

der abuse than their younger peers. Fourth, recall bias is an intrinsic problem of retrospective, cross-sectional research.

Further research with independent corroborators is needed to ascertain the ways in which the confounding nature of retrospectively recalled memories affects adults' self-reported childhood maltreatment (8). As an alternative method, one might begin with records of determined cases of child maltreatment in state or county child protective services files and compare victims' adult psychological functioning by means of a case-control cohort design. Finally, we need to know more about the "ordinary magic" of resilience (9) relative to psychiatric outcomes of child maltreatment. The majority of victims do not succumb to suicidality or psychopathology, suggesting there may be mediating and moderating factors related to resilience vis-à-vis risks.

Dr. McHolm et al. told us little about the policy or applied practice implications of their research. For many readers of the *Journal*, these aspects are certain to be more important than the specific source of the researchers' sample. Replicating the consistent findings of prior research with community-based samples contributes little to our knowledge base. Research in this area could be enhanced significantly by controlling for the developmental stages at which the victims' maltreatment occurred as well as the historical contexts that shaped attitudes toward abuse and neglect. More sophisticated research is needed, and ultimately, clinical science and our clients benefit with higher standards of research and reporting.

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Dr. McHolm and Colleagues Reply

TO THE EDITOR: We thank Ms. Mathy for her interest in our work. We agree that high standards of research and reporting benefit both clinical science and clients; however, we disagree with a number of her comments.

Ms. Mathy cites two articles to challenge our statement that no studies have examined suicidality among a community-based sample of adults with a childhood history of physical abuse. The article by Silverman et al. (1996) regarding the long-term sequelae of child and adolescent abuse followed individuals only to age 21; our work extended this line of investigation to the entire lifespan. The article by McCauley et al. (1997) examined a clinical group of patients from primary care internal medicine practices. This fact was noted by Santa Mina and Gallop in their review (1998), another of Ms. Mathy's citations. Further, Santa Mina and Gallop reported that there were "no studies specific to childhood physical abuse" and underscored the need for "additional studies of...specifically physical abuse from...community subgroups." Although we agree with Ms. Mathy that it is important to consider the overlap between forms of maltreatment, the majority of existing research examines a combination of maltreatment types or focuses on childhood sexual abuse. Far less is known about the association between childhood physical abuse specifically and psychiatric impairment.

In terms of the methodological concerns expressed, her comments reflected general issues that researchers in this field have grappled with for some time. Ms. Mathy suggests that our research "ignores" the relationships between duration, intensity, and frequency of abuse. As noted in the article, data were derived from a comprehensive mental health survey. Although it would have been interesting to include more parameters of maltreatment, practical issues of response burden prevented us from doing so. Ms. Mathy also identifies recall bias and aspects of the timing of maltreatment (e.g., the developmental stage) as limitations of the retrospective research design. We acknowledged potential limitations, such as recall bias, in our Discussion section. Alternative methods that would address such limitations are not easily applied to community samples. Ms. Mathy suggests the use of child protection cases of maltreatment and matched comparison subjects as a preferred research design. In fact, Widom (1) has published widely since 1989 on the results of such a cohort study. However, this case-finding approach precludes the opportunity to study a community sample, as was our focus.

Finally, we must disagree with Ms. Mathy's characterization of our research as a "replication study." We do not claim to have produced the definitive study of the complex interrelationships between suicidality and its correlates. We do, however, suggest that the article contributes to our understanding of suicidality in depressed women through its 1) examination of correlates from multiple domains, 2) focus on childhood physical abuse, 3) investigation of the cumulative impact of psychiatric comorbidity, and 4) separate exploration of suicidal ideation versus attempts within a community sample. Given the potential clinical significance of research in this area, we encourage Ms. Mathy and others to join us in the challenge of furthering our understanding of the relationships between suicidality and correlates such as childhood physical abuse and depression.

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The Nosology of Juvenile Mania

TO THE EDITOR: Ellen Leibenluft, M.D., et al. (1) presented an informative and useful realignment of the nosology for juvenile mania. The authors considered an array of important “methodological and conceptual issues” in their analysis, but they did not clearly distinguish between the methodological and the conceptual. That is, to what extent do the authors put forth the new categorization on the basis of the difficulties in assessment of DSM criteria in the context of the juvenile population? Or do they believe that there is a fundamental distinction among the categories they propose? If the latter, to what extent is the conceptual distinction limited to the juvenile population, or should it be applied or adapted for adults as well?

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Dr. Leibenluft and Colleagues Reply

TO THE EDITOR: We appreciate Dr. Pincus's comments on our article. We suggested these clinical phenotypes for juvenile mania because of the difficulties that arise when clinicians and researchers try to apply the DSM-IV criteria to children. The question of whether there is a fundamental distinction between these categories is an empirical one, and in the article, we suggested research strategies for addressing it (see our Table 1). For example, it is important to ascertain whether there are consistent differences between the phenotypes in neuropsychological and physiological function, longitudinal course, familial variables, etc. Should such differences exist, subsequent studies in adults would be warranted.

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Delusional Thoughts in Alzheimer's Disease

TO THE EDITOR: The article by David Sultzer, M.D., et al. (1) provides strong additional support, by way of correlation analyses of the observer-rated severity of delusions, for the contribution of right frontal brain dysfunction to the appearance of abnormal beliefs in Alzheimer's disease. This form of analysis has the merit of accounting for the contribution of other variables, such as age, age at onset, and severity of dementia, as well as the behavioral factor of agitation, to variations in regional brain metabolism. There are, however, some comments to be made about the interpretation of the results and,

perhaps more important, about the method of study adopted by the authors.

The findings were seen as evidence for a linear relationship between delusional “severity” and the degree of impairment of metabolism in areas of the right frontal cortex. There are challenges to this interpretation. It is equally possible that the content and personal significance of the delusions described (about half of those outlined could reasonably be considered elements of a misidentification syndrome) might have had some variable influence on the behavioral assessment of delusion severity on the Neurobehavioral Rating Scale. In other words, an association of the nature, as much as neuropsychiatric severity, of abnormal beliefs with quantitative variation in regional brain metabolism has not been fully examined. Equally, there is evidence from case studies that delusions that have a substantial impact on behavior (and would have been highly rated on the Neurobehavioral Rating Scale) may appear at the minimal stage of Alzheimer's disease in association with subtle and confined cortical dysfunction and that they impair a specific set of cognitive abilities (2, 3).

The results of the study extend previous evidence from cross-sectional studies of similar populations. Reliance on a dimensional approach in a group showing diverse delusional phenomena, however, may continue to divert attention from methods more likely to foster an analytic understanding of delusional states. These methods will rely on the study of multiple single cases, as has been so fruitful in the analysis of Capgras syndrome (4), and will likely combine detailed clinical phenomenology, functional imaging, and cognitive neuropsychology (5). The discrimination of delusions with a factual content satisfying traditional clinical criteria from affectively laden persecutory beliefs may well be of heuristic value but will not sufficiently inform etiological studies in both organic and functional delusional disorders. Firmly held factual delusional beliefs can arise from specific memory failures and be affectively laden when the disorders of memory or other aspects of cognition involve issues of autobiographical knowledge and personal identity.

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Drs. Sultzer and Mendez Reply

TO THE EDITOR: We appreciate the comments by Drs. Shanks and Venneri that address the interpretation of results from