

our study. Cerebral mechanisms involved in the development and expression of delusions in Alzheimer's disease are complex and multifactorial. In their letter, two important concepts are highlighted: 1) specific brain regions and neural processes involved in the expression of delusions are variable and depend on the content of the delusional thought, and 2) deficits in particular cognitive abilities may play an important role in the development of delusional thoughts, perhaps more important than the extent of isolated cortical dysfunction. We agree. Regional cortical hypometabolism and specific cognitive deficits are probably interdependent contributors to delusions.

In our Discussion, we noted several factors that may influence the relationship between regional brain function and delusional thoughts in Alzheimer's disease. We also presented a hypothesis linking dysfunction in specific cortical areas to delusions with particular content or emotional valence. This hypothesis has some empiric support but needs to be rigorously tested. The models of cognitive and physiological contributions to the development of delusions that are cited by Drs. Shanks and Venneri (Ellis and Lewis, 2001, and Frith, 1999) provide a coherent view of possible intermediary processes. Studying patient groups with uniform delusional thoughts and including cognitive neuropsychological evaluations along with measures of regional brain function, as proposed by Drs. Shanks and Venneri, can help test these hypotheses and further refine etiological models.

There are some practical challenges in this approach. Groups of patients with uniform delusions tend to be small, patients often have more than one delusion, and the content can change within certain bounds over time. Moreover, a delusion's interpersonal meaning, emotional valence, or cognitive link to personal identity can be difficult to measure but may be critical in its cognitive or physiological development. There also may be a conceptual advantage to studying patients with a variety of delusional thoughts. Regardless of thought content, memory deficits, or perceptual influences, patients with delusions are unusually willing to accept an implausible conclusion. This cognitive process that extends beyond specific thought content may itself have discrete neurobiological underpinnings.

A complementary approach may be the optimal research strategy for the near term. Studying larger groups of patients with well-characterized thought content and cognitive abilities can help clarify etiological mechanisms. We are currently studying a larger group of patients with Alzheimer's disease with targeted neuropsychological assessments, hoping to shed more light on the factors that influence the development of delusional thoughts.

An additional critical goal is to examine the effects of neural systems rather than individual cortical loci. Discrete nodes in cortical circuits probably each contribute certain aspects to delusional thought content or its genesis, but broad systems are involved in the overall expression. Studies that examine activity in multiple cortical sites and their interactions, along with an assessment of specific content and cognition, as suggested by Drs. Shanks and Venneri, may contribute to an etio-

logical model that elaborates brain mechanisms and can be applied to clinical care.

DAVID SULTZER, M.D.
MARIO MENDEZ, M.D., PH.D.
Los Angeles, Calif.

Suicide and Psychotic Depression

TO THE EDITOR: I was intrigued by the findings of Meena Vythilingam, M.D., et al. (1) who reported that patients with psychotic major depression have a greater risk of death than patients with nonpsychotic major depression. This finding was based on a study that used survival analysis to assess the outcomes of 61 psychotic and 59 nonpsychotic depressed patients who were followed up for 15 years after hospital admission. The authors also reported that a positive dexamethasone suppression test (DST) result was associated with psychotic depression.

My colleagues and I reported relevant data some time ago that contrasts with these findings (2). We examined suicide rates in a group of 1,593 patients who had been hospitalized for unipolar and bipolar mood disorders. Subjects were followed up for 14 years. We concluded that psychotic and nonpsychotic subjects had similar risks for suicide and that among patients with a mood disorder, psychosis did not predispose to suicide. Dr. Vythilingam and co-workers did not specifically address the issue of suicide. We also reported that for a subset of 423 subjects given the DST, a positive result was significantly associated with the presence of delusions (3). A positive DST at baseline was not associated with subsequent suicide.

References

1. Vythilingam M, Chen J, Bremner JD, Mazure CM, Maciejewski PK, Nelson JC: Psychotic depression and mortality. *Am J Psychiatry* 2003; 160:574-576
2. Black DW, Winokur G, Nasrallah A: Effect of psychosis on suicide risk in 1,593 patients with unipolar and bipolar affective disorders. *Am J Psychiatry* 1988; 145:849-852
3. Black DW, Monahan PO, Winokur G: The relationship between DST results and suicidal behavior. *Ann Clin Psychiatry* 2002; 14: 83-88

DONALD W. BLACK, M.D.
Iowa City, Iowa

Drs. Vythilingam and Nelson Reply

TO THE EDITOR: We are pleased that Dr. Black was interested in our article on psychotic depression and mortality. He states that he and his colleagues reported relevant data some time ago that contrast with our findings. Namely, they found no increase in suicide in psychotic depression. Dr. Black then notes that we did not address the issue of suicide.

The contrasting findings that Dr. Black refers to in his letter are unclear. We will reiterate our findings. First, we reported that psychotic depression was associated with all causes of mortality—not suicide. Thirty of the 37 deaths were from medical causes, and the increase in mortality in psychotic depression appeared to be from medical causes. In fact, Dr. Black's findings are similar to ours in that the suicide rate in psychotic depression was not significantly different from the rate in nonpsychotic depression. Further similarities are

highlighted by the absence of a significant relationship between DST results and suicide.

Second, Dr. Black overlooked our discussion of suicide in the Results and Discussion sections. In our group, there were three definite suicides and one death that was suspicious for suicide. These four cases were evenly split between the psychotic and nonpsychotic groups.

We do acknowledge, however, that the findings from our study are in contrast to those from another study that demonstrated an association between DST nonsuppression and suicide risk in patients with affective disorders (1). Prospective studies in larger numbers of subjects with psychotic and nonpsychotic depression could help tease out the complex relationships between neurobiology, clinical symptoms, and mortality.

Reference

1. Coryell W, Schlesser M: The dexamethasone suppression test and suicide prediction. *Am J Psychiatry* 2001; 158:748–753

MEENA VYTHILINGAM, M.D.
J. CRAIG NELSON, M.D.
Bethesda, Md.

Suicide Among Police Officers

TO THE EDITOR: I have several comments regarding the methods employed in the study of suicide among New York City police officers by Peter M. Marzuk, M.D., et al. (1), as well as the interpretation of the police occupational context.

This study compared police officers with the general population of New York City. While age, gender, race, and region were statistically adjusted for, an inaccurate comparison of suicide rates may have resulted. The comparison involved a healthy and psychologically tested working group (the police) with the New York general population, which included the unemployed, institutionalized, incarcerated, and mentally ill. These population groups generally experience higher suicide rates. Thus, the study compared a New York population containing segments that have high suicide rates with the police, who *should have relatively low suicide rates*. Even if this study were accurate, the fact that police officers have suicide rates equal to those of the New York population demonstrates that suicide is a problem.

The work exposures involved in policing are confounders that add considerable weight to an analysis of suicide. Incidents such as witnessing death, encountering abused children, and street combat weigh heavily as precipitants to depression, alcohol use, and suicide. The study may have better compared the police with an occupation similar in confounder weight distributions, in addition to including such confounders in the analysis to assess their impact.

While psychological testing is an important screening tool for bringing in officers suitable for police work, it does not tell the whole story. Exposure and job socialization in policing have profound impacts on officers. It was interesting that the mean age of suicide for police officers in this study was 33.5 years, an age much younger than the national norm for suicide. It was also interesting that police suicide rates were noticeably unstable, while population rates remained stable over the 20-year period. The high police suicide rate in 1994, for example, occurred during a time of citywide internal investigations into a police drug scandal.

Some researchers have stated that occupation is not on the list of suicide risk factors. While we cannot yet be certain that police work by itself is a suicide risk factor, we can state that it serves as a fertile arena for suicide precipitants, including relationship problems, culturally approved alcohol use, firearms availability, and exposure to psychologically adverse incidents. This job is part of the causal chain of suicide.

In sum, this study reflects statistics that tell us that we need to look deeper into police suicides and their root causes. While statistics such as rates per 100,000 tell us about numbers, they do not tell us about suicide risk. We may be better informed if we know the inherent risk of police suicide in both a quantitative and a contextual sense. Policing is a psychologically dangerous occupation. We still have a way to go with police suicide research, but I remain with the premise that police are at a significantly higher risk for suicide.

Reference

1. Marzuk PM, Nock MK, Leon AC, Portera L, Tardiff K: Suicide among New York City police officers, 1977–1996. *Am J Psychiatry* 2002; 159:2069–2071

JOHN M. VIOLANTI, Ph.D.
Buffalo, N.Y.

TO THE EDITOR: In their article, Dr. Marzuk and colleagues reviewed the rate of suicide among New York City police officers and compared it with the rate of suicide among New York City residents. After adjusting for demographic differences, they found that the rate of suicide was lower among the police officers (14.9 per 100,000 person-years) than among the general population (18.3 per 100,000 person-years).

It is estimated that over 90% of the individuals who commit suicide suffer from diagnosable psychopathology, including substance abuse and dependence, mood disorders, and psychotic disorders (1, 2). New York City police officers are screened for the presence of psychiatric disorders at the time that they are hired. No doubt, some preemployment psychopathology is missed, and some officers develop addiction problems and other psychiatric disorders while working in the department. It would be interesting to know the incidence and prevalence of such disorders among police officers during the period studied.

If we assume that few of the officers who committed suicide had diagnosable preemployment psychopathology, it may not be a fair comparison to look at their rates of suicide compared with the residents of New York City generally unless the prevalence of serious psychopathology among the comparison population is accounted for. The fact that the rate of suicide among police officers is about 80% of that of the general population may speak to the enormous stresses associated with police work, and the more proper conclusion may be that being a police officer greatly increases the risk of suicide in individuals suffering from no significant preemployment psychopathology.

References

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