**Resumo do artigo de Oostenbroek *et al.* (2016)**

**“Comprehensive Longitudinal Study Challenges the Existence of Neonatal Imitation in Humans” com o re-exame de Meltzoff *et al.* (2017).**

Oostenbroek *et al.* argue that the scientific literature about imitation in infants remains controversial due to mixed findings resulting from methodological limits. Thus, the authors performed a longitudinal analysis in order to investigate neonatal imitation in humans. They presented a total of 106 enfants with nine social and two non-social models and scored their responses at 1, 3, 6, and 9 weeks of age. They interpreted their results as showing an absence of relation between infants behavior and models, undermining the assumption of neonatal imitation and of related theories based on this consideration. Yet, Meltzoff *et al.*, a group of experienced researchers on neonatal imitation, re-examined these unexpected results and found 11 flaws that would have biased Oostenbroek *et al.* results towards null effects. First, they consider that the presentation of 11 models to infants, lasting too long and switching too fast, are impertinent in relation to concentration and cognitive abilities. Second, some models are considered inadequate to test imitation as infants may be unable to perform them. Third, the response periods during which imitation was assessed was also considered too brief in relation to infant neuromotor processing. Fourth, the reviewing authors disagreed on four points regarding the response criteria used to assess imitation. Fifth, some models were judged to lack environmental consideration with mixed stimulation such as simultaneous hand and involuntary face stimulation. Sixth, the acceptance threshold for infants’ alertness was considered too low. Seventh, the study design introduced a bias by allowing uncontrolled exposures to experimental stimuli. Eighth, subject selection for the longitudinal sample induced partial and controversial use of data. Ninth, a protocol violation was found as some stimuli presentations have been shortened. Tenth, the experimental procedure lacked a differential order of models’ presentation, used for controlling protocol biases. And eleventh, the experimental design, eliciting a possible body instability of the infants, was considered a bias towards null effect as it has been shown to be disruptive for infants. Moreover, Meltzoff *et al.* re-analyzed the data from Oostenbroek *et al.*, comparing behaviors in imitation phases with mean control phases, rather than the original cumulative and statistically less pertinent procedure. Despite the study’s biases, they found significatively more tongue protruding after the corresponding model presentation than mean control phases, suggesting a particularly robust neonatal imitation for this behavior. Finally, Meltzoff *et al.* call for a more parsimonious and cumulative approach on investigating neonatal imitation.

**Questões sobre o artigo Jones, S. S. (2009)**

**“The development of imitation in infancy.”**

This text elicited two different kinds of questioning. The first one, more practical, is about the discussed concepts and results regarding imitation. In particular, the tongue protruding behavior. From the beginning of the review, during the part on infancy imitation, I wondered why it was tongue protruding that has been a recurrent behavior in infants.

I would assume that the labial sensitivity of infants may be particularly developed and thus a stimulation would be pertinent. This sensitivity would make sense in relation to the importance of sensitive information in food intake through nipple localisation and milk intake. Then, I wondered: Could it be a response to an increased arousal in an unrelated paradigm, as the author suggests? And even though tongue protruding could be related to increased arousal, could not it be as well an imitation during model presentation of tongue protruding? Can the study of frequencies or durations of tongue protruding, compared between unrelated and related presentations give us more insight? For instance, higher behavior matching in related model presentations than unrelated ones could be supportive of tongue protruding at lower level during arousal and higher level during imitation. Nevertheless, the idea of imitation for such particular behaviors raised subsequent questions. What could be the evolutive function of being able to imitate tongue protruding..?

Interestingly, later in the paper, the author addresses this question, stressing on the fact that it is too often disregarded.

Then, the author’s reports that tongue protruding, or other challenged infant’s imitative behaviors, seemed replaced by reaching movements along development. It thus occurred to me that tongue may be the only mobile and reaching organ that an infant can activate. From a developmental perspective, it seems that infants gain control over mouth parts first, and then head part, limb parts and body trunk… Thus, considering an aroused individual, longing for exploring an attractive stimulation while it is impossible to use limbs or head, the tongue protruding starts to be environmentally pertinent to me.

Throughout the reading of the review, I admit to having had a change of perspective. Due to the reading of the summary texts first, I was initially considering that infants could imitate behaviors, especially tongue protruding. However, I am now more in agreement with the author’s view that we do not appear to have enough empirical support to defend a theory in particular. It relates to my second kind of questioning. I was really delighted by the author’s point of view and development. She seemed to have made a fair account of the area’s knowledge, truly considering different results and their related suggestions rather than confronting results going with or against her opinion on the matter. To me, she has a real scientific stance. What comes out is more about considering that we don’t know yet rather than considering that anyone is wrong. To me, there is no personal conflict in this review, in other words, there is no “ego-fight”. And the review consequently seems much more efficient in eliciting reflection on the field and giving an overview. Even though, through the citations, we guess what view she can have on the debate, there is no discredit of the researchers defending different considerations. And after all, how could one say another is scientifically wrong? Why wouldn’t it be just a different result, that we do not understand yet?

It makes me think of the saying that “I only know that I know nothing”. And I think it is a wise statement to take into consideration in science.