

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 conclusions were compromised by my methodology: Steinbeck wrote, "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 student in occupational therapy at Texas Women's University, was that the amount of time the subjects performed the purposeful activity was significantly longer \((p < .05)\) than the time they performed the nonpurposeful activity. I reported the time as "nonsignificant" in my paper because of a misreading of a statistics table. Nonetheless, I maintain that a physiologic 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 activities used in his study were recorded at equal levels of activity on the electromyogram and heart rate recordings, they were of comparable work load, even though the subjects performed a significantly greater number of repetitions in the purposeful activity. If one performed more repetitions of one activity, or performed that activity for a longer period of time than another activity, to reach the same point of perceived exertion 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 demanded 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 purposeful and nonpurposeful lower extremity 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 similarities or dissimilarities of work load, that most probably affected the outcome of the experiment.

The heart rate and electromyogram findings both were significantly higher for the purposeful hand activities in the Steinbeck study. No question of preference for the purposeful hand activity was reported in his paper. The hand activity results supported the results of my own experiment, making each one a stronger assertion. The results of his lower extremity activities as yet remain on their own. We need more studies to decide this issue.

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Author's Response

I welcome Marjorie Kircher's comments concerning my article "Purposeful 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 performance 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 purposefulness of one activity over the other. Kircher, on the other hand, did not carefully control for differences in the physiologic requirements of the activities 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 difficult to duplicate in an activity designated 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 between 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 comparison of purposeful and nonpurposeful hand activities in my study did yield results similar to those reported in her study. However, I considered these findings the result of a failure on my part to adequately control the force exerted by the subjects in the upper extremity activity. Although 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 extremity activities used in my study. Comparison showed a significantly greater number of repetitions performed on the purposeful lower extremity activity recorded at equal levels of exertion than with the nonpurposeful lower extremity activity. Kircher's conclusion that this difference in performance was the result of a discrepancy in work load can only be made by ignoring the data. Recordings of heart rate and electromyogram indicated that subjects performed both activities at equal levels of exertion. Kircher erroneously assumes that heart rate must increase with increased repetitions of an activity. And although it is difficult to follow her argument, she also seems to be drawing an improbable correlation between preference for an activity and heart rate when she proposes that the reason heart rates were not higher in my purposeful lower extremity activity was because half my subjects had not indicated a preference for that activity. 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 sentiments of many in our profession when she calls for more studies to examine 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 future studies of that nature follow a more controlled methodology as I believe was demonstrated in my study.

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