

Discussion

Accounts for the N170 face-effect:  
a reply to Rossion, Curran, & Gauthier

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**Abstract**

In their commentary, Rossion, Curran, and Gauthier (Rossion, B., Curran, T., Gauthier, I. (2002). A defense of the subordinate-level expertise account for the N170 component. *Cognition*, 85, 197–202) (RC&G) raise a series of arguments against the domain-specificity account for the N170 face-effect (Carmel, D., & Bentin, S. (2002). Domain specificity versus expertise: factors influencing distinct processing of faces. *Cognition*, 83, 1–29). This effect consists of a large difference (always significant) observed in the amplitude of a negative component peaking at the lower posterior-temporal sites in response to human faces relative to many other stimulus categories. As an alternative to domain specificity, RC&G advocate a “subordinate-level expertise” account, by which the N170 effect can be obtained for any type of stimulus for the individual identification of which the perceiver is an expert. While considering some of their arguments well taken and interesting, we believe that, overall, RC&G’s interpretation of our current data (as well as some of theirs) and of our position ignores several important aspects and, therefore, their critique is not persuasive. © 2002 Elsevier Science B.V. All rights reserved.

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Rossion, Curran, and Gauthier (this issue) (RC&G) reject the N170 effect as evidence for domain specificity in face processing, by making the following arguments:

1. Negative potentials at similar latencies are elicited by non-face categories, and the amplitudes of these potentials vary considerably. According to RC&G, this pattern suggests that the N170 might relate to general categorization processes (e.g. Tanaka, 2001).
2. N170 increases in amplitude when the task requires categorization at the subordinate, compared with a basic level (Tanaka, Loo, Weisbord, & Keifer, 1999).

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3. Comparing the brain activity elicited by faces with that elicited by other objects is moot because faces are automatically categorized at the individual (within category) level whereas other objects are not.
4. Given the complexity and diversity of extrastriate brain regions, it is very likely that the N170 is modulated by a number of brain sources, each involved in a different type of perceptual process.

We agree completely with the last point (cf. Sagiv & Bentin, 2001). Indeed, far-field potentials, recorded at the scalp, are almost always modulated by more than one brain source. This is why the consistent N170 discrimination of visual stimuli carrying physiognomic information from any other perceptual category is so striking. It is the researcher's role to use experimental designs and post-hoc analytical methods in order to disentangle the influence of the different sources. This strategy contrasts the view that we should lump together all the effects that may modulate a particular event-related potential (ERP), a view advocated by RC&G. In the present case, the negative component showing the N170 effect, in addition to its association with face-selective visual mechanisms, is most likely modulated also by non-selective visual activity that is always reflected by N1. Consequently, it is not surprising to find that any visual stimulus elicits a negative component at about this latency. Furthermore, being modulated by physical characteristics of the stimulus, the N1 elicited by different stimulus categories may vary in amplitude considerably. Fortunately, however, as we demonstrated in our recent paper (Carmel & Bentin, 2002), the influence of these two sources can be easily disentangled. The N170 effect is biggest at the mastoids, and significantly reduced (sometimes even completely absent) at O1 and O2, sites at which the N1 is maximal. Indeed at the posterior lateral sites the difference between faces and any other stimulus categories (tested under similar experimental conditions) exceeds by far differences between other categories that might be sporadically observed. Failing to make this distinction led RC&G to make arguments 1 and 2. Support for our position can be easily found considering the data of Tanaka et al. (1999). As evident in all the figures and in the BESA analysis, the potential that has been modulated by the type of categorization was limited to the left hemisphere, peaking at posterior parietal sites, far from the sites at which the N170 effect is observed. Ignoring this topographical distinction, RC&G erroneously associated the interesting brain activity reported by Tanaka et al. (1999) with the N170.

Regarding the third argument above, we believe that RC&G ignored our position regarding the function of the mechanism to which the N170 is putatively associated. The present evidence strongly suggests that the N170 is sensitive to isolated face parts and it is not sensitive to face familiarity (e.g. Bentin & Deouell, 2000). Hence, we proposed that it is associated with a structural encoding mechanism, which probably *precedes* the within-category identification processes (Bentin, Allison, Puce, Perez, & McCarthy, 1996; see also Carmel & Bentin, 2002). Our current view is that this component is, in fact, associated with an extrastriate face-detection mechanism that triggers the structural encoding process in the fusiform gyrus (see Sagiv & Bentin, 2001). If our view is correct, the fact that the within-category identification process might be automatic for faces is irrelevant to our present discussion.

RC&G criticize our suggestion that domain specificity may account for the absence of

task effects on the N170 elicited by faces (while such effects were found on the ERPs elicited by cars at the same latency), by making the following arguments:

1. Faces attract attention automatically; therefore, finding little task-induced modulation of the N170 elicited by faces is not relevant to domain specificity. Attentional effects due to task instructions should be interpreted in the context of attentional differences due to the perceiver's acquired processing biases for a category.
2. In passive (oddball) tasks, faces and objects of expertise should automatically recruit additional processes as compared with categories for which the perceiver is not expert.

The possibility that faces attract attention automatically is intriguing, but so far the evidence is not unequivocal. In fact, the major evidence for this claim is the fact that unilateral extinction is reduced when faces are presented in the contralesional field of right-hemisphere damaged patients (Vuilleumier, 2000). In normal participants, however, selective attention did not influence face processing (Cauquil, Edmonds, & Taylor, 2000). Note that in the Eimer (2000a) study cited by RC&G as evidence for attention effects, the task required memorizing and retrieving attended items (detecting repetitions, or matching exemplars to memorized items). This confound could explain the differences found between conditions for faces, as well as the finding that this modulation was mostly evident in components later than the N170 (see Eimer, 2000a, Fig. 3); the N170 was almost identical for attended and unattended faces, with a difference of about 1  $\mu$ V between them. Moreover, Eimer (2000b) reports that when attention was directed *away* from faces, the N170 was not modulated by the task. Parenthetically, RC&G seem to ignore that in the study that they cite, as well as in several others, Eimer favors a domain-specificity interpretation of the N170 (Eimer, 2000a–d). Finally, we believe that the whole argument regarding attention stems from a misinterpretation of our position. We have never claimed that face processing is totally immune to attention manipulations, and did not use such an argument to discard the “expertise view”. We simply demonstrated a clear difference between the significant and explainable task effects on the processing of other items (cars) and the absence of such effects for faces, suggesting that this difference supports domain specificity in face processing. Regarding the possibility that faces would recruit different processes than items for which the perceiver is not expert, it seems to us that this is simply an enunciation of the expertise view which is the focus of the present argument and, as detailed below, still needs to be proved.

In Experiment 2 (Carmel & Bentin, 2002), we compared the N170 elicited by human faces (for which humans are experts at the within-category identification level) with that elicited by faces of apes (which share the general configuration of the human face but for which our subjects were not experts at the within-category identification level). RC&G criticize our exploration of the putative expertise effects raising the following arguments:

1. We did not actually manipulate expertise because the stimulus type effect was confounded with task relevance. In Experiment 2 the participants were instructed to press a button each time they saw a human face. Hence, despite appearing at equal relative frequencies, human and ape faces were not task-equivalent: furthermore we had no objective (behavioral) evidence for the ability of our participants to individually

- identify human and ape faces or to the extent at which these stimuli were processed in a holistic manner.
2. Behavioral experiments comparing human and monkey/ape faces do support the expertise hypothesis. For instance, the face inversion effect in animals and humans is found only for faces for which subjects have developed an expertise. Furthermore, in a recent study, face inversion effects on N170 were found for human but not for monkey faces (de Haan, Pascalis, & Johnson, 2002).
  3. Expertise modulated the N170 elicited by dogs and birds in a previous experiment (Tanaka & Curran, 2001). This evidence, in concert with fMRI (e.g. Gauthier, Skudlarski, Gore, & Anderson, 2000) and behavioral data that showed an effect of expertise (e.g. Gauthier & Tarr, 1997), outweighs our “single null result”.

First, we should specify that showing similarly conspicuous N170 effects (with an identical scalp distribution) for human and ape faces was *not* a null result. We did not fail to find a predicted difference. Rather, we ran a crucial experiment testing predictions based on two theoretical frameworks, domain specificity versus expertise, and the data supported the former. Further, we should note that the modulation of N170 by expertise to dogs and birds in the Tanaka and Curran (2001) study was considerably smaller (albeit significant) than the modulation usually found for faces. In addition, as far as we can tell from the figures, these small effects were also more dorso-medial than the standard N170 effect. Hence, while very interesting, at the very least these effects need to be replicated, and their relation to the face-N170 further elucidated, before becoming a base for strong theoretical conclusions. Regarding both the fMRI and the behavioral evidence, we suggest that these data tapped a different level of processing than that tapped by the N170 effect, a level at which, for example, holistic or component-based processing strategy is relevant. To this end, we should only add that, as the type of processing was not relevant to our study, this issue was completely ignored in our paper.<sup>1</sup>

Did we, indeed, manipulate expertise? We agree with RC&G that it would have been nice to have an independent measure of expertise for ape faces. Yet, as the participants in this study never worked in a monkey lab or had any unusual experience with apes, it stands to reason that they were not experts in individual recognition of ape faces (or, at the very least, they were significantly less expert than in identifying human faces). As for the fact that human faces were given a target status in this experiment, note that if this factor affected face processing, it should have augmented the N170 relative to that elicited by ape faces (or the non-target faces of Experiment 1), which is the opposite of what was found.

The face inversion effect found for human but not for monkey faces led de Haan et al. (2002) as well as RC&G to conclude that “experience drives the specialization of the N170” and that human expertise with human faces does not generalize to faces of apes. However, it is interesting to note that in a different study (Pascalis, Demont, de Haan, & Campbell, 2001)

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<sup>1</sup> It is interesting to note, however, that data concerning holistic processing for items of expertise are far from convincing (at least as far as disproportionate inversion effects or part-whole paradigms are evidence). For example, Gauthier et al. (2000) found such evidence for car experts but not for bird experts. By contrast, Tanaka and Gauthier (1997) found such evidence for dog experts but not for car experts. Note also that even thousands of trials were not sufficient to develop holistic processing for inverted faces (McKone, Martini, & Nakayama, 2001).

the same authors report that human infants at various ages showed an inversion effect for faces of both humans and apes, but not for sheep faces, and concluded that the inversion effect generalizes to primates. Of course, opinions may change in light of new data, but note that the former study is in agreement with a study in chimpanzees (Parr, Dove, & Hopkins, 1998). In that study (cited by RC&G as evidence supporting the expertise view) inversion effects in chimpanzees were found only for human and chimpanzee faces, but not for capucin monkey faces. Whereas the extension of chimpanzees' response specificity to human faces could have been, in principle, explained by their experience with human faces (being raised in captivity), for the very same reason their experience with the chimpanzee faces might be doubtful (we have no information about how these chimps were raised). At the very least, these data could be equally explained by both the domain specificity and the expertise views, hence providing no strong support to either. Furthermore, on the basis of this discussion RC&G seem to be in complete agreement with us that without special training humans are not particularly expert for individual identification of apes. If so, how does the expertise hypothesis account for the conspicuous N170 effect elicited by apes in comparison to faces of other animals (cf. Bentin et al., 1996)? Note that, in contrast, the absence of a face inversion effect on the N170 elicited by ape faces in the de Haan et al. (2002) study can be easily accounted for by our model of the N170 effect. Recall that face inversion affects face identification, impeding global processing. This manipulation does not, however, affect component-oriented detection of physiognomic features. Ape faces are similar to human faces in general configuration, but not at the level of individual features. If indeed the N170 effect is associated with the component-based detection of physiognomic information, impeding global face processing by inversion may enhance the amplitude of the N170 for human but not for ape faces. Note that a similar effect was found for schematic faces and accounted for in detail by Sagiv and Bentin (2001).

In conclusion, it seems to us that the present critique published by RC&G is not persuasive. Whereas we accept the possibility that extensive expertise with a particular set of stimuli might change the strategy of processing those stimuli, the present evidence does not imply that this change starts at the level of processing at which the N170 effect is modulated. Whether or not the special efficiency that normal people show for face processing is innate is not clear. While reviewing present evidence on this issue, Carmel and Bentin (2002) took no stand. It is clear, however, that, while faces are not more frequently seen than many other stimuli, they are the only visual category to develop an expertise naturally, without explicit training. To this end, we believe that their processing is domain-specific.

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