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Uncertainties in Assessing and Treating Assaultive Patients

To the Editor: In an article in the April issue Quanbeck and colleagues (1) emphasized the heterogeneity of assaultive acts committed by state hospital patients. Such observations stimulate further discussion of the report from Pennsylvania on reducing use of seclusion and restraint (2). Although Quanbeck and colleagues' report did not specifically address seclusion and restraint, it underscores the importance of having a range of available interventions to reduce the adverse consequences of assaultive behaviors.

Although the Pennsylvania group demonstrated the feasibility of reducing seclusion and restraint (2), further clarification of secondary risks appears warranted. The rate of mechanical restraint declined by 60% from 1998 to 2000, from 2.96 to 1.20 episodes per 1,000 patient-days; however, staff injuries with lost work time resulting from patient assaults apparently increased by 30%, from .10 to .13 episodes per 1,000 patient-days.

On the basis of the stated census of 2,800, this rate increase represents an increase in the number of injuries from 102 to 133. Whether this apparent increase in injuries was related to reduced seclusion and restraint requires consideration. Added to this figure would be self-injuries, injuries to patients from patient-on-patient assaults, and less severe and unreported assaults on staff (3,4).

Many rightly state that seclusion and restraint should be a safety measure of last resort, used only when there is an imminent risk of harm and no other safe and effective intervention is available (5). However, assessing imminent risk and the availability of an alternate intervention is fraught with uncertainty and might be considered within the framework of true- and false-positive predictions. True-positive assessments correctly predict that restraints are required to prevent violence; false positives incorrectly predict such need. True negatives correctly predict that restraints are not needed; false negatives incorrectly predict that restraints are not needed. Some have called for bringing restraint rates to zero (5), which would imply that true- and false-positive rates can approach zero. For complete safety, the false-negative rate would also be zero: no violence would occur that is preventable only by restraint. In addition, violence may still occur among those with a true-negative assessment if caregivers make errors in carrying out preventive interventions. The data suggest, however, that even when experts carry out assessments and interventions, it may be unrealistic to completely and safely eliminate restraints in the complex clinical setting of the state hospital.

In state hospitals, dangerousness based on aggression is a common criterion for admission, and as underscored by Quanbeck and colleagues, psychiatric hospitals may retain some high-risk persons who engage in criminal violence. Realistic preventive treatments may not exist for many of these patients.

Assault rates in our state hospitals are too high, and the suffering of as-

sault victims must also be addressed. In our hospital, patients' assaults on staff in recent years have caused facial fractures requiring reconstructive surgery, concussions with neurological sequelae, and a death. Reducing seclusion and restraint may lead to unacceptable risk, as suggested by a near tripling of assault rates in one setting (3) and the apparent increase of 30% in Pennsylvania (2).

Strategies, such as those described by Quanbeck and colleagues, to characterize subtypes of assaultive behaviors may help identify optimal preventive interventions for those at risk of aggression, thereby reducing the rates of true and false positives and false negatives.

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In Reply: We think Dr. Becker makes an extremely important point. Although our report did not specifically address use of seclusion and restraint, the most common reason for their use is the management of assaultive behavior (1). Therefore, if an intervention succeeds in reducing the number of aggressive incidents

committed by patients in a psychiatric facility, use of seclusion and restraint will also decrease as a natural consequence.

Numerous organizations have expressed concern about the deleterious psychological effects and physical risks associated with seclusion and restraint (2). The Centers for Medicare and Medicaid Services and the Joint Commission on Accreditation of Healthcare Organizations have emphasized the need to limit their use to emergency situations. In response, psychiatric facilities across the nation have made concerted efforts to reduce seclusion and restraint use, and many have succeeded. However, the actual success of these efforts should be examined closely. As Dr. Becker points out, some facilities have reported that a decrease in seclusion and restraint use has been accompanied by a corresponding increase in the number of assaults and injuries to staff. In our opinion, a successful seclusion and restraint reduction program achieves a significant decrease in the number of aggressive incidents along with a decrease in use of seclusion and restraint.

Psychiatric hospital administrators must achieve a delicate balance when implementing policies that are intended to reduce use of seclusion and restraint. They must protect psychiatric patients from unnecessary or prolonged confinement in seclusion and restraints. At the same time, they must be careful that a policy is not so restrictive that clinicians are unable to justify use of seclusion and restraint when necessary to prevent injuries to staff and other patients. Staff injuries from patient assaults can exact a heavy toll in terms of physical disability, psychological damage, lost work days, and poor staff morale. Further, psychiatric patients are entitled to "safe conditions of confinement," and mental health professionals in many states have a statutory duty to protect third parties—for example, as set forth in *Tarasoff* (3).

We believe that the most effective approach to this problem is to target the root causes of aggressive behavior rather than rely on administrative

policies that seek to restrict or even ban use of seclusion and restraint (4). Although our current understanding of the causes and optimal treatment of aggressive behavior remains limited, and this lack of knowledge likely plays a role in the misuse and overuse of seclusion and restraint, staff education can have positive effects. For example, after an urban psychiatric hospital implemented a staff training program on the management of assaultive behavior and mandated weekly team meetings to discuss strategies for treating aggressive patients, a decrease in both staff injuries and the rate and duration of restraint use was observed (5). The results of our study suggest that inpatient aggression is a multifaceted problem that requires heterogeneous treatment interventions; any "one size fits all" approach will not be effective in the long term.

Cameron David Quanbeck, M.D.

Barbara E. McDermott, Ph.D.

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Concerns About Study Design

To the Editor: An article in the May issue of *Psychiatric Services* by Perry and colleagues (1) compared public attitudes toward depression in childhood and adulthood. Although the authors made an interesting comparison and they acknowledged several design flaws in their

study, they did not address certain weaknesses and underplayed many that they did mention.

As the authors note, the vignettes used in the study were not identical. The most salient difference was that only the vignette describing the child mentioned suicidal ideation. The authors referred to the vignettes as "equivalent but not identical," but they did not give a clear explanation of how equivalency was assessed. They noted that this may have had only minor impact on the results because the differences in "danger to self" ratings were small and the differences in "danger to others" ratings were larger. However, it is insufficient to assume that the differences between the vignettes can be dismissed on the basis of outcomes. For the two vignettes, 79% of respondents rated the child as "likely to be violent toward self," and 74% of respondents rated the adult in this way. In both cases the rating was high. The ratings of danger to others were much lower, 40% and 30%, respectively.

Given highly publicized recent events, such as school shooters killing others and themselves, it is possible that the suicidal ideation mentioned in the child vignette caused the difference in ratings of danger to others. If the adult vignette had been the only one to mention suicidal ideation and if the study had been conducted in the early 1990s when post office and workplace shootings, rather than school shootings, were highly publicized, the results may have been reversed.

This brings us to our next point. The adult vignette was used for data collection in 1996, and the child vignette was used in 2002—six years later. The authors suggest that this had little impact on their findings. It is well known in social sciences that a cohort effect can occur when data are collected at distinctly separate times. In addition, because the vignettes were used at two different times, there was no randomization in the populations given the vignettes. The authors did not report whether there was a significant difference between the participant groups. Data provided

were limited to gender, age, and race. Of particular concern in a study such as this one are education level, knowledge of depression, and socioeconomic status. And finally, there was a dramatic surge in school killings between 1996 and 2002, and many books, news magazines, and other media outlets hypothesized that mental illness plays a role in children's violent acts. There is thus reason to believe that public perceptions of children might have changed.

Additional problems with the study include not providing information about age—or age range—for the adult and the child in the vignettes. Respondents' varied perceptions of their ages would have made a substantial difference in their interpretation of the vignette. Binary coding of data may not have appropriately captured participants' views, particularly the views of those who were ambivalent about the topic. Also, given the large sample, reporting analyses of power, in addition to *p* values, would have been appropriate.

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In Reply: We are pleased that Lenz and Houlihan's notions of the contributions and limitations of our study converge with our own, and we welcome the opportunity to address these issues in more detail.

First, the authors note that the vignettes describing adult and child de-

pression were not identical. As we acknowledged, the inclusion of suicidal ideation in only the child vignette may have skewed public attitudes. However, the similarity in participants' ratings of danger to self for the adult and the child suggests that suicidal ideation was not a "red flag" for respondents. Moreover, both vignettes, which were developed by psychiatrists, represent the typical symptom profiles of adults and children meeting *DSM-IV* criteria for major depression. Because suicidal ideation is of particular importance in the clinical assessment of childhood depression, its presence in the vignette is appropriate.

Second, the six-year lag between the two surveys may account for the observed differences. One possibility that Lenz and Houlihan mention—that public attitudes toward children with depression were shaped by media reports linking school violence to mental illness—is, in fact, one explanation we offered. There are no data to directly test this assertion, but it remains a plausible interpretation. However, it is telling when the public views youths with depression as more dangerous than adults with depression. The cultural origins of public attitudes are of little consolation to providers, families, and children who face barriers to treatment and recovery.

Third, Lenz and Houlihan suggest that our results might have been different had the comparison been conducted in the 1990s. In fact, there were several highly publicized workplace shootings in 1995 and 1996, which suggests that the serendipitous timing of the 1996 and 2002 General Social Survey provides an apt comparison of the media-propagated culture of fear surrounding adults and children with depression. Moreover, if these workplace shootings skewed attitudes toward adults with depression negatively, our analyses may actually have underestimated differ-

ences in attitudes toward adults and children.

Fourth, as noted in our introduction to the series (1), the age of the vignette child (eight or 14 years old) varied randomly. Respondents believed that the problems of the 14-year-old were more serious (Pescosolido BA, Jensen P, Martin JK, et al, unpublished manuscript, 2006), and they were more likely to prefer social distance from the 14-year-old (2). However, post hoc analyses revealed that the public did not believe that adolescents with depression were more likely than children to harm others. This finding suggests that both are vulnerable to stigmatization associated with perceptions of dangerousness.

Finally, regarding the use of power and the binary coding, we ran *t* tests as well as multinomial logistic regression analyses using the categorical dependent variables. These results led us to the same conclusions as those obtained by use of binary logistic regression models. We presented odds ratios to facilitate interpretation of the effect magnitude of receiving either a child or an adult vignette.

We hope that future research will continue to raise questions about how the public views mental illness among children, adolescents, and adults. Understanding the larger cultural context will assist clients, providers, and families in overcoming challenges associated with stigma.

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Jane D. McLeod, Ph.D.

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