Letters to the Editor

Pointers on Purposeful Activity Study Argued

I would like to commend Thomas M. Steinbeck for his well-designed study entitled "Purposeful Activity and Performance," which appeared in the August 1986 issue of the American Journal of Occupational Therapy (pp. 529-534), and to congratulate him for receiving the 1986 AOTA Cordelia Myers Writer's Award for that article. It was gratifying for me to see that my study, "Motivation as a Factor of Perceived Exertion in Purposeful versus Nonpurposeful Activity," published in the journal in March 1984 (pp. 165-170), was used as a model for building further research. My study won the same writer's award for 1984, and I am very appreciative of the recognition that AOTA has given to these experimental studies of a topic that is important to support the philosophy of the occupational therapy profession.

However, I must respectfully disagree with Steinbeck on his point that my conclusions were compromised by my methodology. Steinbeck wrote, "the heart rates recorded in Kircher's study indicate that her subjects performed two dissimilar activities in terms of work load and, therefore, her conclusion that the differences in performance were due to purposefulness is compromised by her procedure" (p. 533). I would like to refer to a Letter to the Editor in the February 1985 issue of the journal (pp. 114-115), in which another writer challenged my research on a similar point. My response was that potential dissimilarities in work load in the two exercises had little bearing on the outcome of perceived exertion, in the way that my research was designed (pp. 115-116).

Steinbeck made the accurate point in his article that other researchers have established that heart rate can be used to classify exercise in relative intensity, with a higher heart rate indicating a greater work load. This point was made and referenced in both my article and response to the Letter to the Editor. It is because of this research that he and I were able to use the Borg scale as a valid measure of perceived exertion in our research. The significance of this to our particular study models (and I'll paraphrase my response to the 1985 Letter to the Editor) is that people ordinarily can perceive their exertion level accurately, regardless of how strenuous the exercise is. If either the purposeful or the nonpurposeful exercise had been a more strenuous exercise, and all other factors had been equal, the subjects simply would have reached the designated level of the Rating Scale of Perceived Exertion (RPE) sooner in the harder exercise and stopped at a comparable heart rate. Because they did not do so, but continued in each study to a higher physiological level of exertion—as was evidenced by a higher heart rate in one of the two compared exercises although perceived exertion was the same for both exercises—one may conclude that there was some factor operant other than mere differences in work load of exercise. I assert that it is likely that this other factor was motivation. It seems that this interpretation could also apply to Steinbeck's results, because, in his study, the heart rates were significantly higher in the purposeful hand activity than in the nonpurposeful hand activity. Further, I would propose that the reason the heart rates were not higher in Steinbeck's purposeful lower extremity activity may lie in his own discussion of preference for the compared activities. He explained that 6 subjects actually reported a preference for the nonpurposeful lower extremity activity over the purposeful activity and that there was no significant difference in expression of interest for female subjects (50% of the subjects) between the nonpurposeful and purposeful lower extremity activities. In my study, volunteers were chosen to be subjects if they stated that they enjoyed competence at jumping rope as children and demonstrated competence by performing the activity in a pretest for 1 minute. All of my subjects were female because it was discovered earlier that males, in general, tend not to enjoy jumping rope and not to be as competent as females. Interest in activities does have some sex specificity as yet in our society.

Although I explained above that it was the physiological point (heart rate) at which the subjects stopped performing the exercise (the level of perceived exertion being the constant), and that therefore a possible difference in work load of the two exercises would not affect the outcome, I nonetheless set up my study so that the heart rates of the performers were monitored at 1-minute intervals to determine if there was a possible difference in work load. Analysis by t test revealed no significant difference in the rate of change of the heart rate when the two exercises were compared! As mentioned in my 1985 response to the Letter to the Editor, there was a misprint in my 1984 article: "7-minute interval" should have read "1-minute interval" (March 1984 issue, p. 167).

Another error of my own in the
paper, which was pointed out later by
Carol Knedeisen, then a graduate stu­
dent in occupational therapy at Texas
Women's University, was that the
amount of time the subjects per­
formed the purposeful activity was
significantly longer (p < .05) than the
time they performed the nonpurpose­
ful activity. I reported the time as
"nonsignificant" in my paper because of
a misreading of a statistics table.
Nonetheless, I maintain that a physio­
logic measure such as heart rate—not
time or number of repetitions—is the
most crucial, objective measure of
how the perception of exertion can be altered by another factor, such as
motivation.

Steinbeck asserted in his paper
that because the lower extremity ac­
tivities used in his study were re­
corded at equal levels of activity on
the electromyogram and heart rate re­
cordings, they were of comparable
work load, even though the subjects
performed a significantly greater
number of repetitions in the purpose­
ful activity. If one performed more
repetitions of one activity, or per­
formed that activity for a longer pe­
riod of time than another activity, to
reach the same point of perceived ex­
terion at a similar heart rate, I would
not conclude that the two activities
were similar in work load. I would
conclude that the activity that de­
manded more repetitions to reach the
same heart rate and level of exertion
was the lesser of the two in work
load. It seems to me that the purpose­
ful and nonpurposeful lower extrem­
ity activities in Steinbeck’s study were
questionable in terms of preference
for the subjects, and I feel it is this
point, rather than their relative simi­
larities or dissimilarities of work load,
that most probably affected the out­
come of the experiment.

The heart rate and electromyo­
gram findings both were significantly
higher for the purposeful hand ac­
tivities in the Steinbeck study. No ques­
tion of preference for the purposeful
hand activity was reported in his
paper. The hand activity results sup­
ported the results of my own exper­
iment, making each one a stronger as­
sertion. The results of his lower
extremity activities as yet remain on
their own. We need more studies to
decide this issue!

Marjorie A. Kircher, MS, OTR
Portland, Oregon

Author’s Response
I welcome Marjorie Kircher’s com­
ments concerning my article "Pur­
poseful Activity and Performance"
(American Journal of Occupational
Therapy, August 1986). In that study I
sought to determine whether the
presence of a goal or purpose would
have an effect on an individual’s per­
formance on paired purposeful and
nonpurposeful activities requiring
equal levels of exertion. To do so I
believed it was imperative that all
physiologic factors be controlled and
equal in both activities in order to
reasonably attribute differences in
performance to the inherent purpose­
fulness of one activity over the other.
Kircher, on the other hand, did not
carefully control for differences in the
physiologic requirements of the ac­
tivities she chose for her study. In her
February 1985 Letter to the Editor,
Mary Kasch pointed out that jumping
rope—the purposeful activity in
Kircher’s study—involved factors of
pacing and isometric exercise of the
upper extremities that would be diffi­
cult to duplicate in an activity design­
ned as nonpurposeful. Kircher, in
her response to that letter, conceded that "some of the subjects jumped
harder and higher without the rope"
(p. 115). Those observations and the
significant difference of 11.15 beats
per minute in ending heart rates be­
tween Kircher’s purposeful and
nonpurposeful activities are indica­tions that the physiologic factors were not
well controlled in her study, and it is
over this difference in methodology
that we are in apparent disagreement.

As Kircher points out, the com­
parison of purposeful and nonpur­
poseful hand activities in my study
did yield results similar to those re­
ported in her study. However, I con­
sidered these findings the result of a
failure on my part to adequately con­
trol the force exerted by the subjects
in the upper extremity activity. Al­
though the results ultimately support
the hypothesis that a purpose or goal
would have an effect on the number
of times an individual would repeat a
desired motion, that argument can be
made more convincingly when all
physiologic factors are equal—as they
were in the case of the lower extrem­
ity activities used in my study. Com­
parison showed a significantly greater
number of repetitions performed on
the purposeful lower extremity activ­
ity recorded at equal levels of exertion
than with the nonpurposeful
lower extremity activity. Kircher’s
conclusion that this difference in per­
formance was the result of a discrep­
ancy in work load can only be made
by ignoring the data. Recordings of
heart rate and electromyogram indi­
cated that subjects performed both ac­
tivities at equal levels of exertion.
Kircher erroneously assumes that
heart rate must increase with in­
creased repetitions of an activity. And
although it is difficult to follow her
argument, she also seems to be draw­
ing an improbable correlation be­
tween preference for an activity and
heart rate when she proposes that the
reason heart rates were not higher in
my purposeful lower extremity activ­
ity was because half my subjects had
not indicated a preference for that ac­
tivity. This, too, is contrary to the data
and once again ignores the effect of
the intrinsic motivation inherent in
the purposeful activity.

Kircher is echoing the senti­
ments of many in our profession
when she calls for more studies to ex­
amine the issue of purposeful activity
and motivation. Although her study
provided valuable support for the use
of purposeful activity, it is support
that is open to question because of a
failure to control for differences in
work load between her two activities,
and I would therefore hope that fu­
ture studies of that nature follow a
more controlled methodology as I be­
lieve was demonstrated in my study.

Tom Steinbeck, MOT, OTR/L
Gig Harbor, Washington

September 1988, Volume 42, Number 9