Reviewer's report

Title: Eating in the absence of hunger: interaction between stress, macronutrient intake and disinhibition

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Reviewer: Marian Tanofsky-Kraff

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This study reports the results from a randomized crossover 2 x 2 design in which the wanting/liking, eating in the absence of hunger, and mood of university students was measured in a stress (vs. rest) condition and after a protein (vs. carbohydrate) meal. The stated purpose of the study was to investigate whether macronutrients affect wanting/liking, eating in the absence of hunger, and mood in stressful or restful states. The design of the study is interesting and the objective methodology used for eating in the absence of hunger and wanting/liking is commendable. However, there are a few significant issues with the manuscript, which require attention.

Major:

My biggest concern is that the manuscript is poorly organized and difficult to follow in multiple places:
- The abstract and introduction is confusing and hard to follow and should be re-worked for greater clarity
- The method section requires some major re-organization. For example, the wanting/liking test session is mentioned early without any description, but then later on, it is in described in a bit too much detail. This makes it very hard to follow what has been done.
- In the method section, line 153, the test meal procedures are discussed, but there is no mention of a screening session referred to earlier in the participants section. Also, the placement/order of this section seems strange with some measures/procedures discussed in the participants section.
- The meal had to be consumed in entirety. It should be mentioned earlier on in the methods section how the researchers adjusted for different energy needs of men and women of different body sizes.
- Lines 289-300 should be specified in the statistical plan rather than the results section itself. A rationale for this hypothesis should be clear from the Introduction. Also, a “role for disinhibition” needs to be clarified – main effect? Interaction (if so, with what)?

A second major concern is the choice or lack of covariates. What covariates if any did the authors account for? In the results section, it is unclear what is meant by: “Since there were neither gender differences, nor differences according to
weight status concerning possible conditional effects of stress vs. rest and of high-protein vs. high-carbohydrate on data of the questionnaires (POMS, STAI, VAS), on data of the ‘liking’ and ‘wanting’ computer test, and on energy intake, data were analyzed for all participants together.” It seems very odd not to control for BMI, as we know BMI has been related to liking/wanting and strongly related energy intake.

Other:

The hypotheses stated on page 5 should be clarified. The authors hypothesize that a high-carbohydrate meal would increase subsequent eating in the absence of hunger during stress relative to what? Relative to protein under stress? Carbohydrate and protein under no stress? Ditto for protein.

What was the rationale of the BMI inclusion range? The criterion for “only visceral overweight participants” is completely unclear. How is “overweight” defined? Does this mean if someone had a BMI of 26 but was not viscerally fat by the authors’ definition, they would be excluded?

How was food intake measured?

Was statistical power calculated? How did the authors decide upon the number of participants included in the study?

There are a number of stylistic issues with the manuscript throughout, including the use of abbreviations that are unclear such as “En% P/C/F” or the use of “i.e.” not within parentheses in multiple places throughout the text. There are no indentations at the start of paragraphs. The manuscript would benefit from the review of a native English-speaker.

Minor:

Regarding the TFEQ page 6, line 145, “unrestraint” and “restraint” are awkward – perhaps “unrestrained” and “restrained”? The statistical plan requires a bit more detail. For instance, specify the levels within “time.”