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A Case of Neutralized Findings?

To the Editor: A funny thing happens on the way to determining whether an element of coercion is effective in some cases of outpatient treatment. No one wants to find out. The study by Phelan and colleagues (1) in the February issue is another example. It is beyond me why these excellent researchers would have used their time and New York State's scarce mental health resources to do this work.

The study is particularly seductive because its elements appear to be the concrete in the foundation of evidence-based findings. The study compared outcomes of an experimental group treated under New York State's assisted outpatient treatment statute (Kendra's Law) and a control group discharged from a psychiatric hospital and attending the same outpatient facilities as the patients in the experimental group. The authors state that because assisted outpatient treatment includes not only coerced treatment but also enhanced services, "We cannot conclude which of these elements of the package deal contributed most to the generally positive outcomes for participants." However, the researchers knew this before they began the study. The study as de-

signed had no possibility of evaluating coercion per se. The article's last sentence—"We therefore caution against using our results to justify an expansion of coercion in psychiatric treatment"—is disingenuous at best.

More puzzling is the last sentence of the abstract, which goes even further: "As such, the results do not support the expansion of coercion in psychiatric treatment." But the results don't refute the expansion of coercion either, which the authors could just as easily have stated in their concluding sentence.

Furthermore, the findings actually do, in some respects, support the use of coercion. The authors found that the patients who received assisted outpatient treatment were significantly less likely to engage in serious violent behavior, had a lower risk of suicide, and had better illness-related social functioning. There was also a trend for patients in this group to have a better quality of life. These findings could result from the coercion plus ancillary services. However, as a take-home message the authors offer their additional finding that the experimental group reported marginally less ($p < .10$) coercion and stigma than the control group. Thus some degree of coercion led to their experiencing less coercion and stigma. In and of itself, that is a profound argument for the benefits of the coercive component in assisted outpatient treatment.

Currently, a debate is going on in New York State about whether to make Kendra's Law permanent. Despite the authors' gyrations to neutralize their own findings, their study provides data that supports Kendra's Law, and it provides absolutely no evidence that refutes the concept that a "tincture of coercion" (2), as used in assisted outpatient treatment, is an integral element in the treatment of a subset of patients in community-based psychiatric care.

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In Reply: Dr. Geller wants to know whether "coercion is effective in some cases of outpatient treatment." He is disappointed that our article did not answer the question that he assumes to be the only reason for conducting a study such as ours. Dr. Geller's question is interesting but by no means the only question of importance in evaluating outpatient commitment under Kendra's Law. We begin by indicating why we cannot answer the question Dr. Geller poses and then discuss the value of what we did learn.

As discussed in our article, one of the constraints on researchers who study outpatient commitment is that such research must be conducted within existing systems of treatment and criminal justice. Kendra's Law mandates both court-ordered treatment and enhanced services for individuals assigned to assisted outpatient treatment. The legislation stipulates that the interventions be delivered as a combined package. Our opportunity as researchers was to study the effects of this "package deal" by comparing individuals assigned to assisted outpatient treatment with those who were not.

At the outset of our study there was substantial disagreement about the utility of outpatient commitment under Kendra's Law as it was implemented, with the full combination of legal coercion and enhanced services. Some people predicted that the policy would be harmful, whereas others were convinced that it was beneficial. Therefore, it is important to point out that our results could have been dramatically different, such that Dr. Geller would have had to critique us instead for being unable to know whether coercion is harmful in some

cases of outpatient commitment. We learned something important about a real-world policy.

Specifically, we learned that outpatient commitment, as enacted under Kendra's Law, was associated with less violence perpetration and suicide risk and improved illness-related social functioning. Equally important, we learned that outpatient commitment was not associated with perceived stigma or coercion among those receiving the intervention. These are things we did not know before the study was conducted, and they were not the answers that many would have predicted. Our study of Kendra's Law was largely motivated by career-long concerns over the problem of stigma associated with mental illness. We were surprised that individuals assigned to assisted outpatient treatment did not report more stigma or coercion.

Dr. Geller is correct that our results neither "justify an expansion of coercion in psychiatric treatment," as we stated, nor "refute the expansion of coercion either," as he stated. However, because our results generally reflected favorably on the effects of outpatient commitment, we felt that unwarranted expansion of coercion was a more likely consequence of our publication than unwarranted restriction of coercion. This is why we chose to emphasize the former in our conclusion.

No study can answer all questions. We recognize the value of disentangling the combined package of legal coercion and enhanced services. But this in no way undermines the importance of studying the success or failure of real-world policies as implemented in real-life settings with consequences for real people.

Jo C. Phelan, Ph.D.
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Helpful and Unhelpful Risk Assessment Practices

To the Editor: Snowden and associates' (1) report in the November 2009 issue on a validation study of the Clas-

sification of Violence Risk (COVR) is an example of a worrying trend toward the promotion of unhelpful risk assessment practices in mental health services (2).

The authors' frequent use of the terms "future violence" and "prediction" might lead some readers to conclude that they are describing a method for predicting future violence. In fact, risk assessment does not, and cannot, predict the behavior of individuals but instead places the individual being assessed in a group of persons whose likelihood of violence is similar but of whom only a proportion will ever actually be violent.

This confusion between a population forecast and an individual forecast is then compounded by the authors' use of the area under the curve (AUC) statistic as the main measure of the effect size of risk assessment. The AUC is a single summary statistic of the trade-off between sensitivity (the true-positive rate) and specificity (the true-negative rate). As a consequence the size of the AUC is not affected by the base rate of the harm being considered. In this study the AUC is the probability that when we randomly pick one patient who went on to be violent and one who did not, the instrument will have assigned a higher score to the violent patient than to the non-violent one. Although this is an interesting property of the COVR, it hardly amounts to a prediction.

A far more useful risk assessment measure is the proportion of high-risk categorizations that are later found to be correct—the positive predictive value (PPV). PPV has the advantage over AUC of being easy to understand, and although it is dependent on the base rate of harmful events, PPV is more useful precisely because the accuracy of categorizing patients as high or low risk is highly dependent on the base rate. When combined with the proportion of correct negative predictions (negative predictive value), PPV provides a comprehensive description of the actual performance of risk assessment categorizations in the population being considered.

The prediction of rare events, such as homicide by a person with schizo-

phrenia, is inevitably associated with a low PPV even when the sensitivity and specificity (and thus AUC) is high, whereas more common events, such as physical aggression in a hospital, could be associated with a higher PPV, even if the risk assessment instrument has a lower sensitivity and specificity. Even after careful review of the paper by Snowden and colleagues, the reader cannot truly judge the utility of the COVR to predict aggression in an inpatient setting because only the AUC and correlation were reported. In our view the AUC should be reported with other measures, including the optimal sensitivity and specificity of the instrument recommended by the researchers and measures of the accuracy of positive and negative predictions in the reported sample.

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Dr. Large is affiliated with the Prince of Wales Hospital, Dr. Ryan is with Westmead Hospital, and Dr. Nielssen is with the St. Vincent's Hospital, all in Sydney, Australia. Dr. Large and Dr. Nielssen are also with the School of Psychiatry, University of New South Wales, Sydney. Dr. Ryan is also with the Centre for Values, Ethics and the Law in Medicine, University of Sydney.

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In Reply: The letter from Large and colleagues describes issues that have appeared in the literature many times. The interested reader should start with Harris and Rice (1). Here we have room only for a précis.

First, our study was predictive in the sense that this word is used in risk assessment. We assessed at time 1 and assigned a probability of violence based on group membership and

then followed the patient for three months.

Second, Large and colleagues suggest that risk assessment has nothing to say about an individual. The COVR classifies people into groups (low risk to high risk and so forth) and on the basis of previous research assigns a “risk rating” according to how many people in that group go on to commit the target behavior. So, for example, we might find that among people in the highest-risk group, 75 of 100 go on to commit the specified behavior, and among those in a lower-risk group, only 25 commit this behavior. Thus would it not be sensible to use this information to allocate resources and plan accordingly? Let us take another example. Perhaps I want to take a trip somewhere to find some sun. How should I choose where to go? I could look at the previous number of days of sunshine for different regions. I might find that New South Wales has 75 days of sunshine per 100, whereas South Wales has only 25 per 100. Which should I choose? I presume Large and colleagues would say it doesn't matter because these are only group data and do not apply to any individual day.

The third issue is in how to present the efficacy of a risk-assessment tool. Most scientific studies tend to use ROC analysis because it is independent of the base rate and therefore we can assess the efficacy across different contexts, changes in the definition of violence, and so forth (2). Indeed the AUC statistic (also known as C) is well established in medicine as the preferred index of test accuracy (3,4). Testing accuracy was the aim of our study, and hence we used this statistic. However, risk assessment and management are not merely a matter of accuracy. Clinicians and managers must deal with such matters as policies and limited resources. These factors have an impact on decision making and on selecting one or more thresholds at which actions are taken. Hence statistics such as the positive predictive value and negative predictive value are very important for the real-work situation (with the COVR or without any formal aid), but they

are context dependent and thus not a good way of comparing and contrasting risk assessment techniques.

The independent validation of such instruments is a crucial step toward evidence-based practice that can aid the clinician in making very difficult judgments related to risk. It is hard to see how such science could be regarded as a “worrying trend toward the promotion of unhelpful risk assessment practices in mental health services.” Do the authors suggest that we should abandon such aids and simply guess about risk?

Robert J. Snowden, Ph.D.
Nicola S. Gray, D.Clin.Psych., Ph.D.

Acknowledgment

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Lifting the Veil

To the Editor: The Personal Accounts column by Maggie Jarry (1) in the December issue is a call to action for psychiatrists to pay attention to the needs of children whose parents have mental illness. Other developed countries, such as the United Kingdom and Australia, have extensive programs to support these children and young adults, but the United States lags substantially behind (2).

Young caregivers want to be respected, included, and acknowledged by the professionals treating their parent. They have a range of needs—

from the child who is coping well and needs information to the child who needs support, treatment, and perhaps protection. The extent of a child's vulnerability depends on many factors, such as the developmental stage of the child and the degree of social isolation of the family.

Until the United States can develop a more comprehensive approach, each psychiatrist can make an individual effort to work with the children of parents with mental illness. We should include children in family meetings about the parent's illness. Parents may want to exclude children “to protect them” or out of fear that children will be removed if the parent is seen as unfit. However, children often have questions about the parent's illness and unfounded fears that are distressing and that distort their understanding of the situation, and children can benefit from support and education. When children visit the parent in the hospital, they can be given age-appropriate literature.

Psychiatrists should advocate for a broader vision of health and recognize that parenting is an essential human role and can be a significant rehabilitative factor in helping a person reach wellness. When a person with a mental illness is a parent, activities supporting his or her parenting should be discussed as part of recovery. Most important, children and other family members should be invited into the room.

There are many barriers to delivering comprehensive care to families. Prominent barriers include the lack of awareness among adult psychiatrists of the needs of these children and the lack of collaboration between child and adult psychiatric services, which keeps the needs of these children invisible. As noted above, parents' fears also conspire to keep children's needs hidden. Finally, perhaps the greatest barrier is a systemic practice in which one person is the identified patient and the needs of other family members remain unrecognized. For example, the current reimbursement system does not facilitate preventive screening of children who might be at risk.

Changes in policies and in health care delivery are needed. Recently it has been recommended that mothers of children with depression also be assessed and treated for maternal depression (3). Can we ask for the reciprocal: that children of parents with mental illness get assessed, and supported and treated if necessary? The Family Committee of the Group for the Advancement of Psychiatry is exploring ways to raise awareness of this issue and to develop interventions that might be accepted and easily incorporated into the clinical arena.

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In Reply: More than five million children in the United States have a parent with a serious mental illness, such as schizophrenia, bipolar disorder, and major depression, and some of these parents have co-occurring addictions. It is estimated that 68% of women and 57% of men with a serious mental illness are parents (1) and that 73% of women and 68% of men with posttraumatic stress disorder are parents (2). Yet it remains common to find mental health professionals who are unaware of or unwilling to see the

relevance of this topic to their work and to the success of the people they serve.

With this in mind, we strongly urge readers to consider the concrete recommendations in the letter from Dr. Heru and her colleagues. However, the scope of this dialogue and their recommendations should go further. Our key message is this: Just as we now recognize that employment is a viable option for people with mental illness, we need to recognize that parenting is also a viable life choice for these individuals, including those with illnesses regarded as severe and persistent. Supported parenting should be integrated into all adult mental health systems.

Training for mental health professionals should include an understanding of the benefits of working with families. Professionals should be aware of and make available the supportive resources needed to address consumer parents' deepest concerns for their children and thereby contribute to parent and family wellness. Strengthening parenting skills enhances familial attachment and motivates parents toward recovery while also addressing the developmental needs of their children. Data should be collected on the parenting status of people who use adult mental health systems in order to understand where supportive parenting resources can be incorporated. Resources to enhance psychoeducation for young family members exist and are being rapidly developed in the United States and abroad (the fall 2009 special issue of *Psychiatric Rehabilitation Journal* provides an overview). These resources can be used today while we develop a national infrastructure for supported parenting. The Substance Abuse and Mental Health Services Administration should publish materials for supportive education of children who have a parent with mental illness, as it has for children with addicted parents.

Furthermore, parents should not fear losing custody of their children

when they disclose their personal health information to any authority, nor should adults who access mental health services be discouraged from having children if that is one of their life goals. For these reasons, mental health systems should partner with child and family services systems to support families.

Understanding the unique experiences of daughters and sons who have a parent with mental illness should be a goal of future research. Daughters and sons need access to information that identifies and normalizes their common experiences. Research should go beyond estimating the statistical likelihood of future mental illness among these children to encompass other aspects of this group's experience and demographic characteristics. Daughters and sons often report difficulty with identity formation and preoccupation with the belief that they will experience the same mental illness as their parents. Therefore, we urge that the recommendations in this letter be balanced with an understanding that the need to screen children for illnesses is small compared with the need to unveil fortifying messages of resilience and hope within the family.

Maggie Jarry, M.S.

Lindy Fox, M.A., L.A.D.C.

Carol Coussons de Reyes, M.S.

Ms. Jarry, author of the December Personal Accounts column, is joined in her response by Ms. Fox, who is affiliated with the Dartmouth Psychiatric Research Center, Concord, New Hampshire, and Ms. Coussons de Reyes, a certified peer specialist with the Office of Consumer Affairs, Division of Behavioral Health, Lincoln, Nebraska.

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