable calamities. Decisions about the eventual issue of artificial feeding should have been made much earlier.

Hospitalization for pneumonia rarely serves the best interests of patients such as the one discussed by Goold et al. Administering antibiotics and fluids, and providing nursing care at home are almost always superior strategies. If the episode of worsened illness appears to be overwhelming, support for dying at home is almost always better for patient and family than death during vigorous hospital treatment. We must learn to notice our routinized, systematic failures to serve patients and families and to turn our newfound discomfort with ordinary error into motivation for change.

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1. Goold SD, Williams B, Arnold RM. Conflicts regarding decisions to limit treatment: a differential diagnosis. *JAMA*. 2000;283:909-914.

**To the Editor:** I applaud Dr Goold and colleagues<sup>1</sup> for their attempt to offer an alternative to the concept of medical futility in managing conflicts in end-of-life care. Futility is a conflict resolution strategy based on power, not persuasion. It is gratifying to read an article that presents understanding and negotiation as superior alternatives.

However, the authors do not discuss the role that disagreement among the medical treatment staff plays in fostering and sustaining conflicts. The article conveys the impression that the physicians and nursing staff present a united front. However, our research into decisions to withhold and withdraw life support from critically ill patients identified physician-physician disagreement as the principal cause of half the conflicts we identified.<sup>2</sup>

Moreover, by constructing a differential diagnosis of interpersonal conflicts, Goold et al present a familiar framework for thinking about an unfamiliar problem. This may reinforce the perception that difficult communication issues in end-of-life care can be managed through a standard medical model. However, such disagreements are rarely resolved by providing additional information. These are disputes about how to respond in tragic circumstances under conditions of uncertainty. Conflicts arise over the *meaning* of this information, rather than the information itself.

Finally, Goold et al describe many of the factors that cause confusion and impair decision making in critical care. Their analysis suggests that disagreement about treatment plans is to be expected in an environment that requires rapid decision making under conditions of uncertainty. Therefore, skills to manage such disagreements should be part of every physician's training, especially those who work in critical care.<sup>3</sup>

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1. Goold SD, Williams B, Arnold RM. Conflicts regarding decisions to limit treatment: a differential diagnosis. *JAMA*. 2000;283:909-914.
2. Prendergast TJ, Luce JM. Increasing incidence of withholding and withdrawal of life support from the critically ill. *Am J Respir Crit Care Med*. 1997;155:15-20.
3. Prendergast TJ. Resolving conflicts surrounding end-of-life care. *New Horiz*. 1997;5:62-71.

**In Reply:** Dr Caplan describes a particular type of caregiver response, the identification of which will aid clinicians trying to resolve conflicts around end-of-life decision making. It could be classified as an example of extreme grief, guilt, or both.

Dr Lynn expresses understandable dismay at a "system" that does not encourage continuity with clinicians, end-of-life advance planning, and attention to families' needs. While we agree that such cases are unfortunate, our focus was on clinicians' responses to these situations when they do occur. The current environment of health care does not favor prevention or information-giving, whether for biomedical, social or emotional aspects of illness. Clinicians should work to decrease the frequency of conflicts and work to resolve disputes with understanding, empathy, and negotiation. Renewed attention must be placed on organizational aspects of caring.<sup>1</sup>

We also agree with Lynn that support for dying at home is almost always better for patients and their families. However, even with adequate support (which is generally not available in our health care system) families may, for a variety of reasons, request that a patient be transferred to the hospital at the time he or she becomes acutely ill; refusing this request will not alleviate the patient's or family's suffering.

We agree with Dr Prendergast that "futility" as a conflict resolution strategy represents most often the power imbalance between clinicians and patients. We hope that our article will help physicians to diagnose and manage disagreements. We used the differential diagnosis to clarify communication and psychosocial issues in end-of-life care, which Prendergast rightly points out as the typical sources of such disputes. In a departure from most biomedical differential diagnoses, however, we suggested that physicians question not just the family, patient, or medical record but also themselves and the context of their work. We also suggested involving a primary clinician with the family to minimize the anxiety that interprofessional disagreements can cause, but we did not recommend that such disagreements be concealed. Disagreements within the health care team can illustrate the uncertainty about prognosis and may serve as a springboard to a productive discussion with the family. Finally, we agree with Prendergast that the construction of a differential diagnosis is only a starting point in understanding end-of-life conflicts. Effective treatment requires a range of communication and relation-building skills.

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## Informed Decisions for Extremely Low-Birth-Weight Infants

To the Editor: Dr Saigal and colleagues<sup>1</sup> found that parents of teenagers who had been extremely low-birth-weight (ELBW) infants gave higher quality-of-life scores to hypothetical scenarios of disability than did health care professionals. The authors conclude that parents are willing to accept life-sustaining care for extremely premature infants, despite the likelihood of severe disability. They further suggest that neonatal caregivers are less willing to provide such care and may influence parents to decline intensive treatment.

This is the opposite of our own experiences. As parents of extremely premature infants, we were given little information about probable outcomes and few, if any, choices about the treatment. Instead of being encouraged to limit care, many of us were threatened and made to feel like criminals for questioning even the most extreme medical measures. We are aware that experienced neonatal caregivers may be reluctant to have their own ELBW infants resuscitated and treated;<sup>2</sup> however, they were all too willing to force this care on our children against our wishes or without our informed consent.

As parents and family members of severely disabled children, we find the scenarios that Saigal et al presented for quality-of-life evaluations to be out of touch with the harsh realities of our children's lives. Where is the description of the months or years of grueling hospitalization with the associated gastrostomy tubes, jejunostomy tubes, and funduplications; the tracheostomies, shunts, and orthopedic, eye, and brain surgeries; hyperalimentation, oxygen tanks, and ventilators? Similarly, there was no mention of bankruptcies, divorces, mental and physical breakdowns, deaths in late childhood, neglected siblings, and suicides caused by the extreme burdens of caring for severely medically and developmentally compromised children.

We feel the study by Saigal et al needs to be viewed within the context of documented public opinion. Polls have shown that 70% believe that parents should be able to ask physicians not to take special steps to keep a brain-damaged child alive,<sup>3</sup> and 76% agree with the statement that they would want the right to decline lifesaving measures for an infant whose survival would impose major burdens on the family.<sup>4</sup> We wonder if the parents questioned by Saigal et al would have given different answers if the questions had been phrased, not as evaluations of static, artificially constructed "health care states," but as questions involving their rights as parents to decline a lifetime of arduous medical procedures and disability for a suffering infant.

Incredibly, Saigal et al use their findings to call into question the very small area of parental decision-making currently allowed by the Canadian Paediatric Society<sup>5</sup> and the American Academy of Pediatrics.<sup>6</sup> Although the guidelines of these organizations are rarely mentioned to parents or honored by neonatologists, they are at least steps in the direction of genuine family-centered neonatal care.

If physicians and policy makers are really interested in what parents want from neonatology, they could find out easily enough by informing all expectant parents realistically about the treatment and outcomes of extreme prematurity as a part of standard prenatal and perinatal care. They could then offer genuine choices. It would be tragic if neonatologists use the results of the study by Saigal et al to further limit the choices of parents.

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**Acknowledgment:** We would like to thank Ms Helen Harrison for her assistance.

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**In Reply:** We thank Ms Culver et al for their thoughtful reflections on our study. While we cannot comment specifically on their personal experiences, we would like to address several of the points they raise.

Some of our comments appear to have been misinterpreted. We did not conclude that parents are "willing to accept life-sustaining care" for all ELBW infants. Rather, we wrote that parents were "more accepting of the severely disabled health states than HPs [health care professionals]." What this study did not do, and what needs to be done in future research, is to explore the processes by which parents and health care professionals make decisions and the bases on which they do so.

We agree that the scenarios presented in our study describe only the health and functional states of the children and not the dilemmas that a family might face in raising a child with complex disabilities. Clearly, it is not possible to address all such issues in a single study. We believe that more should be done to explore these multidimensional issues, which are part of the realities of families of children with disabilities.

We completely agree about the importance of parents being involved in decision-making for their children's treatment. Our studies show that parents of both ELBW and control infants overwhelmingly (>97%) believe that the final decision regarding life-sustaining treatment should be made by parents, and fewer, although a substantial proportion (ELBW, 66%; control, 77%), support the role of physicians in such decisions.<sup>1</sup> It is our perception that parental involvement is currently encouraged to a much greater extent than it was in the past.

We are at a loss to understand why Culver et al feel that we "call into question the very small area of decision-making" currently allowed by the Canadian Paediatric Society<sup>2</sup> and the American Academy of Pediatrics.<sup>3</sup> In fact, we conclude that "parents are the most appropriate agents" when making decisions on behalf of their infants in the neonatal intensive care unit because there was greater consistency between parents and adolescents than between adolescents and health care professionals in the valuation of the severely disabled health states. We have repeatedly emphasized the importance of parents and health care professionals being aware of the differences in perspectives within and between groups and recommend individualized counseling for parents facing complex decisions about neonatal intensive care.<sup>1,4</sup>

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### Screening Mammography in Elderly Women

To the Editor: Dr Kerlikowske and colleagues<sup>1</sup> attempt to delineate a clinical strategy to identify groups of older women who could realize the greatest benefits from mammography screening at the most reasonable costs. The importance of achieving this goal is underscored by converging epidemiologic and de-

mographic trends: breast cancer is a disease of old age, and by the year 2030, 1 in 5 women will be aged 65 years and older. The greatest increase in this older population will be among the group for which there is the least data and consensus on the value of screening: women aged 80 years and older.

As Kerlikowske et al acknowledge, older women are physiologically heterogeneous. For instance, there are important age-related variations in comorbidity, mammographic sensitivity, natural history of disease, and morbidity associated with breast cancer and its treatments. All of these factors affect screening-related survival benefits. Women also differ in preferences for health outcomes. For example, some older women are willing to undergo aggressive breast cancer treatments for small increases in survival, while others value quality over quantity of life.<sup>2</sup> Risk is also variable: the relative risk of developing breast cancer is 2.7 for women in the highest quartile of radial bone mineral density (BMD) (vs the lowest),<sup>1</sup> 4 to 5 for those aged 60 years (vs 40), 2 to 6 for family history (vs none), and 2 to 4 for personal history (vs none).<sup>3</sup>

Based on this heterogeneity in aging, preferences, and lack of a perfect risk predictor, we propose a somewhat different approach to addressing the question of whether there should be an upper age limit for breast cancer screening. In this approach, we extend our earlier work<sup>4,5</sup> and model the effects of physiological, not chronological, age on screening decisions. We ask, (1) is there a threshold, based on age-specific life expectancy associated with combinations of common comorbidities, below which screening will not yield sufficient benefit to be considered cost-effective? (2) would estimating the probability that a given population was destined to develop an estrogen receptor that is positive vs negative or an indolent vs an aggressive tumor change screening decisions or intervals? (3) would clinical breast examination by nurses be a cost-effective alternative to mammography? and (4) for a given life expectancy, what are the boundaries of society's preferences for health (and time) that would change screening decisions?

Among older women, several competing trends are likely to affect future breast cancer incidence, morbidity, mortality, and costs (eg, use of hormone replacement therapy or selective estrogen receptor modulators, changes in diet and exercise habits, increases in screening use and declining rates of screen-detected prevalent cases, shifts in treatment paradigms, and declines in cardiovascular mortality). Developing a feasible, broadly applicable method of making public health and clinical decisions about the use of breast cancer screening in the expanding older population is a key challenge facing cancer control and gero-oncology researchers.

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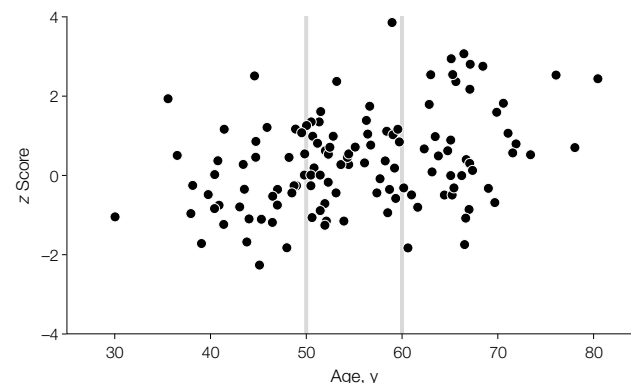
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**To the Editor:** Risk factors for breast cancer may be used to identify a particular population that would benefit from screening mammography. Several authors have observed that elderly US women who have an elevated BMD also have an increased risk of breast cancer.<sup>1,2</sup> Thus, BMD may be an indicator of lifetime estrogen exposure, suggesting that BMD may be used as a screening tool not only for osteoporosis but also for breast cancer risk, as suggested by Dr Kerlikowske and colleagues.<sup>3</sup>

Another implication is that women with breast cancer are relatively protected against osteoporosis. This may be quite important, since these women rarely receive hormone replacement therapy. To test this hypothesis, we measured the BMD of Belgian women with newly identified breast cancer in our institution (n=118; mean age [SD]; 55 [10] years; range 30-82). We measured their BMD at the vertebral L2 through L4 site using a standard dual-energy x-ray absorptiometry (1000, Hologic, Waltham, Mass) and compared it with that of the normal reference population using the z score (ie, the difference between the patient's BMD and the mean BMD for that age category divided by the SD for that age). The BMD of the breast cancer patients in relation to age expressed in z score is shown in the FIGURE. Women with breast cancer who were younger than 50 years had a mean (SD) z score of 0.09 (0.85); women between the ages of 50 and 60 years, 0.38 (1.12); and women older than 60 years, 0.76 (1.23). In our sample, increased BMD is only observed in elderly women with breast cancer; peri-

**Figure.** Bone Mineral Density (BMD) of Patients With Breast Cancer Expressed in z Score and Shown in Relation to Age



The z score equals the difference between the individual patient's BMD and the mean BMD for that age category divided by the SD. The mean (SD) z score for women younger than 50 years was 0.09 (0.85); for women between the ages of 50 and 60 years, 0.38 (1.12); and women older than 60 years, 0.76 (1.23).

menopausal and early postmenopausal women have either comparable or only a moderate increased BMD compared with the general population. Therefore, some women with breast cancer will still be at risk for osteoporosis.

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Brussels, Belgium

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**To the Editor:** Dr Kerlikowske and colleagues<sup>1</sup> neglect significant costs that entirely negate the small gain in life expectancy they report. Biennial screening mammography for a period of 10 years requires at least 5 visits to the mammography facility. Given the mobility constraints in the population in question, the 7.2 hours to 2.1 days added life expectancy is likely to be consumed by the screening procedures themselves. It does not seem reasonable to recommend medical procedures that only extend life long enough to perform the procedure being recommended.

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1. Kerlikowske K, Salzman P, Phillips KA, Cauley JA, Cummings SR. Continuing screening mammography in women aged 70 to 79 years: impact on life expectancy and cost-effectiveness. *JAMA*. 1999;282:2156-2163.

**In Reply:** To make evidenced-based recommendations related to breast cancer screening of elderly women, the efficacy (or lack thereof) of screening mammography needs to be better defined. Additional time preference and utility data are also needed, and guidelines for a threshold for average life expectancy gained and cost per year of life saved are required. In the meantime, elderly women and their physicians must decide whether to continue or discontinue screening mammography at age 70 years. In an effort to build a clinically relevant and useful model to facilitate this decision, we focused on the 2 most important factors that would influence decisions about screening in the elderly: (1) level of breast cancer risk determined by age and BMD measurement, and (2) time preference or discount rate.

Even without precisely defining an individual woman's physiologic age, breast cancer risk, discount rate and utilities, a clinician can make a reasonable recommendation based on our model. Elderly women with normal or high BMD and a strong preference for preventive care may choose to undergo mammography while women with existing comorbid conditions and low BMD whose chance of dying of breast cancer is very low may choose not to undergo mammography since the chance of benefiting from screening is very small. Given the small average gains in life expectancy (2.1 days) from screening elderly women with normal to high BMD, even those at relatively high risk and without comorbid conditions, the chance that average life expectancy would be increased to more than 30 days (a gain from a preventive intervention considered to be large<sup>1</sup>) is small.

Although the number of cases is small, Dr Rozenberg and colleagues report some interesting data that support the association between breast cancer and high BMD. Other studies have found that high estrogen levels are associated with breast cancer,<sup>2</sup> suggesting a central role for estrogen in both diseases.

Technically, Drs Seidenwurm and Breslau are correct that time costs are part of the total cost of an intervention. However, it is often assumed that quality adjusted life years incorporate these time costs. Therefore, it would be redundant to include them as a separate cost.<sup>3</sup>

It is important to recognize that older women differ significantly in their risk for breast cancer and their preferences for a small gain in life expectancy and the potential harms of screening mammography. The emphasis should be to identify groups of elderly women willing to undergo mammography (with its attendant harms—time, money, discomfort, additional tests and surgeries) and who are also most likely to benefit from screening. Conversely, it is important to identify elderly women who

are unlikely to benefit from screening so they will not be subjected to the potential harms of mammography.<sup>4</sup> Our goal is to help physicians identify those elderly women who may benefit the most from screening mammography and to help elderly women make an informed decision about continuing screening mammography.

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3. Gold MR, Siegel JE, Russell LB, Weinstein MC, eds. *Cost-effectiveness in Health and Medicine*. New York, NY: Oxford University Press; 1996.
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## CORRECTIONS

**Incorrect Numbers:** In the Original Contribution entitled "Effect of Out-of-Hospital Pediatric Endotracheal Intubation on Survival and Neurological Outcome: A Controlled Clinical Trial" published in the February 9, 2000, issue of THE JOURNAL (2000;283:783-790), there were incorrect numbers in 2 tables. On page 787, in Table 3, the numbers "3/22 (5)" for "Child maltreatment" under "Survival by Final Diagnosis" for "ETI" should have read "1/22 (5)." The related odds ratio and 95% confidence interval values remain correct. On page 788, in Table 5, the numbers "27 (7)" for "Gastric distention" under "ETI" should have read "98 (27)." The related *P* value in that table remains correct.

**Incorrect Academic Degree and Missing Citations:** In the Original Contribution entitled "Impact of Disseminating Quality Improvement Programs for Depression in Managed Primary Care: A Randomized Controlled Trial" published in the January 12, 2000, issue of THE JOURNAL (2000;283:212-220), an incorrect academic degree was cited and 2 citations were omitted in the text and reference list. In the byline on page 212, the academic degrees for Lisa V. Rubenstein should be MD, MSPH. Also, the following 2 references should have been cited in the article:

32. Muñoz RF, Miranda J. *Group Therapy for Cognitive-Behavioral Treatment of Depression, San Francisco General Hospital Depression Clinic, 1986*. Santa Monica, Calif: RAND; 2000. Document MR-1198/4.
33. Muñoz RF, Aguilar-Gaxiola SA, Guzmán J. *Manual de Terapia de Grupo para el Tratamiento Cognitivo-conductual de Depresión, Hospital General de San Francisco, Clínica de Depresión, 1986*. Santa Monica, Calif: RAND; 2000. Document MR-1198/5.

These references should be cited with reference 31 on page 215, middle column, at the end of the first sentence in the first full paragraph.