Clarification on altitude training

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We congratulate our colleagues for their careful reading of our work in their Hot Topic Review 'Does 'altitude training' increase exercise performance in elite athletes?' (Lundby & Robach 2016) but strongly believe that they have a selective appreciation of the available literature that requires clarification.

Living High Training low (LHTL)

The authors report that many LHTL studies in normobaric hypoxia (NH) "*failed to show a positive outcome*", which in our view is only partially true. In a cross-over design, we (Saugy *et al.*, 2016) recently tested if LHTL in hypobaric hypoxia (HH) would lead to larger performance enhancement than in NH. Our hypothesis was that HH (i.e. natural altitude) would lead to larger enhancement than NH but the results were contrary to this hypothesis. So we cannot support the affirmation by Lundby & Robach that "*natural altitude remains the best approach*". We acknowledge that observed changes in hemoglobin mass (Hb_{mass}) on the same experiment might have been influenced by the training camp conditions. However, again, a fair review would cite our response (Wehrlin *et al.*, 2016) detailing the controlled training and environmental parameters along with the high level of reproducibility of the duplicate Hb_{mass} method used.

Repeated Sprint Training in Hypoxia (RSH)

The authors question how RSH might lead to "performance gains" as high as 55%. Precisely, improved repeated-sprint performance post-RSH training in the quoted study (Faiss et al. 2014) was highlighted by longer exercise duration (or a larger number of completed sprints) before reaching exhaustion (carefully defined as 70% of peak power). While such performance improvement remains very specific (test protocol, participants background, training content), the reader needs to be cognizant that even a small change in an individual's power produces a large change in time to exhaustion (Allen & Hopkins, 2015). Such specific performance enhancement is in fact discussed carefully in our articles as to avoid any over-interpretation of our data (Faiss *et al.*, 2015). Montero and Lundby (2015) criticize the "criteria for fatigue" we have used and based on fancied ex nihilo calculations, claim that the outcomes we have reported result from "differentiated criteria" between intervention groups. We contend that this has no foundation and have carefully replied to this criticism, explaining how inaccurate their reasoning is (Faiss *et al.*, 2015). Curiously, in their first RSH study (Montero & Lundby, 2016), the authors have used the same criteria. How coherent is this?

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A careful reading of the available literature would mention the *very likely* greater gains in a specific repeated agility test (cumulated times) after RSH compared to similar training near sea-level (Brocherie *et al.*, 2015a). Additionally, in (Brocherie *et al.*, 2015b), the LHTL+RSH group had a twice larger RSA enhancement than the LHTL + RSN group and maintained this positive outcome for three weeks.

We also disagree with Lundby and Robach's interpretation of the study by Gatterer et al. (2014) who actually reported a training × group interaction on the fatigue slope calculated during repeated sprint exercise. This in fact confirms a better "resistance to fatigue" and Gatterer et al. concluded "*this type of hypoxic training led to larger improvements in RSA when compared to normoxia training*". That is the opposite of Lundby and Robach's conclusions.

The authors then cite their recent study (Montero & Lundby, 2016) where 12 different tests in 3 days post-intervention were conducted. We (Girard *et al.*, 2016) pointed out many methodological flaws in this study, questioning how individuals were able to maximally perform so many exhaustive exercise tests in such a small time period without any confounding deleterious influence of residual fatigue, likely to induce negative pacing strategies e.g. no control of the training intensity or of the pacing during RSA test. We concluded that many methodological shortcomings may explain why no additional effect of RSH *versus* RSN was observed.

A rare point agreed with the authors is that the readers/practitioners should critically assess the strength and weaknesses of the different studies and of the different sides of this debate.

Finally, we predict that the authors may change their mind in the future regarding RSH as they did regarding the efficiency of LHTL. A few years ago with the method they use for measuring Hb_{mass}, a larger than 10% increase in Hb_{mass} was reported for 250 h of exposure at 2500-3000 m, corresponding to an increase > 4% per 100 h in elite endurance athletes (Brugniaux *et al.*, 2006). One of the authors then stated "*LHTL improves VO_2max and associated power output. This improvement represents a marked increase, especially for elite athletes*". Whereas recently (Robach *et al.*, 2012) they stated "*the positive effects of LHTL on oxygen transport appear to be negligible among elite cyclists who already possess very high aerobic capacities conferred by high Hb_{mass} and VO_2max*".

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