their mental status and provide an early warning sign of a possible suicide attempt.

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Dementia With Lewy Bodies, Visual Hallucinations, and Medications

TO THE EDITOR: In their recent study, Clive G. Ballard, M.R.C. Psych., M.D., et al. (1) "confirm" high frequencies of visual hallucinations and delusions in dementia with Lewy bodies and also conclude that visual hallucinations are significantly more persistent in this disorder than in Alzheimer's disease. Although extensive clinical evaluations were performed before death, the authors do not report the medication status of their patients. The impact of dopaminergic drugs on the mental state of demented parkinsonian patients should not be ignored. It is interesting that 66% of the patients with dementia with Lewy bodies in this study had visual hallucinations. A prior meta-analysis of dementia with Lewy bodies reports noted that 68% of the patients with dementia with Lewy bodies receiving dopaminergic drugs had visual hallucinations, but only about half that rate was found in medication-free patients (2). Dr. Ballard et al. may be prematurely attributing visual hallucinations to the pathological process of dementia with Lewy bodies per se rather than to an epiphenomenon, i.e., medication status. A review of their patients' medications could shed light on this question.

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Child Psychopharmacology, Effect Sizes, and the Big Bang

TO THE EDITOR: We read with interest the article by Karen Dineen Wagner, M.D., Ph.D., et al. (1) in the June issue. In their study comparing citalopram to placebo, we were surprised to find the authors reporting an overall effect size of 2.9. With the commonly cited criteria set forth by Cohen, effect sizes can be considered trivial (<0.2), small (0.2 to <0.5), moderate (0.5 to 0.8), or large (>0.80).

By these metrics, the reported effect size can be characterized as gargantuan, big bang-worthy. The value does not appear to be a benign typographical error for "0.29," given that "2.9" appears twice. An accurate effect size cannot be manually calculated with the information provided in the article. However, in order to arrive at the effect size of 2.9, it can be estimated that a pooled standard deviation of the change score of 2.1 would have been required. Such a narrow standard deviation of the change score seems improbable (a manual calculation with the Ns and standard deviations in the article yields a value of 15.6, for an effect size of 0.4). Moreover, such a low standard deviation of the change score would suggest uniformity in response that is far from consistent with comparable studies.

We surmise one of two possibilities. The first is that a simple arithmetic mistake occurred and was not picked up, despite otherwise meticulous attention to detail. A trickster decimal point may be to blame, and a demoted effect size of 0.29 may gain in honesty what it loses in the sex appeal of an inflated 2.9 status. A smaller effect size seems more plausible, and not only because a meta-analysis of 33 trials of selective serotonin reuptake inhibitors (SSRIs) for the treatment of adult depression (2) arrived at a pooled effect size of 0.4 but because the current study, although statistically significant, was not *that* clinically impressive. Only 36% of the patients treated with citalopram responded, compared to 24% of those with placebo (for a lukewarm number needed to treat of 8). These results, while modest, are respectable in their own right and nothing to sneeze at in a clinical area that has been short on proven therapeutic options. But a majestic sequoia of 2.9 they are not.

Alternatively, the authors may have used a different definition or formula to calculate the effect size. This would be unfortunate because the basic job description of an effect size is to facilitate communication *among* investigators and *across* measures. The gargantuan 2.9 becomes an unfortunate jarring screech of nails against the chalkboard: it robs from the melody of welcome that this timely contribution otherwise merits.

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TO THE EDITOR: Dr. Wagner and colleagues reported on a randomized clinical trial for the treatment of depressed children and adolescents with citalopram. The standard of random-

ized clinical trial reporting has been described in the CONSORT statement (1); one of its recommendations is to describe the flow of the subjects in the study (number screened, proportion randomly assigned, etc.). Dr. Wagner and colleagues did not report the proportion of subjects who were excluded from the random assignment after the single-blind period. This information is critical because a placebo run-in period might help to "wash out" nonspecific responders, allowing sharper evaluation of treatment-specific effects as shown in some pharmacotherapy studies (2).

An additional concern is the elicitation method used for adverse events at a time when the safety of SSRIs in youth has been called into question (3). The adverse events were: "reported by patients or observed by investigators" (Wagner et al., p. 1080). The reliability of this practice is questionable because some adverse events, even very severe ones, could neither be reported by the patient nor observed by the investigator and would need to be specifically assessed (4).

Finally, it is somewhat surprising that the authors do not compare their results with those of another trial, involving 244 adolescents (13–18-year-olds), that showed no evidence of efficacy of citalopram compared to placebo and a higher level of self-harm (16 [12.9%] of 124 versus nine [7.5%] of 120) in the citalopram group compared to the placebo group (5). Although these data were not available to the public until December 2003, one would expect that the authors, some of whom are employed by the company that produces citalopram in the United States and financed the study, had access to this information.

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TO THE EDITOR: We read with interest the study by Dr. Wagner et al. We have a number of concerns about this study. In the Method section, it is not clear how the patients were recruited. One is also left in the dark about the method of random assignment and if the random assignment list was concealed. The authors also give no indication of how they arrived at the sample size and if a power calculation was done. Given the recent concerns about the risk of suicidal thoughts and behaviors in children treated with SSRIs, this study could have attempted to shed additional light on the subject. The authors called the analysis of data an intent-to-treat analysis, although four patients who were lost to follow-up were excluded. In a true intent-to-treat analysis, all patients are analyzed in the groups to which they were initially assigned, regardless of whether they received the treatment or not. We consider the use of the term "intent-to-treat" in this context misleading.

Dropouts from the study have been accounted for by using the last observation carried forward. Treatment response in depression is frequently followed by a subsequent return to original or baseline values on a scale such that the last observation carried forward may be an unduly optimistic estimate. The classification of dropouts as treatment failures is based on safer assumptions than the last observation carried forward.

Our greatest concern is with the results and conclusions drawn. There is no table showing the results in detail. The authors have only stated that 36% of citalopram-treated patients met the criteria for response, compared to 24% of patients receiving placebo. This response rate, while in itself marginal compared to other studies of antidepressants, does not in itself show that citalopram is better than placebo.

We calculated the absolute benefit increase of using citalopram as 0.12 (95% confidence interval [CI]=–0.015 to 0.255). The relative benefit increase that could be attributed to citalopram was 50% (95% CI=–135% to 6%). The odds ratio, i.e., the odds of improving while taking citalopram compared to placebo was 1.75 (95% CI 0.92 to 3.43). The number needed to treat, i.e., the number of children who need to be treated with citalopram for one additional positive outcome was eight (95% CI=4 to infinity). None of these shows that citalopram is any better than placebo.

We would argue that the authors did not provide sufficient evidence to support their claim that "citalopram produces a statistically and clinically significant reduction in depressive symptoms in children and adolescents" (p. 1082). We are surprised that the most respected psychiatric journal in the world published a study that is misleading to its readers in the extreme.

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Dr. Wagner and Colleagues Reply

TO THE EDITOR: Dr. Mathews and colleagues request further information about the randomized, placebo-controlled trial of citalopram for treatment of depression in children and adolescents. Randomization was on a 1:1 basis and was stratified by age group. The random assignment list was concealed from the investigators, which is fundamental to the claim that