Below we offer a few brief comments on the paper by Szufnarowska et al. (2014).

1. The effect sizes are reported as d values (which, we assume, indicate Cohen's d values). Cohen's d values for single-sample t-tests, like the ones reported in this paper, can be calculated from the t-values as d=t/sqrt(n), where n is the sample size (here, n=22). However, the reported d values do not match the ones calculated from this formula. Rather, they seem to have been calculated from a different formula, d=2*t/sqrt(df), where df is the degrees of freedom (here, df=21). This latter formula is valid only for two-sample t-tests, and using it for assessing the effect size for single-sample tests results in overestimating the effect size by more than a factor of 2. When we calculate the effect sizes from the appropriate formula, only one of those reported in the paper and its supplementary material was 'large' (duration measure for Nodding), and only one of them was 'medium' (frequency measure with Direct gaze) by conventional labelling.

2. The effect size calculations for the paired-sample t-tests seem to have employed the same formula. This is also inappropriate.

3. The error bars on Figure 3 do not represent the SE values, although this is asserted in the figure legend. For example, the error bar depicted on the second column suggests that the difference score (DS) for Direct gaze was about 0.235, and the corresponding SE value was about 0.052. As in this case t=DS/SE for the single-sample t-test comparing DS to zero, such values would result in t=4.519 rather than t=2.156, which was reported in the text. The other error bars are also smaller at least by a factor two than what would be expected by the reported t values.

4. The figure legend for Figure 3 states that "pluses indicate p-values = .05" while the text reports that for both columns, marked by pluses, $p > .05$.

5. Under what circumstances one can use one-tailed, instead of two-tailed, statistical tests is a matter of debate in psychological research. While we do not necessarily object to applying one-tailed tests in this study, it should be pointed out that this choice of statistics produced more relaxed criteria of acceptance of statistical significance than almost all studies with comparable methodology, which, as a rule, adopted more stringent two-tailed tests (see, for example, Gredebäck et al., 2008). For a comparison, using the statistical criteria applied by the original study that the present study intended to partly replicate (Senju & Csibra, 2008), only two of the results reported in the main part of the paper would be considered significant: the facilitation of gaze following by Direct Gaze compared to the baseline (i.e., zero) and to the No cue condition. Remarkably, this replicates Senju & Csibra (2008).

6. An important difference between the design of the present study and that of Senju and Csibra (2008) is that the latter applied a between-subject design. This design was intentionally chosen to avoid the possibility of carry-over effects.

7. Szufnarowska et al. note that the stimuli in the non-ostensive condition of Exp. 1 in Senju and Csibra (2008) could have drawn infants' attention to themselves, and thus away from the actor. If 'attention' here means overt visual attention, than this was not the case: the cartoon image was presented on the head to make sure that infants looked at the actor when she turned her head. Senju and Csibra (2008) measured and reported that infants looked at this stimulus as much as they looked at the face in the ostensive condition. In contrast, the design of the present study did not ensure that infants' overt attention would be on the head at the moment of the crucial cue: infants' looked at least 250 ms less to the face during the No cues condition than during any other condition, and hence were less likely to observe the directional cue of head turning. The authors did not report whether these differences was statistically reliable, but this seems very likely given the confidence interval values they included in the paper. Thus, the present results, unlike the ones reported by Senju and Csibra (2008), could indeed be explained by the difference in overt visual attention that infants paid to the absence of cues versus the various cues employed in the other conditions.

8. If the term ‘attention’ refers not to overt visual attention but to some other mediating mechanism, it would be necessary to supply a definition or a behavioural or physiological measure that would allow for the testing of the engagement of such a mechanism. Without a definition and/or an independent measurement of attention, it will be difficult to avoid the circularity of arguments represented by claims such as “young infants’ attention is high in response to attention-grabbing human actions” and that “Future work should continue to explore the features of cues that infants respond to in order to determine the nature of their attention.” These sentences suggest that ‘attention’ is used in the present paper as a descriptive term rather than as an explanatory mechanism.

9. It is also proposed that, in Senju & Csibra (2008), “infants could have been confused because of the sudden absence of the attractive stimulus” after the disappearance of the cue in the non-ostensive condition. Had they been really confused, it would be difficult to explain how could they follow the actor’s gaze after the very same visual stimulus in the infant-directed speech condition of Exp. 2.

10. We were surprised to see that the paper by Triesch et al. (2006) was cited as “research suggesting that attention-based mechanisms can account for infants’ acquisition of gaze following.” The computational model offered by Triesch et al. (2006) did not include any mechanism corresponding to ‘attention.’ The term ‘attention’ used in that paper (see also Deák et al., 2014) exclusively to refer to overt visual behaviour of infants and caregivers (and their equivalents in the computational model).

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