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FAILING THE DUCK TEST: REPLY TO BARBARO ET AL. (2016)

**Failing the Duck Test:**

**Reply to Barbaro, Boutwell, Barnes, and Shackelford (2016)**

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**Abstract**

In this reply, we respond to the critique by Barbaro, Boutwell, Barnes, and Shackelford (2016) in regards to our recent meta-analysis of intergenerational transmission of attachment (Verhage et al., 2016). Barbaro et al. (2016) claim that the influence of shared environment on attachment decreases with age, whereas unique environmental and genetic influences increase, which they felt was disregarded in our meta-analysis. Their criticisms, we argue, are based on a misunderstanding of the core tenets of attachment theory. Barbaro et al. unify parent-offspring attachment, attachment representations, and romantic-pair attachment under the same conceptual and empirical umbrella, even though these constructs serve different behavioral systems. We show that excluding the incompatible twin data on pair bonding from their analysis undercuts their argument. Statements about the role of the shared environment in attachment beyond early childhood are highly uncertain at this point. Importantly, even if the role of the shared environment were to wane with age, its effects may still be causally important in later childhood or adult outcomes, as either an indirect factor or as a factor influencing earlier developmental outcomes.

Keywords: attachment, intergenerational transmission, meta-analysis, shared environment

The statement ‘If it looks like a duck, swims like a duck, and quacks like a duck, then it probably is a duck’ illustrates the use of abductive reasoning, instigated by philosopher C. S. Peirce. The qualifier ‘probably’ is crucial. Applied to attachment, sorting parent-offspring attachment and attachment representations into the same category as romantic-pair attachment seems plausible enough. From the perspective of attachment theory, however, offspring-parent attachment may *predict* pair bonding, but it is not the same thing; parent-offspring attachment belongs to a different ‘behavioral system’ than pair bonding, as Bowlby (1969/1982) argued extensively. Furthermore, meta-analytic data have documented the absence of meaningful associations between measures of attachment representations within the parent-offspring domain (such as the Adult Attachment Interview) and pair-bonding styles (Roisman et al., 2007).

Barbaro et al. (2016) combined incompatible twin data on attachment relationships, representations, and pair bonding, to support their hypothesis that with age, the influence of the shared environment wanes, and that of genetics and unique environment waxes. Unpacking their summary of twin studies in Table 1 shows that the suggested role of age is confounded with differences in the concepts being measured. [Specifically](#), the Donnellan, Burt, Levendosky, and Klump (2008), Crawford et al. (2007), and Brussoni, Jang, Livesley, and Macbeth (2000) studies are all studies of adult romantic-pair attachment, [which, as noted above, are qualitatively distinct from measures of parent-offspring attachment and attachment representations](#). Moreover, Barbaro et al. erroneously include the Fearon et al. (2006) study, which is a sub-sample of the Bokhorst et al. (2003) sample. Deleting these four studies leads to a shrinkage of the unique environment (which includes measurement error) from 58% to 47%, and an even more drastic reduction of the genetic component from 21% to 13%, whereas the shared environment component almost doubles, from 20% to 39% ( $k = 5$ ;  $N = 1,469$ ). Of course, just summing the ACE components across studies with different sample sizes is imprecise at best. Rather, the available data do not yet allow for any firm statement about increasing or decreasing A, C, or E influences on parent-offspring attachment across the life span, as such a judgment would be based on only one study in adolescence (Fearon,

Shmueli-Goetz, Viding, Fonagy, & Plomin, 2014). The possibility raised by Fearon et al. (2014) -that parent-offspring attachment becomes more heritable with age, and that shared environmental influences shrink—is intriguing and potentially important, but at this stage still highly hypothetical.

But what if, for argument's sake, the influence of the shared environment on attachment does not extend beyond childhood? Would this make the study of intergenerational transmission of attachment obsolete? Certainly not. First, even if it were the case that shared environmental influences were limited to early childhood, these effects are important in their own right. From a developmental perspective, it is absurd to argue that only effects that persist *unchanged* into adulthood are important. The results of our meta-analytic work show continuing evidence for the intergenerational transmission of attachment: our analyses demonstrated a moderate sized correspondence ( $k = 83, r = .31$  [95% CI .18, .37]) between parents' attachment representations and infant-parent attachment relationships. Although a declining effect size and publication bias were also indicated, the effects were even highly significant and not trivial in size among the unpublished studies ( $k = 43, r = .25$  [95% CI .18, .32]), as well as among those performed in the last decade ( $k = 44, r = .26$  [95% CI .20, .32]), and even in the unpublished studies in the last decade ( $k = 27, r = .21$  [95% CI .14, .29]). Thus, the evidence supports the view that intergenerational transmission is a potentially important developmental phenomenon in the infancy period that is worthy of further scientific study. Second, indirect, cascading, and interactive effects from early development to later outcomes can mean that early shared environment effects are no longer detectable in adolescence or adulthood, but *these may still be part of the causal chain*. For example, if, in families where there was a smoker, children's exposure to cigarette smoke was 50% determined by shared environment, and such passive smoking affected lung function later in life with an effect of  $r = 0.30$  (i.e. not a trivial effect), then the increase in the twin-twin correlation for lung function that would arise due to the original shared environment would be just  $r = .05$  for both MZ and DZ twins – virtually undetectable.<sup>1</sup> Although this hypothetical example ignores measurement error, passive gene-environment correlation and GxE, and the choice of effect size is arbitrary, it illustrates how the

downstream footprint of individual, time-limited, but moderately persistent effects of an early shared environmental exposure can be difficult to detect in a cross-sectional twin study. It is thus wrong, in general, to conclude from a modest role of shared environmental factors in later outcomes that shared influences were not part of the causal process implicated in those later outcomes. An age-related drop in shared environmental variance is in itself no reason to abandon the understanding of shared influences altogether. Moderator effects are a further case in point – if a substantially shared environmental variable, such as early attachment, has a stronger effect on outcomes for some children than others, as in case of differential susceptibility (Bakermans-Kranenburg & van IJzendoorn, 2015), or has a stronger effect in some circumstances than others, and if these moderating factors are at least partially non-shared, then shared components of attachment will be contributing directly to non-shared variance in outcomes. Third, Barbaro et al. (2016) imply that all early attachment effects must be mediated by later attachment; but even if attachment changes later on, early insecurity may have had an impact on developmental outcomes prior to that change, for example, in the domain of behavior problems, and those effects could persist. Taken together, we believe the claim that intergenerational transmission of attachment is no longer of interest is misguided.

Do parental attachment representations explain everything we want to know about attachment? Of course not, which leads us to our final point. Barbaro and colleagues claim that “Verhage et al. (2016) inaccurately conclude that transmission...is primarily responsible for the development of infant attachment, ... ignoring the significant role of the non-shared environment”. This was not our conclusion; indeed, our paper clearly shows that parental attachment representations leave the majority of the variance in parent-offspring attachment unexplained.<sup>2</sup> Our paper was an investigation of intergenerational transmission of attachment, not an investigation of all causes of individual differences in attachment. Finding intergenerational transmission of attachment, as we did, says nothing in particular about the significance of the non-shared environment, and in other publications we have written extensively about the importance of the

non-shared environment for attachment (see Bokhorst et al., 2003; Fearon, Groh, Bakermans-Kranenburg, van IJzendoorn, & Roisman, 2016; Fearon et al., 2014; Fearon et al., 2006; van IJzendoorn et al., 2000).

In sum, we conclude that the evidence supports the position that [intergenerational transmission and](#) shared environment [more generally are](#) important for understanding the development of attachment, as is the non-shared environment, and that it is crucial to keep making the clear conceptual distinctions between parent-offspring relationships and pair-bonding which have existed since the genesis of attachment theory.

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Notes.

<sup>1</sup> The power to detect such a shared effect is not constant, but varies as a function of the genetic parameter as well; however, large to very large samples would be required in most realistic scenarios.

<sup>2</sup> We were also puzzled when Barbaro et al. (2016) stated “...transmission and other parent-based variables known to be negligible in the development of attachment”. It is difficult to discern what evidence, if any, they refer to here. The convergent evidence regarding the role of parenting in the development of attachment is strong, and spans behavioral-genetic evidence, observational studies, and experimental trials (Bokhorst et al., 2003; Cicchetti, Rogosch, & Toth, 2006; Fearon et al., 2016; Roisman & Fraley, 2008). They may be referring to the now-discredited notion that absence of shared environment means that parenting is unimportant. Not only is this wrong in general, attachment research is a poor example to choose because there is in fact good evidence of shared environment in this case.