KLINIKUM DER ALBERT-LUDWIGS-UNIVERSITÄT

ABTEILUNG KLINISCHE NEUROLOGIE UND NEUROPHYSIOLOGIE
ÄRZTLICHER DIREKTOR: PROF. DR. R. JUNG

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11.4.78

Department of Ophthalmology,
NYU Medical Center,
550 1st Avenue,
New York,
NY 10016,
U.S.A.

Dear Dr.

Enclosed is an MS entitled "The perception of suprathreshold sinusoidal flicker measured by light- and dark-phase matching". This is a revised and severely curtailed version of an MS "Frequency-response characteristics of the visual system for temporal modulation above threshold" submitted to Vision Research through Dr. Shipley, and it is resubmitted to you according to the new guidelines.for

The MS reports an experiment on the perception of suprathreshold sinusoidal flicker using a technique involving brightness matching of the maxima and minima of the flicker cycle, and we compare the resulting frequency-response functions to flicker threshold functions. Our first version of the MS received a rather rough treatment from the referee, whose comments are enclosed for your information. By way of explaining why we resubmit the MS (rather than simply drop dead) I wish to make some replys to the referee's more specific points, marked 1-6 in his comments.

1-2) We were in fact familiar with the work of both Bryngdahl (not Bryndahl) and Boynton et al. The relevance of Bryngdahl's work is principally methodological since he used a parallel procedure in his studies on the perception of suprathreshold sinusoidal gratings.(as do Fiorentini & Maffei later, J.Physiol. 231, 1973), and he might of course have been cited in the previous MS. In view of some of the other comments by the referee (see below) we have now included two specific references to his work.

Boynton et al. measured the increment threshold for a test-flash presented at various times relative to the flicker cycle of square-wave flicker presented at 15 or 30 Hz, and their results show that the distributions of thresholds in the high-frequency range describe more or less a sine-wave, leading to the same conclusions as the de Lange experiments. The specific relevance of these high-frequency square-wave experiments to our brightness matching experiments with pure sine-waves (0.5 - 15 Hz) and to the interpretation of our results remains a secret to me, and the referee's comments are not very helpful.

(6 point in the Discussion)

Our conclusions regarding sine-wave and square-wave flicker referred to by the referee, relates to the fact that previous experiments on the perception of suprathreshold square-wave flicker (Vision Res. 17) at high modulation amplitudes showed a substantial low-frequency decrease in the psychophysical response whereas the present experiments with sine-waves show no such decrease. At higher frequencies (above 5-6 Hz) our experiments indicate no differences between the sine- and square-wave frequency-response functions (ineagreement with Boynton et. al.) but at low frequencies the findings are contrary to what is observed at threshold. There is nothing meaningless about this perfectly empirical result (not obviously expected) and it requires an explanation. In our discussion we speculated about possibl differences in brightness integration times for sine- and square-waves at low frequencies. We have, however, omitted this point in present curtailed version.

3) We did not use the quantity (Lmax-Lmin)/(Lmax+Lmin) to denote the modulation of non-sinusoidal wave-forms. We used $(L_{cf}max - L_{cf}min)/$ $L_{cf}max + L_{cf}min)$ as an index of the subjective depth of modulation, where $L_{cf}max$ and $L_{cf}min$ are the comparison luminances required to match the brightness maxima and minima. This is in fact exactly the parallel to Bryngdahl's definition of "subjective contrast" in the spatial domain.

In all fairness to the referee it is possible, however, that confusion might arise from two sources in the previous MS, now corrected: Firstly, rather than using $L_{cf}max$ and $L_{cf}min$ we adopt Bryngdahl's Bmax and Bmin to specify that we speak of brightness (expressed in terms of matching luminance, again as in Bryngdahl's study). Secondly, in the previous MS we used the term "apparent amplitude of modulation" to denote this quantity, and I agree that this is misleading. It has now been changed to "subjective temporal contrast" with an explanation of the choice of this term.

4-5) It is difficult to make serious replies to these comments because they are completely besides the point and seems to reflect some misunderstanding ~~of aspects~~ of suprathreshold psychophysics.

If one wish to measure brightness (or some other perceptual dimension) several response indicators are available: for example, it may be required that the subject gives a direct numerical estimate of the brightness, perform a cross-modal match (adjust this sound to be as loud as the light is bright), or perform a brightness match to a comparison light. All of these procedures (and several others) are equally sound (and, in a way, equally arbitrary), and as proven several times the general shape of the resulting curves would be more or less the same. When one uses brightness matching to evaluate brightness variations across changes in spatial or temporal parameters, the spatio-temporal properties of the comparison stimulus are unimportant as long as thy remain fixed during changes in the test stimulus. Since the relationship between luminance and brightness is known to be approximately logarithmic (for fixed spatial and temporal values) luminance variations of the comparison thus serves as a convenient measure for brightness variations of the test stimulus. Of course, the specific luminance values required for a match depend upon the spatial and temporal characteristics of the comparison (i.e. a 100 msec flash requires less luminance to attain a given brightness than a 400 msec flash (Broca-Sulzer effect)), but the general shape of the resulting curves would be the same. Since the referee does not seem to realize this simple but fundamental point, his comments become meaningless.

6) Again, the referee is simply wrong. There is no methodological principle from which the greater scientific soundness of either of these two psychophysical procedures can be derived, they are equally common in the literature, complementary and give similar results. For example, in experiments on brightness enhancement of flickering lights van der Horst and Muis (Vision Res. 9, 1969) used a fixed comparison and we (Exp. Brain Res. 22, 1975) used a variable ~~reference~~ com-parison and the results are closely similar.

In conclusion, it is difficult to see the justification of the referee's judgement of our MS. It is (to my knowledge) the first study of suprathreshold sinusoidal flicker including low frequencies, it uses an acknowledged psychophysical method, and it presents new results. Of course, as Shipley once wrote in a Vision Res. editorial, in a way "the referee is always right", and we have in the present condensed version attempted to clarify any doubious points. We have also changed the title to a more descriptive one. The MS should now be more suitable for a Research Note (or possibly a Letter).

I look forward to hear from you.

Yours sincerely

Svein Magnussen

In reply please use my current address:

Neurologische Universitäts-Klinik mit
Abteilung für Neurophysiologie,
78 Freiburg im Breisgau,
Hansastrasse 9,
West-Germany

EKSEMPEL 3

Reprint Series
22 February 1980, Volume 207, pp. 908–909

SCIENCE

En interessant publiseringshistorie. Denne artikkelen ble først sendt til Nature, som automatisk returnerer 70% med begrunnelsen at artikkelen er for spesiell. Etter et tilsvar, ble artikkelen sendt til konsulent, som misforsto poenget i artikkelen. Etter nok et tilsvar innrømte konsulenten at han nok hadde misforstått, men opprettholdt sin vurdering! Artikkelen ble så sendt til Science, som aksepterte den. Kommentarene fra Science illustrerer også forskjellen i konsulentuttalelser.

Adapting to Two Orientations: Disinhibition in a Visual Aftereffect

Svein Magnussen and Wolfgang Kurtenbach

nature

In reply please quote:
M02363 JW/DJ

2nd March 1979

Dr. S. Magnussen,
Institute of Psychology,
University of Oslo,
Blindern,
Oslo 3,
Norway.

Dear Dr. Magnussen,

In view of the pressure on space in Nature, I am afraid that on this occasion we are unable to publish your manuscript.

We are therefore returning it so that you can resubmit it without further delay to a more specialised journal such as Vision Research, where I am sure you will have no difficulty in publishing it.

Yours sincerely,

Editorial Assistant.

Encl.,

PSYKOLOGISK INSTITUTT

10.3 1979

Your ref: MO2363

Editorial Assistant,
NATURE
4 Little Essex Street,
London WC2R 3LF
England

Dear Dr.

Thank you for letter of 2.3-79. I am a regular reader of Nature and have also from time to time submitted papers when I thought them suitable for the journal. However, they have always been returned, and for reasons that they are to "specialized" (whatever that means, cf. a random recent issue of Nature), that there is pressure on space an so on. The papers have always been accepted by other journals so Nature's rejection only means a delay in publication. However, the reason that I submit to Nature in the first place is that I consider it most suitable for this particular manuscript. In the present case I am particularly astonished because at least 25 papers (mostly by British authors) on visual after-effects and related topics have been published in Nature during the last 7-8 years, and I can offhand remember only a 3-4 whose results are so theoretically descisive as ours. Furthermore, the subject of our paper is very central in current psychophysical and neurophysiological theorizing the paper should therefore be ideally suited for Nature. All this makes me wonder which manuscripts by which authors receive a serious consideration, and whether there is any point in submitting to Nature at all. Of course I understand that limited space makes it necessary to reject otherwise acceptable papers, but in this case I ask you to reconsider your desicion and therefore take the liberty to resubmit the manuscript.

Yours sincerely

Svein Magnussen, Dr. philos.

nature

Macmillan Journals Ltd
4 Little Essex Street
London WC2R 3LF
Telephone 01-836-6633
Telex 262024

In reply please quote: M02363R
MR/CP

11th May, 1979

Dr. S. Magnussen,
Institute of Psychology,
University of Oslo,
Blindern,
OSLO 3,
Norway.

Dear Dr. Magnussen,

Your manuscript has now been seen by a referee in the light of whose comments I am afraid we are unable to reverse our original decision.

We are therefore returning the manuscript once again, with the referee's comments which we hope you will find helpful.

Yours sincerely,

Jonathan Weitz

Associate Editor.

Encs.

REFEREE'S COMMENTS

The experiment is clearly described but I am not persuaded that it supports the authors' claim that adaptation is a result of inhibition. I have minor worries about the method, since it doesn't permit easy comparison with the results of other experiments, and I have a major worry about interpretation.

Three aspects of the method bother me:

a) During the adaptation phase of the experiment the gauge is allowed to move along a bar, while in the test phase fixation is required. It doesn't really seem necessary to avoid the production of after-images in the adapting phase of the experiment and it would be nice to know that the eye-movements don't have some peculiar effect on the results.

b) An odd method is used to measure the change in perceived orientation. Does line C rotate about its end or its middle? Does the observer set it to appear co-linear with the vertical test line, or what? By putting the C line end-on to the test line one adds a new complexity whose effect on base-line results needs to be known.

c) Given a) and b) one really wants to be assured that the authors could reproduce the 'standard' result relating magnitude of the tilt after-effect to the difference between the orientations of the test and adapting lines. Unless they can, it is impossible to interpret their figure.

I also have a more substantial worry about their interpretation. I agree that the angular function for the "reduction" effect is similar to the angular function of the after-effect itself. But it seems to me that this could be produced by the addition (albeit incomplete) of an after-effect that has a standard angular function (produced by line A_2) and a fixed effect (produced by line A_1). There is clearly a quantitative problem because the sum of the separate effects would be greater than that displayed in Fig. 1, but I see no reason to expect perfect addition anyway.

It is misleading (in Fig. 1) to connect the data points for 6° tilt to the dashed line at the ordinate.

YKOLOGISK INSTITUTT

22.5.1979

Dr. [illegible],
Associate Editor,
NATURE,
4 Little Essex Street,
London WC2R 3LF
England

M02363R
MR/CP

Dear Dr. Robertson,

Thank you for your letter of 11th May, and the referee comments on my manuscript "Adapting to two orientations: disinhibition in the tilt after-effect". I feel a little uneasy writing this reply, seeing the problems of editing a journal like Nature and the vast numbers of researchers who wish to publish their papers, but my problem is that the referee's worries are so easily answered that a couple of minor revisions and additional statements should satisfy any doubts. I enclose a copy of the referee's comments and answer them below.

a) Scanning a fixation-bar during the adaptation phase is a widely accepted procedure adopted as a precaution against possible confounding with conventional after-images, used in the wast majority of contemporary studies. However, steady fixation gives similar results.

b) The method used to measure perceived orientation is not at all odd, to the contrary it is used and recommended by most authors. Line C was rotated about its middle, and as stated on p. 2 in the manuscript the subject's task was to set it parallel to the apparent orientation of the test line. Whether the test and comparison line is presented side by side (in that case a separation of several deg. visual angle is required) or above each other is a matter of chöice, we used the latter method because it increases the preciseness of the settings, and

c) <u>of course</u> we reproduce the standard result with this method (if necessary, an additional figure showing these results may be included); otherwise we would not publish <u>this</u> experiment.

I do not at all understand the referee's "substantial worry" about the interpretation. The results can not be explained by summation of two after-effects (arising from A_1 and A_2, respectively), because then the datapoints would lie above the baseline (a mirror-image of what we find), and that is the very point of the paper (discussed on p.3) and what makes the experiment a crucial test between the "inhibition" and "neural fatifue" hypotheses.

In view of the above points, I would very much appriciate your comments as to whether a suitably revised version would be acceptable. Please excuse all this extra trouble I am causing, but as all other researchers I am of course eager to publish my results in the best and most suitable journals. Looking forward towards your reply,

Yours sincerely

Dr. Svein Magnussen

nature

Macmillan Journals Ltd
4 Little Essex Street
London WC2R 3LF
Telephone 01-836-663
Telex 262024

In reply please quote:
LL02363RR LR/LS

21st June 1979

Dr.S. Magnussen
Institute of Psychology
University of Oslo
Blindern
Oslo 3
Norway

Dear Dr. Magnussen

Your manuscript has now been seen once again by our referee whose comments are attached. As you will see, he now agrees that he misunderstood but nevertheless cannot recommend publication in Nature. We are therefore returning it once again, so that you can resubmit it elsewhere as he suggests.

Yours sincerely

pp Associate Editor

REFEREE'S COMMENTS

I have re-read the paper, and I now think my previous "substantial" criticism was based on a misapprehension.

However, I'm still not convinced that their interpretation is correct. My doubts remain centred around the necessity of their hypothesis. I grant that some inhibitory dual effect of line A2 on A1 would account for their results, but that inhibition need only be exerted during adaptation; it's not necessary to suppose that the inhibition is prolonged. One might suppose that, during adaptation, line A2 inhibits the mechanisms that rspond to line A1, with the result that these (now less excited) mechanisms become less fatigued than they would in the absence of line A2. Since they are less fatigued, the after-effect will be smaller. There seems to be no obvious reason to suppose the existence of any prolonged inhibition. I don't know how Magnussen and Kurtenbach's experiment would exclude that hypothesis.

I feel that "crucial" experiments like this are valuable only when the hypotheses they test are very explicit. The alternative hypotheses of "fatigue" and "prolonged inhibition" are here not developed explicitly enough for one to be confident that the experiment really excludes one of them.

My feeling is that the results and theory might be better developed more fully in a longer article, perhaps in Vision Research.

SCIENCE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
1515 MASSACHUSETTS AVENUE, NW, WASHINGTON, D.C. 20005 • 202—467-4350

29 October 1979

Dr. Svein Magnussen
Institute of Psychology
University of Oslo
Box 1094 Blindern
Oslo 3, NORWAY

Dear Dr. Magnussen:

Although we are interested in publishing your paper, we are not able to accept it in its present form. Our referees raise some objections that should first be met, and the comments are enclosed.

Accordingly, I am returning the manuscript in the hope you will make the appropriate revisions.

Sincerely,

Assistant Managing Editor

JER/cw
Enclosure

SCIENCE

Author MAGNUSSEN, S. & KURTENBACH, W.

Title Adapting to Two Orientations: Disinhibition in a Visual After-Effect

Comments: Contrast illusions and aftereffects in contour perception occupy an area where a theoretical unification of many effects is in the air, and an open discussion of concepts, models and data are all needed. In this context, the present work contributes needed data, but is not pointed enough in its discussion of the data's implications. Perhaps it is only a matter of taste, but it would be my preference as a scientist to make at least some of the following changes and to consider some of the following issues:

1. Drop the statement, "We can not of course generalize to other visual after-effects." This work is important precisely because a general account of most contour illusions and aftereffects is at stake.

2. Be explicit in reference to two-spatial-frequency experiments. Perhaps the Dealy & Tolhurst (1974) study should be described, as this is what the already-cited Stromeyer, Klein & Sternheim (1977) critique is aimed at. The experiment of Sharpe (Vis Res 1974, 14, 41) is exactly analogous for interactions between colors and might be an easier example for readers. Compared to the intricacy of a Dealy-Tolhurst-type experiment, the greater elegance of the present demonstration should shine nicely.

3. Much psychophysical work in the past decade has been concerned with "channels" defined by the selective adaptation and aftereffect paradigm. If prolonged inhibition causes adaptation, the channels are not a door to single-unit excitatory tuning curves but rather reflect inhibitory processes among populations of units. Although pointed out before (e.g., Blakemore, Muncey & Ridley, 1973 Vision Res.), it is a major implication worth stating for a general readership.

4. The current uncertainty/controversy over the bandwidth of spatial frequency channels (Nachmias, Weber, Vision Res 1975, 15, 217; Stromeyer & Klein, Vis Res 1974, 14, 1409) may reflect their determination by interactions among many units (greater chance for variability to enter) or even determination by several mechanisms.

5. Fourier analysis or analysis based on any other series of orthogonal components

(cont'd.)

(Continue on additional sheet if necessary) can not be used to construct "an elegant theory of pattern perception" (as Furchner, Thomas & Campbell Vis Res 1977, 17, 827 describe the pioneering approach of Campbell & Robson, 1968) if spatial frequency channels fail to have independence. For orientation, the present work shows interaction, the opposite of independence. Independence is important, and the lack of it here is in keeping with the lack of it elsewhere. For spatial frequency, one could choose among Henning, Herts & Broadbent, Vis Res 1975, 15,887; Tolhurst & Barfield, Vis Res 1978, 18, 951; and Furchner, et al. loc cit.. Or, one might put the paper's Footnote 9 into the text and enumerate the domains in which interactions have been documented or found useful to postulate theoretically. This would follow an introductory paragraph suggesting the significance of the point for the issues I have tried to enumerate here: excitatory vs. inhibitory; single-unit vs. populations; bandwidth; passive tuned detectors vs. active processing; independence vs. interaction.

6. (lesser point) The present thesis is that aftereffects are caused by persistence of the same mechanisms which cause simultaneous contrast. There are several points of similarity between simultaneous and successive versions of the perceived frequency shift demonstrated in two papers already cited (Klein et al. 1974 and Tolhurst & Thompson, 1975). If the thrust of the present paper is to be the generality of domains where one mechanism will suffice for two effects, then these similarities in the realm of spatial frequency should be mentioned.

My overall comment is that the present result is important because of its elegant simplicity and because it is the first result of its kind in the well-understood orientation domain, rather than the spatial frequency domain, where the neurophysiological substrate is less well understood in both structure (columns, slabs) and function (tuning curves, intracortical interactions).

EKSEMPEL 4

Perception, 1985, volume 14, pages 265-273

Visual half-field symmetry in orientation perception

Svein Magnussen, Nils Inge Landrø¶, Tore Johnsen
Institute of Psychology, University of Oslo, Box 1094, Blindern, Oslo 3, Norway; ¶Sunnaas Rehabilitation Hospital, 1450 Nesodden, Norway
Received 24 February 1984, in revised form 12 November 1984

Abstract. The perception of orientation in the left and right visual half-fields has been investigated. No evidence for interfield differences was obtained for the discrimination of single lines by line matching or in magnitude of the systematic orientation distortion in orientation contrast and rod-and-frame experiments. Furthermore, increasing the time interval between test and comparison lines in successive matching provides no evidence for a differential operation of short-term spatial memory in the two hemispheres. It is concluded that hemispheric asymmetries do not arise at the level of sensory processing of spatial signals.

1 Introduction
It is generally accepted that the cerebral hemispheres are not functionally symmetrical (Beaumont 1982; Bradshaw and Nettleton 1983), but the nature of the asymmetries is a matter of some debate (Bradshaw and Nettleton 1981). Of special relevance to vision is the hypothesis that the right hemisphere surpasses the left in handling visual spatial signals, processing stimuli holistically as opposed to the analytic mode of processing by

Denne artikkelen ble først sendt til Neuropsychologia hvor tre konsulenter avviste den, hovedsakelig fordi de ikke likte resultatene. Interessant nok ble tilsvarende eksperimenter med svake metoder og svakt design, men som viste (sannsynligvis tilfeldige) positive resultater, publisert i samme årgang. det er imidlertid ikke grunnlag for parlamentering når konsulenter og redaktør ikke finner resultatene interessante nok (men foreslår andre tidsskrifter).

Artikkelen ble så sendt til Perception, som aksepterte den. Bemerk igjen forskjellen i grundighet.

Fontenot and Benton 1972; Atkinson and Egeth 1973; Kimura 1973; Umiltà et al 1974; Sasanuma and Kobayashi 1978; Scobey 1982), curvature (Longden et al 1976), and stereoscopic depth (Durnford and Kimura 1971; Julesz et al 1976; Pitblado 1979a; Grabowska 1983) differences between the left visual field (LVF) and the right visual field (RVF) are sometimes found, sometimes not. Among these, half-field testing of the perception of single-line orientation apparently gives stable field effects; but the available results are inconclusive as regards the role of sensory mechanisms. For example, performance on the widely used match-to-sample technique may reflect either the sensory capacity, spatial short-term memory, or the reporting process (Scobey 1982). Dee and Fontenot (1973) found that interfield differences vary with the delay between test and comparison stimuli, which suggests that memory may be an important factor in matching experiments. A similar difficulty of interpretation arises in connection with reaction-time measurements; for example, the much cited experiment of Umiltà et al (1974) required the subject to compare the test stimulus to a memory set of orientations.

NEUROPSYCHOLOGIA

An International Journal – Une Revue Internationale – Eine Internationale Zeitschrift

EDITOR-IN-CHIEF – M. JEANNEROD
Laboratoire de Neuropsychologie Expérimentale.
U.94 – INSERM, 16 Avenue du Doyen Lépine
69500 Bron. FRANCE.

Bron, November 14, 1983.

Dr. Svein Magnussen
Institute of Psychology
University of Oslo
Box 1094
Blindern,
Oslo 3, Norway

Dear Dr. Magnussen,

Your paper entitled "Processing of line orientation by the left and right cerebral hemispheres in normal subjects" has now been analyzed by the 3 referees to whom it was submitted. As you will notice from the enclosed comments, they all came to the same conclusion, namely that your experiments are competently done but do not reach results of a sufficient significance to warrant publication in Neuropsychologia.

In fact, I agree with the referees that the experiments you report were not designed to disentangle the right/left processing problem, but rather were designed to test more "peripheral" mechanisms of visual perception. Therefore, it might be that your study would be more suited to a journal devoted to psychophysics or to the study of perception than to Neuropsychologia.

I am sending back to you the original manuscript and thank you for your interest in our journal.

Sincerely yours.

...NERC.

Encl.

Published by PERGAMON PRESS Oxford · London · New York · Paris

Comments on the paper by S.Magnussen et al.: Processing of line orientation by the left and right cerebral hemispheres in normal subjects.

This paper reports three experiments on perception of line orientation in the right and left visual fields. The experiments appear to have been performed in a competent and careful way. No differences between the fields were found in terms of accuracy of matching of line orientation (Experiment I), orientation contrast effect (Experiment II) and rod-frame interaction (Experiment III). The conclusion that hemifield and hemispheric differences emerge at a rather late stage of processing is reasonable, but I am not sure that the present data are strong enough to support it. Davidoff (see reference list) stated that the demonstration of a left visual field advantage with discrimination of line orientation requires considerable information in the display and/or little time allowed in the processing. If these conditions are met, the left visual field advantage for orientation discrimination seems fairly robust. It is reasonable to suggest that the results could have been different in Experiment I if the Authors had used a reaction-time paradigm, and perhaps the same suggestion applies to the two other Experiments. On these grounds, I feel that the data are too weak to allow any conclusion about hemispheric differences on this task in normals. Neuropsychologia therefore is not the appropriate medium for publishing this paper. Experiments II and III may be of some interest to an experimental psychology journal.

[illegible] er 2

Processing of line orientation by the left and right cerebral hemispheres in normal subjects

Magnussen, S., Landrø, N.I. and Johnsen, T.

In this paper, the authors report their negative findings in three experiments that investigated hemispheric differences in the processing of line orientation. The authors state that these experiments were based on two hypotheses. The first hypothesis refers to the "superiority of the right hemisphere over the left in handling visual spatial signals". The second hypothesis is concerned with differences in the mode of processing between the two hemispheres, left (analytic) and right(holistic).

While the authors discuss adequately the background to the first hypothesis no such discussion is offered for the second hypothesis. For example, in p.5 the following statement is made: "...we considered half-field testing of this sort of spatial illusions relevant perceptual tests of the analytic-versus-holistic-processing hypothesis of hemisphere function". The relevance, though, of the analytic/holistic hypothesis to these perceptual tests is not made clear. In view of the fact that there is a lack of general agreement over a clear definition of what the terms "analytic" and "holistic" processing imply the authors should provide their definition of these terms in relation to their stimuli and state their predictions. For example, how could the orientation illusion phenomena tested in experiments II and III be differentially affected by analytic or holistic processing.

The reporting of the Method and procedure of experiment I is not complete. For example each time the subject issues a "left" or "right" instruction to the experimenter, what is the magnitude of the change in the orientation of the "comparison line" (presumably the change of orientation was of a constant magnitude)?

What was the orientation of the comparison line in relation to the test orientation?

1 Were there any differences in the number of instructions issued by the subject in relation to the visual field of presentation?

The information concerning the statistical analyses given in the Results sections is also incomplete. Other than one *F* value for experiment I no other statistical parameters are given. In addition, whenever the *F* values are reported the degrees of freedom must also be stated.

Given that the three experiments reported do not stem out of a precise definition of theoretical concepts, the negative results reported are not critical contributions to this field of investigation, therefore, this paper is not suitable for publication in Neuropsychologi

Comments on the paper "Processing of line orientation by the left and right cerebral hemispheres in normal subjects" by S. Magnussen et al.

In this paper three experiments are reported in which the authors studied the perception of line orientation in the right and left visual fields. The first experiment is methodologically weak. In this experiment the subjects were required to match the orientation of a "comparison" line with a "test" line flashed to the right or left hemifield. The test line was presented as many times as requested by the subject in order to be satisfied with his match. This procedure lacks one of the perquisite necessary for hemispheric differences when the stimulus discrimination is easy: short exposure. One wonders, if under more pressure, the customary right hemisphere superiority would not have emerged.

The other two experiments are carefully done, but their interest is limited. The conclusion that hemispheric asymmetries do not arise at the level of sensory processing of spatial signals is not proved by the experiments. What is proved is that some visual illusions originate at some rather peripheral level. I am afraid that these findings have a too limited interest to deserve publication in Neuropsychologia. It is my opinion that at this stage of the study of hemispheric differences sharper experiments and a more solid theoretical frame are required to improve our knowledge of the field in a substantial way.

Journal Editor, Brain and Perception Laboratory
Department of Anatomy, The Medical School
University Walk, Bristol, BS8 1TD, England · telephone Bristol (0272) 24161
PERCEPTION

Ref. 1279 TT

8th May 1984

Dr. S. Magnussen,
Institute of Psychology,
University of Oslo,
Box 1094,
Blindern,
Oslo 3,
NORWAY

Dear Dr. Magnussen,

I enclose two referees' comments on your paper, ms. no. 1279 TT, on "Visual half-field symmetry in orientation perception".

Both referees recommend publication subject to a number of revisions. I look forward to receiving this revised version.

Yours sincerely,

R. Graham.

Af

Encs.

Ref #2

General Comments

This is a generally well-presented paper except for the large number of minor errors indicated in the attached notes.There are, however,one or two points that worry me.

The number of practice trials in the experiments is very low which could produce unwanted variance in the data.When the object is to show that there is no laterality effect (not that this result was forseen) then it is incumbent upon authors to show that unwanted variance has not obscured a potentially positive finding. I am aware that laterality differences have been found in some experiments to vanish or reverse as experimental sessions proceed but this question could have been examined by appropriate analysis of the data.

In Experiment 1 (page 6) was experimental session entered as a factor in the analysis of variance? Also, was the significance of the left-right difference in mean standard deviation for the 30 degree condition tested?

The data plotted in the authors' Figure 1 refer to the variability in the subjects' settings for the different meridians tested.I have pointed out that the mean discrepancy between the subjects' settings of the comparison lines and the true orientations of the test lines could be larger for a particular meridian even though the mean standard deviation of such settings was smaller (see accompanying notes).The mean "discrepancy values" should therefore be reported. I assume that what the authors refer to as "the mean standard deviations of the equality settings" were derived by 1) calculating for each subject the difference between the subject's setting of the comparison line and the true setting of the test line 2) calculating (for each : the standard deviation of the settings for a given meridian and 3) averaging the standard deviations across subjects.This -or whatever the authors do mean by their term- requires to be made explicit in the text.

Along the same lines,there could conceivably be a difference between left and right visual fields in the extent of the mean difference between true and "obtained" orientations.The mean values,and not simply the standard deviations,should therefore be reported.

In Experiment 2 how do the authors know (page 7) that an inter-stimulus interval of 0.5 msecs is sufficient to avoid masking? This is relevant in view of suggestions that left and right visual fields differ in their response to adaptation (Tei and Owen,Percep.&Psychophys. <u>28</u>,479-483; see also Meyer,Nature,<u>264</u>,751-753).Could a greater masking effect in one visual field have counter-acted a greater sensitivity to orientation in that same visual field?In this connection (as well as more generally) I should like to see a same-different analysis of the data.A difference between fields may obtain only for,say, different trials ,and not for same trials,leading to an overall lack of visual field effect -as reported by the authors- when the data are collapsed across this factor.A left-right difference for different but not for same trials could indicate that masking is effective over a greater range of orientations in one or other visual field.Although this was not found to be the case in the experiment by Beaton and Blakemore (1981) (using an adaptation paradigm) the orientations used by the latter differed by at least 10 degrees while those used by Magnussen et al. differed by 5 degrees which might conceivably reveal a difference.

To anticipate the authors' response I am not convinced given the 5 msec. exposure duration that the subjects'

Ref. 1

1. Show diameter of centre grating in inset to Fig. 3.

2. Re-word reference to Fairweather (lines 7-8 of page 10) - the aside enclosed by dashes is not clear as to what is meant. (what does "not encouraged" mean?).

3. Expand paragraph on lines 3-10 of page 11. What prediction about the experimental results would have been made on the analytic-wholistic dichotomy? What was the line of reasoning of Bertelson and Morais?

EKSEMPEL 5

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HIGHER-HARMONIC ADAPTATION AND THE DETECTION OF SQUAREWAVE GRATINGS*

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(*Received* 28 *November* 1985; *in revised form* 29 *May* 1986)

Abstract—Adaptation to a high contrast sinewave grating of 1 c/deg spatial frequency causes a large increase in the contrast threshold for a 1 c/deg test grating, but fails to raise the threshold for a squarewave grating of 0.33 c/deg, although the sensitivity of the "channel" tuned to both the third and fifth harmonic components of the squarewave test grating should be thoroughly suppressed. Following sequential adaptation to sinewave gratings of 1 and 3 c/deg spatial frequency, detection of squarewave gratings at 0.33 c/deg likewise remains unaffected. In contrast, after adaptation to a 0.33 c/deg squarewave grating with missing fundamental the contrast threshold for a squarewave test grating of the same frequency is increased by 0.25 log unit, although the higher harmonic component frequencies are less affected than by sequential sinewave adaptation. The results suggest that independent spatial frequency channels detecting harmonic components are not alone sufficient to account for the visibility of low frequency squarewaves.

Linear filtering   Adaptation   Low frequency detection   Complex gratings

INTRODUCTION

At low spatial frequencies, sensitivity to sinewave gratings falls off linearly, but remains approximately constant for squarewave gratings (Campbell and Robson, 1968). Th...

Denne artikkelen har en lang publiseringshistorie med en repetisjon av hovedeksperimentet, flere tilleggseksperimenter, supplementerende statistiske analyser, computersimulering av resultatene på grunnlag av en formalmodell og tre redaksjonelle omganger med totalt seks konsulentuttalelser. Resultatene har tråkket mange på tærne. To konsulentuttalelser er gjengitt for å illustrere den perfide tonen i enkelte vurderinger.

account for the ... to sinewave and squarewave gratings at low spatial frequencies. They, therefore, suggested the existence of multiple channels, each tuned to a different spatial frequency, acting independently (see also, Sachs *et al.*, 1971; Graham and Nachmias, 1971). A complex grating is, accordingly, either detected by the most sensitive channel or as a result of probability summation over channels (Wilson and Bergen, 1979). For squarewave gratings of low spatial frequencies this means that the channels with peak sensitivities at the third, fifth, and higher harmonic components might be employed to detect the squarewave grating at threshold (see Jaschinski-Kruza and Cavonius, 1984).

Further evidence of the multichannel model ... might encode the various frequency components of the retinal image. A fairly limited number of channels is usually assumed (Wilson and Bergen, 1979; Watson and Robson, 1981; Sekuler *et al.*, 1984; Wilson and Gelb, 1984).

Campbell *et al.* (1981) more recently put forth the idea of a "watershed" in spatial vision. According to this idea, harmonic analysis is conducted for spatial frequencies above 1 c/deg, whereas local, gradient detection is done below 1 c/deg. Jaschinski-Kruza and Cavonius (1984) have demonstrated, however, that gradient detectors are not necessary to account for low-frequency detection, since this can be accurately predicted by their space-domain model. It remains, therefore, to be shown whether a low-

*Part of these findings were presented at the 8th *European Conference on Visual Perception*, Peniscola, Spain, September, 1985.

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SP769

M.W. Greenlee and S. Magnussen

p.1. It is not self-evident from the preceding that "the third harmonic of ;of low spatial frequency squarewaves should be detected before the fundamental." The authors should make their reasoning explicit.

p.6. The comments made on the previous paper regarding psychophysical methodology and the standard error apply here. In this paper, results from a second observer are presented, ;but only in one of the figures.

p.7 and 8. These pages contain longwinded and rather silly attempts to convince us of the unimportance of certain differences; they are also confusing because of the mixture of linear and logarithmic units for contrast. I urge that these pages be boiled down to a few clear sentences in which contrast is discussed in the the same units.

p.9. The rationale for adapting to a squarewave of frequency 3f (rather than, say, of f) does not become made clear until the discussion section.

p.10. Frankly, I could not make head or tail of the theoretical notions presented at the end of the paper and in the last figure. If I am not alone in being confuesd, the authors should be urged to clarify their thinking, or their exposition, before resubmitting the paper.

EKSEMPEL 6

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NOTE

DETECTION OF MOVING AND STATIONARY GRATINGS IN THE ABSENCE OF STRIATE CORTEX

SVEIN MAGNUSSEN and TOVE MATHIESEN*

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(*Received* 3 *November* 1987; *accepted* 29 *December* 1988)

Abstract—A 30-yr-old woman whose left occipital lobe had been removed because of an arteriovenous malformation was tested for her ability to detect stationary flashing and moving luminance gratings in the right (blind) visual field. With stationary gratings the performance ... moving gratings her performance on a 2AFC task rose to an

Dette eksemplet viser eksemplariske og seriøse konsulentuttalelser, hvis ønsker desverre bare delvis kunne etterkommes, og en fornufting og nøktern dialog med redaktør.

localization of unseen lights ...
and rudimentary discrimination of form [27, 28] in perimetrically blind regions of the visual field. According to TORJUSSEN [25] this non-conscious "blind-sight" [22] performance may even in some cases be turned into conscious perception with suitable bilateral presentation of stimuli in symmetric positions in the normal and blind visual hemifields. Explanations of these results generally refer to the "two visual systems" concept [8, 11] and the role played by the mid-brain projections to the superior colliculus [see reviews in 5, 27].

However, neither the results nor their interpretations have gone completely unchallenged. Several studies do not find evidence for blind-sight with similar or improved testing techniques [5, 9, 14]. In a critical review CAMPION *et al.* [5] identified several sources of experimental errors in the early studies, notably that of ocular stray-light, and pointed to the possibility of processing by spared cortical tissue. The present study eliminates both these sources of ambiguity. First, our subject B.N. has had her complete left occipital lobe surgically removed; this makes her an ideal blindsight subject. Second, rather than introducing experimental strategies such as a blind-spot control condition [24, 28] to meet the stray light objection, we circumvent the problem by using electronically generated grating stimuli whose exposure involves changes in contrast but not in mean luminance. We tested B.N.'s ability to detect both stationary and moving luminance gratings using two-alternative forced-choice procedures.

METHOD

Subject

B.N. is a 33-yr-old woman who was operated on in 1983 because of a huge arteriovenous malformation in the left parieto-occipital area. The whole occipital lobe was removed from the midline, along the falx down to the cerebellum; laterally above the parieto-occipital sulcus down along the superior-temporal sulcus to the posterior part of the infra-temporal gyrus, according to the surgeon's description. The operation has left her with a right homonymous hemianopia with less than 2° macular sparing, sufficient for accurate fixation. Figure 1a shows the visual field borders determined for left and right eye by the Goldman perimeter. The visual field of the left eye has an approx. 15° wide intact sector in the upper right quadrant, and the binocular visual field has a 10° intact sector in the

Ref 1

COMMENTS ON REVISED PAPER BY GREENLEE AND MAGNUSSEN

The revised paper is crisper than the original, which highlights both the strengths and weaknesses of this work. The strengths are such that I sincerely hope this paper finds its way into the literature soon. Its weaknesses - described below - are such that I believe it is still not ready for publication.

METHODOLOGY: The authors still insist on giving standard errors in linear units, which are meaningless, for the reasons stated in the previous review. They also still fail to state on how many independent staircases their standard errors are based: [redacted]. (A minor point: they can well afford the space to specify their staircase procedure more fully, by stating (a) "the specified amount" by which the contrast was altered, and (b) describing the adaptation procedure more fully: was the stimulus steady or flickered? what was the observer supposed to do with his eyes, etc?

DATA: We are promised data from two observers, and mostly what we get is data from one or the other; we only get data from both on sine/square discrimination before and after adaptation. Why isn't figure 3b as complete as 3a? Surely the contrast sensitivity function for observer JG was also measured. In contrast, only data for observer JG are presented in figure 4. That is an important experiment, for which we deserve to see MWG's data as well (it is hard to believe they do not exist). The apparent omission of data that must have been collected may raise unnecessary doubts in reader's minds.

DISCUSSION: It is mercifully briefer than last time. However, it highlights the fact that the authors' conceptualization of the problem has not advanced beyond its original formulation twenty years ago. In the meantime, several people have reported that the presence of the fundamental can actually facilitate the detection of the third harmonic, so it is by no means obvious why Campbell and Robson's original predictions worked as well as they did, and may explain why they do not work here even without adaptation. The authors must at least make a nod at this issue; I will be glad to supply them with relevant references if they are not aware of them.

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Comments on the paper by S. Magnussen and T. Mathiesen "Human movement and pattern detection in the absence of striate cortex".

This article, which has the format of a Research Note, deals with the important issue of "blindsight", that is with the unconscious detection of visual stimuli presented to a perimetrically blind area of the visual field. The main thrust of this study is that a moving pattern can be detected by a patient who has undergone a unilateral removal of the occipital lobe even when the stimulus is presented to the hemianopic area of the visual field. When a similar pattern is held stationary, above-chance detection is still possible but is much less obvious than when the pattern is moving.

In principle, this paper is acceptable for publication in Neuropsychologia as a Note but only if the authors provide a satisfactory reply to the following queries or discuss the following points:

METHODS- 1) A better description of the surgical removal should be provided and in addition it would be useful to show a CT- scan of the patient. This is especially important since there was a considerable degree of sparing of vision in the affected field and it would be interesting to have an idea of the possible structures subserving such vision.

2) The description of the testing procedure is unclear and incomplete. What exactly is a two-interval forced choice procedure? Was the pattern presented on every trial (in such a case a correct detection would have depended upon the decision being made in a given time-window) or rather was intermingled with the presentation of the background only ? 3) The system used for eye movement monitoring might have been sufficient for detecting saccades but what about steady shifts of fixation ? 4) The position of the stimulus in reference to the spared portions of the hemianopic field should be clearly stated.

RESULTS- 1) Which statistical test was used ?. In the in toto analysis, which score was used ?

DISCUSSION- 1) The authors claim that stray light was controlled, however, even though the mean luminance of the display did not change, the local luminance of the pattern did obviously vary and such an effect may not be necessarily negligible given the low spatial frequency used.

2) Before one can conclude that the patient's performance is a genuine phenomenon, it would have been important to carry out some psychophysical assessment of the blindsight effect. For example, one could have increased the spatial frequency of the stimulus up to a thresold point. By the way, why just one, very low spatial frequency, was used ? As to the generality of the observed effect, the authors argue that some evidence quoted in the Introduction might be just a chance score (e.g., Marzi et al, 1986). This kind of reasoning obviously applies to their present single-case evidence too! It should be clearly stated in such a respect that one does not expect all hemianopic patients to show blindsight and therefore it is very unwise to consider the presence of blindsight only in a few subjects of a given population as chance scores.

2

0,5 : x 15

42 5

45

12,7 5

Comment on the paper by Magnussen & Mathiesen entitled "Human movement and pattern detection in the absence of striate cortex".

This is an interesting paper, and the experiments reported can be evaluated as an important contribution to the study of spared visual functions in perimetrically blind field regions after unilateral postgeniculate brain damage. The quality of the paper is such that it can be recommended for publication in NEUROPSYCHOLOGIA.

There are, however, some points which the authors should consider in order to improve the paper.

1. With respect to the fact that cases with "controlled" removal of parts of the occipital brain are very rare, and considering the fact that nowadays we have not to wait for structural verification until the patient dies because the brain can be mapped in vivo, CT or MR-images should be presented which show the whole extent of the removal, i.e. also in the vertical dimension. [illegible] [illegible]
2. On page 3, bottom, the authors refer to the literature on "blind-sight" and cite the work of Meienberg et al (14). These authors did not test "blindsight" in a straightforward way, i.e. they did not use any of the methodologies which have been found to "demonstrate" blindsight, and which seem, therefore, to represent an essential if not crucial prerequisite for testing "blindsight".
3. p. 4, top: Even though Campion et al. "questioned the control for processing by spared cortical tissue" the authors should either cite these "questioned" studies, or should point out that.the brain damage has been verified and presented in a number of other studies dealing with "blindsight".
4. The clinical presentation of the case is not very accurate. Visual functions are -except for the visual field- not even mentioned; it is not quoted which neuropsychological tests have been used, and the statement that "her general intellectual performance is well above average" is not very helpful even though nobodoy would doubt these statements. From the point of view of presentation of a single case, authors should rely more on empirical data than on mere description.
5. p. 6, top: Is "detection of moving patterns" synonymous with "move-ment detection"? Since the authors did not test patients' ability

to differentiate stationary and moving patterns, they should use the first term to prevent misunderstanding. This would also include the title.

6. p. 6, middle: Even though one would agree that a trained observer can detect small changes in fixation, there remain some doubts whether this could be done "reliably". It would be helpful if the oberver's reliability would be tested more directly, for example, to record the gaze shifts of the patient or even of a normal subject (in case the patient is no longer available), and to correlate the detection performance of the observer with the amplitude of the (voluntary) gaze shifts which can be calculated from the objective recording. Since the centre of the grating was 13.5 deg apart from the fovea, and the edge of the grating was thus at about 9 degrees eccentricity (is this correct?) it may not be that crucial but it should be done in order to improve the control (see discussion on artifacts!).
7. p. 8, top and middle: The statements that "B.N.'s blind-sight performance ... cannot possibly be based on information routed via the geniculo-striate pathway" and "the extent of B.N.'s lesion makes this possibility unlikely" again requires the presentation of empirical data on the exact location and extent brain damage (c.f. point 1).

Some minor points:

1. Authors should look for typing errors in the ms.
2. The visual plot in Fig. 1a is misleading in the sense that this type of a clear-cut hemianopia is very unusual, especially with respect to the complete correspondence with the 75-deg meridian in the upper and the 285-deg meridian in the lower half-field. Furthermore, the field extent in the upper periphery, especially along the 90-deg meridian, is usually not 60 deg. In this map, it seems much larger than 60 degrees because there is no "tendency" of the field border to finish! Could the authors check this again?

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Prof. ...
Laboratoire de Neuropsychologie Experimentale
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69500 Bron, France

Dear Prof. T ... d,

Enclosed are 4 copies of a revised manuscript "Detection of moving and stationary gratings in the absence of striate cortex" with T. Mathiesen, submitted to Neuropsychologia. I thank you for the comments on the first version (Dec. 3, 1987), and apologize for this ridiculously long delay in revision. Several factors contributed to this, my collaborator moved to another part of the country, and I have been enjoying a sabbatical in Lothar Spillmanns laboratory in beautiful Freiburg. Thus communications were less than optimal.

I thank the referees for their balanced comments and suggestions for improvement. In the revision I have tried to incorporate all their points: Further information on neuropsychological testing and ophthalmological examination are supplied. Figure 1 is redrawn to show visual field borders for each eye separately, and the size and location of the stimulus in relation to the border are indicated in an inset. The procedure is explained in some more detail, and the system for monitoring eye movements described — if requested, a figure can be supplied. Furthermore, I have changed the title as suggested, added a short comment on the choice of a low spatial frequency, the statistical test, dropped the reference to Marzi et al., and generally made conclusions a little more tentative than in the first version.

On one major point we have unfortunately not been able to supply the requested information: According to the neurologists contacted CT would in BNs case be of little valued bacause the silver clips residing after the operation is a strong source of artifacts and image distortion. BN herself is less available, and was reluctant to subject to CT. We have also tried to obtain a better description of the operation, but when the surgeon finally answered (after several months) he could not give us more information. I do agree with the referees that it would certainly increased the value of the paper to have this information, but hope it is not a major obstacle to publication.

One minor referee comment I do not fully understand: How can there be "local stray light" considering that the stimulus consists of 12 (on average) light and dark bars whose luminance change symmetrically in opposite directions?

Please convey my thanks and comments to the referees.

I look forward towards hearing from you. Please reply to my Oslo address — unfortunately my time in Freiburg is running out.

Sincerely

S. Magnussen

Svein Magnussen

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EKSEMPEL 7

Journal of Experimental Psychology:
Learning, Memory, and Cognition
1992, Vol. 18, No. 1, 151–156

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Retention and Disruption of Motion Information in Visual Short-Term Memory

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Department of Neurophysiology
University of Freiburg, Federal Republic of Germany

Velocity discrimination thresholds for drifting luminance gratings were measured as a function of the time interval between test and reference gratings, using a two-interval, forced-choice procedure. Discrimination thresholds, expressed as Weber fractions ($\Delta V/V$), were independent of interstimulus intervals (ISIs) ranging from 1–30 s, demonstrating perfect short-term retention of velocity information. When a third grating was briefly presented halfway through a 10-s ISI, memory masking was observed. Discrimination thresholds in memory masking were unaffected by maskers of the same velocity but increased by 100% when test and masker velocity differed by a factor of 2. The results are interpreted with reference to a model where the short-term memory for simple stimulus attributes is assumed to be organized in terms of arrays of memory stores linked in a lateral inhibitory network.

The processes of visual perception involve bottom-up analyses of incoming signals generated at successive levels of the visual pathways and top-down comparisons of this information to memory representations of previously observed stimuli. Psychophysical and neurophysiological approaches to vision have concentrated on the bottom-up aspect of perceptual processing and the coding of simple spatial and temporal

Dette er et interessant tilfelle. Selve temaet for denne undersøkelsen er midt i blinken for dette tidsskriftet, men artikkelen bryter med en teoretisk og empirisk publiseringstradisjon. To konsulenter stemmer for refusjon på dette grunnlaget, to stemmer for akseptering. Redaktøren er i tvil, men åpner for diskusjon. Forfatterenes argumentasjon blir vurdert av den samlede redaksjon, og artikkelen aksepteres. Dette er mønstergyldig redaksjonell behandling. Bemerk nok en gang forskjellene på konsulentuttalelsene.

1985; Vogels & Orban, 1986) and the spatial frequency of simple (Magnussen, Greenlee, Asplund, & Dyrnes, 1990; Regan, 1985) and complex gratings (Harvey, 1986). There is extensive neurophysiological and psychophysical evidence that spatial frequency (size) and orientation of visual stimuli are important features in early cortical processing, encoded by a set of stimulus-specific neural mechanisms—or channels—of the visual cortex (De Valois & De Valois, 1988; Olzak & Thomas, 1986). Similarly, neural mechanisms have been isolated that encode the direction and relative velocity of moving visual stimuli (Movshon, Adelson, Gizzi, & Newsome, 1986).

In the experiments of Regan (1985) and Magnussen et al. (1990), the short-term memory of spatial frequency information was assessed by the psychophysical determination of the discrimination threshold for suprathreshold luminance gratings, using a two-alternative forced-choice procedure in which the stimuli to be compared were separated by a variable interstimulus interval (ISI). The logic of this approach is that if the spatial resolution of the sensory image can be inferred from discrimination thresholds for simultaneously viewed

ual luminance gratings revealed a truly remarkable storage capacity of visual memory, demonstrating perfect retention of spatial frequency for ISIs in the range of 1 s to 30 s (Magnussen et al., 1990; Regan, 1985). Within this time span, at least, discriminations based on memory and simultaneous discriminations based on sensory information are identical, with thresholds down to 3–5% under optimal conditions. The delayed discriminations must rely on pure visual representations, as it would be impossible to encode the stimuli semantically in a fashion that would permit the discrimination of such minute differences in the spatial frequency of two gratings. The method therefore permits the visual component of short-term storage to be studied independent of higher order categorical processes.

Magnussen, Greenlee, Asplund, and Dyrnes (1991) further showed that a third grating displayed briefly during a 10-s ISI may interfere with delayed discrimination. We have called this *memory masking* because it operates well outside the temporal range of conventional sensory masking (Breitmeyer, 1984). Contrary to sensory masking, where the effects of masking are most pronounced when test and masker have the same frequency, memory masking is greatest when the spatial frequencies of the test and masking gratings differ by

This research was supported by the Norwegian Research Council for Science and the Humanities, the Alexander von-Humboldt Stiftung, and Deutsche Forschungsgemeinschaft, SFB 325, B4.

We thank J. Obergfell-Fuchs and Ina Fuchs for their careful observations.

Correspondence concerning this article should be addressed to Svein Magnussen, Vision Laboratory, Institute of Psychology, University of Oslo, Box 1094, Blindern, 0317 Oslo 3, Norway.

A

Comments on

Retention and disruption of motion information in visual short-term memory

by

Svein Magnussen and Mark W. Greenlee

I recommend that this paper be published in the *Journal of Experimental Psychology: Learning, Memory, and Cognition* following minor revisions. It makes an important contribution to the memory literature in two ways: it is a quantitative study of short-term visual memory, something that is relatively rare in the literature; and it relates the findings to the large body of electro-physiological and psychophysical knowledge therefore forming a bridge between these fields and the psychology of memory. There are a few minor quibbles that I have which I think should be incorporated into a revision.

At the top of page 5 the authors state "Obviously such delayed discriminations must rely on pure visual representations, as there is no means at the subject's disposal to conceptually encode such minute differences in the spatial frequency of two gratings." Although I personally think that this statement is true, the authors really have no direct evidence that it is true. They should soften the statement by removing the word "obviously" and just point out that they believe that it would be difficult, perhaps impossible, to encode the stimuli semantically in such a fashion as to permit discrimination.

I am not convinced by the authors' conclusion (page 12-13) that there is no unmasking for velocities far removed from the test velocity. Of the four individual data sets shown in Figures 3a and 3b three of them show a reduction of the masking at the extreme values. Only the "same" condition data of MWG do not show this effect. When the four sets of data are averaged, Figure 3c, the reduction in masking effect is washed out by this single set of data (or so it appears to me). The authors should apply a statistical test to the extreme data points to learn if they are significantly lower than the maxima in the curves. The maximum-likelihood method they have used to measure thresholds does allow the computation of a confidence interval for each estimate, so a statistical test would be possible.

The word masking is misspelled on the third line of page 14. Finally the ending paragraph of the paper needs to be strengthened. I don't recommend the introduction of a new concept ("visual-spatial scratch pad") in the second to last sentence without giving the reader any concrete idea what it means. The fact that it is in quotes indicates that it is jargon, but one that conveys little meaning to the reader. If they are going to reject this concept, they should develop it more fully first so the reader can understand what is being rejected. I hope that these comments are useful to the authors.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY: LEARNING, MEMORY, AND COGNITION

B

Manuscript Evaluation for Author(s)

uscript. No.: 91-023 Author(s): Magnussen, Greenlee

le: "Retention and DIsruption of Motion Information in Visual Short-Term Memory"

Re: MS #91-023

This paper adds original material to the literature on short term memory. The proposed interpretation of the "memory masking" experiment is intriguing. I recommend that the paper be published.

C

Comments on "Retention and disruption of motion information in visual short-term memory", by Magnussen and Greenlee

I should state at the outset that I am not an expert in the area addressed by the present manuscript. My comments reflect only the viewpoint of a minimally knowledgeable outsider.

This paper describes an experiment examining thresholds for discriminating the velocities of successively presented moving gratings. Manipulations included the velocity of the reference grating, the interval between presentation of reference and comparison gratings, and the presence/absence of a "masking" grating during the interval and -- if presented -- the relationship between the velocities of the reference and masking grating. Results indicated relatively high velocity discrimination thresholds, increasing at higher velocities, and interference for masking gratings that differed in velocity from the reference grating.

The findings reported here are of interest -- in particular, the finding of very accurate memory for velocity over relatively long ISIs. I think, though, that the contribution is a little thin, and that the paper may not be most appropriate for the audience of JEP: LMC. The "thinness" is at both empirical and theoretical levels. A single experiment is reported (with not too many observers or observations per cell, even for psychophysical standards), and some parametric extensions of the work could substantially increase the impact of the contribution. For example, further exploration of the question of *why* velocity discrimination thresholds are so much higher than thresholds for discriminating differences in spatial frequency and other stimulus properties seems called for. In addition, the finding that velocity and direction of motion apparently are processed separately seems quite
2 important: do other properties (like spatial frequency and orientation) also get treated independently of velocity?

With respect to the model advanced to account for the data, in the form presented in this paper it seems to be little more than a redescription of the basic results. That is, "lateral inhibition" is used to account for performance decrements (presumbably, if interference selective to the velocity of the reference grating had been observed, then a more specific form of inhibition would have been postulated). This model may well be spelled out in a more detailed, quantitative fashion by the investigators cited as proposing it; however, for purposes of the present discussion, it comes off as vague and qualitative. Thus, any reworking of this paper should include a more detailed account of the model and its applicability to the present situation.

In summary, I do not wish to be unduly negative about this paper. I do feel that the findings are interesting, but I don't really see quite where they fit into our knowledge about visual memory processes and representations. As I stated at the outset, this paper might well be more appropriate for <u>JEP: Human Perception and Performance</u> or some other journal oriented toward visual scientists. The findings might have more impact on researchers in the field of motion perception, and such individuals could evaluate this paper with more expertise than the present reviewer can.

(D)

Comments on 91-023

This paper deals with subjects' ability to discriminate the velocities of successively presented moving gratings. The velocity of the reference grating, the interval between the reference and comparison grating, the presence of a "masking" grating, and the velocity of the reference and masking grating (when present) were all manipulated in the experiment. It was found that subjects had relatively high velocity discrimination thresholds (which increased at higher velocities) and that there was interference for masking gratings that differed in velocity from the reference grating.

My feeling is that the work reported in this paper was competently executed and that the paper is well-written. However, I guess I have a serious concern about whether or not the JEP:LMC is the right outlet for this work. On the one hand, the work does relate to short-term memory performance, which is certainly with the purview of the journal. On the other hand, the methodology employed in the paper is more typical of studies published in psychophysically oriented journals. I can't recall seeing papers in LMC that had only three or four subjects; further, there are virtually no statistical analyses reported in the paper and typical readers of this journal may not appreciate the methodology. Finally, most articles in the journal usually consist of a whole series of experiments or at least two experiments.

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2.5 1991

Dear Prof.: [illegible]

Thank you for your detailed comments on MS #91-023 "Retention and disruption of motion information in visual short-term memory." I can see your dilemma concerning the paper and I half-way expected comments in that direction.

I have no trouble with the comments of reviewer A, whose requests are easily incorporated into a revised version of the MS. However, the objections of reviewers C and D are more fundamental, reflecting a difference between the psychophysical research strategy of performing detailed measurements on a few subjects, and the more conventional memory research reporting group differences. The question of which strategy to choose will, in my opinion, depend on the "purity" and precision of the experiment, and how fundamental the underlying mechanisms are.

In our experiments on delayed velocity discrimination we use high-fidelity psychphysical techniques and computational procedures (Best-PEST) to measure delayed discrimination thresholds, each data point for single subjects represents 120-200 trials. This is an amount of data well above average for the typical psychophysical experiment (cf. reviewer C) and we report results for three subjects. The necessary statistical tests (ANOVA) for the masking experiment was performed (p. 12 in the MS), and standard errors of grand means are indicated in the figure. If necessary, error bars could be added to Fig. 1 too. If other tests are required, they can easily be run, but I am uncertain about which tests to perform and what they would tell.

In a recently published study, cited in the present paper, we reported similar experiments on the short-term memory of spatial frequency information (Europ. J. Cogn. Psychol. 1990). Since that was a first demonstration of this type of short-term memory, we tested a total of 40 subjects on the critical conditions of 1 and 10 (15) seconds, and the group means were similar to the detailed measurements of individual subjects. In an "in press" paper in Vision Research (also cited) we report analogous memory masking results for spatial frequency. In view of our data on spatial frequency memory, I feel quite confident about the results of the parallell experiments on motion with new subjects. Since a methodological point of the paper is the advantage of precise psychophysics, I hesitate to comply with the reviewers request for testing more subjects in these quite time-consuming experiments. There is very little to learn from testing more subjects, all the subjects we have tested so far in the various memory experiments behave as replicates of each other provided a sufficient number of trials are run. In addition, we are currently expanding our memory research along the dimensions of both time and complexity, combining memory for separate attributes in a single trial, and these experiments will keep us quite busy for the next few months. For your information, copies of the memory papers published so far are enclosed.

Two minor replies to reviewer C: First, discrimination thresholds expressed as Weber fractions vary enormously between stimulus dimensions and sensory modalities. The difference between velocity and spatial frequency is well established (as documented by citations) and not specific to the present experiments, and a discussion of that point is outside the scope of the paper. Second, I agree that the results do not fit in very well with the present knowledge about visual memory and visual representation. This, in a sense, is the point of the paper. We present a fresh approach.

In conclusion, I find it hard to see why experimental strategies which have proven quite successful in vision and visual processing cannot be applied to the study of the fundamental mechanisms of visual memory (after all, Ebbinghaus reported single-subject data). On the other hand, I appreciate your concerns about the paper on grounds of journal policy and readership. So, unless a revised version based on the current set of data is publishable, I feel that it would be most appropriate to withdraw the paper.

I am looking forward to hear from you.

Sincerely

Svein Magnussen

Enclosures: copies of two papers

Journal of Experimental Psychology: Learning, Memory, and Cognition

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May 15, 1991

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Dear Dr. Magnussen:

Thanks for your letter of May 2nd. After I reading it over, I decided to check with my Associate Editors to see what their recommendation might be. I did this because, in some ways, they are more representative of the field of memory research. I have reported studies in JEP:HP&P with very few subjects, but with lots of data on each subject, whereas they all typically run lots of subjects in their studies. Somewhat surprisingly to me, they all came back with the same recommendation. Namely, they indicated that if the methodology was sound (which I believe it is) and you understandd that your paper might not receive as much attention in the journal as it might get elsewhere, that we should publish it.

Therefore, if you make the revisions that are called for by the reviewers, I am prepared to publish the paper. If you are agreeable, please send three copies of the revision to me.

Sincerely,

Keith Rayner

Journal of Experimental Psychology:
Human Perception and Performance
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Competition and Sharing of Processing Resources in Visual Discrimination

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University of Oslo

Mark W. Greenlee
University of Freiburg

Discrimination thresholds for spatial frequency and contrast tested individually were compared with dual discrimination of contrast and spatial frequency, and dual discrimination of 2 contrast or spatial frequency components. The components were presented overlapping, forming a compound grating or as side-by-side simple gratings. When observers had to judge contrast and spatial frequency simultaneously, discrimination thresholds increased by an amount predicted by a model of stimulus uncertainty for orthogonal dimensions (1.7); when they had to judge 2 frequency or contrast components, discrimination thresholds increased by a factor of 3–6 compared with the single-judgment task. The relative spatial location of the components did not interact with task complexity. The results are consistent with a model assuming a set of parallel special-purpose attentional mechanisms.

Current theories of attention distinguish between large-capacity, parallel-processing mechanisms and limited-capacity, serial-processing mechanisms (Johnston & Dark, 1986; Neisser, 1967; Posner & Petersen, 1990). In vision, it is believed that elementary features such as color, orientation, size, and movement are coded in parallel at a preatten-

ence is observed (Allport, 1971; Chua, 1990; Stefurak & Boynton, 1986; Wing & Allport, 1972). A recent study by Vincent and Regan (1995) showed that independence applies even when the observer has to report three dimensions simultaneously, in their study contrast, orientation, and spatial frequency.

Denne artikkelen ble akseptert etter fjerde vurderingsrunde. Ved første forsøk var samtlige konsulenter positive, og fremhever spesielt at eksperimentene er kritiske for flere moderne teorier om oppmerksomhet (Spesielt John Duncans formuleringer), men vil ha noen oppklaringer. Det er imidlertid skjult bombe i kommentaren fra reviewer A. Men siden kommentaren var noe perifer, tok vi den bare opp i følgebrevet i revisjonen. Samme reviewer aksepterer de fleste forandringer, men forsterker det samme punktet. I tredje versjon siterte vi konkrete data som viste at konsulentens innvendig ikke er valid, det er dessuten et marginalt poeng ved artikkelen. Men konsulenten har bestemt seg for å blokkere publisering og vil ikke akseptere dokumentasjonen. På dette tidspunkt skjønner vi også hvem konsulenten er og hvorfor han er negativ, redaktøren forstår også at dette bærer galt av sted og gir flere åpninger for at alle kan redde ansikt. Vi valgte å gjøre et lite, men unødvendig tilleggseksperiment, nevnt på en linje i resultatseksjonen, og artikkelen ble akseptert.

tested alone but that when the observer has to identify two shapes, two spatial frequencies, or two orientations, interfer-

which of two successively presented stimuli had the higher value; in the dual-judgment task, either dimension could vary, the observer had to attend to both dimensions and decide, first, which dimension changed and, second, which interval had the higher value. The results confirmed that discrimination thresholds were higher in the divided-attention condition, but for both contrast and spatial frequency the thresholds did not exceed the value predicted by a model based on statistical decision theory for independent sources. In other words, when the effects of stimulus uncertainty were discounted, the discrimination thresholds were comparable in single- and dual-judgment tasks, indicating parallel processing of the dimensions of contrast and spatial frequency. These findings recently were confirmed and extended (Magnussen, Greenlee, & Thomas, 1996).

Svein Magnussen, Vision Laboratory, Department of Psychology, University of Oslo, Oslo, Norway; Mark W. Greenlee, Department of Neurology, University of Freiburg, Freiburg, Germany.

This research was supported by Alexander von-Humboldt Stiftung and the Norwegian Research Council. We thank J. P. Thomas for advice in the application of the model.

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Journal of Experimental Psychology: Human Perception and Performance

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August 1, 1995

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re: ms 95-75 by Magnussen & Greenlee

Dear Dr. Magnussen:

Enclosed are three reviews of your paper with Greenlee, "Competition and sharing of processing resources in visual discrimination". All are from experts on the issues you address. Reviewers B and C are members of the Journal's editorial board.

The message conveyed by these reviews is clear and consistent. All the reviewers believe the work is potentially very important, but none believes the paper is quite ready to publish. Three major questions arise. One concerns the model of stimulus uncertainty effects that provides a baseline against which to assess resource limitations. At present, the model is assumed and its details omitted in lieu of a reference to another paper. The reviewers would like the details spelled out at least enough to understand the model, and Reviewer B would like to see your particular model situated with respect to other approaches and defended where it differs from other approaches in ways that would alter the baseline estimates it provides for your analyses. A second question concerns the discrepancy between your results and the widely cited findings of Duncan and others in which objects seem to be the basis for allocation of resources rather than dimensional analyzers. All the reviewers want to see a compelling discussion of this discrepancy. The third question is related to the second. Reviewer A suggests that your arguments hinge on being confident that your task arrangement forces simultaneous or concurrent processing of multiple dimensions and multiple objects. Because of the properties of the sequential trial line you employed in your two-interval forced choice procedure, the reviewer is not convinced that this is true. The reviewer would like to see this methodological point discussed, and suggests that adding an experiment that more clearly forces subjects to deal with all stimulus information concurrently would strengthen your argument.

Given these reactions, I don't think I can accept the paper at this point in its development, but I would be pleased if you would be willing to undertake a revision that deals effectively with the three questions described above. If so, please send four copies of the new version of the paper to East Lansing whenever it is completed to your satisfaction. I'll send it to one or two of the present reviewers, or perhaps to one of the present reviewers and one new reviewer, depending on the reviewers' availability at the time the revision arrives and the extent and nature of the changes that are made. Whether the revision should include new data in response to Reviewer A is a decision I'll leave up to you at this point. In the meantime, I'd like to thank you for sending this very interesting work to JEPHPP. I look forward to seeing the revision.

Sincerely,

Editor

A

Review of ms 95-75 (Magnussen and Greenlee)

The results of a series of discrimination experiments were interpreted as showing that visual attention can be divided without cost between two spatially separated objects or two attributes of a single object provided the two discriminations are made along different dimensions (in this case, spatial frequency and contrast). Increases in discrimination threshold observed between single dimension and dual dimension discriminations under these conditions were viewed as within the bounds of what would be expected by a stimulus uncertainty model. However, when two discriminations were required along two spatial frequency or two contrast dimensions, regardless of whether they are embodied within a single stimulus or two spatially separated stimuli, observed increases in discrimination threshold, relative to single discrimination cases, are consistently greater than would be expected from the uncertainty model. This result was interpretated as implying an attentional limitation attributable to competition between the two discrimination requirements for common, dimension-specific, limited capacity processing resources. This is a potentially important set of results because it appears inconsistent with several current and popular models of visual-spatial attention, including spotlight, zoom lens, and object-based models.

A critical assumption underlying the authors' interpretation of the results is that their experimental conditions force the simultaneous processing of multiple stimuli or stimulus attributes, thereby requiring the splitting of attention. This may not be the case: stimuli to be compared were presented in succession, the first for 100 ms, the second for 100 ms following a 1 or 3 sec ISI. Stimuli were not masked. For these reasons, it is possible that on some unknown portion of the experimental trials Ss could attend to the two attributes/stimuli in succession. If so, the experimental results cannot be used to argue effectively against models predicting performance decrements when attention is (simultaneously) spread between two spatially separated stimuli or between two attributes of the same stimulus. It should be noted however that the account I have described does not explain why single-to-dual discrimination performance decrements should be so much larger under "same" versus "different" dimension discrimination conditions. However, the authors' account readily accomodates this pattern of results. Nonetheless, the conclusions would be much more compelling if the present results could be obtained under conditions more likely to force the splitting of attention across spatial locations/stimulus attributes. This seems particularly important given the number and influence of the attentional models potentially challenged by the findings, and given the seeming inconsistency between these results and some of those reported elsewhere (e.g., Duncan).

Several more specific comments:

[illegible] interpretation of the findings relies heavily on a stimulus uncertainty model [illegible]ced but not described in the ms. This model should be characterized in greater [illegible] the ms so the reader doesn't have to consult the Greenlee & Thomas (1993) [illegible] understand it. The model is attractive, and deserves space here.

2. Insufficient detail is provided of the conditions of stimulation. I think I may have figured out what could vary between stimuli or stimulus components during each interval within a trial under each experimental condition, but I'm not sure. This problem could be handled in part be including a more useful legend for Figure 1.

3. The ms contains a number of typos throughout.

4. In general, the ms is well-written. An exception is the first sentence in the abstract, which is too long and complex. And, as indicated above, more attention should be given to explaining stimulus and experimental conditions.

-

B

MS #95-75 by Magnussen and Greenlee

This paper investigates the nature of attentional limitations in processing separate dimensions of one or two objects in the visual field. The current view in the field of attention is that separate dimensions such as color and shape can be processed in parallel from a single object but that interference occurs when attention must be divided across separate objects. The authors report results which contradict this view.

The experiments involve presenting a single grating at fixation or two gratings side by side. In single task conditions, the subject is asked to make judgments about a single dimension such as contrast. In dual task conditions, both contrast and spatial frequency dimensions require discrimination. These dimensions may belong to a single object or two objects. Duncan's theory would predict dual task and single task performances to be equivalent only in the same object condition. The authors report that this result holds for both the single and dual object conditions.

The reason for the discrepancy with Duncan's results are not clear and the authors don't really address this issue. Duncan is not the only one to report this kind of result. Farah, Egly, Driver, and others have reported similar findings. In addition, neur
opsychological data reported by Humphrey, Driver and others is also consistent with these results. Overall, the literature review of the object results in inadequate and the authors need to provide some argument as to why they have found results different
than those of many other labs. There may be a couple of possibilities. First, the present experiment uses a Pest like threshold adjustment procedure. Is this as sensitive to small differences as using a forced choice accuracy measure.

Second, the authors use a normative model to evaluate statistical effects of dual task conditions. Dual-task discrimination must be worse than that
predicted by the model to qualify as an attentional effect. Again, there is an extensive literature on these models (e.g.. Shaw, Eriksen and Spencer, Kinchla, Lappin, etc.) and this literature needs to be reviewed. In any case, the model used by the authors is not presented in this paper so it is difficult to evaluate. They should provide enough details for this paper to stand alone. There are many possible models that can be used to provide an estimate of the expected dual-task decrement. These models may differ in the form of the distributions used for signal and noise (gaussian, double exponential, etc.) as well as the decision rule used to combine information across channels (independent decisions, weighted

integration, etc.). Therefore the predicted statistical effect of increasing the number of channels will depend on the particular model. The failure to find an attentional effect in the present experiment may depend on the particular model employed. Shaw provides an upper bound for these effects that is independent of these assumptions. In the present experiment, these difficulties could be surmounted by simply requiring the subject to make separate judgments about each dimension (e.g.. contrast and frequency). It isn't clear to me why the subjects in the present experiment first have to decide which dimension changed and then the direction of change. It is this aspect of the procedure which requires model based estimates and may lead to a lack of power in detecting attentional effects.

Review of Magnussen & Greenlee, "Competition and sharing of processing resources in visual discrimination" (ms. 95-75)

This paper describes several experiments in which discrimination thresholds for making two simultaneous perceptual judgements are measured. At issue is whether and when such judgements interfere with one another.

According to one view, the visual system consists of a set of independent modules, each specialized for dealing with a single visual attribute (e.g., spatial frequency or orientation). Simultaneous judgements about two different attributes can be made without interference (decrements in performance for simultaneous judgements can be attributed to statistical uncertainty effects), but two simultaneous judgements about a single attribute will cause interference and true performance decrements. According to this view, whether the attributes to be judged are spatially contiguous (as they would be if they were attributes of a single object) or not is irrelevant; what matters is whether the two attributes to be judged are the in the same domain (e.g., spatial frequency or orientation) or not.

A competing view (e.g., Duncan, 1993) holds that two judgements made about attributes of a single object will never interfere, but two judgements about two different objects will yield performance decrements, and this holds whether the two attributes are in the same dimension or different dimensions.

I found the paper rather difficult to follow and it took quite a bit of effort on my part to pull out the description above (and if the authors do not find it to be a satisfactory account of what they are saying, then the paper needs still more clarification). The data are, however, convincing in supporting the first view described above.

The least satisfactory aspect of the paper, aside from the difficulty I had in extracting the theoretical issues at stake, is that the authors do not offer any explanation for the different results reported by Duncan (1993). The two studies come to diametrically opposite conclusions, and the data in both cases seems strong. The authors would do well to offer an explanation to readers.

Although the comments above are somewhat critical, let me say that the issues addressed by this paper are extremely important, even foundational, and the empirical results are
r. I think this paper is likely to make an important
ontribution after suitable revisions are made.

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Deres ref. MS 95-75
Vår ref.

Dato: 31.10 1995

Dear Dr.

Thank you for letter of August 1 and the positive reviews of our MS "Competition and sharing of processing resources in visual discrimination". Four copies of the revised MS and a new set of figures are enclosed.

In the revision we have followed the reviewers recommendations. First, a description of the uncertainty model proposed by Mark Greenlee and Jim Thomas is included as an Appendix to the paper (all reviewers). The description could alternatively be inserted after the Methods section. Second, the discrepancies between the present results and those of Duncan (1993) are discussed at some length on p. 19-20 of the MS (reviewers B and C). Third, the legend to fig. 1 have been greatly expanded to explain the stimulus conditions of the various experiments (reviewer A). Fourth, a reference to the recent studies by Vecera & Farah and Egly et al. are included in the introduction (reviewer B); however, none of these are designed to answer the questions raised in our paper, they just show that objects and locations may both guide attention and this is no surprise if attention is modular. Fifth, we have corrected a number og typos (sorry!).

Two points raised by the reviewers appear to be based on a misapprehension: We have a much stronger version of the two-alternative forced choice method than employed by studies using fixed values because in our experiments the values are selected individually by the computer based on previous rights and wrongs; it is not an adjustment method (reviewer B). Also there is no possibility that observers can attend to the two components or dimensions in succession because they are presented as part of the same stimulus or as twin gratings presented simultaneously for less than 200 msec (reviewer A), what is presented in succession are the stimuli to be compared. This point is perhaps made clearer by the new legend to Fig. 1.

I hope the revisions have answered the reviewers' questions and clarified dubious points, and look forward to hear from you.

Sincerely

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Journal of Experimental Psychology: Human Perception and Performance

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December 13, 1995

re: ms r95-75 by Magnussen & Greenlee

Dear Dr. Magnussen:

Enclosed are two reviews of your revised paper with Greenlee, "Competition and sharing of processing resources in visual discrimination". Reviewer A is a member of the Journal's editorial board and is new to the paper. Reviewer B saw the original.

Reviewer A finds the paper interesting (as did the reviewers of the original). This reviewer thinks that you've made the case for modularized, parallel processing of frequency and contrast, but that you may not have made the case against object-defined allocation of attention. The reviewer asks for independent evidence of some kind that perceives really do parse the overlapping stimulus displays one object rather than two. This may be a tall order, but the reviewer's point is well taken, I think.

Reviewer B recounts the three criticisms this reviewer made of the original, and says that two of them have been overcome. But the reviewer believes the most important one remains -- the possibility that even given the relatively brief exposure durations, attention could have been allocated sequentially to the two features in the target display rather than simultaneously. The argument you offer against this possibility is an assertion that 200 ms is too brief a time for sequential attentional allocation, but you offer no theoretical or empirical evidence in support of this assertion. The reviewer can't generate an obvious defense of it, and is uncompelled. Note that unless this criticism can be fended off, it casts doubt on the point that Reviewer A accepted. Hence, between the two reviewers, both major points of the manuscript have met with skepticism.

Where does this leave us? I expect this will be frustrating, but again I don't think I can accept the paper at this point in its development. Again, however, I would be pleased if you would be willing to undertake a revision. I will be inclined to accept the paper if you can deal in an effective way with Reviewer B's criticism, which seems to me to be the more fundamental issue given the arguments you wish to make. If you cannot prove whether perceivers parsed the overlapping displays as one object or two, then discussion acknowledging this would probably be sufficient, as long as the basic argument for modularity still goes through.

If you are willing to try one more time, then please send four copies of the new version of the paper to East Lansing whenever it is ready. I'll give it to these same two reviewers for their opinions. In the meantime, I'd like to thank you for returning this paper to JEPHPP. I'm sorry the outcome was not more positive, but I look forward to the possibility of seeing another revision.

Sincerely,

A

Magnussen & Greenlee. JEP:HPP r95-75

This is an interesting paper. It nicely confirms the observation that processing of stimuli in different modules (Spatial frequency and contrast) can take place in parallel, whereas processing within a module (frequency-frequency or contrast-contrast decisions) is impaired.

The more contentious claim is that processing of different properties within an object is not more efficient than processing properties from different objects (e.g., Duncan). The most relevant data to this issue is the within domain decision (frequency-frequency and contrast-contrast) as this is where attentional resources seem to be most limited. In these studies the between object condition is where two gratings are presented side by side (Figure 1B) and the within object is where two gratings are presented superimposed (Figure 1C). Performance appears to be equivalent in these two situations, whereas the object-based model of Duncan predicts better performance in the superimposed within object situation.

What is not clearly specified in these experiments is what an object is. It is assumed that when the two gratings are superimposed they are encoded as one object. However, this is a crucial and fundamental assumption. There is plenty of evidence (e.g., Duncan; Rock & Gutman, 1981) that two separate objects can be perceived even when they are superimposed. Thus it is not clear in the current studies whether the two gratings in Figure 1C are perceived as one coherent object containing two spatial frequencies, or as two separate objects. Indeed it may be the case that as subjects are required to make separate decisions about each spatial frequency, they parse this scene as containing two objects. If this were the case, then Duncan would predict no difference between superimposed and separate objects.

Therefore I feel that the authors should consider this possibility carefully. They need to unequivocally demonstrate that the two superimposed gratings are indeed perceived as one coherent object containing multiple spatial frequencies.

= =
= =

B

Review of ms r95-75 (Magnussen and Greenlee)

I criticised the original version of this ms on three grounds: (1) that the conditions of stimulation were such that successive processing of simultaneously presented objects/dimensions could not be precluded; (2) that the Greenlee and Thomas model used in interpretation of the present data was not presented in sufficient detail; and (3) that the conditions of stimulation were not presented in sufficient detail.

The revised ms adequately addresses the second two concerns. However neither the ms nor the cover letter adequately addresses the first concern, which is the most important of the three. Let me repeat my criticism. A critical assumption underlying the interpretation given by the authors is that the experimental conditions forced Ss to simultaneously process multiple stimuli or stimulus attributes, thereby forcing the splitting of attention. I am not persuaded this was the case. Stimuli were presented for 100 ms and were not masked. Because of the absense of masking, the effective duration of useable information from the stimuli and their residua was doubtless greater than 100 ms. In their cover letter to the revision, the authors argue that since stimuli were presented for less than 200 ms, there is no possibility Ss could sequentally attend two components in succession. It's not clear why the authors believe that the components of multiple component stimuli less than 200 ms in duration cannot be sequentially attended. Perhaps they mean the stimulus components could not be sequentially fixated, which is probably true. But we know attention can be shifted much faster across visual space than can the point of fixation. And given useful stimulus information likely could be extracted from the stimulus residual beyond its 100ms display duration, the likelihood of sequentially attending stimulus components on at least a significant portion of trials seems high. If this were the case, their interpretation is invalidated.

For this reason, and because the results as interpreted by the authors run contrary to findings in the literature, I again recommend against publication of the ms in its present form. My criticism can be easily evaluated using stimulus conditions that preclude sequential attentional analysis (masking and somewhat shorter durations). This should be done before this ms is accepted.

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Deres ref. MS 95-75
Vår ref.

Dato: 28.1 1996

Dear Dr.

Thank you for letter, reviews and E-mail correspondence concerning the revised MS "Competition and sharing of processing resources in visual discrimination". Four copies of a second revision are enclosed.

The reviewers' comments relating to theories of spatial attention have forced us to look at the MS with critical eyes. Our previous rather summarily dismissal of space and object based theories was based on the pattern of results rather than single conditions, and was not intended as the main contribution of this paper, which is the modularity of contrast and spatial frequency in perceptual discrimination.

In the revision we have reorganized the Discussion to show this priority: we first discuss modularity and then add a separate section on "Implications for theories of spatial attention". In this section we discuss the data from the various conditions in more detail, dealing both with the question of whether complex gratings can be considered one or two objects, and the possibility of attention shifts. In the latter context we now cite results from experimental estimates of the time course of spatial attention shifts (Müller, Findlay, Tsal, Stoffer) which all indicate durations in the order of 200-400 msec for the shift process alone under conditions where the initial engagement is just defined by fixation. If we add the task of encoding very fine differences in contrast and spatial frequency (the critical condition here) there is not sufficient time, particularly remembering that dual encoding and shift process must be perfect on the vast majority of trials under this condition (which is required since the data conform to the uncertainty prediction indicating independence and no load on attention). Never the less, we have tuned this part of the discussion down, making less strong claims regarding these theories.

In addition we have omittesd the last sentence in the Abstract and changed the wording slightly in the Conclusions.

We hope the revised version answers the reviewer's questions. We believe the paper has profited greatly from the comments and have added a statement in the Acknowledgments to that point. Since you kept the original figures, we have not included a second set.

Sincerely

Svein Magnussen

Journal of Experimental Psychology: Human Perception and Performance

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Dr. Svein Magnussen
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Institute of Psychology
University of Oslo
Box 1094
N-0137 Oslo
NORWAY

March 21, 1996

re: ms 2r95-75 by Magnussen & Greenlee

Dear Dr. Magnussen:

Enclosed are two reviews of your again-revised paper with Greenlee, "Competition and sharing of processing resources in visual discrimination". They are from the same two reviewers who saw the previous version. One is now quite happy. The other remains unconvinced because of the possibility of sequential processing or attention shifting. This reviewer is inconvinced by the evidence you cite against this possibility, countering that both the Muller and Tsal experiments involved the need to interpret an informative cue before the diretion of an attention shift could be determined. This would appear to mean that the estimates of 200-400 ms to achieve an attention shift that they obtained are quite a bit longer than your subjects would probably need. How much longer is the critical question. Where else could one reasonably look to get estimates of preprogrammed or stimulus-driven attention shifts that might be more comparable to what your subjects could be doing? I'm not sure. One place might be the visual search literature, where both slopes of reaction time functions and estimates from threshold setting procedures like those of Zacks & Zacks (JEPHPP 1993 plus or minus) suggest that if attention is distributed serially in conjunction search conditions, it often moves from item to item much more rapidly than once every 200-400 ms. Maybe there are other places that a creative thinker might come up with. For the moment, however, it seems to me that the reviewer's concerns remain legitimate ones.

What can we do? There are three options. One is to add another experiment of the type that both reviewers mention, with either shorter presentation times, post-stimulus masking, or both. The second is to describe the results of your additional experimentation (the results you mentioned in your email of January 9), but not report them in full. This option, of course, depends on those results being available in the literature so that interested readers can easily find them. The third is to find just the right situation already in the literature in which attention switching time has been estimated, discover that the estimated time is still too long for your subjects to have switched attention, and add that citation to the discussion in your paper. At present, I leave this choice up to you. But unless one or another of these options is followed to remove the remaining doubts about attention switching, I'm not sure that I can accept the paper. This is a hard choice for me. The paper is very good, and it will stimulate interest in its readers, and skeptics can always carry out followups themselves if they don't believe your account. The results SHOULD be published -- somewhere. The problem, though, is that perhaps they should not be published in this particular journal unless this remaining doubt can be eliminated in a more powerful way.

Please let me know what you want to do. If you choose to pursue publication in JEPHPP, I'll be very happy to consider one more revision that does one or more of the three things just described. Please send three copies of such a revision to East Lansing whenever it is completed to your satisfaction, along with a cover letter describing what you've done. I'll read the letter and the paper and try to make a final decision without further external review if I can.

Sincerely,

Editor

A

Review of ms# 2r95-75 (Magnussen and Greenlee)

One of my original criticisms of this work was that the conditions of stimulation were such that successive processing of simultaneously presented objects/dimensions could not be precluded. If successive processing of objects/dimensions did occur, their conclusions about the cost-free splitting of attention across spatial locations are rendered invalid. To be more specific, I argued that given stimuli were presented for 100 ms and were not masked, it is possible Ss could shift attention between locations and extract task relevant information prior to disappearance of the stimuli and their iconic residua. In their current revision, the authors argue against this possibility based on data by Muller and his colleagues and by Tsal that indicate it takes 200-400 ms for voluntary spatial shifts of attention in detection and identification tasks. The problem with this argument stems from the fact that the studies cited measured attention shifting time under conditions in which the direction in which attention had to be shifted was uncertain for Ss until they were provided an informative cue: they had to interpret the cue (an obviously time-consuming process), then move their attention in the direction indicated. However in the Magnussen and Greenlee study, no uncertainty existed regarding the locations of the two stimuli to be interrogated, and Ss may very well have been able to pre-program the attentional mechanism to serially fixate on the two known stimulus locations. Since the Magnussen and Greenlee runs contrary to findings in the literature, their arguments relative to this possibility need more substance than is provided in this ms in order to convince me publication is warranted. I recommend once again that they carry out an additional study using stimulus conditions that preclude sequential attentional analysis of simultaneous stimuli (use shorter durations and mask).

B

Magnussen and Greenlee. JEP:HPP, 2r95-75

The revised version of this paper adequately answers the criticisms of the previous version. First, the authors now acknowledge that their data do not provide unequivocal evidence for of against object-based accounts. On page 22 they are appropriately cautious concerning this issue. The second concern was that after 100 ms mask free stimulus presentations there may be time for attention switches. This is a critical issue for their arguments of parallel encoding. On page 20 they now discuss evidence that supports their suggestion that attention could not be disengaged, moved and engaged in that time. However, on this latter point, I think the article would be stronger if they investigated the stimulus presentation boundaries. That is, can the same results be observed at 50 ms exposure, what about 25 ms etc.? This latter experiment would make their claims for parallel processing much stronger, but the experiment is not absolutely critical for publication.

Eksempel 9

MEMORY, 2003, *11* (3), 319–327

Memory for a staged criminal event witnessed live and on video

Cecilie Ihlebæk, Tonja Løve, Dag Erik Eilertsen and Svein Magnussen
University of Oslo, Norway

Memory for a staged robbery was tested in two groups of participants witnessing the event either live (n = 62) or on video (n = 64). Immediately after the event participants filled out a questionnaire probing memory with emphasis on the timing of the event and robber characteristics. The results showed that participants who watched a video recording of the event reported more details and with a higher accuracy ... participants who were present on the scene, but the pattern of memory errors were similar in the two ... concluded that laboratory experiments may overestimate the memory of eyewitnesses but ... essential aspects of memory performance in naturalistic contexts.

Denne artikkelen ble opprinnelig sendt til *Journal of Applied Psychology*, som etter lang ventetid returnerte den med en negativ kommentar fra en reviewer som åpenbart ikke hadde lest artikkelen skikkelig – den er totalt misforstått. Redaktøren henger seg på uttalelsen. Vi svarer, og redaktøren skjønner at dette er nok ikke helt bra, men kan ikke innrømme at han heller ikke har lest artikkelen, og åpner i stedet for at vi reviderer. Vi reviderte, men fikk samme type uforståelig svar i neste omgang. Manuskriptet ble deretter sendt til en mer basalt orientert tidsskrift, *Memory*, som akseptere den.

in real-... laboratory make it p... memory performances of labo... not representative of those of witnesses ... crimes (McKenna, Treadway, & McClosky, 1992; Tollestrup, Turtle, & Yuille, 1994; Yuille, 1993; Yuille & Wells, 1991).

In memory research the pros and cons of laboratory versus naturalistic studies of memory form a theme of continuous debate (e.g., Banaji & Crowder, 1989; Ceci & Bronfenbrenner, 1991; Conway, 1991; Koriat & Goldsmith, 1994, 1996; Loftus, 1991; Neisser, 1978). In the context of eyewitness research, laboratory experiments and

... ensic contexts differ along three dimen- ... may challenge the ecological ... nts: First, many ... h are

... less ... different geo... the action and thus ... tunity to observe; they may com... happening at different times and thus ... and remember the event through different time windows. In violent crimes such as an armed robbery witnesses may be forced to interact with the perpetrator(s) or be ordered down on the

Requests for reprints should be sent to Svein Magnussen, Department of Psychology, University of Oslo, Box 1094 Blindern, 0317 Oslo, Norway. Email: svein.magnussen@psykologi.uio.no

We thank P. Eggum of Securitas for giving us access to the training courses, and the management of the "robbed" companies for permission to collect data. Comments from two reviewers contributed significantly the final version of the paper.

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Journal of Applied Psychology

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November 8, 2000

Dr. Svein Magnussen
Department of Psychology
University of Oslo
Box 1094
Blindern, 0317
Oslo, Norway

Dear Dr. Magnussen:

I apologize for the delay in completing my review of your paper "Witness to Robbery: A comparison of Memory in Realistic Field Settings and in the Laboratory" (No. 00-258), authored with C. Ihlebaek, T. Loveand D. Eilertsen. I had originally requested input from two reviewers. One was unable to evaluate this paper, and this reviewer's replacement was also unable to provide a timely review. Based on my evaluation of the issues raised by Reviewer B, I thought i was not in your best interest to delay this review further by seeking additional input. The concerns raised by this reviewer are, in my view, both legitimate and fundamental. I am very sorry, but I cannot accept this paper for publication in Journal of Applied Psychology.

The enclosed review raises a number of points, three of which strike me as most important. First, I agree with the reviewer that comparison between participants' reports and observers' reports are extremely difficult to interpret. The video camera necessarily "sees" different things than a human observer sees. The number of details, the type of details, the length of time devoted to each detail, etc. are all virtually certain to be different when viewing a videotape than when serving as a participant observer, and it is virtually impossible to sort out the roles of attention, information acquisition, and memory here. Second, the reviewer is right in raising questions about ecological validity. If you had achieved ecological validity, it is almost certain that the arousal levels would be considerably higher for participants than for observers. Finally, the reviewer raises questions about your interpretation of data bearing on the arousal -accuracy relationship.

I am sorry I cannot be more positive regarding this manuscript. The paper was interesting in a number of ways, but the issues raised above are not the kind that are readily addressed in a revision, and I will not encourage one. I hope, however, that you will find the enclosed reviews useful in designing future research. I also hope you will continue to consider JAP as a potential outlet for your work.

Sincerely,

Use additional pages, if necessary. Please return **FOUR** copies.

Manuscript: 00-258

Title: Witness to Robbery: A comparison of Memory in Realistic Field Settings and in the Laboratory

Reviewer: B

COMMENTS TO THE AUTHORS

This study compares memory of witnesses of a staged event with witnesses who viewed the event on video. I should note that I conducted a very similar study 15 years ago. I realised as I ran the study, and particularly as I analysed the data that the comparison between live and video versions of the same event is pointless. The fundamental difference between a live version and the video version renders any comparison meaningless. By its nature the video camera focuses on certain aspects of the event while the live viewer is free to focus attention where he or she wishes (or where their attention is drawn). If an attempt is made to provide a video view of the entire scene, the resolution is so poor, relative to a live view, that no comparison can be made. In effect, the video camera imposes a view on the viewer that changes what is seen and attended to. Thus, any differences in quality of memory between live and video perspectives is a consequence of the video medium; comparing the two is comparing apples and oranges. In fact, the authors of this paper admit that the live witnesses had "less overview of the situation" (p. 15). In fact, some of the Live witnesses "were forced to interact with one of the robbers by handing over the money or ordered to get down on the floor" (p. 15). Comparing witnesses who watch an event on video with those who are ordered on the floor during the event makes no sense at all. In summary, the procedure is fundamentally flawed - the live and video taped versions might be compared in other ways but a comparison of detail and accuracy of memory is meaningless. The witnesses to the live and to the video event are doing different things, attending to different aspects of the event, have a different perspective, etc. There are so many differences between the live and video versions that it is impossible to attribute the source of any memorial differences.

The authors argue that the staged robbery is ecologically valid. This is clearly not the case. The live witnesses rated their level of arousal only barely higher than the witnesses watching the video tape. I doubt that the authors would argue that victims of actual robberies are only slightly more aroused than when they watch television.

The authors make a comparison between the absolute amounts of recall in the archival study of Tollestrup et al and those found in their study. This is completely unwarranted. The number of details recalled in the Tollestrup et al study were a consequence of what the police recorded in their files (not what the witnesses actually remembered). In other words, the police recorded what was relevant to their investigation : height weight, age, etc. Thus, the fact that the number of details in the present (15.5) was close to the number of details in the Tollestrup et al study (14.7) is a coincidence and has no meaning whatsoever. Suggesting that there is a similarity here is an unfounded attempt to conclude that "the conditions of the present study are representative of forensic contexts" (p. 15).

The authors reported an inverted U-shaped function relating arousal and accuracy in the filed or live condition. However, they report no analysis to support this conclusion. They note that the relationship looks like an inverted U but to my eye it appears to be a random relationship.

In summary, the procedure employed in this study is fundamentally flawed. The conclusion that the authors sought, that laboratory studies are forensically relevant, could not be addressed with the method employed.

E-mail

Dear dr. [illegible]

Thank you for E-mail and letter of November 8, with the reviewer's comments on our manuscript "Witness to robbery……" (No. 00-258). I must confess that this was astonishing reading. What the reviewer has raised as objections *are in fact precisely the questions this study attempts to answer.* Please consider the comments below.

1) The introduction relates to the ongoing debate on laboratory versus field studies in eye witness psychology, and quotes previous authors who have identified three critical differences between laboratory and real-life conditions in evaluating eye witness memory: *First*, the emotional arousal elicited in forensic contexts versus the quiet conditions of viewing a video or slide show in the laboratory (page 3 of the MS); *second*, the differences between the conditions of observation in the laboratory and real life (relating specifically to factors such as differences in the opportunity to observe among forensic witnesses due to attentional capture, being in different geographical positions with respect to the action, being forced to interact with the perpetrator and so on, versus laboratory witnesses who are cast in the role of passive observers with a uniform view of the scene, page 4), and *third*, the composition of witnesses in the laboratory studies are typically not representative for actual forensic witnesses (students versus the general population, page 4). In developing the rationale for the study we explicitly states that the effect of emotional arousal cannot be modeled in planned studies (I would like to see the ethical committee that would allow you to *really* frighten uninformed subjects), but that the other two factors can be evaluated by suitable comparisons of laboratory and staged events in field settings. And that is precisely what we have done*: The "objection" raised by the reviewer is the experimental variable of the study! Why else should one be interested in comparing field and laboratory*???? And to achieve this we have used a situation (a very realistically staged event on the spot – bank or service station) where the persons involved are the same (the robbers are acted by two well rehearsed police officers), the memory items probed in the two conditions are the same and relate to information present in both the video and field condition, and the subject samples in both conditions are representative for robbery exposed professions (third point above). The debate has been going on for many years, but this is the first attempt to evaluate what the differences between the field and the laboratory (second point above) actually mean for the memory performance obtained in the two conditions. Surely, these questions are as legitimate questions of scientific research as they are of scientific debate.

2) The next comment by the reviewer. "The authors argue that the staged robbery is ecologically valid. This is clearly not the case. The live witnesses rated their level of arousal only barely higher that the witnesses watching the video tape. I doubt that the authors would argue that victims of actual robberies are only slightly more aroused than when they watch television". Has the reviewer read the paper?? To the contrary, we explicitly state in the introduction that one cannot model real life in this respect, and in the discussion we make the following comment: "While the present field study successfully recreated the *physical* conditions of armed robberies in Norwegian forensic contexts where the actual firing of guns are fairly rare, it is clear that such forewarned staged events do not elicit the emotional reactions likely to be elicited by actual robberies. In both field and laboratory conditions emotional arousal clustered in the lower half of the 7-point scale" (p. 16). We do nowhere claim ecological validity with respect to emotional arousal. However, we did include a rating of emotional arousal in the study. To my knowledge we are the first to do this, so we can at least to some degree evaluate this factor in these two contexts. (In most studies no measure of arousal are included, and the question of arousal is often studied with manipulation of "emotional" versus "neutral" picture sequences in the laboratory), The forewarning in the field condition in the present study was very indirect and given a couple of minutes before the event took place. The staged robbery was so realistic that persons with a heart condition and pregnant women are advised not to participate in the course.

3) The reviewer writes: "The authors reported an inverted U-shaped function relating arousal and accuracy in the field or live condition . However, they report no analysis to support this conclusion. They note that the relationship looks like an inverted U but to my eye it appears to be a random relationship". This is not correct. The legend to Fig. 3 informs that this inference is based on a curve fit. Anyway, we also comment that the relationship is a mild one, and repeat comments made above.
4) The reviewer comments on our comparisons with a study by Tollestrup et al. on the memory of real witnesses. This is a minor discussion point of the paper, and can be dropped. However, the information we elicited from the witnesses were not too different from that probed by the police (the staged robberies were after all part of a course developed by police officers), thus the comparison is not completely unwarranted. And the reviewer quote only half the our statement on p. 15. The complete sentence read: "This suggests that except for an overall lower level of induced emotional arousal among field witnesses, the conditions of the present study are representative for forensic contexts".

Of course, one must be prepared that from time to time papers are rejected by international journals. But in this case it is impossible to simply accept the rejection. The comments of the reviewer are so off the mark that I very much doubt he actually read the paper, except perhaps for the Discussion-section, which he has misrepresented. Both the purpose of the study, the rationale of the experimental design and the interpretation of the results have been misunderstood. And my years of experience in writing for scientific journals, including APA journals, convince me that the paper itself is quite clearly written.

In the light of these comments I would ask you to reconsider your decision.

Sincerely

Svein Magnussen

Envelope-to: s.j.magnussen@psykologi.uio.no
X-Sender: krm10@email.psu.edu
Date: Mon, 27 Nov 2000 11:01:39 -0500
To: Svein Magnussen <s.j.magnussen@psykologi.uio.no>
From: <krm10@psu.edu>
Subject: Re: review MS 00-258

Dear Dr. Magnussen

I apologize for taking so long to respond to your e-mail concerning your
paper "Witness to robbery...." (No. 00-258). I am sorry that the review
was not a satisfactory one, and I wanted to let you know what your options
might be.

First, the reviewer who provided input is someone who is generally
knowledgeable, careful and constructive in reviewing manuscripts, and I an
convinced that this individual gave your paper a careful look. I also read
through your paper with some care. Your letter suggests that we both
missed some very important aspects of the research you presented, which is
very possible. On the other hand, I think that if two reviewers, one quite
expert in your area and another generaly knowledgable and trying to do a
competent job both completely misunderstand your paper, it is likely that
the readers of Journal of Applied Psychology would also tend to
misunderstand it.

I cannot accept your paper for publication, but if you are willing to
prepare a revision of the paper that helps to clarify the points the
reviewer and I missed, I will be happy to consider it for publication. We
will consider this manuscript without predjudice (i.e., the fact that a
revision was not originally invited does not make any difference in
considering a revised manuscript), and I will try and get additional
reviewer input for the revised manuscript.

If you have any questions or issues you would like to go over before
submitting a revised manuscript, please feel fre to contact me. Also,
please send four copies of the revision together with a letter describing
in detail the changes you made to the manuscript

Sincerely

To: Kev... <krm10@psu.edu>
From: Svein Magnussen <svein.magnussen@psykologi.uio.no>
Subject: Re: review MS 00-258
Cc:
Bcc:
Attached:

Dear dr. ...,

Thank you for your response to my E-mail, and your generous offer of a second evaluation of a revised version.

In the meantime I have conducted an informal survey, giving the manuscript and the reviewer's report to three cognitive psychologists, one of whom is an American colleague who is currently with us, and who has extensive experience both as journal and handbook editor. Without knowing each others evaluation (or my own comments) they all came back with essentially the same verdict: The reviewer's main objection does not make sense. As one of them remarked, it is like saying that you cannot study the effect of light and dark adaptation on perceptual performance because people do not see that well in the dark. The logic of our study is analogous to the light and dark example: We have attempted to evaluate the effect of observation conditions (laboratory versus field) on the number, type and accuracy of details subjects are able to report in these two conditions. This might be difficult, but it is legitimate and should be interesting to anyone concerned with questions of ecological validity. So, I am a little uncertain about what to revise in the MS. However, I will take you up on your offer and try to amplify these points in a revised version. In addition, I will drop the results on arousal level which are not important in study.

I'll send you four copies of a revision with a coverletter explaining the changes made.

Thank you.

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At 11:01 27.11.00 -0500, you wrote:
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OSLO

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e-mail: svein.magnussen@psykologi.uio.no
Tel: +47 22 85 53 75
Fax: +47 22 85 44 19

2000-12-04

Dear Dr. [illegible],

Referring to your letter of November 8, and subsequent E-mail communications, I hereby resubmit a revised version of the MS "Witness to robbery….." by C. Ihlebæk, T. Løve, D.E. Eilertsen & Magnussen (No. 00-258). In the revision I have made the following changes in response to the reviewer's comments:

1) As mentioned in my E-mail response we were a little astonished by the reviewer's main objection because the "objection" is the experimental variable of the study. As noted in the introduction to the paper: "In the context of eye witness research, laboratory experiments and actual forensic contexts differ along three dimensions, all of which challenge the ecological validity of laboratory experiments", and these are arousal, subject composition and the differences in the physical and geographical conditions of observation listed by the reviewer. In developing the rationale for the study we explicitly stated that the effect of emotional arousal cannot be modeled in planned studies (no ethical committee would allow you to *really* frighten uninformed subjects), but that the other two factors can be evaluated by suitable comparisons of laboratory and staged events in field settings. Surely this is a legitimate research question. The study attempts to evaluate what the differences listed by the reviewer actually mean for the memory performance. In order to do this in a meaningful way, you have to have comparable situations and test the memory for information that is available in both field and laboratory conditions. In the present study we are as close to achieve this as on can expect, while preserving the authenticity of the field condition. We used situations (a very realistically staged event on the spot – bank or service station – and video version of the same event)) where the persons involved are the same (the robbers are acted by two well rehearsed police officers), the memory items probed in the two conditions are the same and relate to information present in both the video and field condition. Keeping the the subject composition in both conditions similar (both representative for robbery exposed professions), it is then possible to evaluate what the differences in physical and geographical of observation mean for the memory performance. In the present version we have made this point stronger, first, by changing the title to "Witness to robbery: *An experimental* comparison of memory in realistic field settings and in the laboratory" and, second, explained the purpose of the study in more detail at the end if the Introduction section (p. 5). In line with conventional memory research we are using the term "memory performance" but realize of course that non-remembered items, errors and distortions can be due to factors operation in the encoding, and make no claim about the source of memory errors (first paragraph, last sentence in the reviewer report).

2) The reviewer further comment "The authors argue that the staged robbery is ecologically valid. This is clearly not the case. The live witnesses rated their level of arousal only barely higher that the

To: Kev[illegible] <krm10@psu.edu>
From: Svein Magnussen <svein.magnussen@psykologi.uio.no>
Subject: Re: review MS 00-258
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Attached:

Dear dr. [illegible],

Thank you for your response to my E-mail, and your generous offer of a second evaluation of a revised version.

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witnesses watching the video tape. I doubt that the authors would argue that victims of actual robberies are only slightly more aroused than when they watch television". This is a surprising comment since we explicitly stated in the Introduction that one cannot mimic real life in this respect, and in the Discussion made it clear that we do not claim ecological validity with respect to emotional arousal. In the present version we have reinforced this point. It is possible, however, that including the data on rating of arousal by the subjects might give an impression that we attached more importance to this aspect of the study that we actually did. The study included the rating because it might allow us to analyze the relationship between arousal and acuracy *within each conditions* but we did not claim to be able to compare across conditions. To be able to do so one would have to have a common anchor, as the use of such rating scales might be context dependent. Without a common anchor, a comparison between the conditions is ambiguous. However, our presentation might have been confusing on that point. Since we have these ratings for only half the field witnesses, and they are of little importance to paper, we have left them out of the present version, and dropped the lengthy discussion of memory and emotional arousal in the Discussion.

3) The reviewer comments on our comparisons with a study by Tollestrup et al. (-94) on the memory of real witnesses. This is a minor discussion point of the paper. However, the type of information we tested in our subjects is similar to the information probed by the police, thus the comparison is not completely unwarranted and we have kept it, but not drawn any conclusion (p. 13).
4) Finally, we have moved a section headed "Between-condition comparisons" from the Methods to the Results section (p. 9).

The paper has been shortened by two pages, and has the format of a Research Report.

I look forward towards hearing from you.

Svein Magnussen

Sincerely

Svein Magnussen

Eksempel 10

APPLIED COGNITIVE PSYCHOLOGY
Appl. Cognit. Psychol. (in press)
Published online in Wiley InterScience
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Displayed Emotions and Witness Credibility: A Comparison of Judgements by Individuals and Mock Juries

JANNE DAHL[1], IDA ENEMO[1], GURI C. B. DREVLAND[1], ELLEN WESSEL[1], DAG ERIK EILERTSEN[1] and SVEIN MAGNUSSEN[1,2]*

[1] *Department of Psychology, University of Oslo, Norway*
[2] *Centre for Advanced Study, the Norwegian Academy of Science and Letters, Norway*

SUMMARY

Mock juries of 5–7 jurors viewed one of three video-recorded versions of a rap…
role-played by a professional actress. The statement was given in a f…
three kinds of emotions displayed, termed congruent, neutral …
The juries were requested to reach a decision on ite…
credibility of the witness and judgements of…
asked to complete the questionnai…
filled out the questionnai…
judged credibili…
emotions …

Det virker her som konsulentene ikke har lest artikkelen, men skummet manus med en forutinntatt holdning om at dette ikke er profesjonelle spillere i internasjonal vitenskap (vi er nokså nye i feltet vitnepsykologi, og førsteforfatterne er studenter). Konsulentene demonstrerer eklatante lesefeil og kommer med anbefaling om at vi bør gjøre det vi faktisk har gjort. Redaktøren ser problemet og inviterer til ny innsendelse, hvor vi foretar noen klargjøringer og kosmetiske endringer. Artikkelen ble akseptert.

… time in everyday life … police investigators in the … courtroom. Recent reviews of the research … discriminate between truthful and deceptive behaviour, … we believe to be associated with deceptive behaviour are in fact … with deception (DePaulo et al., 2003; DePaulo & Morris, 2004; Strömwall, Granhag & Hartwig, 2004; Vrij, 2000). However, the absence of reliable relationships between behaviour and truthfulness does not prevent us from using behavioural signs in the judgement of other people. One type of signal that is important in forming impressions of other people, and in judgements of credibility, are the non-verbal signals indicating the

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—APPLIED—
COGNITIVE
PSYCHOLOGY

Professor Svein Magnussen,
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Box 1094 Blindern,
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14th March 2006

Dear Professor Magnussen,

Re: Manuscript N°. 779

Thank you for sending us your paper (with colleagues) on the impact of expressed emotion on jury decision making, which has now been reviewed. I must apologise for any delay in reviewing your paper, which has been due to the commitments of one of our distinguished reviewers.

I recognise the difficulties of conducting realistic jury research and the importance of such work. However, having read your paper in conjunction with the reviewer's comments, I fear that I am not convinced that your paper is suitable for publication in Applied Cognitive Psychology. Briefly, as both reviewers note, there are methodological difficulties, not least the difference in procedure between the reported study and the control condition, which is included in a separate paper. As reviewer B notes, this is an awkward arrangement which does not assist in evaluating your findings. There is also the statistical problem highlighted by Reviewer A, which requires a revised form of analysis, with potential consequences for the data reported and the interpretation of the results.

In the circumstances, and with regret that I must, on this occasion, reject your paper. It may well be that a socio-legal journal might take a more positive view of your paper and I would encourage you to revise your paper with such an outlet in mind. As it is, I must thank you for your patience and hope that our reviewers' comments are helpful in developing your research in this important area.

Sincerely,

Graham Davies
Founding Editor

'Displayed emotions and witness credibility: A comparison of judgements by individuals and mock juries' (Ms779) is a clearly written paper with an interesting finding: Displayed emotions did influence mock jurors' judgements when they assessed the "victim" independently and without a jury discussion; the effect, however, disappeared after jury deliberations. I like this paper and I think the finding is important. However, since I am not a jury deliberation researcher, I can't judge how "new" this finding is. Frankly, I find it hard to believe that something similar has not been examined before. At the very least, the authors need to discuss the jury deliberation research in more detail and should make clear how their study differs from previous jury studies.

There is a big problem with the data analyses. For example, in Figures 1 and 4, three groups are compared: (a) juries, (b) individual jurors who made their judgements after jury deliberations, and (c) individual jurors who made their judgements without such deliberations. Comparisons between groups (a) and (b) are within-subjects comparisons, but comparisons between group (c) with groups (a) and (b) are between-subjects comparisons. This is awkward. Moreover, I am not sure what the data of the juries look like. Are these the scores per jury? In that case there would be 31 scores (31 mock jury groups) or are they the scores for each individual participant? In that case there would be 174 scores (174 participants). In the first instance a within-subjects design cannot be used, because 31 data points (jury scores) cannot be compared with 174 data-points (individual juror scores after deliberation). In case the individual juror scores were used in the jury groups data the problem arises that many of these scores are not independent from each other (all participants within the same jury will have the same score). The jury deliberation research provides examples of how to analyse such data. All problems, however, could be avoided if the jury scores (group a) would be left out, leaving over groups (b) and (c) which could be easily compared in a between-subjects design. In other words, all analyses need to be redone, and so I won't further comment on the Results section in this review.

I will list my other points in order of appearance in the manuscript.

- Page 5: The authors need to discuss in much more detail why and how jury deliberations would affect individual jury members' assessments after the deliberations.

- Page 5: The jurors were instructed that they had to reach common decisions. How realistic is this in real life? I noticed (footnote 1) that full agreement is not necessary in Norway. Why then was this instruction given?

- Page 6: The data of the present study are compared with some data of a previous study (control group). Ideally, the authors should have collected new data for this control group.

- Page 8: Comparing data of two different studies may be problematic give the fact that the questionnaire used in the present study was an abridged and slightly modified version of the questionnaire used in the original study. This could create a confound. At the very least, the authors need to explain in detail what the exact differences were between the two questionnaires.

- On page 15, gender differences are discussed. In case the authors find it important to discuss gender comparisons, they should introduce this issue properly in the Introduction, and ideally, formulate a gender-related hypothesis.

- On page 16 the authors claim that the tendency to vote "guilty" closely followed the credibility judgements. They need to statistically test this assumption (by correlating the two measurements).

Manuscript #779

The paper deals with the decision making process of a mock jury in terms of credibility and guilt verdict in a case of rape, presented according to three different scenarios according to how the victim the alleged rape: congruent, neutral, incongruent.

The study is in line with several researches conducted in the field that look at the effect of extralegal factors on credibility of witnesses (and victims).

The study, though interesting because it looks at the effect of jury discussion on the mitigation of individual decisions, it has some limitations and methodological problems, rendering the paper not ready for publication.

In the review of the literature, though appropriate, there should be a wider review towards cognitive process during decision making, and why group discussion should influence individual judgement (the hypotheses that caught the authors to come to their design).

Procedure

The study makes use of a previous conducted study to make comparisons between group results. This implies that the reader should be able to have immediate access to that article to read about comparability of the sample (where the same people?), which video are we referring to. Results comparisons are here presented with regard to the no-jury-discussion (individual no-jurors). The authors then say that although the video is the same as in Kaufman et al. (2003), this was an 'abridged and slightly modified version of it': with regard to what? How can comparisons be made if the variables (items) were not all the same: if the item were used the same, this should be mentioned and also that the two samples (if two different samples were used) were comparable.

The whole paper includes virtually and practically this other work, but it would have been preferable to have the data and include them in the analyses. One is brought to considered that the same study was the same, and that prior to group discussion and decision, individuals were asked to rate their judgements. This procedure would have been more preferable, more straight-forward and comprehensible.

A table presenting a summary of mean values for the main and interactive effect would help interpret results; figures are of immediate visibility, but they lack to provide exact values.

The sample only stated that it was based on students (which faculty of the University of Oslo? It would be interesting to know if they were from Law or Psychology or Medicine. Was this checked for any possible bias in the decision process?) And how were they recruited? The sample is 19-52 mean age 23.5 yrs, I would like to see the standard deviation which I would assume is rather big, given that there are a few over 30 students. If these are outliers they might be removed from the sample.

It is not clear about the measure used: how many items measuring credibility? To have a reliable scale, at least 3/5 items for each construct should have been used, to assure reliability of measure.

The discussion should be a critical review of the findings and possible explanation not another description of what was found; thought the authors indicated that assessing what can influence the credibility of the victim does not mean that the person is found guilty, this I found the strongest limitation of the study, that no story of a suspect is presented (even if with no added levels of manipulation.) This strongly affects ecological validity of the study; group cognitive decision processes takes place by assessing emotional-stereotyped preconceived ideas that are based also on what he does or does not. In their paper there is no reference to the importance of including a script of 'a suspect', even if this was the same in all experimental conditions.

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2006-03-21

Dear Professor Davies,

Thank you for your reply to our submission to APS, Ms 779, "Displayed emotions and witness credibility: A comparison of judgments by individuals and mock juries" by J.Dahl, I. Enemo, G.C.B. Drevland, E. Wessel, D.E.Eilertsen & S. Magnussen.

Of course, one must be prepared to have papers rejected by major journals from time to time, but sometimes the rejection is hard to accept. This is one of these times, and the reason why rejection is hard to accept, is that we did not make the fatal errors the reviewers claim we made, and we did in fact carry out most of the procedures they recommend; in addition there are some misreadings/misunderstandings of the procedures, for which we possibly are to blame, and which are easily rectified.

A more detailed response to the reviewer's comments follow below, which we hope you will consider when deciding on our appeal.

Reviewer A.

According to the reviewer the paper is clearly written, with an interesting and important finding. The reviewer's main objection is the data analysis:

"There is a big problem with the data analyses. For example, in Figures 1 and 4, three groups are compared: (a) juries, (b) individual jurors who made their judgements after jury deliberations, and (c) individual jurors who made their judgements without such deliberations. Comparisons between groups (a) and (b) are within-subjects comparisons, but comparisons between group (c) with groups (a) and (b) are between-subjects comparisons. This is awkward"

We don's see how this is awkward. The figures display means for purely descriptive purposes, and whether these are within- or between-subject is not relevant. It would be a relevant objection for statistical purposes, but no such analyses have been performed, and this ought to be clear from the text.

"Moreover, I am not sure what the data of the juries look like. Are these the scores per jury? In that case there would be 31 scores (31 mock jury groups) or are they the scores for each individual participant? In that case there would be 174 scores (174 participants)."

We don't see that there should be any doubt about that. It was explained in the Methods section and the reported degrees of freedom on p. 10 are 28. But the informatioin may also be included in the figure caption.

"In the first instance a within-subjects design cannot be used, because 31 data points (jury scores) cannot be compared with 174 data-points (individual juror scores after deliberation). In case the individual juror scores were used in the jury groups data the problem arises that many of these scores are not independent from each other (all participants within the same jury will have the same score)."

We agree with the reviewer – the analysis decribed would be impossible – and that is why we never did it, and we don't really see see how the text could convey this erroneous impression.

"All problems, however, could be avoided if the jury scores (group a) would be left out, leaving over groups (b) and (c) which could be easily compared in a between-subjects design."

The reviewer recommends that we exclude the jury data, but in our opinion excluding data that show the "homogeneity process", would reduce the importance of the paper. The between-subjects comparisons (group b versus c) were in fact done and are reported on p 10 and 12 in the MS. So there is nothing wrong with the data analyses - we have not carried out forbidden analyses - and there is no need to do them again.

Additional points by the reviewer:

"Page 5: The jurors were instructed that they had to reach common decisions. How realistic is this in real life? I noticed (footnote 1) that full agreement is not necessary in Norway. Why then was this instruction given?"

We may not have been completely clear on that point. The juries in Norway are requested to reach a verdict, and the process by which this is reached is not known outside the jury. The standard phrase of "more than six votes" for a guilty verdict could mean that they all agreed, or that one, two or three members disagreed. We will never know. If there are fewer members in favor of guilty verdict, the accused is aquitted. So our phrase "common decision", would probably better read "decision", since we have no idea about how the decision was reached, as is the case of real juries.

"Page 6: The data of the present study are compared with some data of a previous study (control group). Ideally, the authors should have collected new data for this control group."

This paper is part of larger experimental program supervised by myself, and in recent time also by author D.E.E. The data collection for the Kaufmann et al, (2003) study, which we use as controls, and those of the present study were all collected within a period of a few months, with subjects drawn from the same population. So it is a real control. However, for various reasons the submission of the present paper has been delayed. We may clarify this point in a revised version.

"Page 8: Comparing data of two different studies may be problematic give the fact that the questionnaire used in the present study was an abridged and slightly modified version of the questionnaire used in the original study. This could create a confound. At the very least, the authors need to explain in detail what the exact differences were between the two questionnaires".

Obviously, we have not been completely clear on that point. For the jury study we shortened the questionnaire, but the common items on which we compare the samples were exactly the same. On the single item that was phrased differently in the present study, we do not compare across samples.

"On page 16 the authors claim that the tendency to vote "guilty" closely followed the credibility judgements. They need to statistically test this assumption (by correlating the two measurements)."

We agree. Such an analysis was in fact done, but by mistake not reported. We will include it in a revised version.

Reviewer B

This reviewer also finds the study interesting, but notes some limitations and methodological problems, detailed below.

"The study makes use of a previous conducted study to make comparisons between group results. This implies that the reader should be able to have immediate access to that article to read about comparability of the sample (where the same people?), which video are we referring to. Results comparisons are here presented with regard to the no-jury-discussion (individual no-jurors). The authors then say that although the video is the same as in Kaufman et al. (2003), this was an 'abridged and slightly modified version of it': with regard to what? How can comparisons be made if the variables (items) were not all the same: if the item were used the same, this should be mentioned and also that the two samples (if two different samples were used) were comparable."

Again, we may not have been completely clear here. The videos presented to the controls and to the participants of the present study were the same, the questionnaire the same (see response to reviewer A), and the samples were drawn from the same population. This will be clarified in a revised version.

"The whole paper includes virtually and practically this other work, but it would have been preferable to have the data and include them in the analyses."

This comment is very surprising, since it is presisely what we did, and it should be obvious from the statistical analyses (see p. 10 and 12 of the MS).

"One is brought to considered that the study was the same, and that prior to group discussion and decision, individuals were asked to rate their judgements. This procedure would have been more preferable, more straight-forward and comprehensible."

The suggested design may have been more "straight-forward and comprehensible" but definitely not "preferable"; if the participants were tested for individual judgments before joining a "jury duty", it might have created unpredictable confounds. We compare two different samples drawn fro the same population.

"The sample only stated that it was based on students (which faculty of the University of Oslo? It would be interesting to know if they were from Law or Psychology or Medicine. Was this checked for any possible bias in the decision process?) And how were they recruited? The sample is 19-52 mean age 23.5 yrs, I would like to see the standard deviation which I would assume is rather big, given that there are a few over 30 students. If these are outliers they might be removed from the sample.
It is not clear about the measure used: how many items measuring credibility? To have a reliable scale, at least 3/5 items for each construct should have been used, to assure reliability of measure"

These points are easily clarified in a revised version. Participants were students from the faculties of Social Sciences and Humanisties, recruited during lectures. Removing the occasional outlier regarding age, did not affect the result. And a discussion of the reliability and validity of the measures may be included in a revision.

"A table presenting a summary of mean values for the main and interactive effect would help interpret results; figures are of immediate visibility, but they lack to provide exact values".

No problem, but most journals warn against dual representation of the data.

"The discussion should be a critical review of the findings and possible explanation not another description of what was found; thought the authors indicated that assessing what can influence the credibility of the victim does not mean that the person is found guilty, this I found the strongest limitation of the study, that no story of a suspect is presented (even if with no added levels of manipulation.) This strongly affects ecological validity of the study; group cognitive decision processes takes place by assessing emotional-stereotyped preconceived ideas that are based also on what he does or does not. In their paper there is no reference to the importance of including a script of 'a suspect', even if this was the same in all experimental conditions".

The focus of the present study is on the effect of the emotional expression of a witness on the perception of credibility, and how group discussions may modulate this effect. As a supplement we asked the very guarded questions about guilt ("we know that you have not hear the other side of the story"). We do not see how the description of suspect would benefit the study. In another context, a video of the suspect telling his story would be interesting, but that is another study - which is currently in progress.

In sum, the main objections to the study raised by the reviewers are simply wrong – we did not make the mistakes they claim, rather we did preciesely what they recommend (except in the case of bad recommendations), or they imply. All these points are easily clarified in a revised manuscript. The reviewers' comments also includes a couple of more general points regarding the review of the literature and the relevance of a couple of discussion points. We will take second look at the literature and revise accordingly.

In view of the above, we hope you will reconsider your decision, and permit the submission of a revised version to Applied Cognitive Psychology

Yours sincerely

Svein Magnussen

29 March 2006

Professor Svein Magnussen
Department of Psychology
University of Oslo

Dear Professor Magnussen

Thank you for writing to me regarding your paper (ms # 779) which we rejected recently. I apologise for any delay in responding, but I had to recover the file and re-read your paper.

As you say, rejection (and I have had a few in my time) is always distressing, doubly so, when you feel that the reviewers have misunderstood your methodology and mode of analysis. As you yourself point out, it appears that some of these misunderstandings arose from a lack of clarity in the original manuscript. To paraphrase Oscar Wilde, to have one referee misinterpret your paper could be an accident, to have both do so, looks like carelessness! Joking apart, there do seem to be some difficulties over clarity of expression which has led to the misunderstandings you highlight in your letter.

In these circumstances, and given our readiness to publish the earlier paper in the series, I am prepared to invite you to revise and resubmit your paper for further consideration. In the circumstances, we will go back to one of the original reviewers, who will be made aware of this correspondence and one other. Could I urge to take careful account of the other points made by the reviewers in their reports? In particular, the need for greater clarity as to design and methodology and a more focussed discussion section: I think the paper would benefit from some trimming of the latter. Also, please reduce the number of figures if possible (and you should also indicate in the manuscript where they should appear). I agree that we do not want to duplicate information in Tables and Figures: perhaps you could add standard errors to the bars to satisfy the referee.

Finally, I should add that I don't think the problem lies in your grasp of written English-which is excellent -but in clarity and completeness. I sometimes find it helps when such issues arise, to show my paper to a colleague unversed in the area in an effort to identify areas of possible misunderstanding. However, the referees have already done this job for you to a large extent.

Thank you for explaining your case fairly and good luck with your revision.

Sincerely

Graham Davies
Founding Editor: *Applied Cognitive Psychology*

AV DAGENS LESNING.

(Grunnforskningsutvalgets innstilling avgitt)

Grunnforskningen er grunnlaget for ny kunnskap og innsikt, og et samfunns kulturelle nivå er i betydelig grad bestemt av dets evne til å frembringe ny kunnskap for å kunne ta imot kunnskapene fra andre land. Utvalget understreker grunnforskningens store betydning for den anvendte forskning, da den utgjør kunnskapsgrunnlaget for den anvendte forskning og utvikler dessuten de metoder som den anvendte forskning må bruke.