Author's response to reviews

Title: Carbohydrate mouth rinse: does it improve endurance exercise performance?

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Author's response to reviews: see over
Dear Editor,

We gladly acknowledge the compliments and the insightful comments from the reviewers. Please see below our point-by-point answers and comments. As we considered all comments helpful, all of them were addressed in the manuscript. Below each comment, we inserted an answer to the reviewer and the modifications are also highlighted in the manuscript. As suggested by both reviewers, the manuscript was submitted to a deep English review. We believe the manuscript has substantially increased in quality and we hope to meet your acceptance.

Sincerely,

The authors.

Reviewer: Asker A Jeukendrup
Reviewer's report:
Page 1, line 1: Studies investigating the effects of carbohydrate... would be a better start
We agree with the reviewer and inserted the sentence at the beginning of the first paragraph.

Page 1: The study by Levine did not contribute something important. It was a poorly conducted study with an interesting observation but it may have been the first one. I would remove this section.
We agree with the reviewer and this section was removed.

Page 1 bergstrom and hultman did not measure performance they studied endurance capacity (time to exhaustion)
Since Bergstrom & Hultman measured time-to-exhaustion, we agree that the best expression here would be “endurance capacity” and not “exercise performance”.

Page 1: The second section is not logical. First you discuss long duration exercise, then high intensity exercise, then you discuss mechanisms only important during more prolonged exercise.
In this section, the reason why we discussed CHO supplementation during a long duration exercise and then a high-intensity exercise was to draw the attention to the fact that even in some types of exercise where theoretically there is no need for CHO supplementation - high-intensity exercise - benefits were already found within its use. However, after the discussion of the possible mechanisms of action of CHO, we have already stressed this point, quoting the reviewer in this section. We decided to remove it.

Page 2: it would be good to discuss the study by Carter that shows that infusion of carbohydrate has no effect on performance despite high muscle glucose uptake. We followed the reviewer’s suggestion, discussing a little bit more about Carter’s study significance.

Page 2: The group of Carter. Carter was a student of Jeukendrup so perhaps better to refer to this as the group of Jeukendrup. Or “at the University of Birmingham”... Chambers is in the same research group. The citation was corrected in order to better refer to Prof. Jeukendrup’s group.

Page 5, line 8-9. The wording of this sentence is awkward. Please rewrite this section. Reading again this section we realized it was not well written. We accepted the reviewer’s suggestion and rewrote this section.

Page 5, line 14: first 5 min of the test (add the) This correction has been arranged.

Page 5, sentence above INSERT TABLE 1. This sentence is poorly worded and could be improved. Also I am not sure why this is important. If ingesting has no benefit but might be negative why would you do it? The last section of the summary is similar. Why is the comparison between a drink ingested or rinsed so important? Explain? The reviewer perceptively questions an item which is not very clear in this first version of the manuscript. However, in the revised version of the manuscript we added a discussion of a paper recently published (February 2010) in Scandinavian Journal of
Medicine & Science in Sports. This study directly compares the effects of CHO ingestion and mouth rinse in high-intensity endurance exercise, and surprisingly the authors show that rinsing but not ingestion contributed to a reduction in time trial due to a higher average power output for the same levels of perceived exertion. Therefore, we didn’t want to mention that CHO ingestion is negative at all, however there is probably a physiological advantage of avoiding ingesting the drink, i.e. by reducing the required blood supply and energy cost by the gastrointestinal tract to digest and absorb the CHO (which are eventually unnecessary to sustain the exercise of relatively short duration). We discussed the paper by Pottier et al. in a revised version of the manuscript, and we included their data in table 1.

Page 5: there does not seem to be a reference to the table?
The reference to table 1 is on page 4, right before the topic “Carbohydrate mouth rinse and exercise performance”, when we say “Although few studies are available in the literature in the context of CHO mouth rinse and exercise performance, the data available so far point to an interesting dietary strategy to be used in certain sports (Table 1).”

Page 6: add a bit more discussion about why pre-exercise feeding may abolish the effect of the mouth rinse?
As requested by the reviewer, a bit more discussion was added at the end of the topic “Carbohydrate mouth rinse and exercise performance” about some hypotheses (mainly from studies using animal models) by which the pre-meal exercise may abolish the effect of mouth rinse.
Reviewer: Wim Derave
Reviewer's report:

The authors mistakingly mention in the abstract and conclusion that a major lack in the scientific knowledge on this topic is a direct comparison between carbohydrate mouth rinse and ingestion. This experiment has already been done and published (Pottier et al. Scand J Med Sci Sports 2010), but has been overlooked by the authors, possibly because the paper only appeared in print in Feb 2010 (although already online available since end 2008). These (new) data by Pottier et al. should be thoroughly discussed in a revised version of the manuscript, as well as be included in table 1.

We agree with the commentary. This paper went unnoticed by us because, as stated by the reviewer, it only appeared to be printed in February 2010. In order to repair this mistake, we discussed the paper by Pottier et al. in a revised version of the manuscript, and we included their data in table 1, as directed by the reviewer.

A possible explanation for the discrepant findings in the literature is proposed by the authors to be the timing of the pre-exercise meal, with the suggestion that mouth rinse is only effective in the post-absorptive and not in the post-prandial state. The authors propose that this is related to liver glycogen stores, but I tend to disagree with this opinion. Although depleted liver glycogen stores can be expected in the studies by Rollo et al. and Chambers et al., who tested overnight fasted subjects, this is certainly not the case in the studies by Carter et al. (4h fast) and Pottier et al. (3h fast), who also found a positive effect of mouth rinse. Could it be that the lack of effect of Beelen et al. relates to the fact that when the pre-exercise meal is only 2h prior to the test, the stimulation of the oral sensory fibers by the meal is still persistent when the exercise test starts?

This observation made by the reviewer is extremely important and, no doubt, the search for the answer to this question improved our brief report since possible hypotheses to answer it lies in studies using animal models, where the loose-patch technique for recording from taste buds in situ has provided important information about transduction mechanisms in mammals for sweeteners. As stated in recent studies using this technique, responses to sweeteners in any single taste bud are reliable and repeatable for
up to periods of 2 h (we made a brief comment about this topic in the revised version of the manuscript). Therefore, the answer to the second reviewer lies in his own question.

I would appreciate a more in-depth analysis of the type of sensory fibers in the mouth cavity that are stimulated by the different types of experimental drinks. Although the majority of the studies are done with non-sweet maltodextrine solutions, Rollo et al. and Pottier et al. used a sweet drink with simple sugars and sweetened placebo. Clearly the effect is therefore not mediated by the sweet taste receptors, but how can the mouth distinguish between a sweet non-caloric and a sweet caloric drink? Please discuss.

Indeed, this is the reviewer’s interesting observation. Probably the answer to this question can again be answered by studies that used animal models. The mammalian sweet taste receptor combines two G-protein-coupled receptors, T1R2 and T1R3, which respond to both natural sugars and artificial sweeteners. However, results from recent studies have shown that T1R3-knock-out (KO) mice show no behavioural attraction to artificial sweeteners. Yet there was only a modest reduction in preference to caloric sugars and T1R3-KO mice still had a detectable gustatory nerve response to natural sugar. Therefore, the mouth can distinguish between a sweet non-caloric and a sweet caloric drink because there are different receptors for sugars and artificial sweeteners in the mouth cavity, mostly found on the tongue’s surface (again, we briefly commented about this topic in the revised version of the manuscript).

The authors suggest that carbohydrate mouth rinse may have practical advantages over drinking, because it can avoid gastrointestinal discomfort (e.g. during running). However, there is probably also a physiological advantage of not having to ingest the drink, i.e. by reducing the required blood supply to and energy cost by the gastro-intestinal tract to digest and absorb the carbohydrates (which are eventually unnecessary to sustain the exercise of relatively short duration). The results in the paper by Pottier et al. indeed suggest that even in cycling the mouth rinse has a ergogenic advantage compared with drinking.

Once again, the reviewer made an excellent observation. Besides avoiding gastrointestinal discomfort, these physiological advantages of CHO mouth rinse on the intake suggested by the reviewer had not been observed by us, and certainly deserves to be included in our manuscript.