# SUBJECTIVE PERCEIVED EXERTION IN THE JUDO ATHLETES SESSION: SEVEN WEIGHTS AND ONE MEASURE?



## LETTER TO THE EDITOR

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Firstly, we would like to congratulate the authors of the article "Training monitoring in judo: comparison between the load intensity planned by the coach and the intensity perceived by the athlete"[1] on their initiative to investigate a theme this relevant to the training load control. Additionally, we should state that the conclusion is at first surprising: "...although the training program has been designed by experienced coaches, difference between the external load intensity expected by the coach and the internal load intensity perceived by the athlete was detected."[1] (p. 268). However, reflexion on the methods applied may help to understand the results:

1) judo is practiced in seven categories concerning body mass in both genders, and its subgroups resulting from these divisions are quite distinct concerning physical fitness (v.g., Franchini *et al.*[2]). Although the group participated in the same training camp, it is not very probable that the athletes were in the same periodization phase, since fighters of different levels participate in the distinct competition phases of the world ranking system. These two factors affect the subjective perceived exertion (SPE) during the session[3];

2) the time of interval between the *randori* repetitions is not mentioned in the session description, which would be important since this variable remarkably interferes in the physiological response (oxygen consumption and heart rate, for instance) during the performance of this kind of activity[4];

3) it is not mentioned whether the subjects involved in the use of this scale are familiar with it, which would be important since acceptable results measured by the concordance limits seem to be found after three sessions when the participant is exposed to different exercise intensities[3]. Especially concerning the coaches, this information seems to be crucial for suitable classification of the session, mainly due to the fact that the scale has not been validated yet for the modality. Another question which may be raised is whether the coaches would not be planning the intensity of the session having the athletes with best physical fitness as basis;

4) the statistical procedures need to be revised, since it is not clear how the Student's *t* test assumptions were fulfilled for comparison of the SPE of the coaches and athletes, the reason why the lactate concentration data were reported as mean and standard deviation, if non-parametric statistics has been applied and also how the Wilcoxon test was sufficient to compare multiple moments[5];

5) there are studies which reported data contrary to what has been reported in the study for the SE and lactate correlation, e.g., higher values of lactate and low/moderate of SPE[6-9], which would occur due to the dissociation between SPE and intensity of the activities in the fights due to the strong attention focus on the opponent's actions[6]; and

6) the *randori* volume reported is extremely high and the intensities expected by the coaches were between 3 and 5, showing that the focus seemed to be more technical than of physical fitness development, a fact which is corroborated by the low lactate concentration values after training compared to what is commonly observed after fight simulation or competitions[10].

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# USE OF SESSION RATING OF PERCEIVED EXERTION IN JUDO

Letter the Editor

## REPLY TO THE LETTER TO THE EDITOR

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Initially, we would like to thank the authors of the Letter to the Editor for the interest and attention to our article[1]. The concern about the training load control is actually intrinsic both to the academic community and the professionals who participate in the Sports routine. Undoubtedly, the search for solutions for the reliable monitoring of the training load, in a more accessible and coherent with the demands of a practice based on evidence, is a crucial issue. Six points have been mentioned in the letter which will be treated accordingly, following the order they were presented. We also thank for the opportunity to clarify and explain some aspects which may have not been suitably presented/discussed in the original article.

Before we start the discussion on the six topics, it is first important to mention that we did not intend to question in any moment the quality of the prescribed training by the coaches who participated in our study[1]. The success of this modality in the international scenario (World Championships and Olympic Games) is unquestionable. Judo is responsible for considerable number of Olympic medals won by Brazil, and was behind only Sailing. Therefore, it is necessary to clarify that the results interpretation presented in our article[1]does not go through reflexion/judgment on the quality of the Judo training, not only for not being the aim in our study but also for its delimitations would not enable such inferences and generalizations.

In the Letter to the Editor it is suggested that "the conclusion is at first surprising"; however, we do not understand the reason for the "surprise". This excerpt of the Letter to the Editor seems to reflect much more a personal and specific opinion of its authors rather than effectively a hypothesis. The formulation of a reasonable hypothesis, based on previous investigation on the theme, would be existence of incompatibility in the perception concerning the training load intensity between coaches and athletes. Studies previously published in other sports modalities[2,3], support this hypothesis; thus, the result of our study[1] did not "surprise" us. Foster *et al.*[2], for instance, demonstrated that the perception of the training load intensity from the side of runners did not agree with the intensity planned by the coach. More recently, Wallace *et al.*[3] investigated the correspondence between the perception of training intensity between elite swimmers and their coaches. These authors demonstrated that the athletes tend to record higher intensity when compared with the intensity planned by the coaches during the sessions designed to be considered easy (score lower than 3)[3]. Contrary to this, when the coaches designed difficult sessions (score above 5), the athletes tended to report intensity lower than the expected by the coaches[3]. None difference between the coaches and athletes responses was found for the sessions considered moderate (scores between 3 and 5)[3].

The results of the investigation mentioned above[2,3], joined with the ones presented in our study[1], indicate that the session rating of perceived exertion (session RPE) may offer the coaches the possibility of using a monitoring method of the internal training load in an individual manner, providing the necessary adjustment in the external training load. Thus, it would be possible to identify, for instance, in a training camp situation, similar to the one investigated in our study[1], that some athletes are training at the intensity planned by the coach and, contrary to this, others are not being trained according to their prescription. Considering this information, the technical staff could manipulate the training load (increasing or decreasing the external load). This adjustment would maximize the chances for success in the sports training, since without the control of the "suitable dose", the responses induced by training could be compromised or even deleterious.

The first point of the Letter to the Editor approaches the supposition that the athletes could present different levels of physical fitness and could be at different moments of the periodization. Concerning this point, we agree that during the investigated training camp[1], the athletes could be at relatively different conditions, both of physical fitness level (despite the fact that the sample was composed of a select group of athletes – the Brazilian National Team) and the moment of periodization. These two factors, contrary to what has been mentioned in the Letter to the Editor, are not a threat to the study results[1], which had the aim to compare the perception of the training load intensity between coaches and athletes. In the real world, it would be impossible to equalize the level of physical fitness and the periodization phase with athletes of this level. However, these points have not any correlation with the hypothesis that the perception of

the training load could vary between coaches and athletes! In fact, the results of our study corroborate the importance of individually controlling the load imposed to the athletes exactly for the reasons raised by the authors (different levels of physical fitness and distinct periodization phases). We support the hypothesis that the session RPE could be extremely useful in this view, since its application has been widely accepted and incorporated in different conditions and samples, regardless of the discussion about the neurophysiological basis and mechanisms of RPE generation[4-7].

Still about the "periodization phase", although the evaluated group has been composed of athletes trained by different coaches and belonged to different institutions; it is worth mentioning that all the athletes evaluated in our study[1] were there due to their participation in an international event of the modality, in a week previous to the training camp. This information was not suitably presented in the original article and we thank for the opportunity to include it in this reply. This additional information makes it plausible to believe that all the athletes were in a similar period (post-international competition) at the moment of the training camp. The fact that the athletes were training together after an international competition at the same site and at the same environmental conditions, seems, contrary to what has been stated by the authors of the Letter to the Editor, a highly favorable condition for the study design. Undoubtedly, these factors (fitness level and periodization phase) could partly explain the high measurement dispersion (session RPE score); however, despite of that, the mean score of the athlete group was higher (first session: 40%; second session: 20%; third session: 100%; fourth session: 66%) than the score attributed by each coach[1].

Still concerning the "two factors" (level of physical fitness and periodization phase) which could affect the RPE response, it is important to highlight that this argument is a simple speculation from the authors of the Letter to the Editor. Moreover, such statement presents no scientific evidence or is relevant to the aims of the study under consideration. Contrary to the speculations from the authors of the Letter to the Editor, recently, Psycharakis[8] presented the consistency of the RPE response during six months of evaluation, in four data collection moments during this period. The possible alterations of content and the periodization phase did not influence on the reliability of the RPE response. Furthermore, the author has demonstrated that the heart rate percentage and the blood lactate concentration for a specific RPE score did not change during the season. In addition to that, it has been supported that the RPE could be used in the different periodization moments as an instrument to monitor the manipulation of training intensity during the different phases of the process[8]. These data show that the use of the RPE[8] or the session RPE[2,3] for evaluation of the training intensity has the potential to be independent from the exercise type and manner and can be used in different situations, both in the high-intensity exercises and in strength training sessions, or even in plyometric training sessions[2,9,10].

Surprisingly, the reference used by the authors of the Letter to the Editor[11] in any moment mentions the factors "physical fitness level" or "periodization phases", as possible modulators of the RPE alteration. Additionally, it refers to a chapter of *Guidelines* published in 2007[11]; hence, besides not being an original article, it does not consider the variables mentioned by the authors of the Letter to the Editor. Interestingly, in the mentioned chapter, its authors[11] stress that the psychological factors could contribute to the RPE score variability, and that other "modulators" of the session RPE could be mentioned, such as the type of exercise, "audiovisual" distraction, the psychosocial environment or even the competitive environment of the experimental situation. Thus, contrary to the understanding of the authors of the Letter to the Editor, the presented reference[11] does not support the hypothesis that the "two factors" influenced on the results of our study. Actually, the used reference[11] highlights the existence of many RPE modulators which usually are not possible to be considered in different investigations. Such difficulty in controlling these modulating factors goes from the sample used to the environmental conditions. For example, in another recent investigation from our group, it was observed that the internal training load measured by the session session RPE was higher in the official competition compared with a simulation. These data reinforce the hypothesis that the competition environment imposes higher level of stress, which, on its turn, boosts the session RPE response[12]. In the context of our study[1], the athletes were in a real training situation, in the same environment, being observed and analyzed by the coaches, their peers and at similar conditions of psychophysiological stress. Although it was not a real competition situation, the environment and conditions favor the study design, due to the scenario presented.

Regarding the session description (point 2), the authors of the Letter to the Editor state that the time of interval between the *randori* repetitions is not informed and that this information would be important, since this variable remarkably interferes in the physiological response (e.g. oxygen consumption and heart rate) during the performance of this type of activity. We would agree with their comment if the aim of the study was to compare the physiological or even psychological responses, between the different sessions with distinct outlining. In that situation, the inclusion of this variable could add important information; however, as highlighted in the article[1], the aim was to compare the session RPE of the coaches and athletes for the same training session. In our study, four sessions with different content were planned by the coaches themselves and not the researchers. In other words, in case a session was different from another (e.g. more or less time of interval between the *randori* session), both concerning the response in the lactate concentration and the session RPE score, this fact would not influence whatsoever on the results presented (comparison of the intensity between athletes and coaches for the same session), or could not even be considered as a limitation to the study.

Concerning the issue of the "familiarity with the used instrument" (point 3), we agree that this point is crucial to the reliability of any measure and could be better presented in the article under consideration. Nonetheless, when the authors refer to the recommendation presented in the reference of the chapter of *Guidelines*[11], which advocate the use of the RPE after three sessions of physical exercise[11], the authors of the Letter to the Editor do not consider that the reference used deals with populations of many different levels of physical fitness, including people with special needs. In this same chapter[11], the authors affirm that the elite athletes (endurance runners) perceive more accurately ex-

ertion than non-athletes. This more refined exertion perception may be related with the fact that, in a competition situation, these athletes have to deeply concentrate on their sensations to effectively regulate the running pace. Such fact demonstrates the importance of cognition, and especially, of previous experience with the type of exercise in order to boost the measure reliability. In a recent literature revision, Lambert and Borresen[13] state that the experience with the activity influences on the RPE response. Possibly, this association is related with the use of the so-called "anchors" for the RPE response guidance. The individuals should consider as maximum threshold (classification = 10) the highest exertion experienced for a training/competition situation. It is reasonable to admit that, in the case of the population investigated in our study; both athletes and coaches had vast previous experience to consider a training session associated with an intensity classification of "extremely easy" or "maximal", for example. Thus, this is definitely not a threat to the study under question[1]. Possibly, less experienced athletes, students, sedentary subjects, among others, would need many sessions with different intensities to understand what a "light", "moderate" or "strong" exercise means.The higher number of sessions would probably provide the understanding and recognition not only of the scale, but also the magnitude of the exertion in an exercise/competition situation.

The authors of the Letter to the Editor speculate that the coaches could have planned the session intensity based on the "athletes with better physical fitness". Despite being simply speculation, this possibility reinforces even more the relevance of adopting methods to monitor training. Supposing that the coaches have used this strategy, after the application of the session RPE method, they would be able to compare the RPE scores of the "athletes better physically fit" with the scores of the "less conditioned athletes". Moreover, the coaches could compare the mean score of the group (classified by weight category, gender or physical fitness level) with the individual score of each athlete, compare two athletes of the same category or even monitor the session RPE score of similar sessions in different periodization moments for the same athlete. All these possibilities make this methodology extremely interesting, especially for a sport "played in seven categories related with body mass in both genders, being the subgroups resulting from these divisions fairly distinct concerning physical fitness".

Concerning the statistical procedures (point 4), we do not agree with the consideration of the authors of the Letter to the Editor. The comparison between the session RPE score of the athletes and the score of the coach was performed from the one sample *t* test, assuming that the hypothetical value was set by the coach. In the analysis concerning the lactate concentration, there is no limitation for the use of mean and standard deviation in the descriptive analysis, as representations of measures of central tendency and dispersion, respectively, and, subsequently, on the application of the non-parametric test for the hypotheses test. Curiously, in one of the articles mentioned by the authors of the Letter to the Editor for discussion of the lactate response data, the RPE of the athletes was an issue for investigation, and the authors of the study compared the different groups through non-parametric tests[14]. However, in the results, the mean and standard deviation value is presented[14]; however, such approach was not criticized by the authors of the Letter to the Editor. Further studies with Judo to define the citations as well as reinforce our choice of descriptive statistics, also used non-parametric tests for analysis of the results; however, reporting the mean as measure of central tendency and the standard error as measure of variability in the descriptive analysis[15,16]. Thus, it does not seem that there is any contradiction or misunderstanding about this, only different positioning and points of view which should be continuously discussed by the community. Nevertheless, the adopted procedures are far to be considered inappropriate or incorrect.

Concerning the multiple comparisons, despite careful reading by the authors of the Letter to the Editor, for some reason, they did not suitably pay attention to the goal of the analysis and its object. According to table 2[1], the training sessions were separately analyzed, that is to say, from the pre-session to the post-session moment, for each of the sessions. The study did not have the aim to compare the responses between the analyzed sessions, but rather the particular response of each of them, and then think on the associations between the RPE of the coach and the athlete, and the alterations in lactate concentration for each session. Thus, the non-parametrict test used is strongly recommended.

Considering that other studies have reported data in the "opposite direction" of our study (point 5), concerning the correlation between the RPE and lactate (lower RPE scores with high lactate concentrations), a much less linear and simple approach than the one adopted in the Letter to the Editor is necessary. Firstly, the assumption that the dissociation between the RPE score and the lactate concentration "would occur due to the strong focus attention on the actions of the opponent" is highly questionable and purely speculation. However, it emerges as an interesting research problem which could be investigated by researchers interested in the theme. It is worth mentioning that, conversely and opposite to the positioning of the authors of the Letter to the Editor, our study corroborates not only the study mentioned by them[17], but also other results of the literature[8,18-20]. These studies, with different experimental design, sports modalities and populations, generally showed that the RPE response could not be explained only in relation to the alteration of the lactate concentration. Contrary to this, an approach as a whole could be very useful in order to monitor the acute and chronic responses during the training process. The issue of the "opposite meaning", highlighted by the authors of the Letter to the Editor does not apply. This apparent dissociation of responses between the RPE and the lactate concentration has been the issue of discussions in the literature[8]. Thus, Green *et al.*[18], for example, suggest that the non-correspondence between RPE and lactate could be attributed to dissociated temporal responses or even to the sensitivity of the different measures concerning the acute responses.The authors reinforce the consideration that the alteration in the lactate concentration may not be the most important mediator of the RPE response, but rather one of the mediators of this response[18].

Robertson and Noble[21] already pointed to this possibility, emphasizing that the RPE alterations could be attributed to many factors and not have a single dominant factor. This approach reinforces the importance and interesting perspective of using the RPE by the coaches on the training and competition routine. Marcora[7], when proposed an exertion perception model

which was independent from the afferent feedback, in contrast with the integration of afferent and efferent signals in the RPE generation[4], presents a set of studies, in which the RPE response and metabolic stress dissociated, an indication that there is not necessarily a causal correlation between metabolic stimulus and perceived exertion. Despite disagreeing about the mechanisms of RPE generation proposed by Marcora[6,7], Noakes and Tucker[4], supporting the integration of afferent and efferent signals in the exercise control and RPE regulation, they also present evidence that the RPE is determined not only by one factor, namely metabolic stress, but also other factors which comprise a complex afferent feedback system, mediating the perceived exertion response, as demonstrated by Hampson *et al.*[5]. Despite the acceptance of any of the proposed models, it is evident that the discussion about the RPE validity as exercise monitoring seems to have become outdated. Nevertheless, as other monitoring examples, it is not free from limitations.

The last point of the Letter to the Editor (point 6) refers to the "*randori* volume". In our opinion, this comment has no correlation with the hypothesis and the aim of the study. Regardless of the means, methods and contents of the training session, the aim was to compare the intensity perception between coaches and athletes and the fact that the perception was inconsistent for the same training session.

Finally, it is worth highlighting certain contradiction in the argumentation flow of thinking presented in the Letter to the Editor. Firstly, the authors demonstrate concern with the validation of the RPE method for Judo, but they use a study with Tae kwon do to discuss the called "opposite direction" of the correlation of session RPE and lactate concentration[17]. Using the thinking of the authors of the Letter to the editor, the results obtained in Tae kwon do should not serve as grounding for the discussion of the responses observed in judo. In a first moment, the colleagues ignore the consistent body of knowledge built from distinct sports modalities, athletes of different levels, conditions and many types of exercises, which has supported the use of the RPE in the monitoring of the training process, when they state that the specific validation for Judo is crucial. However, in a contradictory manner, they mention the studies which used the RPE in other combat sports modalities to reflect about the reported results (dissociation between the RPE score and lactate concentration) in our study. In another moment, the authors also mention a study about the RPE and lactate behavior in Judo during simulated fights[14], with populations of level lower than the one evaluated in our study, to support their considerations. Curiously, in that moment, the authors of the Letter to the Editor do not demonstrate concern with the possible difference in the "level of physical fitness" between the individuals who participated in the study used as reference[14] and the athletes of the Brazilian Judo National Team[1].

The argumentation presented in the Letter to the Editor also presents another point which deserves attention. The authors of the Letter do not present in any moment the differences between the session RPE, used according to the procedures proposed by Foster *et al.*[2,9] and the "punctual" RPE, responded in a specific moment of the activity. In spite of these methods having presented similar constructs, it would have been expected that the authors of the Letter to the Editor, concerned about the reliability of the training monitoring, observed these differences for their considerations and future discussion.

We would like once again to thank the colleagues for the opportunity to comment and clarify additional points which improved the contribution and relevance of our study[1]. Furthermore, we congratulate the *Revista Brasileira de Medicina do Esporte* on the democratic space for discussion.

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