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Distinguishing the cause and consequence of face inversion: The perceptual field hypothesis

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ABSTRACT

I published a critical review of the face inversion effect (Rossion, 2008) that triggered a few reactions and commentaries by colleagues in the field (Riesenhuber & Wolff, 2009; Yovel, in press). Here, I summarize my original paper and attempt to identify the source of both the agreements and disagreements with my colleagues, as well as other authors, regarding the nature of the face inversion effect. My view is that the major *cause* of the detrimental effect of inversion on an observer's performance at individual face recognition is the disruption of a perceptual *process*. This perceptual process is makes and observer see the multiple features of a whole individual upright face at once. It also makes the percept of a given facial feature highly dependent on the location and identity of the other features in the whole face. The perceptual process is holistic because it is driven by a holistic face *representation*, derived from visual experience. Hence, an inverted face cannot be perceived holistically: the *perceptual field* of the observer is constricted for inverted faces, each facial feature having to be processed sequentially, independently, i.e. over a smaller spatial window than the whole face. Consequently, it is particularly difficult to perceive diagnostic cues that involve several elements over a wide space on an inverted face, such as long-range relative distances between features (e.g., relative distance between eyes and mouth), or diagnostic cues that are located far away from usual gaze fixation (e.g., mouth–nose distance or mouth shape when fixating between the eyes). These difficulties are mere *consequences* of face inversion – the cause being a loss of holistic perception-, and it does not follow that relative distances between internal features are necessarily particularly important to recognize faces, that they should be labeled "configural", or should be given a specific status at the representational level. I argue that distinguishing the *cause* and *consequence*(*s*) of face inversion this way can provide a parsimonious and yet complete theoretical account of the face inversion effect.

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1. Introduction

My original paper published in this journal, and which is the subject of two commentaries, was on face inversion (Rossion, 2008). An upright and an inverted face are strictly identical except for the orientation (i.e., phase) of the visual stimulus. However, we are quite good at recognizing upright faces, but terrible when the same faces are presented upside-down (Fig. 1A). This decrease of performance is known for a long time (e.g., Goldstein, 1965; Hochberg & Galper, 1967) and concerns the recognition of famous, personally familiar, or previously seen (old/new discrimination) faces. We are also significantly impaired and slowed down at matching/discriminating inverted as compared to upright unfamiliar faces (Fig. 1B), and most studies of this phenomenon have used such tasks with unfamiliar faces.

Like many other authors, I believe that a full understanding of this phenomenon will help us greatly in clarifying how the human brain perceives and recognizes faces. However, exactly 40 years after Yin's (1969) seminal study showing a much larger effect of inversion for faces than objects, the reason why face recognition is affected so much by inversion remains unclear.

I wrote this critical review of the face inversion effect mainly because I had the feeling that a series of papers published over the last few years by some of my colleagues (Riesenhuber, Jarudi, Gilad, & Sinha, 2004; Sekuler, Gaspar, Gold, & Bennett, 2004; Yovel & Kanwisher, 2004) were even increasing the conceptual confusion that currently reigns in the literature about the nature of the face inversion effect. I have now read with interest the replies of my colleagues Yovel (in press), as well as Riesenhuber and Wolff (2009) to the critical points that I raised about their original studies (Rossion, 2008). I thank these authors for their replies. In light of the complementary information that they provide in these replies, I may have made a few references to methodological aspects or results of their previous work, or the work of other authors, that were not correct, and I apologize for these mistakes (see Appendix 1). However, these are minor and largely irrelevant points, and I stand completely by the major criticisms that I made of their

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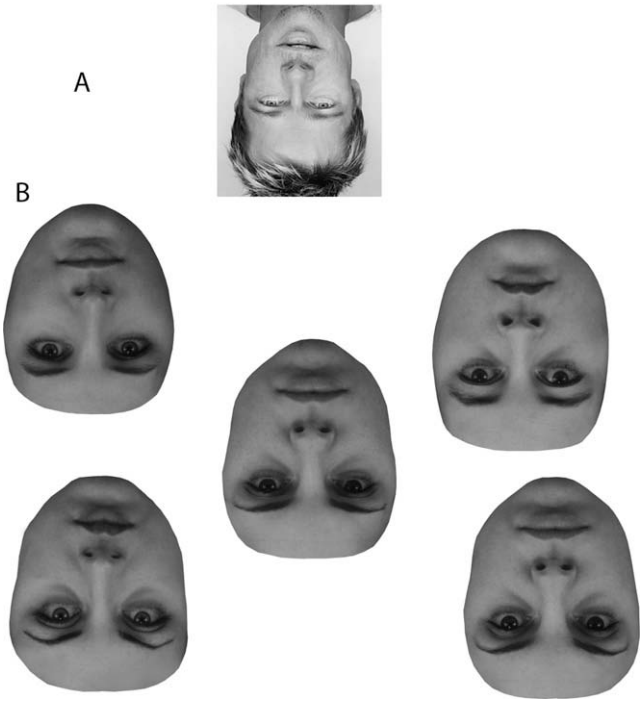

Fig. 1. (A) Inversion affects dramatically the recognition of famous faces, but also personally familiar or previously seen faces. (B) Matching or discriminating unfamiliar faces also suffers from inversion, people making more mistakes and being much slower with inverted as compared to upright faces.

original studies. Most importantly, these authors still cannot reconcile their data with the outcome of previous studies. I provide more information below, but also at the end of the present paper (Appendix 1) regarding these points, for readers interested in the precise scientific argumentation over methodological issues.

My intention for the present contribution is to be more constructive, and to aim at identifying the agreements, and most importantly the source of the remaining disagreements with these authors, in order to make progress regarding the theoretical account of the face inversion effect. I first briefly summarize what I wrote in my original paper (Rossion, 2008). I then clarify the theoretical position I took, which again, like Yovel's (in press) and Riesenhuber and Wolff's (2009) views, does not give a special status to certain diagnostic facial cues (i.e., relative distances between features) over others at the level of face representation. This is a theoretical position that is not novel at all, since it was already explicitly formulated by Tanaka and Farah (2003, pp. 62–64). However, I believe that these authors – including Tanaka and Farah – failed to consider the following: the encoding of relative distances between features could well be affected more than the encoding of local featural cues by inversion, precisely *because* integration of information over a larger spatial range is more critical to encode the former cues (relative distance) than the latter. This point, on which my colleagues and I seemingly disagree, is critical. I conclude the present paper by explaining how this view stands with respect to other theoretical positions in the field regarding the face inversion effect, and make a few suggestions for future research in this field.

2. A summary of my paper and theoretical position regarding the face inversion effect

I started my paper by claiming that, based on the literature, *what inversion does is to disrupt something called "holistic face processing*" (statement #1). This holistic processing disruption would be the major *cause* of the inversion effect (as outlined clearly previously by Tanaka, Farah and colleagues; see Farah, Drain, & Tanaka, 1995; Farah, Wilson, Drain, & Tanaka, 1998; Tanaka & Farah, 1993, 2003).

I then considered another kind of literature supporting the idea that when a face is inverted, the perception of certain cues that are diagnostic to individualize a face is more affected than the perception of other cues (statement #2). This view is what I call a qualitative view of face inversion.

In contrast, according to a *quantitative* view, all diagnostic facial cues are affected the same way by inversion. In other words, upright and inverted faces are processed just the same way, but less efficiently for inverted faces. Valentine (1988), and more recently Sekuler et al. (2004), argued in favor of this quantitative view. However, this view, other than stating that we are less good with inverted faces because we do not see faces in this orientation very often, does not offer any theoretical account of the inversion effect. I also argued in my review that showing that a local area of the face, the eyes in particular, can be the most diagnostic for processing individual faces by means of a distributed aperture method and response classification (Gosselin & Schyns, 2001; Haig, 1985), was not an argument against the view that we process faces holistically. Thus, showing that, under certain circumstances, roughly the same area of the eyes is used (Sekuler et al., 2004) and fixated (Williams & Henderson, 2007) when processing upright vs. inverted faces is an interesting observation, but it does not dismiss the qualitative view of face inversion at all.

I claimed that (#1) is the *cause* of the face inversion effect, while (#2) is merely a *consequence* of it. This cause vs. consequence relationship is a third statement made in my previous paper (#3). I consider this point as particularly important, and perhaps the only original point I made in that paper (Rossion, 2008), and which is emphasized here.

Partly because of the observation (#2), some authors have given a special status to certain face cues, at the representational level, i.e. those that are generally affected the most by inversion (e.g., Carey, 1992; Diamond & Carey, 1986; Maurer, Le Grand, & Mondloch, 2002). These authors have also *argued* that the face inversion effect was largely due to the loss of the perception of these cues, which they believed to be highly diagnostic for face recognition. In doing so, it seems that these authors confused the *cause* and the *consequence*(*s*) of the face inversion effect.

Besides an argumentation based on logic, I provided evidence from three sources supporting (#3): data from faces rotated in the plane over multiple angles; manipulation of relative distances between features in the vertical vs. horizontal direction; and the nature of the face recognition impairment in acquired prosopagnosia. The reader interested in this argumentation should refer to my previous paper (Rossion, 2008; see also Busigny & Rossion, in press; Rossion & Boremanse, 2008).

3. The source(s) of the (dis)agreements

While both Yovel (in press) and Riesenhuber and Wolff (2009) agree with me on (#1), they do not agree with me on (#2).

What we have to discuss briefly now is thus:
Whether (#2) is correct or not?
If so, why do I think that (#2) is the consequence of (#1)?

Finally, if (#2) is correct, does it imply that certain cues – in particular the relative distances between features – must have special status at the representational level?

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