Intersubjective Comparisons Are Possible with an Accurate Use of the Borg CR Scales

To the Editor:

We recently read the interesting commentary of Lambert and Borresen, which discusses the advantages and disadvantages of the various measures of training load in sports. This commentary addressed an important issue in sport science. As a further contribution, we would like to clarify some arguments presented in the commentary, particularly on the determination of the training load using the RPE and the session-RPE method.

Lambert and Borresen reported in the abstract that because the session-RPE method depends on subjective assessment, the intersubject comparisons may be inaccurate. We believe it is important to clarify that this should not be the case, since by choosing Borg’s category ratio scales (CR10 and CR100) for subjective assessment such systematic variance is minimized in agreement with modern psychophysical theory. Indeed, these scales are level-anchored, semi-ratio scales that combine the advantages of a ratio scale with those of a labeled category scale according to the Borg’s range model. This model asserts that the subjective range of intensity (from minimal to maximal) is equal between individuals, and evidence accumulated during the last 40 years has provided support to this theory. This is why the Borg scales are valid for prescribing and evaluating exercise intensity in both the clinical and sport setting. Failure in intersubject comparisons usually arises from using of scales that (1) have not been developed following the appropriate psychophysical procedures (ie, not valid) and (2) have been modified so that the original properties of the scale are lost or changed (eg, self-translation, modification of the numbers and anchors, and similar), or by (3) correlating the perceived exertion with exercise intensity indicators under different contexts (eg, indicators not ranging from minimum to maximum, such as the percentage of anaerobic threshold). Another crucial point for intersubject comparisons is the use of the correct instructions and administration procedures, including a valid interindividual reference point (the so-called fixed star). Furthermore, the appropriate instructions are also important because the CR scales are general intensity scales and the instructions define the domains measured. This underlines the importance of using appropriately validated psychophysical instruments that comprise both the scale and the relative instructions. Accordingly, when using the session-RPE method, the original CR10 (or CR100) scale should be used.

Another point raised in the commentary requiring further discussion was the suggestion that the session-RPE method may be not suitable for quantifying training load in sports such as rugby union and rugby league. However, there is evidence of convergent validity between session-RPE and other measures of exercise intensity (eg, heart rate and blood lactate concentration) during both rugby union and rugby league, with results similar to those of other sports. Additionally, it has also been shown that the match session-RPE is moderately correlated ($r = 0.54$) to the number of tackles completed during a game in professional rugby league.
suggesting that the global perceived exertion is also affected by tackling. Although further studies are probably needed, on the basis of these studies we suggest that the session-RPE method is an acceptable indicator of training load and can be applied to the rugby codes in the same manner as to other sports.

Finally, we generally agree that gaining consensus and providing evidence-based practice guidelines can improve scientific practice, but we do not believe a consensus on training load measures is warranted at this early stage. Rather, more well-designed studies experimentally investigating the theoretical link between the training load and training outcomes are still necessary before such a consensus can be made.

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References

Response

*To the Editor:*

We appreciate the thoughtful comments about our article.\(^1\) We would like to respond to the comments and clarify some of the points we made in the paper.

We accept the theory behind the Borg category ratio scale and acknowledge that the subjective range of intensity is equal between individuals, as you have pointed out. However, in practice the relationship between internal workload and perception of effort is not perfect, and therefore there will always be some error around the measurement.\(^2\) This does not negate the practical value of using the 10-point scale in a measurement such as the session-RPE, as we have mentioned in this article,\(^1\) and others.\(^3,4\)

Our assertion that the session-RPE may not be suitable for assessing training load in collision sports such as rugby union and rugby league is based on the fact that there are further aspects to the game that are not captured simply by measuring session-RPE. The physical demands of the game vary considerably depending on the playing position. For example, the number of tackles a player makes during a game can vary threefold,\(^5\) and considering that the number of tackles is directly proportional to the plasma creatine kinase activity 24 h later,\(^6\) it can be inferred that muscle damage also varies considerably between players. We would argue that the relationship between session-RPE and tackles \(r = 0.54\) is moderate at best and only explains 29% of the variance. Clearly, there are aspects of the physiological stress that are not detected by session-RPE alone.

In order to facilitate the progression of research in this area, we suggest that the methods of quantifying internal workload need to be standardized, given that the training and match demands of different sports vary considerably. For example, aspects specific to the game of rugby (session-RPE, tackles, rucks, lineouts, etc) during practice and matches need to documented. The definition of each aspect of the game needs to be carefully standardized, just as the definitions for injuries have been standardized for cricket,\(^7\) rugby,\(^8\) and football.\(^9\) In our opinion, if training loads are expressed in a more standardized way across studies, the training outcomes will be interpreted with more vigor.

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Erroneous Readings from Ingestible Temperature Capsules Due to Ingestion of Crushed Ice

To the Editor:

Recent studies have consistently shown that the use of cold fluids/slurry in reducing body core temperature ($T_c$) prior to exercise can improve endurance performance and capacity (Lee et al, 2008; Siegel et al, 2010). The recent study by Ihsan et al (2010) supports these findings by feeding crushed ice to lower $T_c$, measured using ingestible temperature capsules, leading to an improvement in a 40-km cycling time trial.

A key consideration when using ingestible temperature capsules to monitor $T_c$ as an alternative to conventional modes (rectal, esophageal, etc), especially in studies involving ingestion of fluids/foods with temperature very different from normal $T_c$, is the uncertainty of its location during measurement due to a range of gastrointestinal transition times among individuals (Byrne and Lim, 2007). The ingested capsule can either be excreted on trial day or there can be insufficient time for it to travel to the distal intestines. The latter can confound the $T_c$ data.

In this study by Ihsan et al (2010), even when the capsules were ingested 8–10 h prior to trial, the $T_c$ data (Figure 1; Ihsan et al, 2010) were likely to be directly influenced by the ingested crushed ice. This assumption is based on a reduction of 1.1 ± 0.59°C in $T_c$ (implying that an individual[s] within the group had $T_c$ lower than 35°C) following completion of the cooling phase, which is physiologically impossible with the approximately 550 g of crushed ice consumed (Siegel et al, 2010; Stanley et al, 2010). If the readings were true, they would have caused a significant degree of thermal discomfort. The reported mean difference in perceived thermal sensation was only about 1.5 units with no difference in mean skin temperature following ingestion of crushed ice. It was previously shown that poor accuracy may
result from using ingestible temperature capsules (even when they were ingested 8 to 11 h prior) with drinks at temperatures lower than normal $T_c$ (Lee et al, 2010), although the degree is subjected to varying gastrointestinal transition time.

With no robust solution to the problem of insufficient transition time for the capsule to reach the distal end of intestines, we are left with three alternatives: (1) removal of data when suspected of transient influence from ingestion of fluids/foods; (2) extension of capsule ingestion time to greater than 12 h prior to experiment (may even be longer for some individuals but will require pilot trial[s] to ascertain), but bear the risk of excretion of capsule upon testing; (3) insertion of capsule as a suppository (Adam et al, 1975; Kenefick et al, 2009). The last option may not be welcomed by some volunteers.

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References


Response

To the Editor:
Thank you for the opportunity to respond to the letter written to you regarding JK Lee’s thoughts on the recently published paper “Beneficial effects of ice ingestion as a precooling strategy on 40-km cycling time-trial performance” (Ihsan et al, 2010).

The insights provided by this letter are acknowledged as potential confounding factors to the use of telemetric pills for the measurement of core temperature ($T_c$)
when consuming crushed ice as a precooling strategy. With this in mind, Ishan et al (2010) chose to use the term gastrointestinal temperature ($T_{gi}$) to represent the data obtained from the pill, as a result of its potential varied location along the gastrointestinal tract. Unfortunately, the investigation by Lee et al (2010) was not published at the time of data collection or paper submission. Therefore, the use of the telemetric pill by Ihsan et al (2010), consumed 8 to 10 h prior to exercise, was appropriate and in accordance with previously published literature (O’Brien et al, 1998).

When considering the comments in the letter that the $T_c$ of the subjects would have been lower than 35°C following the completion of the cooling, it must be highlighted that this assumption has been inferred from a figure, and is unfortunately incorrect. The starting temperature of the participants was 37.1°C, and the postcooling temperature was 36.1°C. Previous research investigating the use of an ice slushy or cold water ingestion as a precooling strategy have also shown $T_c$ to fall to around 36°C (36.4°C, Lee et al, 2008; 36.55°C, Siegel et al, 2010). Because there have been only three published papers regarding ice slushy ingestion on the $T_c$ response, it is likely that further research is required before we can confidently conclude that core temperatures in the low 36°C range (ie, 36.0–36.5°C) are not possible from crushed ice ingestion.

To this end, the authors acknowledge the comments of this Letter to the Editor as plausible confounding factors related to the temperature changes seen when using telemetric temperature pills ingested prior to precooling with crushed ice. However, it is possible that more research is required to confirm the magnitude of temperature change one can expect to see from such a precooling strategy. Additionally, the key findings that a performance improvement of 6.5% can be attained when rapidly cooling the body prior to exercise should not be overshadowed.

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References