Author's Response

My basic intent in my special communication was to show that considerable precedent and evidence exist to support the value of measuring muscle strength in patients with brain lesions. As I indicated in the conclusion, "muscle strength is by no means a variable of such great importance that it can be justified as a sole indicator of status, change, capacity, or outcome in patients with brain lesions." I also recommended that "because considerable variance in patient performance remains unexplained by muscle strength, the search should continue for even better targets of measurement and treatment." Nevertheless, two of the purposes of research are to "focus attention on neglected issues" and to "provoke rethinking of taken-for-granted assumptions."1(p302) My hope is that my special communication will provide clinicians with information that will help them to rethink the notion that muscle strength is not an important issue and is not an appropriate variable for measurement in patients with brain lesions.2

In examining the commentary of Rothstein et al, I note three issues (which I will rephrase as questions) to which I would like to respond. First, how credible is the research that provides evidence for the relationship between muscle strength and function? Second, how should we interpret correlations between muscle strength and function? Third, can or should we seek to predict future status from present status?

How credible is the research that provides evidence for the relationship between muscle strength and function? Much of the research that I cited, no doubt, has actual weaknesses or weaknesses that can be imputed to it because of incomplete explanations of method. Rothstein et al have done their usual thorough job in addressing the weaknesses. Nevertheless, I believe that most of the research that I cited, including my own, has value. I would repeat two points that I have made previously.3 First, I agree with Vanderbroucke that the substance of the problem is more important than the method of the study and that clinicians should keep an eye on the "general principles of validity in the background."4 Second, I reiterate the comments by Lewin that "history teaches us that original publications about major breakthroughs in medicine are very often methodologically weak," that "the niceties of the methodological kind most typically flourish when everybody is already convinced of some theses," and that we should "try to collect new evidence by whatever means, rather than apodictical theorizing about methods, since methods, like alchemy, will never solve the essential problem of what is truth 'out there, in reality.'"5 Particularly in harmony with the last quotation, I believe that the diversity of methods used to measure muscle strength in the studies I cited serves to strengthen rather than weaken the argument that a relationship exists between muscle strength and function. If relationships are examined using different methods, I believe the results may be more valuable, particularly to clinicians who may be using methods similar to those that are different.

How should we interpret the correlations between muscle strength and function? I have no argument with the concerns of Rothstein et al that the correlation between muscle strength and function should demonstrate not only statistical significance but strength as well. I also appreciate, as a close reading of my own studies will verify, the consequence of calculating multiple correlation coefficients. When correlations are reported, their value as well as their significance is available for the reader to judge. Correlations of .40 to .70, the range in which correlations...
between muscle strength and function often fall, have been described by Guilford as moderate and as demonstrative of substantial relationships. I am pleased, personally, when I read of 25% of the variance in one variable being explained by another. I am elated by an explanation surpassing 50%. Although much variance is thus left unexplained by muscle strength, we are provided with at least some meaningful explanation of the dependent variable. Given the diversity of brain-injured patients, sources of explanation other than muscle strength are expected, even if not examined. To expect a "gold standard" predictor variable is as unrealistic as to expect to find a golden fleece or fountain of youth. If, however, we can show correlations between muscle strength and function in the moderate (.40-.70) to high (.70-.90) range, we have done more to scientifically identify potentially fruitful targets of intervention than has been done previously. If many of the correlations are reported in the same study, it only goes to show that the relationship is not limited to a single muscle group. I would think that such a fact would be important to many clinicians.

Can we or should we seek to predict future status from present status? As we are not sovereign over our patients’ outcomes, we should no doubt exercise caution. Still, I believe that a basic, albeit not necessarily widely held, concept in stroke rehabilitation is that future status is a function of present status. Therefore, patients who are admitted to rehabilitation in a severely affected state seldom complete rehabilitation at as high a level as patients who are admitted in a minimally affected state. This is not only true for muscle strength but for speech,7 gait performance,8 and overall functional status9 as well. Although I believe deeply that we can help our patients achieve higher levels of muscle strength and function, I do not have any evidence to suggest that our interventions are nearly as important to outcome as the foundational capacity with which the patient begins rehabilitation.

I trust that the views expressed heretofore, along with my special communication and the commentary by Rothstein et al, will stimulate progress in our profession. Such progress is sorely needed for the benefit of the large number of patients who have brain lesions.

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References