

TECHNICAL RESPONSE

ANIMAL COGNITION

Response to Comments on “Number-space mapping in the newborn chick resembles humans’ mental number line”

Rosa Rugani,^{1,2*} Giorgio Vallortigara,¹ Konstantinos Priftis,² Lucia Regolin²

Mangalam and Karve raise concerns on whether our results demonstrate a mental number line, suggesting auxiliary experiments. Further data analyses show that their methodological concerns are not founded. Harshaw suggests that a side bias could have affected our results. We show that this concern is also unfounded.

Mangalam and Karve claim that some confounds may violate the assumption of trials’ independency of the measures and criticize the use of the Mann-Whitney test instead of the binomial test (1). We have now analyzed chicks’ performance focusing on the first trial only, as this cannot be influenced by any previous trials. We computed the number of chicks that chose the left panel on the first trial in the small number test and the number of chicks that selected the right panel in the first trial of the large number test. We applied the binomial test. Chicks significantly circumnavigated the left panel in the small number test (experiment 1: 14/15, $P < 0.01$; experiment 2: 10/12, $P = 0.04$; experiment 3: 31/37, $P < 0.01$) and the right panel in the large number test (experiment 1: 12/15, $P = 0.04$; experiment 2: 12/12, $P < 0.01$; experiment 3: 32/37, $P < 0.01$).

When, in a further analysis, we selectively considered the first trial of the first test performed, chicks significantly chose the left panel in the small number test (experiment 1: 7/8; experiment 2: 5/6; experiment 3: 16/20; overall 28/34, $P < 0.01$) and the right panel in the large number test (experiment 1: 5/7; experiment 2: 6/6; experiment 3: 13/17; overall 24/30, $P < 0.01$).

¹Center for Mind/Brain Sciences, University of Trento, Rovereto (Trento), Italy. ²Department of General Psychology, University of Padova, Padova, Italy.

*Corresponding author. E-mail: rosa.rugani@unitn.it or rosa.rugani@unipd.it

Mangalam and Karve pointed out that the bias may be related to feeding, with a preferential activation of the right eye (left hemisphere) at the population level. It should be noted that in the paper they quoted (2), selective lateralization for food discrimination and antipredatory responses were simultaneously assessed. Note, also, that the opposite bias is reported in feeding tasks (3). Once a given number is associated with food, the reinforcement expectation may increase in front of larger numbers. Let us arbitrarily suppose the existence of a rightward bias because of use of the right eye in association with feeding during training. The right bias would be somewhat amplified in the large number test. When chicks are subsequently presented with the small number test, the bias would turn to a left bias for some reason, as Mangalam and Karve advocate (namely, a discrepancy in the association or a novelty factor). Whatever the reason for the change in the bias between the first and second tests, there would be reasons to expect that such a shift involved only one direction (plausibly the left one, because of right-hemisphere involvement in response to novelty (4)). Instead, the direction of the bias we found, already on the very first test, was opposite depending on initial training: Chicks trained on 5 associated 8 with the right; chicks trained on 2 associated 8 with the left.

With reference to the experiments with zero, there is a problem when using a blank panel, namely, the change in continuous physical varia-

bles, which would obviously be impossible to control.

In sum, the alternative hypothesis suggested by Mangalam and Karve would at most explain only the data of those subjects that first underwent the “large number test.” It would not explain the preferential choice of the left side for the “small number test,” in particular when this was the first test administered.

Harshaw considers crucial the role of side biases displayed by individual chicks (5). He computes an index of bias that could be negative (leftward bias), positive (rightward bias), or equal to 0 (absence of any bias). The overall bias in our sample (mean = 0.02; SE = 0.02; $t = 1.15$; $P = 0.26$) demonstrates Harshaw’s concerns to be unfounded. In his comment, Harshaw averaged the absolute values of left and right biases, resulting in an overall bias cumulating left and right tendencies as if all chicks had a rightward bias. Correlations reported in his figure 1 (5) are misleading, as absolute values are represented along the x axis.

Concerning Harshaw’s “stricter bias” (arbitrarily fixed at “ $\geq 70\%$ choices on a single side”), his claim that it “would not present a serious challenge to the conclusions of the study if such biases were symmetrical around the mean of 0.50” is rhetorical. Harshaw in fact fails to conclude that of the 22 chicks displaying the bias (34.40% of the 64 subjects), 14 subjects had a right “bias” and 8 subjects a left “bias” (binomial test 14 versus 8, $P = 0.29$). The difference is not significant, so this bias could not have influenced our results.

Overall alternative interpretations to the idea of a mental number line are of course yet possible. However, up to now, our number-space mapping hypothesis (6) is the only one that optimally fits our results.

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