Handbook of Research on Face Processing
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FOUR WAYS FOR FACES TO BE 'SPECIAL'

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INTRODUCTION

The study of face recognition provides a meeting point of interest for scientists from a wide variety of backgrounds. Social, developmental and cognitive psychologists, neurologists, neurophysiologists and ethologists have all taken an interest in the subject at some time. Ellis and Young's chapter admirably conveys the plurality of approaches adopted. However, do these approaches seek the same type of answer to the question 'Are faces special?'

The notion of 'special' explored by Ellis and Young shifts as their chapter progresses. We see that the idea 'special' ability would have the following properties:

1. components would be present from birth - the INNATENESS criterion
2. the ability would be localised in a (relatively) circumscribed area of the brain - the LOCALISATION criterion
3. the responsible part of the cortex would not be involved in any other function - the MODULARITY criterion
4. the processing would differ in some respect from other (visual) processing - the UNIQUENESS criterion

In our view, such a set of requirements embodies a rather strict view of the interaction between the uniqueness of the system design and the way in which it might develop. Even with quite an extensive genetic specification of an ability it would be possible for that ability to manifest itself widely throughout the cortex. It is possible, that is, for faces to be 'special' without any of criteria 2-4 being satisfied.

THE INNATENESS CRITERION

Some prominence is given by Ellis and Young to the finding that new born infants respond more strongly to schematic faces than to scrambled faces. This result, originally reported by Goren, Sarty and Wu (1975) and replicated by Dziurawiec and Ellis (1987), is of double interest. Firstly, the precocity of this response requires that we assign chief responsibility to innate properties of the infant visual system. Even if it were argued that a mere half an hour interaction with the world was enough to allow the child to learn about faces, we would still need to postulate some innate factor in order to account for the specificity and rapidity of the learning. Exactly what it is in the human face that the infants are responding to remains to be determined; we need not necessarily assume that a prototypical human face is specified in the neonate brain. These findings with newborn infants gain extra interest, however, in the light of the failure of a number of investigators to find a preference for faces over non-faces (including scrambled faces) in infants younger than 2 months old (for review, see Maurer, 1985).

The neonate is born with a number of specific skills that relate to faces. The 1-month-old infant does not seem to prefer schematic faces to scrambled versions of the same stimuli. However, by 3 months, preference for faces has returned. What happens to this preference in the intervening time? We suggest that the problem lies in the kinds of choice test that have been used. In the experiments by Goren et al. and by Dziurawiec & Ellis, the neonates followed the stimulus with their head and eyes as it was moved round them. The experiments where 1-month-old infants failed to discriminate between faces and other stimuli either employed a standard preference procedure with two stimuli (Fantz, 1965) or an infant control procedure with a single stimulus (Maurer & Barrera, 1981). In both cases the stimuli were static. We believe that the difference in technique is crucial and reflects the operation of two separate mechanisms.

The anatomical location of the two mechanisms is not crucial for our proposals. However, we feel a certain interest in postulating that the preferential tracking abilities of the newborn are controlled by a primitive attentional mechanism. It appears likely to us that the input to this mechanism is via the retino-collicular visual pathway, since there is strong evidence that this pathway is developmentally in advance of the retino-cortical pathway, which may not be functional till about 2 months after birth (see, for example, Atkinson, 1984; Braddick, Wattam-Bell & Atkinson, 1986). Further, some sub-cortical structures such as the superior colliculus appear to be critically involved in attention and orienting towards stimuli, and also are capable of handling fairly complex visual and acoustic information (for review, see Stein & Gordon, 1981).

We postulate further that, following maturation of the cortical visual pathway at around 2 months old, the control of visual attention passes to mechanisms accessed via the retino-cortical pathway. These mechanisms would be responsible for the emergence of a preference for face-like configurations in standard infant preference tests found at 2 to 3 months (Maurer, 1985; Maurer & Barrera, 1981). Such tests, with their reliance on static nasal visual field presentations, would not generate a discriminatory response from the sub-cortical attentional mechanism.

What of the relationship between the two mechanisms we have discussed? It would be possible to think in terms of them being closely linked in some way. In this case, we would be claiming that both sub-cortical and cortical mechanisms were special in a strong sense. However, it would be sufficient for our purposes if the two mechanisms were unconnected, the first one having the function of focussing the infant's attention upon the human faces in their early environment. This would guarantee that even if faces are subsequently learned about by a general learning mechanism, they will be learned more thoroughly than anything else in the infant's world. Indeed, analogous mechanisms in other species have been shown to have separate neural substrates (Johnson & Horn, 1986, 1987).

Other results with very young infants discussed by Ellis and Young lead us to focus on different issues. Suppose we take the data of Field, Woodson, Greenberg and Cohen (1982). They found that infants at 36 hours would habituate to a face of fixed expression and would dishabituate when the expression was changed. To account for this result alone we do not have to postulate an innate 'expression recogniser' in the infant. There are adequate gross visual cues to differentiate canonical happy, sad and surprised faces. Adequate controls would include having the faces upsidedown, and the use of schematic stimuli. However, the report that the infants also tended to imitate the expressions they were watching is less easy to account for in non-special terms. The data of Meltzoff and Moore (1977, 1983) are even more demanding. In their discussion of these data, Ellis and Young follow the obvious course, and favour the idea that 'neonates have fairly sophisticated routines not only for perceiving faces but for matching the movements of a face before them by their own facial musculature'. Is there any alternative? Suppose we discovered that, in a situation like that used by Meltzoff and Moore, infants responded specifically to a large, dark O before them by opening their mouths, and responded specifically to a light coloured O by pursing their lips. Could we then claim that we had destroyed the idea of imitation? We feel that we could not, since the account which would suggest itself - of a couple of arbitrary responses to a couple of arbitrary stimuli - is less intelligible than the notion of imitation that it purports to supplant. Of course, facial imitation would then be the intention of the mechanism rather than its true description. If the reported facts hold up - and at the moment we can find no great merit in the challenges to the Meltzoff and Moore position (apart from the usual virtues of scepticism) - we feel there would be a cast iron case for specialness.

It seems to be a problem to Ellis and Young that finger movements are also imitated, as Meltzoff and Moore claim. Faces could be regarded as special only by virtue of being visible parts of the human form. The fact that, in addition, hands are special for the same reasons, (see also Leslie, 1984) does not detract from the special status of faces. Faces are still distinguished from clocks, for example. Clocks do not signal their intentions.

We are left with the question of the relation between the imitation data and the two mechanisms we have already proposed. We are forced to conclude that, at the moment, there is insufficient evidence to allow us a profitable speculation on the matter.

THE LOCALISATION CRITERION

Ellis and Young cite, with seeming approval, Teuber (1978) who asked:

> 'Are faces, with their relatively greater dependence on the right hemisphere, 'special' in a similar way to that in which speech, with its dependence (in most of us) on the left hemisphere is said to be 'special'?' (p.890)

Such a formulation not only reduces the question, but also restricts it. Indeed, it seems to us that one would have to have a very particular viewpoint to define 'special' in such a way. Of all the things that are special about speech, its dependence on the left (or any other single) hemisphere is, perhaps, the least interesting. Would Teuber have concluded that for those unfortunates who have a bilateral dependence for speech, the product is less special? Are the only interesting or special parts of speech those that are unilaterally dependent? Should we really conclude that the more localised a function is, the more special it is likely to be?

Let us view the same issue from the other end. Would it actually surprise anyone if a former world chess champion suffered a focal lesion that gravely affected his ability to play chess but had no other serious effects upon his behaviour? We suspect not. Suppose, further, that a number of world class chess players were studied in their dotage and it was found that those with lesions in the right frontal lobe had selective problems with chess, while those with lesions elsewhere had no chess specific problems. We feel that this would be regarded as a triumph for the reductionist view, but doubt whether anyone would draw the conclusion that chess was biologically 'special'. What seems to be the case for a hemisphere is, for us, the case for a lobe or a gyrus. More precise localisation would not change the issue.

An alternative line of thinking might be that localisation is a necessary but not sufficient condition for biological specialness. This forces us to consider the nature of localisation. In classical neuropsychology a one to one mapping between a localised structure and a particular psychological function was assumed. This approach has proved hazardous. To illustrate this point we note that Ellis & Young briefly mention the neurophysiological studies of Perrett and his collaborators on cells sensitive to faces in the monkey temporal cortex. Ellis and Young claim that these studies raise some problems for the classical neuropsychological approach to prosopagnosia. These problems turn out to relate to the localisation criteria. For example, Ellis and Young make the point that the regions of the brain pinpointed by Perrett and his collaborators do not correspond to the areas around the inferior longitudinal fasciculus, the region identified by classical neuropsychology. But, in fact, this discrepancy in itself need not matter. We do not even need to invoke possible differences between monkeys and humans in cortical structure. We could simply postulate that prosopagnosia results from damage to one of the major inputs or outputs to Perrett's STS. Indeed, since the inferior longitudinal fasciculus projects to the temporal lobe, this is a strong possibility. However, if one is going to talk about the cortical localisation of an ability it is first necessary to specify how much of the ability one requires to be localised for it to qualify as special under the rules of the game one is playing. Thus, one could simply say that the region of interest to Perrett was later than the *real* localised face processor. (If one really wants to play this game.) However, it should be noted that contemporary neuroanatomists think more in terms of circuits connecting often widely separated areas, rather than relating psychological functions to single, specific locales (Goldman-Rakic, 1984, in press; Mishkin & Appenzeller, 1987).

What light do these considerations throw on our earlier claim that the preferential responding to faces found in newborns is sub-cortical? This claim was made for two reasons Firstly, there is evidence that the cortical control over the visual behaviour of newborns is minimal. Secondly, evidence from a variety of sources (electrophysiology and lesion studies in animals, blindsight cases etc) suggest that structures in the subcortical visual pathway are involved in attention and orienting. When attempting to analyse the cognitive mechanisms underlying face recognition in early infancy, we considered it reasonable to postulate a cognitive unit whose characteristics matched those derived from neurophysiological evidence. Note however, that the cognitive unit we have postulated could, in theory, be implemented anywhere in the brain. Thus, the sense in which our postulated mechanism is 'special' is not because it may be exclusively located in sub-cortical regions, but because it has certain characteristics and limitations not shared by other cognitive systems.

THE MODULARITY CRITERION

One problem alluded to by Ellis and Young bears directly on the modularity criterion. This is Perrett's finding of cells with a variety of functions mixed in with the cells responsive to face identity. Among such cells are those responsive to the general characteristics of faces. In the same area are also found cells responsive to hands and certain types of head and body movement as well as direction of gaze (Perrett et al., 1985). There are also cells in the same region that are responsive to somatosensory and auditory information about unexpected stimuli (Chitty, Mistlin & Perrett, 1985). Further, recent evidence from Perrett's lab indicates that neurones responsive to faces are not found throughout the STS, but occur in small clumps (0.5 mm to 4 mm across) within particular strip-like sub-regions around the superior temporal sulcus. Consequently, barring a series of extraordinarily precise accidental lesions, damage in the human brain to any region analogous to the monkey STS should result in impairments in the perception of a variety of animate objects and movements, as well as both the identity and general attributes of a face (see Perrett et al., in press). Meanwhile, it is of course possible for us to designate that region of the cortex as dealing with 'biologically relevant objects'.

It should be clear that the neurophysiological findings shed an interesting light on the search for a 'pure' case of prosopagnosia. If prosopagnosia results from damage to brain regions analogous to STS in the monkey, then we would expect it to be accompanied by deficits related to the function of other cells in the region. There is only one study we know of in which any appropriate experiment has been done (Davidoff, Matthews & Newcombe, 1986). In this case, the prosopagnosic patient was unable to respond on the basis of the direction of a human gaze, but was able to tell the direction in which the gun of a model tank was pointing (Perrett et al., in press). This is exactly the kind of result one would expect from the Perrett data referred to above.

Even if we established a 'pure' case of prosopagnosia what would this tell us? One might suggest that since there is a region of the brain exclusively devoted to some stage of face processing, this stage of the processing is unique to faces. That is, one might want to conclude that there is a unique function performed on face-like inputs. However, due to the range of possibilities open to us concerning the nature of cortical development, we cannot conclude even this. For example, we could envisage an arrangement whereby the location in the cortex where a particular range of inputs are processed is a matter of developmental timing. With regard to face recognition the story would run something like this. An innate sub-cortical reflex keeps the infant looking at faces over the first few weeks of life. Around the end of the first month a particular cortical region either becomes structurally mature or gets 'activated' biochemically. The visual information entering the retino-cortical pathway most commonly at this time is faces. This class of sensory inputs then 'organise' that region of cortex in a similar fashion to the way in which other patterned input 'organises' other regions of cortex (Simons & Land, 1987).

A further argument is possible concerning the importance of finding a pure prosopagnosic. It appears to be the case that all prosopagnosics, with the possible exception of the one described by De Renzi (1986), have deficits of identification of other classes of item – buildings, cars, personal goods and so on – sharing a particular characteristic with faces. This characteristic is the identification of individual items within a category. Such data is consistent with the view that an important stage of the identification of all such items is carried out by one and the same system which may be localised in a particular region, R. Face processing, that is, would not be modular in the sense we have used the term. A lesion occurring in region R would then be expected to affect a subset of the classes of item processed by that region. However, it also appears to be the case that not all prosopagnosic patients display the same pattern of accompanying defects. One has, then, to conclude that there is some degree of separateness in the storage or the processing of buildings, cars, personal goods etc., at least in the individuals concerned. Would we want to say that such classes of items were 'special'? Further discussion of this issue is hampered by the fact that the evidence is largely anecdotal.

The only prosopagnosic for which there is good evidence of purity is patient 4 in De Renzi's (1986) paper. This patient was a 72-year old public notary who suffered a stroke. De Renzi reports that 'the identification of relatives and close friends constituted an insurmountable problem if he could not rely on their voices'. His visual skills were good unless the test involved memory for faces. De Renzi also explored this patient's capacity for identifying single familiar objects from a set of objects from the same class. 'He was requested to identify his own electric razor, wallet, glasses and neckties, when each of them was presented together with 6 to 10 objects of the same category, chosen to have a physical resemblance with the target', (De Renzi, 1986, p.249). In this and other tasks the patient 'performed unhesitatingly and correctly'. In addition, according to the patient's wife, he easily recognised his car in parking lots.

How are we to react to De Renzi's patient? It appears that he is a pure prosopagnosic. The loss of feeling of familiarity was apparently restricted to faces. We might note the faint possibility that the patient was using particular features of the objects rather than identifying the objects as wholes. In this suggestion we are proceeding by analogy with a prosopagnosic patient of A. Damasio's (personal communication) who unexpectedly proved able to recognise a photo of his daughter. It turned out that this was by virtue of a blackened front tooth and when photographs of other young women were suitably disfigured the patient made false identifications. Such findings are part of the clinical description of prosopagnosia. Bauer and Rubens (1985) observe: '(Prosopagnosic) patients learn to identify people by using extra facial cues, including clothing, gait, length of hair, height, or distinguishing birthmark', (Bauer & Rubens, 1985, p.207). It sounds unlikely from De Renzi's brief account that an equivalent strategem could be at work with his patient but the possibility of what we might call strategic identification should be borne in mind.

If one or two more patients are established as pure prosopagnosics then we will have some evidence, at least, of an organisation of faces in memory that is separate from all other familiar objects. We might also hope to find patients for whom there are problems of identifying buildings, cars or personal goods, without an accompanying prosopagnosia. It is not clear to us, however, how it would be possible to make a watertight case in favour of specificity of processing of faces on the basis of the existence of pure prosopagnosics or of any other particular patient. There will always be the option of the deficits being attributable to storage.

THE UNIQUENESS CRITERION

Having established that it is difficult to argue for 'special' processing on the basis of the localisation or modularity criteria, we can enquire what type of psychological evidence would allow us to draw such a conclusion. The main thrust has been to establish whether faces are categorised and processed in a similar way to other visual objects. As Ellis and Young conclude, there is as yet no convincing evidence that faces are processed any differently from any frequently encountered class of objects which we require to discriminate amongst.

However, the fact that every face has its own data base in a way that is not true for any other class of stimuli, further guarantees that the mechanisms that analyse and recognise faces would have unique properties, without it being necessary for such mechanisms to occupy a special, circumscribed part of the cortex nor to operate in a unique fashion.

Returning to the four criteria for specialness, we have agreed that only the first criterion, the innateness criterion, needs to be true for faces to be special. The other three criteria may follow as natural consequences of this, but this is not necessary. Indeed, in the case of faces, we conclude that none of the localisation, modularity or uniqueness criteria are special. Furthermore, our own view of the innate mechanism is that it only operates during the first few months of life, and only approximately specifies the face (and perhaps not uniquely the human face). In spite of all this, we agree that faces are the most special thing around.

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