Art and Science

To the Editor:
I was excited when I saw the title of your March 2000 Editor's Note: "Sex Appeal." As I read the editorial, I agreed—something is missing. I also agreed with your comments on the need for research and the application of theory, creativity, and science in moving the profession forward. Yes, we need evidence to justify what we do and to encourage consumer confidence in our services.

I liked your reference to Star Trek. As one who was accused of going where few therapists have gone before, I know the hope of a better tomorrow in rehabilitation. We cannot allow the rest of the world to move ahead of us, and the vision you refer to is there; its potential is just not being maximized.

But you did not balance your comments appropriately. Medicine, and I include physical therapy as a big part of that, is both an "art" and a "science." You emphasized the "science" part, and sometimes in today's evidence-based environment, we forget that there is also an "art" to what we do. If we put too much emphasis on the "science" part, we run the risk of becoming too technical in our approach to service delivery, too "cookbook." A wise person once said, "The long road between scientific work and the care of a patient is a road of uncertain interpretations, many of which are subjective in nature."

The "art" part brings in the intuitive reasoning and the gut passion, those intangibles that make that special relationship between the therapist and the patient not just an intervention, but an experience. The "art" part allows the innovative creativity that you refer to. The "science" part takes from history and fact, then opens the doorway for invention and creative innovation. The "art" part allows us to take the "science" part and apply it toward the vision and the needs of the future. That is why physical therapy is both an "art" and a "science." Now that is sexy.

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Constraint-Induced Movement Therapy

To the Editor:
Blanton and Wolf presented a case report of a patient with upper-extremity hemiparesis due to a stroke who received constraint-induced movement therapy. The Wolf Motor Function Test (WMFT) and the Motor Activity Log (MAL) were applied to assess the effect of the treatment. We are concerned about the use of these outcome measures, the clinimetric quality of which is still uncertain. Apparently, the validity of data obtained with the WMFT and the MAL has never been evaluated. The interrater reliability was reported to range from .95 to .97 for the WMFT and to be .94 for the quality-of-movement scale of the MAL. Despite careful and repeated readings, we were unable to find the reliability value of .94 in the reference cited.

We agree with the suggestion of Blanton and Wolf that "[future studies including large sample sizes within a randomized clinical trial should be considered."

In fact, an observer-blinded randomized clinical trial on the effectiveness of treatment involving forced use of the upper extremities in 66 patients has been performed at our hospital. In this trial, the MAL was used as a secondary outcome measure.

We found 1.2 and 0.7 points mean improvement on the amount-of-use scale of the MAL after the intervention in the experimental and control groups, respectively (corrected for baseline differences and presence or absence of hemi-neglect). The surprising improvement in the control group might be due to a placebo effect or a "hello-goodbye effect." The latter term refers to the observation that patients may exaggerate their problems before therapy, hoping to be eligible for the intervention, and minimize their problems at the end of the intervention to "please" the staff with their improvement.

In our opinion, the risk of eliciting a socially desirable answer from the patient to the MAL questions is enhanced when the MAL is administered daily during the treatment phase, as Blanton and Wolf described. It should be noted that the MAL is essen-
tially a "subjective" measure, meaning that the patient's ratings may be influenced by his or her expectations as well as those of the interviewer, which may be unconsciously expressed in the way the questions are posed. To assess its criterion validity, the use of the affected arm during functional activities should be observed in the patient's home environment, but for many practical reasons this would not be feasible. Alternatively, by using the instrument as a secondary outcome measure in a trial, along with primary outcome measures with known clinimetric properties, more insight can be obtained into its construct and content validity, reproducibility, and responsiveness. We used the Action Research Arm Test and the Rehabilitation Activities Profile as primary outcome measures in our trial, both of which have been shown to yield valid and reliable measurements.1-3

Until more is known about the clinimetric properties of the WMFT and the MAL, we recommend that these instruments should not be applied as primary outcome measures in a trial that is intended to give a valid answer regarding the effectiveness of an intervention for improving upper-extremity function.

**References**


**Author Response:**

We thank Dr van der Lee, Dr Becker- man, Dr Lankhorst, and Dr Bouter for their interest in our case report1 and for the thought-provoking comments they have offered. We will respond to their comments in the order in which they were presented.

There is clear indication that the Motor Activity Log (MAL), developed in both its preliminary form2 and its present form3 by Taub and his team at the University of Alabama at Birmingham, yields reliable measurements when they are obtained repeatedly before intervention and over an extended time interval.4 According to Uswatte and Taub,3 at least one dimension of validity is reflected in high correlations (intraclass correlation coefficient [3,1]=.90) between patient and caretaker scores obtained separately from one another at different points in time.

With respect to the Wolf Motor Function Test (WMFT), we do have preliminary data on the reliability and validity of WMFT scores, but the data are not yet published in a peer-reviewed form. Briefly, our preliminary data show that intraclass correlation coefficients ranged from .97 for raters performing repeated measures among subjects with no known cardiovascular impairments to .99 for the same measures among age- and sex-matched patients with stroke. Moreover, there was excellent concurrent validity with data obtained with the Fugl-Meyer Upper Extremity Assessment Test.5 Although these data are not as yet published, correlations were -.54 (P=.0166) at a first rating session and -.68 (P=.0014) at a second session measured among 19 patients with chronic stroke, with sessions separated by 14 to 16 days.

There was no difficulty in discriminating between the WMFT scores of subjects without stroke and those of subjects who were poststroke. The reliability value of .94 initially ascribed to the Miltner and colleagues4 should have been referenced to a report by Taub et al5 and to an article by Taub et al6 that indicated no change in a control group's MAL scores from pretreatment to posttreatment measurements. Further explanation about the reliability of MAL scores is addressed in a letter to the editor by Taub and Uswatte that appeared recently in Stroke.8

We have begun a blinded, crossover, national randomized clinical trial on constraint-induced movement therapy among patients with subacute stroke. This trial has been given the acronym EXCITE (EXtremity Constraint-Induced Therapy Evaluation). We plan to recruit at least 240 subjects from 7 locations, and we will use the same protocol as described in our case report.1 The subject in our case report was part of the pilot study done in preparation for this EXCITE trial. Our group is well aware of the published randomized trial7 to which van der Lee and colleagues refer. We suspect that the less favorable results seen by the van der Lee group were perhaps due to the fact that their subjects had higher baseline MAL scores than did our subjects. Another variable affecting outcomes...
may be that the intensity of intervention used in their study was not as profound as that used in our work or as used by Taub and his colleagues. In fact, the baseline score on the amount-of-use scale of the MAL reported by van der Lee et al (2.2 points) approaches the exclusion criterion for our clinical trial; that is, their patients may be too high functioning for them to see substantive change.

We do not think that the improvement in the control group in the study by van der Lee et al is due primarily to a placebo effect. Their control patients were actually receiving a physical therapy–based treatment, the intensity of which might easily have fostered some improvement. We question seriously the possibility that our patient was attempting to please us in this case report. In fact, she was even reluctant to participate. During her exit interview, there appeared to be no indication that her effort was directed toward accommodating to the wishes of the investigators. Her continued improvement for the next 3 months would tend to dispel that notion, as any intended or inadvertent behavioral modification on our part would most likely have dissipated.

We do not question the subjective aspects of the MAL. As noted earlier, the high correlation between caregiver and subject responses supports the belief that a high degree of congruence exists beyond subjectivity. The MAL is not intended to condition patients into providing "expected" responses, nor will it be offered repeatedly during the clinical trial. It was administered daily in this case report to provide us with a running account of changes in patient perception of real-world use of the affected limb. At no time did the patient see scores from previous days, nor is there any reason to believe that these previous scores over 10 days were retained in memory.

Another alternative for assessing criterion validity for limb use in everyday activities is to monitor adherence to wearing a constraint for the less impaired arm, which can be done through use of a contact switch embedded within a splint and interfaced to a microprocessor (counter). Additionally, a proxy, but objective, measure for limb usage can be secured through monitoring motions using accelerometry. Uswatte et al have developed this approach. Monitoring adherence to splint use and limb use with accelerometry is an important feature incorporated into our clinical trial.

There is little doubt that other clinical measures, such as the Action Research Arm Test and the Rehabilitation Activities Profile, could be important outcome measures. We feel they may not be most appropriate for subjects with the motoric characteristics sought in our work. We hope that the information presented in this response sheds more light on the psychometric properties of the MAL and the WMFT. Hopefully, clinicians wanting to use these tests will recognize that their value is augmented by their simplicity and the ease with which these real-world and laboratory-based measures can be complemented with more expensive, objective (ie, reliable) physiological tools.

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References
8 Taub E, Uswatte G. CI therapy and massed practice: a reply to van der Lee et al [letter to the editor]. Stroke. 2000;31:987-988.