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Executive Accounts of Theory-of-Mind Development

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Two varieties of executive theories may be distinguished: emergence accounts and expression accounts. The meta-analytic findings are fully consistent with emergence accounts of theory of mind and do not entirely rule out expression accounts.

INTRODUCTION

Wellman, Cross, and Watson (2001) have done the field an extraordinary service by elegantly systematizing the now-voluminous false-belief literature. Their analyses identifying factors that do and do not affect false-belief performance bring order to an otherwise confusing, somewhat contradictory body of research. Moreover, their finding that, across studies, no manipulation moves the performance of children younger than age 3½ to above-chance levels is powerful testimony to just how deep-seated these children's false-belief difficulties really are. In this commentary, I consider the implications of the meta-analysis for executive accounts of theory-of-mind development, which have recently arisen as a serious competitor to the conceptual change account favored by the authors (see Carlson & Moses, 2001; Frye, Zelazo, & Palfai, 1995; Perner & Lang, 1999; Russell, 1996).

DISCUSSION

Executive theories come in two varieties. Expression accounts argue that young children already have a conception of belief but are unable to express it in standard tasks because they cannot inhibit their knowledge of the true state of affairs. In contrast, emergence accounts claim that a certain level of executive ability needs to be in place for the very construction of a belief concept. In this view, acquiring such a concept minimally requires some capacity to reflect on thought and action, some ability to distance oneself from the immediate situation, and some ability to inhibit salient but misleading knowledge.

The factors identified by Wellman et al. as enhancing false-belief performance are either clearly executive (e.g., salience of mental state, real presence of object) or are ones that might indirectly affect executive processes (e.g., introducing deceptive motivation and active participation could well increase the salience of a protagonist's mental states). The fact that these manipulations affect performance is, therefore, good news for executive theories. The problem is that they do not systematically elevate younger children's performance beyond chance. Where does that leave executive theories? Clearly, as Wellman et al. readily acknowledge, their findings are fully compatible with executive emergence accounts. False-belief performance at younger ages may well be relatively impervious to the manipulations in question because children lack the requisite concepts, but executive skills may nonetheless be implicated in the acquisition of the concepts. But what of expression accounts? Is not younger children's apparent resistance to executive manipulations fatal to such accounts? Certainly, resistance of this type constitutes a strong *prima facie* case against the expression hypothesis; in part, for this reason, we have argued elsewhere for an emergence hypothesis (Carlson & Moses, 2001; Moses & Carlson, 2000). Nevertheless, for several reasons, one should proceed cautiously before altogether rejecting the expression hypothesis.

First, certain types of studies were entirely excluded from the Wellman et al. meta-analysis for various reasons. These include studies of false-belief explanations (e.g., Wellman & Banerjee, 1991); deceptive behavior, as opposed to verbal comment on that behavior (e.g., Chandler, Fritz, & Hala, 1989); eye gaze toward the two candidate locations in the false-belief task (e.g., Clements & Perner, 1994); and studies in which the true state of affairs was unknown to children (e.g., Wellman & Bartsch, 1988). The danger here is that, despite the myriad variations among the included studies, inferences in the meta-analysis were drawn from variations on a rather limited methodological theme. For example, in most of the analyzed studies, children were asked to make explicit verbal predictions about an individual's beliefs or behaviors under conditions in which the latter were, or would soon be, at odds with a potentially interfering aspect

Commentary on Wellman, Cross, & Watson, "Meta-Analysis of Theory-of-Mind Development: The Truth about False Belief."

of reality. However, suppose young children possessed a belief concept, but were so executively disabled that they were entirely unable to disengage attention from reality. If so, they would be able to produce "on-line" reasoning about beliefs only when reality was unknown. Under such conditions, young 3-year-olds actually do perform quite well (e.g., Wellman & Bartsch, 1988). These data are not considered in the Wellman et al. meta-analysis because children can hardly be said to be ascribing false beliefs if reality is unknown. My point here is only that such data are at least consistent with the existence of a crippling executive deficit that prevents children from expressing their belief knowledge unless all executive obstacles are removed. Of course, in the real world, where such obstacles are virtually omnipresent, "competence" of this type would be of little practical use. Still, from a theoretical point of view, having a kernel of competence is quite different from having none at all.

Second, for all its quantitative strengths, a meta-analysis is, in some respects, a blunt instrument. The validity of a manipulation depends crucially on the details of its implementation, but meta-analytic procedures are blind to these details, lumping together good with bad and weak with strong instances of such manipulations. To their credit, Wellman et al. do implement a measure of quality control, excluding studies in which performance on controls was poor. Doing so, however, falls far short of guaranteeing the validity of the included studies (weak manipulations can surely coexist with perfect control performance), and so it remains possible that the true effects of certain manipulations have been underestimated. Moreover, the findings of some studies do not fit the general meta-analytic pattern. These findings could, of course, represent statistical aberrations, but the details of the relevant studies might also be instructive. Wellman et al. do examine some studies of this type, but then offer reductive interpretations. However, the fact that the findings in question can be interpreted frugally (and those discussed certainly can), does not rule out the possibility that they, and others like them, truly reflect early competence.

Third, the authors assume that when a manipulation moves performance from below chance to chance, the improvement is likely spurious—a by-product of confused or random responding. The alternative, of course, is that some children are genuinely helped and some are not. This issue is important because executive deficits may occur either with or without conceptual deficits. When conceptual deficits are present, removing executive demands should shift children from systematic to unsystematic errors (i.e., to random responding). In contrast, when conceptual competence is present, removing these demands should induce real improvement. In support of the random-response hypothesis, and hence the conceptual-deficit view, Wellman et al. find that as mean performance moves closer to 50% across studies, children become less consistent over trials. Nevertheless, the correlation between mean performance and consistency accounts for less than 12% of the variance, and so hardly rules out the possibility that "at-chance" performance is actually produced by distributions that are at least substantially bimodal. Clearly, a more fine-grained analysis of the relevant studies is required to sort out these matters.

Fourth, Wellman et al. (2001, p. 672) make much of the fact that even their "best-effects" model—intended to maximize the combined impact of all the enhancing factors—"failed to interactively change the shape of the basic developmental pattern. . . ." This failure, however, would appear to be a mathematical necessity rather than an empirical finding about preschool social cognition. The best-effects procedure, as I understand it, simply involves the addition of a constant (the sum of the enhancing values for each factor times their respective coefficients) to the regression equation at each and every age. Hence, in the absence of Age $\times$ Factor interaction terms, the shape of the function must be identical for best-, worst-, and no-effects models. Moreover, the multivariate model is not well placed to assess combinatorial effects, because some of the critical cells are empty or virtually empty (i.e., no studies manipulate all of the enhancing factors; only one manipulates more than two). Thus, estimates of such effects are extrapolations that go well beyond the data. As Wellman et al. recognize, if all these factors were simultaneously manipulated, they might well interact synergistically, possibly generating strong performance in young 3-year-olds. Surprisingly, after 20 years of false-belief research, some key studies have yet to be carried out.

Finally, executive impairments may occur at either the representational level (e.g., cognitive inhibition) or the behavioral level (e.g., response inhibition): Children may have false-belief difficulties because they cannot inhibit attending to reality, or because the way in which they must respond suffers from a potentially interfering response history (e.g., pointing accurately to where objects actually are). A strong test of the expression account would require reducing both types of executive demands. The relevant studies in the meta-analysis focus only on the representational level (i.e., they manipulate the attentional salience of either mental or objective states). Previous work, however, has found that manipulating response inhibition also enhances performance, at least in the case of deceptive

behavior (Carlson, Moses, & Hix, 1998). Hence, a "no-holds-barred" approach, attacking executive difficulties on both fronts, might well have potent effects.

CONCLUSION

My aim here has been a limited one. I have not presented positive evidence in favor of an executive expression account. Indeed, for young 3-year-olds, compelling evidence of that type is difficult to find. What I have tried to show, however, is that the expression hypothesis is not definitively ruled out by the meta-analytic findings. The conceptual change account represents the best working hypothesis for theory-of-mind development, and Wellman et al.'s meta-analysis surely adds powerful support to it. But whether "the truth" about false belief is at hand is perhaps not as clear.

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