

subjects at baseline (mean=2.66, SD=0.49) and after treatment (mean=2.56, SD=0.51) resulted in $t=0.37$, $df=12$, $p=0.72$, with a 95% confidence interval of -0.48 to 0.68.

Our current clinical observation cautions us that atomoxetine may also be of limited value in children who do not respond to treatment with standard stimulant therapy. The role of atomoxetine needs to be firmly established with a cost-effective analysis if it is to be considered as first-line therapy, and its effectiveness in nonresponders should be demonstrated if it is to be considered for an expensive trial among nonresponders to first-line stimulants.

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Autism, Movement, and Facial Processing

TO THE EDITOR: Ami Klin, Ph.D., et al. (1) showed that a high-functioning autistic adult looked at mouths rather than at the eyes of adults' faces when viewing naturalistic social situations, while a normal comparison adult showed the opposite pattern. According to this argument and others, the authors argued that low orientation to salient social cues embedded in naturalistic situations is a core deficit in autism.

In their Letter to the Editor, Chantal Kemner, Ph.D., and Herman van Engeland, Ph.D., M.D. (2), wrote that when autistic children are shown a static presentation of faces, they do not reach the conclusion of Dr. Klin et al. They argued that the discrepancy between these results is due to a difference in the presentation of facial stimuli, i.e., the dynamic presentation in the study of Dr. Klin et al. versus the static presentation in their own study.

We confirm that low-functioning autistic children are impaired in the processing of physical environmental movement, particularly rapid movement (3), while high-functioning autistic children are much less impaired in the same type of tasks. When biological movement is concerned, autistic children perform relatively adequately in emotional and non-emotional expression-recognition tasks when facial expressions are displayed slowly on video (4). Along the same line, low-functioning autistic children better recognize dynamic facial expressions when displayed slowly than when presented at normal speed. Considering these arguments and others, we proposed the rapid visual-motion integration deficit hypothesis in autism (5). According to this hypothesis, some autistic individuals having major movement-processing disorders from early in their lives will avoid rapid physical and biological movements (considered as aversive stimuli),

thus disrupting secondarily social interaction. Some of these individuals, or some autistic persons having minor motion-processing disorders, will search for, habituate themselves to, and learn to handle and cope with such kinds of stimuli. To summarize, rapid visual-motion processing deficits constitute a core neuropsychological marker of autism and secondarily account for the deficit in social interaction.

Thus, when the autistic subject focuses on the mouths of adults' faces in the study by Dr. Klin et al., he or she probably attempts to capture facial speech information that is difficult to process accurately and efficiently in naturalistic social situations while avoiding looking at the fastest facial movements (i.e., saccadic eye movements).

Therefore, discrepancies between the results of Dr. Klin et al. and of Drs. Kemner and van Engeland might be due to the severity of autism in the subjects tested in their respective studies and to the kind of presentation of facial stimuli.

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Dr. Klin and Colleagues Reply

TO THE EDITOR: We thank Dr. Gepner for his letter concerning our article and the letter of Drs. Kemner and van Engeland discussing their findings (1) relative to ours. A word of accuracy, however, needs to precede our reply. Drs. Kemner and van Engeland were not reacting to our review in the *Journal* (that included a single case illustration) but to our case-control series, which appeared in the *Archives of General Psychiatry* (2). And it was our suggestion (3), not that of Drs. Kemner and van Engeland, that the discrepancy in results between the two studies could be due to the type of stimuli used in the two studies: static, i.e., pictures, by Dr. Kemner and her colleagues (1) versus dynamic, i.e., videotaped social situations by us (2).

Dr. Gepner's hypothesis of a rapid visual-motion integration deficit in autism is interesting, but we must take issue with his explanations of our data. First, Dr. Gepner hypothesizes that some individuals with autism may avoid rapid physical and biological movements (considered as aversive stimuli), which would, developmentally, disrupt social interaction. In our clinical experience, young children with autism may in fact be fascinated with rapid movements, particularly if these are repetitive or create unusual sensory sensations (e.g., shining reflections as in spin-top or repetitive patterns like in a computer screensaver). As in many areas of perceptual research in autism in which a basic process was proposed to underlie more global visual attention to social stimuli, we

feel that it is important to study the given process in both nonsocial and social paradigms so as to ensure that the deficit is general rather than a phenomenon that occurs only in social situations.

Second, Dr. Gepner hypothesizes that our finding of increased visual fixation on the mouth region (rather than the eyes) of people in our videotaped stimuli reflects the participants' attempt to capture information that is difficult to process accurately and efficiently in naturalistic social situations. In other words, individuals with autism would focus on the mouth because they were overwhelmed by the rapid movement nature of naturalistic social phenomena. While this is a possibility, we doubt that this is the only factor accounting for our results. For example, in some of our eye-tracking illustrations (e.g., Figure 3 in our article, p. 900), there is hardly any movement taking place. In addition, our eye-tracking studies of toddlers with autism (4) showed that young children with autism also focus a great deal on mouths, and in our video stimuli of a parent's approach to a toddler, the mouth region is where most of the movement is taking place (because the caregivers are continuously talking).

Third, Dr. Gepner's own work (his references 3–5) suggests that lower-functioning individuals with autism (i.e., those with a degree of mental retardation) are impaired in the processing of physical environmental movement, particularly rapid movement, while higher functioning individuals with autism (i.e., those without accompanying mental retardation) are less impaired in such tasks. In other words, low-functioning children do worse than high-functioning children on visual tasks involving rapid movement. Applying this notion to the discussion of our results and those of van der Geest and colleagues, Dr. Gepner hypothesizes that the discrepancy may be due to the severity of individuals with autism included in the two studies, with our participants being more cognitively disabled than those included in the study by van der Geest and colleagues. Inspection of subject characterization data on the two studies does not support this hypothesis. The participants in our study were both older and more cognitively able than the group of participants in the study by van der Geest and colleagues. In fact, the viewer with autism whose individual eye-tracking data were described and illustrated in detail in our review article was a 38-year-old man who has been followed-up in our center for most of his life and who has a very impressive list of accomplishments, including college graduation in a competitive institution and two master's-level degrees. He was, nevertheless, quite socially disabled and appeared to have a typical manifestation of autism in a cognitively able individual.

In sum, we agree with Dr. Gepner about the importance of future research on this topic but do not believe that his suggestions account for the results obtained in our eye-tracking studies to date.

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Religion, Spirituality, and Mysticism

TO THE EDITOR: In regard to the article by Jacqueline Borg, Psychol., M.Sc., et al. (1), we were intrigued by the potential association between the serotonin system and the character trait of "self-transcendence." However, we urge caution in interpreting these results as evidence of a biological basis for religion/spirituality. A careful definition of terms is required to avoid confusion.

"Spirituality" is a relatively new term that refers to the nearly universal human search for meaning, often involving some sense of transcendence. Some suggest that spirituality is a broader concept than religion and is therefore more appropriate for pluralistic contexts (2). However, it is not clear that spirituality actually describes a more universal phenomenon (3). In fact, the concrete and individualized manifestations of "spiritualities" tend to function not as universals but precisely the opposite: unique religions with only one member (4).

On the other hand, "religion" refers to a particular tradition, practice, or community that shapes a comprehensive worldview sufficient to interpret all of human experience within a specific cultural context (3). Within any religious tradition such as Catholicism, there are many practices that may be considered faithful and normative, such as Benedictine monasticism, "third-order" Franciscan spirituality, or the prayer of the Rosary. Of all the practices of spirituality, a small subset might be called "mystical" or marked by visions, "revelations," or losing the sense of oneself.

Although we doubt a direct correspondence, such mystical experiences may appear similar to the auditory and visual hallucinations induced by psychotropic drugs, such as LSD. Some aspect of such mysticism may be mediated by the serotonergic system. However, Dr. Borg et al. used religion and spirituality interchangeably, and they reduced religion/spirituality to mere mystical experience. This reduction is misguided and potentially confusing. Many faithful religious practices have no mystical component, and in fact, several religious traditions specifically reject the type of mystical experience addressed in this article (5).

Therefore, we suggest that the variability in serotonin may not be best understood as a marker for religiousness, per se, but as a marker for a more general character trait found in both religious and nonreligious people. It would be interesting to repeat a similar study within two populations of religious and secular subjects. We suspect that within both populations, a subset might have lower binding potentials that would correlate not only with "spiritual acceptance" but also with the metrics of mystical experience and perhaps recreational drug use. Finally, given the hypothesis that self-transcendence is environmentally influenced by the differences