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Discussion

The evidence rejects the expertise hypothesis: Reply to Gauthier & Bukach [☆]

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Abstract

In Robbins, R. & McKone, E. (2006). No face-like processing for object-of-expertise in three behavioural tasks. *Cognition* this issue, we showed face-like holistic/configural processing does not occur for objects-of-expertise on standard paradigms including inversion, part-whole, part-in-configurally-transformed-whole, and the standard composite task. In this reply to the discussion by Gauthier, I., & Bukach, C. (2006). Should we reject the expertise hypothesis? *Cognition*, this issue, we focus on several issues: the fact that they do not dispute our review of previous data; the strength of null effects obtained from multiple studies and paradigms; the evidence for domain-specificity of the neural substrate supporting face processing; the difference between the expertise hypothesis (as a theory about the origin of face processing) and studies of how experience affects object processing in general; and the problems with G&B's proposed alteration to the standard composite paradigm. We argue that overwhelming evidence suggests the expertise hypothesis should be put to rest so that researchers can focus on what the origin of "special" processing for faces actually is, and investigate the many interesting changes to object recognition that *do* occur with experience.

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In our article (Robbins & McKone, 2006, R&M) we argued, based on an extensive review and our own new results, that face-like holistic or configural processing does not occur for objects-of-expertise. The commentary by Gauthier and Bukach (2006, G&B) does not provide any critical new evidence that would change this conclusion.

1. No corrections to our review data

The ‘greeble’ studies of Gauthier and colleagues have regularly been cited as having found holistic processing for objects-of-expertise. We summarised the results of those studies, showing that holistic processing was not found. Gauthier has been offered the chance to comment on our representation of her data; crucially, no corrections have been indicated. It thus appears that Gauthier accepts that the greeble studies did *not* show holistic processing in the standard tasks.

Instead, G&B note that we “abandon a conclusion drawn by several peer-reviewed studies”. No references are given as to which studies they have in mind. As far as we are aware, we reviewed all original empirical studies on holistic processing for objects-of-expertise. It is true that many peer-reviewed articles have stated the *conclusion* that holistic processing occurs for objects-of-expertise. Unfortunately, those studies have left unstated, or misrepresented, the actual *data* on which this conclusion rests. In a recent example, the review article of Bukach, Gauthier, and Tarr (2006) states their previous studies “found that putatively face-specific cognitive... mechanisms are recruited for skilled individuation of non-face homogeneous categories” (p. 159). No specific results or paradigms are mentioned, but reference is given to Gauthier and Tarr (1997). Although the results of that paper taken alone were encouraging to the expertise hypothesis, R&M (Table 5 & Section 5.3) showed (a) the expertise effects on the paradigms tested (part-whole, and part-in-configurally-transformed-whole) were not statistically significant, and (b) subsequent studies of Gauthier and colleagues comprehensively disproved that these paradigms showed larger effects in experts than novices. Overall, the fact that claims were peer-reviewed is of no value when they are demonstrably false.

2. Claims based on null results

In emphasising that we reject the expertise hypothesis based on null results, G&B seemingly suggest that null results can never be of any theoretical value. We disagree. Although null results can of course be unreliable in any particular case, the strength of our review was in showing that the same lack of face-like processing in experts was obtained in study after study, with different object classes, types of experts, and methods of measuring holistic processing. With respect to our own experiments, we showed *significant* holistic effects for faces (as well as *significant* differences between faces and objects-of-expertise); it is therefore unlikely that lack of statistical power was a problem.

3. A special neural substrate for faces?

G&B suggest our lack of emphasis on neural substrates is a part of a “divide-and-conquer approach”. Our reasons for focussing on behavioural results were practical: our experiments were behavioural, and our article was already long. In fact, we do not see convincing evidence for the expertise hypothesis from neural substrate studies. Attentional effects in fMRI are not ruled out by a correlation between behavioural performance and FFA response (G&B, Table 2), even when this is obtained over a substantial delay (Gauthier, Curby, Skudlarski, & Epstein, 2005); the correlation could arise because more-expert experts have a greater interest in the object class than less-expert experts and this difference is stable over time. G&B question why an attention effect would be limited to the FFA, but expertise-affected voxels have been found to occur throughout the fusiform gyrus with relatively little overlap between expertise-sensitive voxels and the FFA (Rhodes, Byatt, Michie, & Puce, 2004).

Limited spatial resolution of fMRI can also produce a false correspondence of the neural substrate supporting faces and objects-of-expertise. In two new studies, high-resolution methods clearly dissociate face selective responses from responses to bodies, a stimulus class for which humans have substantial expertise. Schwarzlose, Baker, and Kanwisher (2005) found that, while faces and bodies appeared to activate the same voxels with standard-resolution fMRI, the use of higher-resolution equipment revealed separate adjacent areas. Tsao, Freiwald, Tootell, and Livingstone (2006) used fMRI to guide single unit recording in monkeys to target the functionally defined FFA, and found a striking 97% of neurons were selective for faces and did not respond to bodies (or hands, or manmade objects). For detailed critiques of earlier fMRI results claimed to support the expertise hypothesis, see McKone and Kanwisher (2005) and Kanwisher and Yovel (in press).

4. Expertise effects vs. ‘the expertise hypothesis’

G&B’s commentary confuses the issue of whether there is support for ‘the expertise hypothesis’ with whether there are general effects of expertise on object processing. The expertise hypothesis is not merely a statement that expertise changes the nature of object processing in some way. Instead, it is a specific theoretical claim about the origin of special processing for *faces* (extensive experience in within-class discrimination obtained at any stage of life),¹ which then makes the specific prediction that face-like holistic processing should be obtained for other objects-of-expertise. Rejecting the expertise hypothesis does not preclude study of the many interesting ways in which expertise affects object recognition (e.g., the downward shift in preferred categorisation level in object experts; Tanaka & Taylor, 1991).

¹ Thus, while rejecting the expertise hypothesis does not tell us what the origin of special face processing is, at least it tells us what it is *not*.

Indeed, our own study found dog experts transferred their expertise completely to unfamiliar formats of inverted and contrast reversed dogs; it would be of interest to know what supports this remarkable generalisation ability. Thus, we agree with G&B that expertise *is* interesting to study, but disagree with their presumption that doing so requires accepting the *expertise hypothesis*.

5. Measuring the composite effect

G&B include a lengthy section critiquing the standard composite effect, and proposing a different method. We see this section largely as an irrelevant distraction. No data on objects-of-expertise are presented. No explanation is given for why the standard technique produces a composite effect for faces but not objects-of-expertise (or, indeed, why similar results are obtained in inversion and part-whole paradigms). Further, the standard aligned–unaligned composite method is widely accepted in the literature, and has been used independently by many different labs (Carey & Diamond, 1994; Goffaux & Rossion, in press; Khurana, Carter, Watanabe, & Nijhawan, 2006; Le Grand, Mondloch, Maurer, & Brent, 2004; Robbins & McKone, 2003; Young, Hellawell, & Hay, 1987). In contrast, versions testing additional unnecessary conditions have been used only by Gauthier and colleagues (note that G&B's references to Farah, Wilson, Drain, & Tanaka, 1998 & Wenger & Ingvalson, 2002, are misleading – those studies used a complex variant on the part-whole task, in which subjects responded to two features at once, not the composite effect).

G&B focus on a supposed problem of 'response bias' in the standard analysis. They ignore the fact that some studies have used a *naming* task, where the issue of response bias does not arise; this includes Gauthier, Williams, Tarr, and Tanaka's (1998) finding of no composite effect in greeble experts. More generally, G&B's discussion of response bias confuses rather than enlightens. Bias is a decision-level effect in which, for example, subjects can differ in the criterion they set to respond (e.g., how much evidence they require before saying 'different') even given the same stimulus information driving perception. Calculations of d' and bias can be useful in settings where the decision criterion could vary, such as across age groups, or across stimulus conditions presented in different blocks. R&M, however, randomly intermixed aligned and unaligned trials. Why would response bias differ across mixed conditions? What psychological process could produce this (other than genuine perceptual integration in the aligned condition)? Why would the alignment effect on 'same' trials be present for upright faces, but not inverted faces, or objects? No answers to these questions are given.

We now reply to two of G&B's specific arguments. In their Table 1, they show it is possible for 'same' trials to be poorer for aligned incongruent than unaligned incongruent, while discriminability (d') between *same* and *different* is equal. We do not understand the point here. G&B accept our argument that, while there is a clear prediction that holistic processing will make aligned harder than unaligned for 'same' trials, the direction of any effect on 'different' trials will vary depending on the similarity of the nontarget halves. In contra-

diction, they later say that *discriminability* should be poorer for aligned. This is not true, because discriminability incorporates ‘different’ trials. Accepting the prediction for ‘different’ trials as ambiguous necessarily invalidates any calculation of d' .

G&B also suggest a situation could arise where the standard alignment effect was found while evidence from extra conditions in their design – a lack of congruency effect – clearly showed there was no holistic processing. This situation is illustrated in G&B Table 1. We agree a lack of a congruency effect would be a problem for us: holistic processing should make the identical faces A/B and A/B (congruent) appear more globally similar to each other than the nonidentical faces A/B and A/C (incongruent), making “same” scores higher for congruent than incongruent. However, G&B *present no evidence that a lack of congruency effect actually occurs*. The ‘subject’ in Table 1 is ‘simulated’. In the submitted manuscript cited (Gauthier et al., submitted for publication), real subjects were tested and a *strong congruency effect* was obtained for aligned faces.

6. Conclusion

G&B failed to address the key points of our original paper. No explanation is provided, or even attempted, for why it is that the standard paradigms widely accepted as measures of holistic processing – inversion, part-whole, part-in-configurally-transformed-whole, standard composite – consistently show holistic processing only for upright faces, and not for inverted faces, objects in novices, or objects-of-expertise. Given these findings, a continuing focus on the expertise hypothesis is neither warranted nor useful. It delays progress on the important issues of understanding the origins, exact nature, and neural coding of holistic processing for faces. It also provides a distraction in the area of understanding how object processing changes with experience and expertise. It is time we accept that the hypothesis, while a worthy one when first proposed, has been disproved, and move on.

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