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REPLY

Response to Armstrong and Bergeron

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We are pleased that our recent publication (Hoffman et al. 2018b) has been of interest and that it will receive further attention through our response to the letter from Armstrong and Bergeron (2019). Their letter is largely concentrated on attempting to refute three aspects of our work, which will be the focus of our response.

Their first issue relates to their misrepresentation that we indicated consuming water ad libitum "provide[s] for adequate hydration during prolonged exercise". We carefully related our conclusions to running for 2 h and avoided referring to "prolonged exercise" because of the potential for misinterpretation. Additionally, we clearly pointed out that "the present data do not allow us to make conclusions about the adequacy of ad libitum drinking to support hydration during longer bouts of exercise where it is possible that greater water deficits could develop" (page 2694). Beyond the issue of clarifying the intent of "prolonged exercise", Armstrong and Bergeron also seem to disagree with our conclusion that our data support the adequacy of ad libitum drinking in maintaining hydration during 2 h of exercise, suggesting differing effects between ad libitum and programmed drinking on rectal temperatures and pre-exercise to post-exercise changes in plasma osmolality, plasma volume and total body water. With regards to rectal temperatures, the highest value recorded for any subject during the ad libitum conditions was 40.3 °C, which was identical to the highest value recorded during both programmed drinking conditions; and as reported within, there were no statistical differences between the maximum mean values for ad libitum (39.2 °C) and the two programmed drinking (38.9 °C and 39.0 °C) conditions. Moreover, their statement regarding heat illness appears outdated considering recent developments on the etiology and pathophysiology of heatstroke (Costa et al. 2017). As for differences in plasma volume and body water changes, these were clearly reported in the paper, and we indicated that these effects without evidence for physiological or psychophysiological impairment "could reflect the presence of internal cues that stimulate adequate drinking to limit the water deficit by the end of exercise to a magnitude that will not adversely affect exercise response" (page 2694). Their suggestion about a different pre-exercise to post-exercise plasma osmolality response is incorrect, as there were no significant changes in plasma osmolality, a key indicator of hydration status, for the ad libitum or programmed drinking conditions.

The second issue they raise relates to our indication that change in body mass does not equate to change in hydration status. To support their stance, they state that the "body mass change (kg) was nearly equivalent to the net change of body water (L), with the differences of only 0.1–0.4 L/2 h." The actual discrepancy between change in body mass and change in total body water ranged from 0.1 to 0.3 kg (equal to L, accepting assumptions of equivalence). We noted this to be a maximum discrepancy of 0.5% body mass, and we refer to this effect as being "relatively trivial during short periods of exercise" (page 2693). Nonetheless, there should be no argument about a discrepancy if the calculations we present in the paper are followed, and that while the discrepancy is trivial with short bouts of exercise, it becomes increasingly important during long bouts of exercise, as previously demonstrated (Hoffman et al. 2018a).

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With the third issue raised by Armstrong and Bergeron, we find it interesting that they have attributed to our paper a "pursuit to support" the perspective that "ad libitum drinking during exercise does not impair exercise performance." It is bothersome to see such a misrepresentation of our work, as we made no statements that the data from the work supported any conclusion related to exercise performance. In fact, in the paragraph on study limitations, we noted that the study design "did not include a period of maximum or near maximum exertion to adequately test for performance limitations from the water deficit of 0.7 L with the ad libitum drinking condition" (page 2695). Conversely, a recent meta analysis (Goulet and Hoffman 2019) demonstrates that ad libitum drinking compared with programmed drinking of greater volumes is actually associated with a statistically significant 1% performance advantage (though noted to be irrelevant in a practical sense) during 1–2 h of cycling or running at moderate to high intensities in temperate to warm ambient conditions.

Finally, Armstrong and Bergeron suggest that our request for a thirst rating every 10 min during exercise influenced ad libitum drinking behavior. We accept this potential concern, and raised it in the paper with an indication that we believed it "was unlikely to have had an important effect since a prior study of similar exercise duration that did not assess thirst rating (Lee et al. 2014) found even less decrease in body mass with self-selected drinking than the present work, and it also seems unlikely that the request of thirst ratings would have altered fluid intake for athletes who are already attentive to internal cues" (page 2695). We would now add that this potential confounder best be recognized in designing future studies.

Armstrong and Bergeron also question our thirst rating values, indicating that "relative to our past laboratory research experience, these mean values are low for a 2-h continuous exercise session". Having not provided a supporting reference, we explored their research to ascertain support for the assertion. We identified reports of two laboratory-based studies assessing thirst rating during 90 min of moderate intensity exercise in the heat (33 °C or 36 °C), while allowing ad libitum drinking. Thirst ratings, on a 1–9 scale, were less than 3 in one study (Maresh et al. 2004), and less than 4 in the other study (Ormerod et al. 2003). We are, therefore, confused by their concern about our mean values of 2–4 on a 0–10 scale (with same midpoint as the 1–9 scale) at the end of 2 h of exercise. Nonetheless, it is perhaps more unclear why they would expect that thirst ratings would be high when one is drinking as much or more than desired, and maintaining euhydration.

It is evident that two of the concerns raised by Armstrong and Bergeron come from misrepresentation of our work, and their stance on the other is clearly incorrect. Furthermore, we have already openly pointed out the limitations to our work in which they have focused. It is unfortunate that the topic of proper hydration during exercise has become exceptionally polarized as evidenced by their letter. We welcome legitimate debates on the topic, but such debates will be more fruitful if accurate representation of research findings and assertions are made.

Author contributions MDH drafted the response. All authors contributed to the revisions of the initial draft, and read and approved the final response.

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