

Abstinence and Safer Sex Among Adolescents

To the Editor: The study by Dr Jemmott and colleagues¹ contributes important data to the effort to develop effective interventions that prevent sexually transmitted diseases (STDs) and human immunodeficiency virus (HIV) disease among young people. Although the authors used an ambitious study design and went to substantial lengths to document and validate the self-reported behaviors that were the principal outcome measures, we believe this and other such studies could be strengthened by the use of biomedical markers that identify STDs.

The availability of sensitive and specific urine-based testing for some of the most common bacterial STDs (ie, chlamydia and gonorrhea) makes such assessment feasible, even among subjects who do not report sexual activity. As O'Leary et al² have noted, "the real outcome of interest in intervention research is morbidity: rates of HIV, of other sexually transmitted infections [STIs] in the population or in a group of individuals and incident HIV/STD at the individual level." While we agree that since the population studied was so young (mean age, 11.8 years) and sexually inexperienced (only 25.2% reported ever having sexual intercourse), preventing establishment of at-risk behaviors is a critical outcome, we would still encourage the use of biomedical markers in the evaluation.

The biomedical assessment of STDs can contribute importantly to the assessment. On one hand, experimental interventions can be associated with differences in self-reported behavior change without differences in rates of STD acquisition being observed.³ On the other hand, a recent study found that a counseling-based approach was associated with only a modest effect in reducing self-reported risk behavior, but had greater impact on the rate of acquired STD.⁴

We would like to emphasize that behavioral interventions are not the only primary prevention modality for preventing STDs. The diagnosis and treatment of communicable diseases such as STDs prevent ongoing transmission of disease and constitute both primary and secondary prevention. Screening and treating STDs, particularly as part of broad control programs, is an important primary prevention intervention that is effective in reducing community prevalence of disease and morbidity.

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1. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.

2. O'Leary A, DiClemente RJ, Aral SO. Reflections on the design and reporting of STD/HIV behavioral intervention research. *AIDS Educ Prev*. 1997;9(suppl 1):1-14.

3. Orr DP, Langefeld C, Katz B, et al. Intervention increases condom use among adolescent females. *J Pediatr*. 1996;128:288-295.

4. Kamb ML, Fishbein M, Douglas JM, et al, for the Project RESPECT Study Group.

Efficacy of risk-reduction counseling to prevent human immunodeficiency virus and sexually transmitted diseases: a randomized controlled trial. *JAMA*. 1998;280:1161-1167.

To the Editor: Based on data from the preintervention questionnaire in the study by Dr Jemmott and colleagues,¹ 25.2% of the 659 student subjects reported that they were nonvirgins and 15.4% had engaged in coitus during the previous 3 months. Three months later, when 647 returned to collect their stipend for participating, only about 80% usefully completed the almost identical questionnaire. Thus, only 112 (21%) of 531 reported coitus in the prior 3 months. The same rate of completing the questionnaire occurred in the 6- and 12-month assessments, although just about all the original study subjects returned, presumably to receive their money each time.

Regarding the effect of abstinence education, 77% (136/176) of those taught mainly abstinence, 77% (126/167) of those taught mainly condom use, and 75% (126/167) of the controls began the study as virgins ($\chi^2=0.09$, $P<.75$). At the 3-month follow-up, 4 (3%) of 136 virgins who were taught mainly abstinence, 11 (9%) of 128 virgins taught mainly condom use, and 13 (10%) of 126 virgins of the controls reported coitus during the prior 3 months. This appeared to be a promising trend for mainly abstinence instruction. However, the authors reported this apparent success of marginally higher rates of maintaining virginity at the third month, but did not reveal how many virgins from each group reported sexual debut at the 6- or 12-month follow-ups.

Despite the authors' claim that the frequency of coitus and unprotected intercourse had decreased by the 12-month follow-up among those who initially were nonvirgins, the frequencies for unprotected sex included those children who did not report any sex during the prior 3 months (and so therefore could not have had unprotected sex). A more telling comparison is of children who claimed to have sex at each follow-up. At the 3-month mark, 48% (11/23) of those taught mainly abstinence, 24% (7/29) of those taught mainly condom use, and 49% (19/39) of the controls reported coitus without condoms ($\chi^2=4.82$, $P<.10$). At the 6-month follow-up, the corresponding figures

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Edited by Margaret A. Winker, MD, Deputy Editor, and Phil B. Fontanarosa, MD, Interim Coeditor.

were 40% (12/30), 48% (12/25), and 46% (18/39) ($\chi^2=0.41$, $P<.85$). At the 12-month follow-up, they were 46% (16/35), 33% (9/28), and 45% (18/40) ($\chi^2=1.18$, $P<.60$). Thus, there were no differences between the groups.

Receiving \$200 for attending 2 seminars and 3 follow-up visits is an event for inner-city 12-year-olds (or just about any 12-year-old). Considering that the children attended the same 3 schools in the same neighborhoods, we suspect they compared notes after the seminars about what each had been told for their \$200. The de facto result is that the study participants probably all had the same information, if not the same experience.

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1. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.

To the Editor: The Editorial by Dr DiClemente¹ criticizes the \$50 million per year allocated by Congress for abstinence education as a triumph of ideology over science. There are reasons to believe that the editorialist has made a hasty judgment.

The National Longitudinal Survey of Adolescent Health found that teens whose parents made it clear that they expected them not to have sex and expected their teens not to use birth control were much less likely to have had sex than other teens.² In addition, teens who had made a pledge of abstinence were 3 times less likely to have had sex.

Perhaps failure of the randomized controlled trial by Dr Jemmott and colleagues³ to include parents in the promotion of abstinence was 1 reason the effects were not long-lasting. Also, since the educators were assigned randomly it is possible that those chosen to teach about abstinence had no more commitment to, or confidence in, abstinence than to the safe sex message.

Teen sexual activity, with or without protection, is correlated with high-risk, self-destructive behaviors. Orr⁴ reported that teenagers who are sexually active are substantially more likely to be suspended from school, run away from home, use marijuana, be arrested by police, and for girls, to attempt suicide. Teen sex may reflect "acting out" behavior for deeper problems related to sexual abuse, low self-esteem, poor relationships, and a lack of vision and goals. These are problems that endorsing safer sex cannot solve.

For instance, condom use does not protect from infection with human papillomavirus and herpes, which are spread by skin-to-skin contact throughout the entire genital region, not just in the parts covered by a condom.⁵ Other studies show that condom use provides little to no protection for chlamydia, the most widespread sexually transmitted infection (STI) in the United States.⁶

Students should also be exposed to deeper issues, such as the ethics of love and the positive value of children being raised by 2 parents who love and are committed to each other. Young

people have many questions, but are responsible trusted adults ready to give them real answers? Or shall adults just continue to push latex "Band-Aids" to hide the issues they don't feel comfortable talking about?

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1. DiClemente RJ. Preventing sexually transmitted infections among adolescents: a clash of ideology and science. *JAMA*. 1998;279:1574-1575.
2. Klein JD. The national longitudinal study on adolescent health: preliminary results: great expectations. *JAMA*. 1997;278:864-865.
3. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.
4. Orr D. Premature sexual activity as an indicator of psychosocial risk. *Pediatrics*. 1991;87:141-147.
5. Dervin JV. Condoms won't prevent transmission of human papillomavirus. *Family Planning News*. 1992;22:12.
6. Samuels S. Epidemic among America's young. *Medical Aspects of Human Sexuality*. 1989;23:12-16.

To the Editor: Although it is interesting to see reported that safer sex methods will have an effect even 1 year after the introductory sessions, it disturbed me greatly to note that the mean age of the enrollees in the study by Dr Jemmott and colleagues¹ was 11.8 years. I suggest to the authors that the proper intervention was not education, but rather to contact the appropriate authorities, including child protective services and the police. Sexual activity for 11- and 12-year-olds is by definition, sexual abuse. In my state of Texas, if I am aware of sexual abuse of a minor, I am required by law to report it to the appropriate authorities. For Jemmott and colleagues to ignore this aspect of their research was an outrage. In his Editorial, Dr DiClemente² also completely missed the broader issue of child sexual abuse. I agree with the authors that sexual activity and STI are significant problems in these young children, but I believe the proper intervention is not sex education classes, but the timely application of social resources to stop child sexual abuse.

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1. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.
2. DiClemente RJ. Preventing sexually transmitted infections among adolescents: a clash of ideology and science. *JAMA*. 1998;279:1574-1575.

To the Editor: The Editorial by Dr DiClemente¹ contends that "it is difficult to understand the logic behind the decision to earmark funds specifically for abstinence programs." On the other hand, the authors of the article² discussed in the Editorial conclude from their findings that, regarding abstinence programs, "future research must seek to increase the longevity of these promising effects." DiClemente is right: ideology should not interfere with science. Abstinence programs are promising and should continue to be funded.

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1. DiClemente RJ. Preventing sexually transmitted infections among adolescents: a clash of ideology and science. *JAMA*. 1998;279:1574-1575.

2. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.

In Reply: We agree with Dr Berman et al that using biomarkers for STDs is valuable in HIV prevention research, but the low level of sexual experience of our participants could have yielded insufficient statistical power to make the analysis feasible. Moreover, although a positive STD test result establishes that a person had unprotected intercourse, a negative test result does not exclude this possibility. The test could be negative, not because the person practiced safer sex or abstinence, but because the person had unprotected intercourse with an uninfected partner.

Drs Cameron and Cameron state that only 80% of our participants answered the question about whether they had coitus in the prior 3 months. Actually, 85% of participants answered the question at baseline, and of the original participants, 92%, 89%, and 88% answered it at 3-, 6-, and 12-month follow-up, respectively. Cameron and Cameron misstate our results at 3-month follow-up. The virgins in the abstinence group were not marginally more likely, but significantly ($P = .02$) more likely to remain virgins at 3-month follow-up compared with the control group. The alternative analyses of the interventions' effects on the maintenance of virginity suggested by Cameron and Cameron also support our reported results. The abstinence, safer sex, and control groups did not differ significantly in the percentage of virgins who reported sexual debut by 6- or 12-month follow-up.

Cameron and Cameron incorrectly assert that there were no statistically significant differences in reports of unprotected coitus at any follow-up. Their alternative analysis of our data based on the percentage of adolescents reporting unprotected intercourse that eliminated participants not reporting sex during the prior 3 months actually yields slightly stronger results than our reported results when the appropriate 2-group comparison tests are conducted. At the 3-month follow-up, the percentage of the safer sex group reporting unprotected intercourse (24% [7/29]) was significantly lower than that of the control group (49% [19/39], $\chi^2_1 = 4.26$, $P = .04$), and marginally lower than that of the abstinence group (48% [11/23], $\chi^2_1 = 3.18$, $P = .08$).

Cameron and Cameron assert that the adolescents received \$200 for participating, but the study participants actually received \$100 for the entire 12 months of the project, which involved 20 hours. The rate of \$5 per hour is appropriate compensation. Moreover, it is improbable that cross-talk among participants suggested by Cameron and Cameron could instill the knowledge, attitudes, and skills conveyed by our 8-hour, 2-session interventions delivered by trained facilitators.

The study Mr Panzer cites is not a randomized controlled trial and therefore cannot support causal inferences about effective prevention strategies. Panzer criticizes our randomization of educators. However, all the educators were willing and able to implement all 3 curricula. Randomizing them removed potential confounding between curriculum content and

educators' ideology. Contrary to Panzer, the effectiveness of latex condoms in reducing the risk of STDs, including HIV, is well established.¹

We share Dr Moore's concern about sexual activity and STD among children. Unfortunately, a substantial number of children choose to have sex at an early age.² Turning these children in to "the proper authorities, including the police," is not an appropriate solution. Education is the key. We concur with Dr Masdeu that additional research on the long-term effectiveness of abstinence interventions is needed. Such work would facilitate the development of educational tools to combat STDs, including HIV, among adolescents.

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1. Centers for Disease Control and Prevention. Update: barrier protection against HIV infection and other sexually transmitted diseases. *MMWR Morb Mortal Wkly Rep*. 1993;42:589-591.

2. Centers for Disease Control and Prevention. Youth risk behavior surveillance/United States, 1997. *MMWR Morb Mortal Wkly Rep*. 1998;47(No. 55-3):1-89.

In Reply: Any discussion of adolescent sexuality is bound to create controversy. The article by Dr Jemmott and colleagues¹ and my accompanying Editorial were no exception.

The comments by Mr Panzer need clarification. First, the central theme of the Editorial questioned the exclusivity of the Welfare Reform Act. It is not that abstinence programs would not be a valuable strategy, they would. However, providing unprecedented federal funding for abstinence-only education in the absence of empirical evidence that such programs are effective seems unwarranted. Second, Panzer offers no empirical support for abstinence-only programs. Even though he presents relevant and valuable observational data, it seems inappropriate to equate findings derived from observational studies with data derived from a randomized controlled trial.

Moreover, there are few data to suggest that abstinence-only programs are likely to be effective in convincing many sexually active youth to abstain from sex. And although not 100% effective, appropriate and consistent condom use does afford a high degree of protection from STIs. Thus, the Editorial questioned the rationale for earmarking funds exclusively for the dissemination of unproven abstinence programs while not permitting the use of these funds for the dissemination of other, more effective comprehensive sexuality programs that provide adolescents with much-needed prevention information and risk-reduction skills.

Dr Masdeu suggests that a contradiction exists between the authors of the study and the central theme of my Editorial. No such contradiction exists. The Editorial does not argue with

the need for research to develop more effective abstinence promotion programs; it objects to the dissemination of these programs without empirical justification.

It is important to distinguish funding research studies designed to develop more effective abstinence programs, which is clearly needed, from allocating substantial fiscal resources to widely disseminate unproven programs. Thus, in the absence of evidence demonstrating the efficacy of abstinence programs, a prudent course of action would be to develop a research agenda to stimulate basic social and behavioral science research designed to develop and rigorously evaluate a new generation of innovative abstinence programs.

As Dr Berman and colleagues note, early detection and treatment of STIs can also be an effective prevention strategy. Such a strategy is greatly enhanced by the advent of new, noninvasive diagnostic tests that can be reliably used in nonclinical settings. To maximize the effectiveness of different strategies for preventing STIs, a coordinated, multifaceted, national program that tailors prevention efforts toward the individual, the family, the community, and the health care system is urgently needed.

Effective abstinence programs would be a valuable weapon in the public health armamentarium to confront the growing threat posed by the epidemic of STIs, including HIV. The public health and clinical significance of postponing adolescents' sexual onset cannot be overstated. However, priorities need to be directed away from programs that are based on persuasive philosophy or anecdotal evidence and toward those that are based on solid empirical research and demonstrated efficacy.

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1. Jemmott JB III, Jemmott LS, Fong GT. Abstinence and safer sex HIV risk-reduction interventions for African American adolescents. *JAMA*. 1998;279:1529-1536.

Euthanasia and End-of-Life Care

To the Editor: We were surprised that in their article assessing patients' perspectives of what constitutes quality end-of-life care, Dr Singer and colleagues¹ did not address the issue of euthanasia. Certainly before the availability of highly active antiretroviral treatment, many patients with human immunodeficiency virus (HIV) infection asked us whether active euthanasia would be possible if their suffering became unbearable and they had no further treatment options. During an anonymous questionnaire survey among 315 persons with HIV infection in Belgium, 82% of the respondents felt that physicians should be able to help terminate life at the explicit request of a patient in severe physical pain.²

In practice, many persons with HIV infection, even if they become severely ill, do not request euthanasia. However, the knowledge that euthanasia could be available for them on re-

quest is important in enabling them to cope better with their insecure future. Therefore, to improve the quality of life for patients with chronic and incurable disease, offering patients the option of euthanasia should be legally possible.

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1. Singer PA, Martin DK, Kelner M. Quality end-of-life care: patients' perspectives. *JAMA*. 1999;281:163-168.
2. Fleerackers Y, Colebunders R, Fonck K, Depraetere K, Pelgrom J. Euthanasia and physician-assisted suicide. *Lancet*. 1996;347:1046.

In Reply: When end-of-life care surfaces on television, in the newspapers, or on the radio, 9 times out of 10 the issue arises as "euthanasia" or "assisted suicide." At the bedside, however, the primary concerns of dying patients are the ones in our study—receiving adequate pain and symptom management, avoiding inappropriate prolongation of dying, achieving a sense of control, relieving burden, and strengthening relationships with loved ones. In our study, which involved patients receiving dialysis (n = 48), people with HIV (n = 40), and residents of a long-term care facility (n = 38), euthanasia was mentioned by less than 5% of participants in each group. Is it possible that the dramatic issue of euthanasia and assisted suicide has obscured the more "mundane" personal issues that are of primary concern to dying patients?

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Self-prescribing by Physicians

To the Editor: I agree with Dr Christie and colleagues¹ that self-prescribing antibiotics may be a problem and actually have potential negative health effects for the physician/patient, given that antibiotics are overprescribed for patients even when the physician is not the patient.

However, I am concerned with the overall tone of the article, which almost conveys a sense of immorality on the part of the responsible physician with health problems. For example, if an experienced internist cannot look at a simple lipid profile and make a determination, based on what he or she knows of the patient's lifestyle and diet, whether medical therapy would be appropriate, then what business does he or she have seeing patients with this problem? If the physician has hyperlipidemia, what objectivity is lost in looking at a simple laboratory report?

The authors specifically address physicians' "inappropriate . . . [use of] self-initiated bronchodilators to treat asthma." What would the authors propose for a physician in the middle of a busy day, on call? I imagine the physician trying to commute to an appointment that is likely some distance away (to

avoid seeing one's colleagues and friends for medical care), then having to wait 2 hours in the waiting room and cancelling the day's schedule, leaving patients in the lurch—or the physician could reach for an albuterol metered dose inhaler and get back to work.

I wonder about the moral issues here. I was trained to work as a physician. Whatever it takes to get me to work every day would be morally appropriate, in my opinion. There are far more urgent issues at hand than the self-prescribing of asthma medications, nonsteroidal anti-inflammatory drugs, and antiulcer medications for the profession of medicine to address.

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1. Christie JD, Rosen IM, Bellini LM, et al. Prescription drug use and self-prescription among resident physicians. *JAMA*. 1998;280:1253-1255.

To the Editor: As Dr Christie and colleagues¹ have so aptly pointed out, it appears that many physicians embrace the doctrine of self-prescription a little too well. The real problem does not really lie with antibiotics and allergy medicines, but rather with the self-prescribing of sedatives and pain medicines. This sets up a pattern of use that may culminate in drug abuse and dependence. As suggested by Hughes et al² and others, the self-use of medicines among residents is more for self-treatment than for recreation.

When this pattern of use is carried into practice, approximately 10% to 14% of physicians demonstrate chemical dependence^{3,4} and use opiates and sedatives at a higher rate than their societal cohorts.⁵ Medicine then must walk the fine line between overregulation, which impairs physician prescribing freedom, vs no safeguards against abuse.

A reasonable consideration would be state regulatory structures that recognize physicians' abilities to appropriately self-prescribe for common medical maladies and the emergency use of pain or sedative medicines, while prohibiting the long-term self-prescribing of controlled substances. In any case, residency training must embrace greater emphasis on the appropriate prescribing of controlled substances.

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1. Christie JD, Rosen IM, Bellini LM, et al. Prescription drug use and self-prescription among resident physicians. *JAMA*. 1998;280:1253-1255.

2. Hughes PH, Conrad SE, Baldwin DC, Storr CL, Sheehan DV. Resident physician substance use in the United States. *JAMA*. 1991;265:2069-2073.

3. Kleber HD. The impaired physician: changes from the traditional view. *J Subst Abuse Treat*. 1984;1:137-140.

4. Voth EA. The Kansas physician facing impairment. *Kans Med*. 1992;93:77.

5. Hughes PH, Brandenburg N, Baldwin DC, et al. Prevalence of substance use among US physicians. *JAMA*. 1992;267:2333-2339.

To the Editor: The study by Dr Christie and colleagues¹ presents some very interesting and disturbing findings. However, the study fails to recognize that self-prescription and self-treatment have already been determined to be an unethical practice, according to the opinion of the Council on Ethical and Judicial Affairs of the American Medical Association. The Coun-

cil's policy states, "Physicians should generally not treat themselves or members of their immediate family."² Exceptions are made for emergencies or in isolated instances when no other physician is available. The reasoning behind this policy is similar to the concerns articulated by Christie and colleagues. Self-treatment raises questions regarding professional objectivity and the assurance of quality medical care.

The Council agrees that medical schools, residency programs, and professional societies should continue to openly discuss and monitor the prevalence of self-treatment. However, such discussions should take place with the understanding that self-prescription and self-treatment, according to the ethics policy of the American Medical Association, is unethical.

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1. Christie JD, Rosen IM, Bellini LM, et al. Prescription drug use and self-prescription among resident physicians. *JAMA*. 1998;280:1253-1255.

2. American Medical Association Council on Ethical and Judicial Affairs. Opinion 8.19, self-treatment or treatment of immediate family members. In: *Code of Medical Ethics: Current Opinions With Annotations*. Chicago, Ill: American Medical Association; 1998.

In Reply: Dr Vatcher and Dr Voth help to highlight the loss of professional objectivity that may occur when physicians self-prescribe. As Dr Tenery notes, the American Medical Association's Code of Medical Ethics states, "Physicians should generally not treat themselves or members of their immediate family" because such activity compromises professional objectivity. The code also states, however, that "there are situations in which routine care is acceptable for short-term minor problems."¹ So where should the medical profession draw the line? Vatcher supports the notion that "antibiotics may be a problem," but it is acceptable to self-initiate asthma therapy and "reach for an albuterol metered dose inhaler and get back to work." In many instances, this kind of self-treatment may be perfectly reasonable. However, we feel that a loss of objectivity with self-treatment means that one can lose sight of not only where to draw the line, but also—and perhaps more important—when the line has been crossed. This may be even more of an issue, as Voth points out, when the medications are sedatives or pain relievers.

If the profession views self-prescription as a problem because it represents a potential loss of objectivity, yet studies reveal that the practice is widespread, then physicians either need to change the policy view or do something to reduce the practice. One of the characteristics of a profession is that it takes responsibility for self-improvement.

Opportunities for self-improvement may be greatest during training. Resident physicians are developing what may become lifelong patterns of behavior and professionalism. We do not think that Vatcher's view that "whatever it takes to get me to work every day would be morally appropriate," serves the profession well or delivers the right message to residents. At the same time, a rigid position claiming that any self-treatment is unethical is unlikely to deliver a credible message to

residents. Our study demonstrates that the practice of self-prescription, previously demonstrated to be widespread among established physicians, is also widespread among residents. In part because the concern about this practice is unresolved, residency program directors should put the issue of self-prescription out into the open, so that it can be discussed explicitly, rather than merely winked at.

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1. American Medical Association Council on Ethical and Judicial Affairs. *Code of Medical Ethics*. 1996-1997 ed. Chicago, Ill: American Medical Association; 1997: 186.

Treatment of Attention-Deficit/Hyperactivity Disorder

To the Editor: The American Medical Association (AMA) Council on Scientific Affairs has concluded that “. . . there is little evidence of widespread overdiagnosis or misdiagnosis of ADHD [attention-deficit/hyperactivity disorder] or of widespread overprescription of methylphenidate.”¹

Psychiatrists Marzuk and Barchas² state, “. . . the most significant conceptual shift (from *DSM-III-R* [*Diagnostic and Statistical Manual of Mental Disorders, Revised Third Edition*] to *DSM-IV* [*Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition*]) was the elimination of the rubric organic mental disorders, which had suggested improperly that most psychiatric disorders . . . had no organic basis.” Herein, they assume but do not prove that “most psychiatric disorders” have an organic basis. Goodwin³ writes, “Physicians are consulted about the problem of alcoholism and therefore alcoholism becomes . . . a disease.” Later, he acknowledges “a narrow definition of disease that requires the presence of a biological abnormality.”

Ross⁴ chides, “. . . dealing with symptoms or syndromes as if they were specific diseases reflects a trend in psychiatry to regard mental illnesses as biological entities.” The US Congressional Office of Technology Assessment⁵ concludes, “Mental disorders are classified on the basis of symptoms because there are as yet no biological markers or laboratory tests for them.”

Regarding ADHD, Ernst,⁶ a researcher, laments, “The definition of ADHD has changed over time . . . samples of children with ADHD who were diagnosed according to *DSM-III-R* criteria include children who do not meet *DSM-III* [*Diagnostic and Statistical Manual of Mental Disorders, Third Edition*] criteria.” As a neurologist, I have found no abnormality (disease) in children said to have ADHD. I have written to leading ADHD researchers asking, “Is ADHD a disease with a confirmatory, physical abnormality?” In response to my questions, on Oc-

tober 25, 1995, Gene R. Haislip of the Drug Enforcement Administration replied, “We are also unaware that ADHD has been validated as a biologic/organic syndrome or disease.” On August 5, 1998, William B. Carey, MD, of the Children’s Hospital of Philadelphia, Pa, replied, “There are no such articles [constituting proof that ADHD is a disease].”

Once children are labeled with ADHD, they are no longer treated as normal. Once methylphenidate hydrochloride or any psychotropic drug courses through their brain and body, they are, for the first time, physically, neurologically, and biologically abnormal.

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1. Goldman LS, Genel M, Bezman RJ, Slanetz PJ, for the Council on Scientific Affairs, American Medical Association. Diagnosis and treatment of attention-deficit/hyperactivity disorder in children and adolescents. *JAMA*. 1998;279:1100-1107.
2. Marzuk PM, Barchas JD. *Psychiatry*. *JAMA*. 1995;273:1715-1716.
3. Goodwin DW. *Is Alcoholism Hereditary?* Ballantine Books; New York, NY: 1988.
4. Ross C. Biological tests for mental illness: their use and misuse. *Biol Psychiatry*. 1986;21:431-435.
5. US Congressional Office of Technology Assessment. *The Biology of Mental Disorders*. Washington, DC: US Government Printing Office; 1992:13-14, 46-47.
6. Ernst M. Neuroimaging in attention-deficit/hyperactivity disorder. In: Nadeau KG, ed. *A Comprehensive Guide to Attention Deficit Disorder in Adults*. New York, NY: Brunner & Mazze Inc; 1995:95-117.

To the Editor: By promoting the diagnosis of ADHD and the use of methylphenidate as a treatment, the AMA Council on Scientific Affairs’ report¹ does a disservice. The council report fails to cite any of the dozens of critical publications spanning decades.²⁻⁶ It exaggerates the benefits of methylphenidate, claiming that short-term use improves academic performance. Reviews instead conclude that methylphenidate has no positive effect on learning but can impair it.³ While admitting there are no proven long-term benefits, the AMA report supports the long-term use of the drug. The report ignores methylphenidate’s many adverse effects.³

The council report calls ADHD a “neuropsychiatric disorder,” but it is a diagnosis of exclusion made only in the absence of any known medical or neurological cause. All the “symptoms” are drawn from normal childhood behaviors, such as squirming in a chair, acting bored, talking out of turn, and being forgetful and inattentive. When these behaviors increase in number or intensity, it really signals that the child requires more individualized attention to unmet basic needs, such as a more engaging and individualized educational environment, more rational or consistent discipline in the home or school, unconditional love, or security and safety. An increase in ADHD-like behaviors almost always indicates that we, as adults, are not giving the child much-needed attention.

The council report specifically denies that methylphenidate is used for behavioral control but the diagnostic items are entirely limited to behaviors. The drug is almost always given to suppress behaviors that signal unmet needs in the child or conflicts between the child and adults.

The council report minimizes how widely methylphenidate is being used and abused. By contrast, the International Nar-

cotics Control Board³ and the Drug Enforcement Administration⁴ warn that 90% of the world's methylphenidate is consumed in the United States, that 10% to 12% of boys aged 6 to 14 years are being diagnosed and given methylphenidate, that more high school seniors are abusing the drug than receiving it through physicians, and that methylphenidate is one of the nation's most commonly stolen and diverted substances.

Cookie-cutter diagnoses and assembly-line pharmacological treatments do not do justice to the needs of our children. On an individual level, America's children need much more attention to their personal, family, and educational needs. On a societal level, they need improved schools and family life and a value system that emphasizes their individuality rather than drug-induced compliance and conformity.

Peter R. Breggin, MD
Bethesda, Md

1. Goldman LS, Genel M, Bezman RJ, Slanetz PJ, for the Council on Scientific Affairs, American Medical Association. Diagnosis and treatment of attention-deficit/hyperactivity disorder in children and adolescents. *JAMA*. 1998;279:1100-1107.
2. Armstrong T. *The Myth of the ADD Child*. New York, NY: Dutton; 1995.
3. Breggin PR. *Talking Back to Ritalin*. Monroe, Me: Common Courage Press; 1998.
4. Drug Enforcement Administration. *Methylphenidate: A Background Paper*. Washington, DC: Drug Enforcement Administration, US Dept of Justice; 1995.
5. International Narcotics Control Board. *INCB Sees Continuing Risk in Stimulant Prescribed for Children*. Vienna, Austria: International Narcotics Control Board; 1997. INCB Annual Report Background Note 4.
6. McGuinness D. Attention deficit disorder: the emperor's new clothes, animal "pharm," and other fiction. In: Fisher S, Greenberg RP, eds. *The Limits of Biological Treatments for Psychological Distress*. Hillsdale, NJ: Lawrence Erlbaum Associates; 1989:151-188.

To the Editor: The AMA Council on Scientific Affairs¹ reasoned that review of the treatment and diagnosis of ADHD is of timely importance, as the authors rightly note that there is a public "climate of fear among physicians, parents, and educators" about treatment with psychostimulants, despite the clear efficacy these medications offer. It is, for this reason, surprising to find that the authors failed to mention that methamphetamine is also a US Food and Drug Administration–approved treatment for ADHD. No data exist that prescribed methamphetamine is more likely to be abused than methylphenidate or *d*-amphetamine. Methamphetamine is also regarded as having more potent centrally acting properties and less potent peripherally acting properties than *d*-amphetamine.² With the longest duration of action of any of the stimulants (8-12 hours), methamphetamine has the advantage of offering true once-a-day dosing. In addition, methamphetamine still has a limited role in the treatment of obesity, has antidepressant properties,³ and is an effective treatment for narcolepsy.⁴

John H. Halpern, MD
Harvard Medical School
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1. Goldman LS, Genel M, Bezman RJ, Slanetz PJ, for the Council on Scientific Affairs, American Medical Association. Diagnosis and treatment of attention-deficit/hyperactivity disorder in children and adolescents. *JAMA*. 1998;279:1100-1107.
2. Wonder PH. Methamphetamine in child psychiatry. *J Child Adolesc Psychopharmacol*. 1993;3:iv-vi.
3. Checkley SA. A new distinction between the euphoric and antidepressant effects of methylamphetamine. *Br J Psychiatry*. 1978;133:416-423.
4. Mitler MM, Hajdukovic R, Erman MK. Treatment of narcolepsy with methamphetamine. *Sleep*. 1993;16:306-317.

In Reply: Dr Baughman calls attention to a point that we made in our article, namely that to date, the pathophysiology of ADHD remains unknown. We share with him the hope that further research will elucidate it and thus improve diagnostic accuracy and treatments. We do, however, disagree with his assertion that the absence of "a confirmatory, physical abnormality" makes ADHD not a "real disease" and that pharmacological treatment represents the drugging of "normal," disease-free children. There are many conditions in medicine (eg, schizophrenia, idiopathic epilepsy) without uniform anatomic or well-understood physiological abnormalities. Physicians would do a grave disservice to patients with symptoms of these conditions by withholding empirically established pharmacotherapies until the scientific understanding reached a threshold to satisfy the austere standards proposed by Baughman.

We thank Dr Breggin for highlighting the point made in our article that patients are indeed entitled to a careful, individualized evaluation and treatment plan for suspected or actual ADHD. Indeed, Breggin's well-known, critical screeds about ADHD and methylphenidate were among the many writings that prompted the AMA to request a report from its Council on Scientific Affairs. We do not regard the 2 political books, 1 chapter, and 2 drug enforcement organization reports that he cites in his letter as being in any way scientific refutations of our report and we stand by our conclusions and recommendations.

Dr Halpern is correct that methamphetamine is a Schedule II stimulant that is used rather infrequently by US physicians in the treatment of ADHD and for the other conditions he mentions.

Larry S. Goldman, MD
American Medical Association
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Myron Genel, MD
Yale University School of Medicine
New Haven, Conn

CORRECTION

Incorrect Wording: In the Medical News & Perspectives article entitled "New Marijuana Laws in 2 States Prompt Caution" published in the December 11, 1996, issue of THE JOURNAL (1996;276:1786-1787), the words "and methamphetamine" should be deleted from the end of the second sentence of the 12th paragraph.

cies in his analyses and standards of evaluation and a willingness to use questionable or misleading statistics to support his position.

First, Kleck routinely dismisses National Crime Victimization Survey (NCVS) results on defensive gun use (DGU), claiming, incorrectly, that they capture less than (a nonrandom) 4% of all such events. Yet he uses the NCVS data concerning self-defense gun use, without caveat, to claim that guns are an effective method of self-defense.

Second, Kleck claims that DGU is far more common than offensive gun use. He obtains this result by inappropriately comparing the large overestimates of self-defense gun use from private surveys² with the estimates of offensive gun use from the NCVS. For his self-defense estimates, Kleck argues that the NCVS misses many crimes, yet when estimating offensive gun use he assumes that the NCVS captures all offensive gun uses (the NCVS misses many offensive gun uses in domestic violence and elsewhere). More methodologically correct would be to compare reports of both types of gun uses from the same survey. When this is done, as Kleck knows, whether the surveys are public (NCVS) or private,³ respondents report far fewer DGUs by them than offensive gun uses against them.

Third, Kleck gives a misleading impression about case-control studies of firearms and suicide. There have been 7 case-control studies in the United States and all 7 found a significant and substantial association between a gun in the home and suicide.⁴ Kleck has written that "One of the least productive lines of inquiry in the gun control debate has been to compare the United States with other nations,"⁵ yet he cites, with no caveat, a study that found no significant ($P > .05$) increase in suicide risk from gun ownership. As it turns out, this was a small study from New Zealand, where extensive background checks for gun ownership are common, gun storage requirements are strict, and there are virtually no handguns. The study had only 20 cases of gun suicide, but even so, in homes with guns, the odds of suicide were 40% greater than in homes without guns ($P < .10$).

Kleck's arguments are often inconsistent and misleading.^{2,6} His recent article is no exception.

David Hemenway, PhD
Harvard School of Public Health
Boston, Mass

1. Kleck G. What are the risks and benefits of keeping a gun in the home? *JAMA*. 1998;280:473-475.
2. Cook PJ, Ludwig J, Hemenway D. The gun debate's new mythical number: how many defensive uses per year? *J Policy Analysis Manage*. 1997;16:463-469.
3. Hemenway D, Azrael DR. Gun use in the United States: results of a national survey. Report to the National Institute of Justice; Washington, DC; 1997.
4. Miller M, Hemenway D. The relationship between firearms and suicide: a review of the literature. *Aggression Violent Behav*. 1999;4:59-75.
5. Kleck G. *Point Blank*. New York, NY: Aldine de Gruyter; 1991:188.
6. McDowall D, Lizotte AJ, Wiersma B. General deterrence through civilian ownership: an evaluation of the quasi-experimental evidence. *Criminology*. 1991;229:541-559.
6. Alba RD, Messner SF. *Point Blank* against itself. *J Quantitative Criminology*. 1995;11:391-410, 425-428.

In Reply: I have rebutted Dr Hemenway's claims about DGU elsewhere.¹ It suffices here to note what Hemenway cannot present—technically superior evidence indicating that DGU is as rare as NCVS data indicate it to be. Taking Hemenway's points in order: First, there is no inconsistency in regarding NCVS "estimates" of DGU frequency as inaccurate while using NCVS data to assess the effectiveness of DGU. I take the former position because NCVS-based estimates of DGU frequency have been strongly contradicted by every other source of information,¹⁻³ while I use NCVS data on DGU effectiveness because they are the best available and because there is no evidence indicating that the DGU reports captured by the NCVS are unrepresentative regarding the effectiveness of DGU.

Second, in comparing the number of defensive uses of guns with the number of criminal uses, it is not better to "compare reports of both types of guns uses from the same survey," given that no single survey has provided valid estimates of both parameters. Now that Cook has conceded that NCVS-derived estimates of DGU frequency are too low,³ Hemenway appears to be the last scholar in this field to believe they are accurate. Conversely, no private DGU survey has had the sample size and detailed questioning concerning crime incidents needed to estimate criminal gun uses. Thus, the best course is to do what I have done—use the best available estimates of each parameter, even if derived from separate surveys.

Third, readers may judge for themselves Hemenway's accuracy in describing the New Zealand study as "a small study." It is actually the largest case-control study of guns and suicide ever done, with 499 cases and 1028 controls.^{3(p279-284)}⁴ In similar fashion, Hemenway alludes to "7 case-control studies in the United States," supposedly showing a gun effect without mentioning that 5 of these were merely different analyses of the same sample of 67 or fewer adolescent suicides in Pennsylvania.^{3(p279-284)}

Gary Kleck, PhD
Florida State University School of Criminology
and Criminal Justice
Tallahassee

1. Kleck G, Gertz M. The illegitimacy of one-sided speculation: getting the defensive gun use estimate down. *J Criminal Law Criminology*. 1997;87:1446-1461.
2. Kleck G, Gertz M. Armed resistance to crime: the prevalence and nature of self-defense with a gun. *J Criminal Law Criminology*. 1995;86:150-187.
3. Kleck G. *Targeting Guns: Firearms and Their Control*. New York, NY: Aldine de Gruyter; 1997:149-162.
4. Beautrais AL, Joyce PR, Mulder RT. Access to firearms and the risk of suicide: a case-control study. *Aust N Z J Psychiatry*. 1996;30:741-748.

CORRECTION

Incorrect Affiliation: In the reply letter entitled "Treatment of Attention-Deficit/Hyperactivity Disorder" published in the April 28, 1999, issue of THE JOURNAL (1999; 281:1491), the affiliation for Larry S. Goldman, MD, was incorrect. Dr Goldman's affiliation should have been the University of Chicago, Chicago, Ill.