Hi Uri,

Thanks for giving us the opportunity to comment on the blog post. We appreciate the work you guys did applying p-curve and small telescopes to provide an interpretation for the comprehensive results.

Below are a couple of points that may or may not be worth mentioning. Points 3 and 4 may be the more important ones for the blog post, since they address Table 2 in Carney, et al.'s, 2015 comment. However, we believe points 1 and 2 are also quite important since they address the extent to which there is evidence that power-posing affects hormonal levels.

Best regards,
Eva, Anna, Magnus, Susanne, Sunhae and Roberto

1) One of the most striking findings from the original study is the result that, "power posing . . . can affect testosterone and cortisol levels in the brain" [*quote taken from TED website:* https://www.ted.com/talks/amy_cuddy_your_body_language_shapes_who_you_are]. But, to our knowledge, none of the 33 published studies other than the original Carney, et al. 2010 (CCY), paper and our replication study the effect of high- vs. low-power poses on hormones. So, the results of these other studies are orthogonal to this important finding, and it seems to be just our paper and CCY that provide evidence on this.

2) Even within the CCY paper, the hormone result seems to be presented inconsistently.

For example, on page 1366: aggregate testosterone levels "were in the normal range at both Time 1 (M = 60.30 pg/ml, SD = 49.58) and Time 2 (M = 57.40 pg/ml, SD = 43.25)" (page 1366), implying an *average decrease of about 2.9 pg/ml* for the population. But, looking at Figure 4, we see that testosterone increases by about 8 pg/ml for the high-power group and decreases by about 5 pg/ml for the low-power group, implying a net average *increase* in testosterone for the combined sample (assuming roughly equal treatment samples, at some point we calculated that the samples must be 22 and 20).

Similarly, on page 1366: "Cortisol levels were in the normal range at both Time 1 (M = 0.16 μg/dl, SD = 0.19) and Time 2 (M = 0.12 μg/dl, SD = 0.08)." This implies an *average decrease of 0.04 μg/dl in the overall sample*. However, the cortisol changes reported in Figure 4 show that participants in the low-power condition experienced mean cortisol increases of about 0.025 μg/dl and participants in the high-power condition experienced mean cortisol decreases of about 0.03 μg/dl. If the samples are roughly equal this implies an aggregate *decrease of only about -0.005 μg/dl*.

We are not sure what to make of these inconsistencies, but they seem important since CCY is the only paper that shows any hormonal effects. We should note that the latter inconsistency is pointed out by Steven Stanton ("The Essential Implications of Gender in Human Behavioral Endocrinology Studies . Frontiers in Behavioral Neuroscience. 2011;5:9. doi:10.3389/fnbeh.2011.00009; footnote 2), who notes other issues with the interpretation of the hormonal results in CCY.

3) Regarding the differences in designs, it is certainly possible that moderators like those CCY list in Table 2 might affect the presence or absence of an effect. But, in many cases we think it unlikely, as the evidence does not support many of these specific examples as probable moderators in this instance.

For example,

- "*Gender ratio – Gender could be a moderator*": We analyzed the data separately by gender (see our Supplementary Material) and find no effect in either sample.

- "*Time in poses – Duration and comfort of poses are very likely to be moderators*": We analyzed the data separately for those subjects who reported the poses to be comfortable and this does not change our results (see Discussion section of our paper).

- "*Filler task during pose – The social nature of the task is a known moderator (Cesario & McDonald, 2013)*":

  In a social condition in which subjects looked at faces, Cesario and McDonald find no effect of high-power poses on risk-taking relative to a control; they find only a negative effect relative to a control of low-power poses (Experiment 1, Figure 2). So, CCY's claim in Table 1 of their comment that Cesario and McDonald (2013) show that "expansive posture → increased risk taking only when context was social," does not seem to be true relative to a no-posing control. In fact, in neither a social nor a non-social condition does a high-power pose increase risk taking relative to the control condition in which poses are not manipulated.

  Moreover, in Experiment 1 (page 267) both the statistical results supporting the moderating effect of social context (an "interaction between context and the dummy variable comparing the constrictive and expansive poses") and the main power-posing effect ("participants who viewed faces in the expansive position gambled more than those in the constrictive position") are sustained by identical test statistics of "$\chi^2 = 1.98, \ldots p = 0.048$" (degrees of freedom not specified). Unless we are missing something, the critical value for a chi-square test statistic with one degree of freedom to have $p < 0.05$ significance is roughly twice as high. So, in fact, neither result may be statistically significant at $p < 0.05$.

  In Experiment 2, Cesario and McDonald find no effect of high- vs. low-power poses (see Figure 3 in their paper) in two separate conditions.

  Taking all of this together, rather than providing compelling evidence that "the social nature of the task is a known moderator," Cesario and McDonald (2013) seem to provide further evidence, across four comparisons, that high-power poses have no positive effects on risk-taking. Given that this paper provides a sample of 383 subjects (a substantial portion of the 2,521 noted by CCY in their 2015 reply) in which there is little evidence that power-posing increases risk taking, this seems worth noting.

4) Finally, one big potential moderator that is not mentioned in CCY's Table 2 is that we employed an experimenter-blind design, in which the researcher conducting the experiment did not know whether the subject had been assigned to a high- or low-power pose at the time of conducting the behavioral (risk and competitiveness) tasks. This difference between our designs might be the factor that most plausibly serves as a moderator of the effect.