Reviewer's report

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

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Reviewer: David Levitsky

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The manuscript by Lemmens et al. describes a well-designed, well-controlled, and well-executed study of the relationship between stress and food intake in humans. The literature on the effect of stress on human food intake, and especially as a cause of human obesity, is confusing at best due to the ambiguity of terms and non-standardized methodologies used.

The authors' examined the effect of inducing a stress (high or low stress) and the role of the protein/carbohydrate content (high protein vs. high carbohydrate) of a meal on mood, “liking” and “wanting” of food, and energy intake. Despite all the possible effects and interactions that could have resulted from observing these variables, only obvious effects (e.g., satiety increases following eating) and only two interactions were observed that directly related to the question posed by the investigators: an interaction between diet and stress and “wanting” and an interaction between diet and stress and energy consumed. However, neither effect was statistically demonstrated by an interaction.

One of the difficulties in appreciating these results lies in the description of the statistics used to analyze the results. The statistics were described as being a “Factorial ANOVA with repeated measures” and Student t tests. It is not clear whether all the factors were subjected to a single ANOVA or the analysis was broken up according to which dependent variable was measured. Moreover, it was not stated whether the t tests were corrected for repeated testing. No explanation was provided as to why standard post-hoc tests were not used?

It is interesting that two subject variables, that were not part of the hypothesis being tested, were reported as significant: dietary restraint and feelings of hunger. However, a question that was not asked was the correlation between these two variables? Does one still observe differences in the feeling of hunger when dietary restraint is controlled? The answer would be of interest to many involved in human research as well as clinicians working on human eating disorders. Unfortunately, no information concerning the correlation between these two factors was provided.

In line 293, it was stated, “Since there were neither gender differences, nor differences according to weight status concerning possible conditions 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.” On
what basis were no “differences” made. Were there no interactions between any
of the psychological variables and gender observed?

The description of the relationship between stress and disinhibition and eating
behavior described between lines 298 to 309 is quite confusing. The first
sentence (Butters et al. showed that acute psychological stress leads to eating in
the absence of hunger, especially in vulnerable individuals characterized by
disinhibited eating behavior.) should be in the discussion section, not in the
results. The rest of the paragraph discusses “wanting”, not food intake and the
relationship between disinhibition and hunger, two subject variables, neither of
which was related to the hypothesis discussed in the introduction. Moreover,
although it is stated that “An effect of disinhibited eating behavior was detected
for post-meal average food ‘wanting’ (‘wanting’ for food items from all the five
food categories taken together) and energy intake”. Unfortunately, no data are
presented, not allowing the reader to get a grasp of the magnitude of the effect.
The text goes on to say “Hence, data were analyzed firstly for all participants
together and secondly for individuals characterized by high vs. low disinhibited
eating behavior.” But the data presented in the text was the relationship between
disinhibition and feelings of hunger, not disinhibition and intake.

The data central to the hypothesis being tested, that composition of the meal
(high or low protein) affects in interaction between stress and intake, is shown in
figures 4 and 5. But the data are not complete. Figure 4 shows the overall effect
of meal composition and the administration of the stressor on meal consumption.
There was no overall effect of either stress or meal composition, a finding that
gets lost in the discussion. Figure 5 shows the data only for the subjects with
high disinhibition scores. The critical figure would be a comparison in intake of
the subjects with high disinhibition to those with low disinhibition as a function of
stress administration and an accompanying significant triple interaction between
disinhibition, stress, and dietary composition. The same criticism applies to the
figures 2 and 3, which argues for the effect of meal composition on “wanting”.
Indeed, it appears that the only difference that emerged from this work occurred
on the test day where the participants were tested under non-stressful conditions
following the ingestion of the high protein meal. Could something have happened
when that particular condition was being tested that was different from the other
three testing conditions?

Eating in the absence of hunger was used in the title and throughout the
manuscript, but there was no discussion of why this is an important variable.
Eating in the absence of hunger generally refers to eating of snacks, not of a
succeeding meal. More discussion of the definition of eating in the absence of
hunger should be devoted in the discussion section, since it nothing about its
significance is mentioned.

The study is important not for what it found, but what it didn’t find. The discussion
should be re-written to reflect this observation. The study basically did not find an
effect of overall stress on eating or wanting or needing, except under a very
specific condition: the non-stressful condition when the test meal was high in
protein. How does this finding relate to the subject of the introductory sentence – stress might be the cause of increasing obesity? The study did not find intake of a meal was related to gender or body weight. They did not find an overall effect of protein composition on food consumption or psychological indicators of stress. They did not find an overall effect of protein composition of the meal on measure hunger of satiety, an observation that appears to contradict other findings by this group. In fact, they observed a dissociation between measures of hunger and satiety and protein content of the test meal because of the decrease in intake observed in the high disinhibition participants, but this condition was not reflected in hunger of satiety measures.

Finally, I believe the title should be changed for two reasons. First, the study does not examine the issue of eating in the absence of hunger as an independent variable. Second, the lack of effects of stress should be emphasized in the title rather than the existence of effects.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

No.