


Re-evaluating the neonatal imitation hypothesis

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We appreciate Meltzoff et al.'s (2017) detailed commentary on our study of neonatal imitation (Oostenbroek et al., 2016) and the opportunity to clarify critical aspects of the findings. First, however, we wish to highlight that our data were collected as part of a larger longitudinal investigation, in which we set out to examine potential relations between neonatal imitation and later emerging aspects of social cognition (Suddendorf, Oostenbroek, Nielsen, & Slaughter, 2013). Our aim was not to test the veracity of the phenomenon of neonatal imitation, but to assess whether early individual differences predict later imitative and other socio-cognitive capacities, which would support the view of neonatal imitation as the foundation of social cognition (Meltzoff, 2002). When coding was finalized, it became clear that, to our surprise, the data challenged the very existence of the phenomenon whose trajectory and consequences we sought to chart.

Meltzoff et al. (2017) claim that methodological problems biased our results towards null effects. We assert that there is no compelling reason to think that any of these critiques undermine our challenge to the field. For instance, they argue that too many stimuli were used in a within-subjects design, which may have led to neonatal fatigue and disengagement. If that were the case, however, then initial trials should have elicited imitation but not later ones. We found no such order effects in any of our analyses (see Supplementary Material in Oostenbroek et al., 2016). Meltzoff et al. rightly note that infants cannot be expected to imitate behaviors that they are incapable of producing. Our data show, however, that with the exception of the 'eee' vocalizations and sad faces, infants produced all target behaviors either occasionally or regularly. Furthermore, Meltzoff et al. state that our stimulus and response periods were too brief. As we

set out to test imitation of multiple gestures, we indeed sought to keep the presentation time relatively short. Note, however, that we built upon Meltzoff and Moore's (1977) original Study 1 procedure, where significant effects were reported for all four modelled gestures even though the minimum stimulus-presentation period was 15 seconds (their procedure allowed for a maximum of three stimulus presentations if infants did not watch the first demonstration). Our total modelling time for each gesture was typically 30 seconds, and supplementary coding (not reported in our original paper) revealed that 95.6% of our modelling-and-response trials lasted 60 seconds in total (i.e. 30 seconds of modelling and 30 seconds of response time).

The literature on neonatal imitation has a long history of post-hoc theorizing. In response to early failures to replicate the effect, for example, it was stipulated that neonates should be *unfamiliar* with the model's face to maintain interest during the experiment (Meltzoff & Moore, 1983a). Later, however, it was argued that infants imitate *familiar* faces to communicate recognition (Meltzoff, 2005; Meltzoff & Moore, 1994), and that 'imitation is not modified by which person serves as the model' (Meltzoff & Moore, 1992, p. 492). Now, Meltzoff et al. again argue that the model should be *unfamiliar*, even suggesting that this is the 'key' to eliciting neonatal imitation. Setting aside the obvious contradictions here, it seems untenable to claim that the 'engine and mechanism for the growth of social cognition' (Meltzoff, 2002, p. 7) would manifest itself only with unfamiliar people. Meltzoff et al.'s other methodological critiques also fail to harmonize with previous literature. For instance, they suggest that infants should be placed in a padded seat to provide adequate postural support, and yet studies using infant seats have produced both positive (e.g. Meltzoff & Moore, 1977, 1983b) and negative (e.g.



Hayes & Watson, 1981; Koepke, Hamm, Legerstee, & Russell, 1983) outcomes. In the broader literature, there have even been mutually exclusive rationalizations of null results, with some authors attributing them to small sample sizes (Meltzoff & Moore, 1983a) and others to large sample sizes (Vincini, Jhang, Buder, & Gallagher, 2017).

Despite the concerns regarding our methods, Meltzoff et al. (2017) nevertheless argue that our data do contain evidence for imitation of one gesture, tongue protrusion (TP), based on a series of post-hoc analyses comparing the frequency of TPs elicited by the TP model to the *average* frequency of TPs elicited by the other ten models. In our original analyses, by contrast, we conducted separate pairwise comparisons for each of the control models, revealing that infants failed to protrude their tongues significantly more often in response to the TP model than to the mouth opening, happy face, or sad face models. The logic for conducting pairwise comparisons is to guard against the possibility that a *general category* of stimuli (such as dynamic faces) is more likely to elicit a response than other types of stimuli, which of course would not be evidence of imitation (for similar arguments, see Meltzoff & Moore, 1977; Paukner, Pederson, & Simpson, 2017). The averaging approach, on the other hand, does not account for this possibility, as it collapses across models that may yield low TP responses (e.g. manual actions) and models that may yield high TP responses. To our knowledge, the averaging approach Meltzoff et al. propose here has been used in only two previous studies (Fontaine, 1984; McKenzie & Over, 1983), both of which reported null effects in neonates.

In sum, Meltzoff et al. (2017) draw two contradictory conclusions from their re-analysis of our data: (1) our study was methodologically flawed, which is why we were unable to detect general imitation effects, and (2) our data do in fact contain imitation effects, but only for tongue protrusion. We suggest that, not only are the authors attempting to 'have their cake and eat it too', but they are also neglecting that (2) is not evidence for flexible imitation (see Meltzoff, 1996). Rather, it is more consistent with non-imitative accounts of TP matching in the newborn period, such as the arousal (Jones, 1996, 2006), reflexive (Anisfeld, 1991; Anisfeld et al., 2001) or associative accounts (Heyes, 2016; Ray & Heyes, 2011).

Meltzoff et al. (2017) conclude by making five design recommendations for future research (including the 'unfamiliar experimenter' stipulation). We welcome proposals that may improve methodological rigor, but we also want to call attention to the observation of Zwaan, Etz, Lucas, and Donnellan (2017, p. 18) that: 'uncritical acceptance of *post hoc* context-based explanations of failed replications ignores the possibility that false positives ... ever exist and seems to irrationally privilege the chronological order of studies over the objective characteristics of those studies'. If Meltzoff et al. are correct in claiming that slight methodological variations can bias results towards null findings, then it should be possible to detect significant methodological moderators of the neonatal imitation effect in a systematic meta-analysis of the existing literature. The field would clearly benefit from such an analysis, as it would from large scale, pre-registered replications from independent lab groups.

Our results do not undermine the central importance of imitation to human sociality (Meltzoff, 2007) and cumulative culture (Legare & Nielsen, 2015). Until more compelling evidence emerges, however, a propensity to imitate from birth should no longer be considered an established phenomenon.

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