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Self-Study from Web-Based and Printed Guideline Material

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## Distant Healing

**TO THE EDITOR:** Astin and colleagues' literature review on distant healing (1) was disappointing. While the authors admit that methodologic limitations in the research they reviewed "make it difficult to draw definitive conclusions about the efficacy of distant healing," they assert that because 57% ( $n = 13$ ) of these flawed studies showed some positive response to at least one measured variable, the interventions must warrant further study. The studies they reviewed, however, do not support such a conclusion. Only two of the five reviewed studies on prayer claimed positive results that reached significance. Harris and colleagues' study measured 33 different variables and found no significant difference between the prayer group and the control group. The prayed-for patients in Byrd's study had a higher percentage of readmissions to the coronary care unit and needed four times the number of temporary pacemakers and three times the number of permanent pacemakers compared with the control group. When taken as a group, the Therapeutic Touch studies reviewed by Astin and colleagues present a clear picture of the randomness of results one would expect to achieve by chance or placebo response alone. The two anxiety studies by Quinn contradict each other; Meehan's pain study contradicts Keller and Bzdek's; Simington and Laing showed no significant treatment response; and Wirth and associates' series of five studies run the gamut of significant response to treatment, no response to treatment, and significant response to placebo.

When one takes into account the numerous flaws in methods, the strength of the response to placebo, the wide variations in results even from the same researcher, and negative physical outcomes consistently associated with reported positive findings, it becomes clear that this genre of studies has not borne fruit despite repeated attempts by committed researchers. We should decline to dedicate further scarce research funds to these endeavors.

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1. Astin JA, Harkness E, Ernst E. The efficacy of "distant healing": a systematic review of randomized trials. *Ann Intern Med.* 2000;132:903-10. [PMID: 0010836918]

**TO THE EDITOR:** The thoughtful systematic review of the efficacy of "distant healing" by Astin and colleagues raises many questions (1). For me, the most interesting conundrum is not whether distant heal-

ing works (I'm highly skeptical) but how such a discussion reveals the contradiction within medicine between a rational perspective, exemplified by laboratory-based scientific knowledge, and the clinical-empirical approach currently based primarily on randomized, controlled trials (2). Will more trials of distant healing with increased methodologic rigor be helpful? If the results of such trials are negative, there is no problem: Rational and empirical knowledge agree. If the results of such trials are positive, would the evidence be persuasive for the medical community? I don't think so. The situation resembles the predicament with homeopathy trials, another seemingly implausible intervention, where the evidence of multiple positive randomized, controlled trials (3) will not convince the medical community of its validity (4). Additional positive trials of distant healing are only likely to further expose the fact that the underpinning of modern medicine is an unstable balance between British empiricism (in the tradition of Hume) and continental rationalism (in the tradition of Kant).

This predicament of an ultimate clash between epistemological and ontological knowing can be posed from an entirely different direction. For example, if enough people could be recruited, one could stratify a distant healing intervention trial by Jewish, Christian, Muslim, and Buddhist prayer. Would such a trial finally give the empirical evidence to settle theological disputes? I suspect that groups that were not as successful would find many shortcomings with the empirical evidence. It seems that the decision concerning acceptance of evidence (either in medicine or religion) ultimately reflects the beliefs of the person that exist before all arguments and observation (5).

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**IN RESPONSE:** We continue to stand by what we feel is a reasonably cautious and conservative interpretation of the findings. Because 57% of the trials we examined did show a significant effect on at least one outcome (and the overall pooled effect size was significant), we do feel that, at a minimum, additional research should be carried out in these areas. While it is true that the results were not uniformly positive, the mathematical odds (based on a simple binomial test) that 13 of 23 studies would show a significant treatment effect ( $P < 0.05$ ) are greater than 1 in a million. It is therefore unlikely that these results are due to chance alone. However, as we noted, it is true that the single-blind designs used in the Therapeutic Touch studies cannot entirely rule out a placebo effect (although the designs in the other trials did theoretically rule out such an explanation because patients had no presumable way to know whether they were receiving distant healing). As we note in our paper, several studies had some methodologic problems. However, overall the trials we reviewed were judged to be of fairly high methodologic quality (1). With regard to the specific issues concerning two of the prayer studies, we refer readers to a recent exchange (2, 3) about these matters.

If we understand Dr. Kaptchuk correctly, he is right in stating that in many cases no amount of empirical evidence is sufficient to change one's prior beliefs, particularly if such beliefs are held to strongly. This appears to be the case whether such evidence refutes a layperson's belief based, say, on faith (for example, "my religion is true") or a scientist's skepticism that something (such as distant healing or homeopathy) is not possible. (Ironically, although scientists frequently argue that their lack of belief in certain phenomena is based on reason and rationality, such skepticism shares much in common with religious dogma in that it is based largely on a set of untested assumptions and is not easily refuted by contradictory evidence.) Understanding the complex reasons underlying people's unwillingness to alter their perspectives even in the face of evidence is of paramount importance because oftentimes (whether in medical science or in our personal lives), only by letting go of previously held beliefs can new learning and discovery ever take place.

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et al and a response to critics. *Arch Intern Med*. 2000;160:1735-7. [PMID: 0010871965]

### The Alcohol Hangover

**TO THE EDITOR:** Wiese and colleagues' statement that hangover-induced absenteeism and poor job performance costs the U.S. economy \$148 billion each year is incorrect, as are the claims that the annual cost is \$2000 per worker and that light-to-moderate drinkers are the primary source of the problem (1). The authors cite a report by Stockwell, but the \$148 billion lost-productivity estimate is not found in this source. However, the National Institute on Drug Abuse and National Institute on Alcohol Abuse and Alcoholism estimate that the total cost of alcohol abuse in the United States is \$148 billion (2). Of note, the agencies' estimate includes all costs from alcohol—not simply the costs of hangovers—and it has been criticized as seriously inflated, with other published estimates running from \$12 to \$30 billion per year (3). Furthermore, even though a significant part of the agencies' estimate is from lost productivity, the loss comes entirely from alcohol abusers. In contrast, many studies suggest that moderate drinkers, on average, have higher wages than abstainers or abusers (4).

The authors' \$2000-per-person cost figure is equally misleading. This estimate comes from a telephone survey ( $n = 635$ ) in which 22 respondents reported alcohol-related problems for which costs could be estimated (5). However, the author of that report specifically noted that the bulk of the costs were attributed to people who drank five or more drinks every day or every other day, not to light-to-moderate drinkers.

Effective programs, policies, and treatments require a broad and balanced understanding of alcohol-related behaviors and problems. Unfortunately, the suggestions that hangovers cost the United States \$148 billion annually and that light-to-moderate drinkers are mostly to blame is a misreading of the literature and a disservice to responsible consumers.

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**IN RESPONSE:** Mr. Becker is correct in pointing out that the 1992 estimates from the National Institute on Drug Abuse and National Institute on Alcohol Abuse and Alcoholism are not specifically directed at the cost of the alcohol hangover. We regret that this statistic may have been misleading. It is important to note, however, that both of these models emphasize the chronic alcoholic and may underestimate the total lost productivity due to light-to-moderate drinking (1).

The light-to-moderate drinker has fewer and less frequent missed work days than the chronic alcoholic, and this makes detecting lost productivity of this group using survey models difficult. Although light-to-moderate drinkers account for fewer absent days per drinker, they represent the overwhelming majority of alcohol consumers. Because there are many more light-to-moderate drinkers than chronic alcoholics, their contribution to the total lost productivity due to alcohol is substantially larger (2). The \$148 billion estimate may be an overstatement of the cost of the alcohol hangover, but for this reason it may also be an underestimate.

Mr. Becker has identified an important point in assessing the cost of the alcohol hangover: Namely, that few scientific and economic models assessing the cost of alcohol have been designed to specifically study the alcohol hangover or the impact of alcohol on the light-to-moderate drinker. We thank Mr. Becker for his point of clarification, and we hope that further research will precisely identify the cost of the alcohol hangover to consumers.

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2. Kreitman N. Alcohol consumption and the preventive paradox. *Br J Addict.* 1986;81:353-63. [PMID: 0003461846]

### Self-Study from Web-Based and Printed Guideline Material

**TO THE EDITOR:** Bell and colleagues (1) suggest that Web-based self-study materials are more efficient than printed materials. They note, however, that the residents participating in their study sought correct answers to questions most often, sought relevant guideline passages less often, and sought evidence supporting those guidelines least often. The authors rightly acknowledge the importance of un-

derstanding this behavior. I suggest that the existence of guidelines per se leads to passivity regarding review of the evidence.

A published guideline indicates that a problem has been solved and the "best answer" is known. Moreover, the rationale underlying the publication of guidelines is to save busy clinicians the time necessary to review the literature. Residents certainly qualify as busy clinicians. In many situations, however, competing guidelines disagree. In still more situations, no applicable guidelines exist and the individual physician's clinical judgment must be tapped.

The price we pay for the time savings is that the learning objective, in the large sense, has shifted from evaluation of one's clinical experience and reading of the literature to assimilating experts' evaluations of the evidence. The reward system, as embodied by board examinations and related measures, undervalues data evaluation. These findings should serve as both a warning and a challenge to those involved in medical education. We must work hard to incorporate the skills of critical reading of the primary literature into the syllabus. To neglect this need will promote reliance on recall of algorithms rather than development of judgment. This is a difficult task, but one that our patients' best interests and our professional standing demand.

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**IN RESPONSE:** We thank Dr. Blank for highlighting the tension in medical education between teaching simplified rules for patient care and teaching the underlying primary evidence. We agree that the latter approach should support more robust decision making, but it also generally requires more time. Our intervention was intended to facilitate learning both guideline recommendations and the evidence that underlies them, but, as we noted, few of the participants pursued the evidence in depth. The participants may have shared an attitude that was found recently among British general practitioners—they do not have time to pursue critical appraisal of the primary evidence (1). Given that all physicians face time constraints, it remains possible that generalists could best improve their practices by focusing on simple, clear messages about actions that have proven benefit. On the other hand, it is also possible that if generalists invested more time learning the primary evidence, they would remember best practices with less repetition, effectively making their initial learning more efficient. Because many physicians fail to learn and apply even simple recommendations supported by both strong evidence and expert opinion, research that addresses these questions is urgently needed.

We disagree that the mere existence of guidelines contributes substantially to physicians' lack of interest in learning evidence.

Well-written guidelines are structured to communicate the evidence in support of their conclusions. Physicians who choose to focus only on the conclusions probably would not have sought out and synthesized the evidence in the absence of the guidelines. Furthermore, if guidelines did not exist, we would also face more difficulty in setting educational priorities and standards for quality improvement. However, we agree with Dr. Blank and also with a recent editorialist (2) that physicians may shun the evidence in part because of poor critical appraisal skills.

As medical educators, we should redouble our efforts at disseminating the most basic of these skills. We must also acknowledge, however, that all humans are limited in the amount they can learn in a given period. To improve the productivity of computer-based medical education, we believe that more research is needed to understand the cognitive mechanisms that lead physicians to learn and retain new material, and to improve their performance.

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### Hypothyroidism in Two Patients after Hepatic Arterial Chemoembolization

**TO THE EDITOR:** We describe two patients who had not previously received thyroid therapy and became hypothyroid after arterial chemoembolization.

A 66-year-old woman with metastatic carcinoid and a serum thyroid-stimulating hormone (TSH) level of 3.0 mIU/L (normal, 0.3 to 5.0 mIU/L) underwent hepatic arterial chemoembolization twice within 3 months. She developed clinical hypothyroidism 4 months after the first chemoembolization. Her serum TSH level was greater than 100 mIU/L, her serum thyroxine level was less than 19.3 nmol/L (normal, 64.4 to 154.5 nmol/L), and serum antimicrosomal antibodies were undetectable. A 58-year-old woman with metastatic

adenocarcinoma and a serum TSH level of 4.0 mIU/L developed clinical hypothyroidism 2 months after chemoembolization. Her serum TSH level was 18 mIU/L, her serum thyroxine level was 52.8 nmol/L, her free thyroxine index was 0.9 (normal, 1.4 to 3.7), and no serum antiperoxidase antibodies were detectable.

During hepatic chemoembolization, both patients received 10 mL of iopamidol and 20 mL of ethiodized oil (which contained a total of 10.6 g of iodine [1]), mixed with doxorubicin, followed by gelatin sponge material. Both patients showed a prompt clinical and biochemical response to standard L-thyroxine therapy.

Iodide-induced hypothyroidism has been associated with the use of lipid-soluble iodinated radiographic contrast agents, which in the past were used for myelography or bronchography, but is rarely seen with the water-soluble agents currently used for most radiologic procedures (1). Patients with underlying thyroid disease are thought to be more prone to iodide-induced hypothyroidism (2, 3). Excessive amounts of iodine inhibit the synthesis of thyroid hormones (called the Wolff-Chaikoff effect), which is the proposed mechanism for iodide-induced hypothyroidism (4).

The slow release of iodine present in lipid-soluble radiographic contrast agents may cause hypothyroidism after hepatic arterial chemoembolization, even in patients without previous thyroid disease. Symptoms of hypothyroidism in patients with widely metastatic disease may be easily overlooked. Patients who have undergone hepatic arterial chemoembolization should be followed closely for the development of hypothyroidism.

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