

Section 6 : Letters to Michotte

A Selection of Letters to Albert Michotte: 1911-1963

In the course of our research we were fortunate to be offered a collection of letters to Michotte which consisted of correspondence from many of the eminent psychologists of the early twentieth century. The documents had been saved by Albert Michotte and came into the possession of his son, Baron Fritz Michotte of Bruxelles on the death of his father. They comprise 151 letters to Michotte written between 1911 and 1963. The correspondence is by 28 authors whose names are listed below:-

Frederick Bartlett
Alfred Binet
Edwin Boring
Cyril Burt
Frederick Buytendijk
C. Jacques Chevalier
Marcel Foucault
Paul Guillaume
Fritz Heider
G. Heymans
Julian Huxley
Ernst Jaensch
Pierre Janet
David Katz

Wolfgang Kohler
Oswald Kulpe
Kurt Lewin
Wolfgang Metzger
Charles Myers
Jean Paulus
Jean Piaget
Henri Pieron
Geza Revesz
Andre Rey
Edgar Rubin
Edward Thorndike
Robert Thouless
G. Zeliong

Obviously, Michotte was in touch with many of the major figures in psychology and he was clearly aware of the work of the Gestalt school, although he kept himself somewhat apart from this group in his own theorising. Unfortunately, we do not possess Michotte's replies but we reproduce here interesting extracts from some of the letters. Our selection was guided mainly by considerations of space and by the wish to incorporate material that might illuminate other chapters of this book. We have included a selection of the extensive and gossipy correspondence between Bartlett and Michotte, who became life long friends and a selection of the correspondence between Kohler and Michotte, which also took place over more than 30 years.

A warm relationship appears to have developed between Michotte and David Katz, who makes some interesting comments on the nature of film and picture perception (see ^{Section 4} ch.). Katz also appears to have been aware of

the incompatibility between Michotte's nativist views and Piaget's constructionist theory of the origins of perceptual structures (see Ch.) (Although we did have a letter from Piaget to Michotte, it was simply a formal request to attend a conference and so it is not reproduced here).

Some of the letters have been translated from the original German, French or Dutch into English. We have retained only the material that seemed to us of general interest, leaving out much that was personal or formal. Eleven short letters from other eminent psychologists are included as evidence of the esteem in which Michotte was held by many of his contemporaries.

My dear Michotte

..... What do you think of Gestalt developments? Of course everybody has read Kohler's book and practically everybody except a few of the more philosophically minded, was dreadfully disappointed. I can't in the least see why it is necessary to force into Gestalt psychology all this speculative biophysics. I don't know what Wertheimer is doing now but so far as I can see Koffka is the only one of the three who is really seriously trying to develop the movement in an experimental sense. And the greater part of his work seems to me not genuine development but elaborate and very clever reduplication. Also what in the world can be meant by saying that this or that effect depends directly upon "cerebral organisation" and yet has nothing whatever to do with past experience beats me completely. All the time when I read them I got held up by this possible confusion between what something is analytically and what that same thing may legitimately be regarded as having developed out of in a historical sense. Sometimes Koffka seems to me to be quite unnecessarily ^uperverse in the effort to be striking, as when he maintains that perceptually ^htridimensional space is historically prior to bidimensional space. The utmost that his evidence seems to me to suggest is that there may be a stage of visual response in which neither bidimensional nor tridimensional reference is at all clearly made, and that both may have grown out of this. But of course to say that would not be at all sensational, and I wonder whether it is unfair of me to think that that perhaps is why he does not say it. I hope I am not being hypercritical but we all have had the greatest hope of this movement when we first learned about it, and now it seems to be frittering itself away into nothing very much.

I put in a lot of hard work during the year to try to get my own book finished, and towards the end of the summer I got it done. It has to be all revised and typed but part of that is done too, so that I hope it may see the light some time next year. It changed a good bit in the writing and became perhaps rather narrower, so that I have decided to call it simply: Remembering: An experimental and social study. I got a good bit of material for the second, and social, part of this when I was in Africa, and some of it I think is quite interesting. I shall be very glad to get it off my chest, for it has been hanging on far too long, and I have a lot of more specifically experimental work I want to get done. There is I am afraid nothing very terrific about it all. In the main it is simply a demonstration - well, I hope a demonstration - of the fact that remembering is a constructive imaginative effort, and not a reduplicated, repetitive one primarily.....

Yours very sincerely,

F.G. Bartlett

161 Huntingdon Road
Cambridge

18 August 1963

Dear Albert,

About a fortnight ago I was most pleased to get a letter from Basic Books saying that, according to instructions from you, they were sending me a copy of the American edition of the English translation of *The Perception of Causality*. The book itself arrived about the middle of last week and I have been re-reading some of it. There is to my mind, as indeed you know already, no doubt whatever that this is one of the few great psychological books which will last and be read for as long as psychology itself endures. I hope that its appearance in English in the USA will distract some at least of the brighter young people there, away from some of the rather fruitless and trivial things they seem to do now, towards the vital and basic problems that you have shown how to tackle. The book is certainly got up in an attractive manner and I think well edited and excellently translated. My only regret is that it did not appear several years ago. Thank you for the copy, and I wish it a fine circulation and all the influence which it so richly deserves.

I do not find it at all easy to appreciate with proper judgement the present position of psychology. Sometimes it seems to me that we have got into a jam, with overmuch information theory which tends towards preoccupation with interminable somewhat small detail and conducts most of us away from a study of the more humane interests. But it may be that this is only a temporary swing due partly to the overwhelming success of current physics and chemistry and partly, perhaps, to a reaction from the romantic excesses of the psychopathologists. Anyway I hope and believe that your book will call a lot of people back into the proper field of classical psychology, but with a better appreciation of what experiment can achieve than was possible before.

..... Our love to you all, and again, all my thanks for the book.

Yours ever,

Fred.

The University
Manchester

March 4th, 1934

Dear Colleague Michotte,

You will probably have heard that, as so many other colleagues, I have been forced to retire as a non-*Aryan* and lost my professorship in Rostock. The University of Manchester has invited me to do scientific work here and give a lecture from time to time. I am here since October, but my family is still in Rostock, because my future destiny is fully indeterminate.

A few months ago, colleague Revesz had solicited you to become co-editor in the advisory board of the planned Acta Psychologica. The publication of the Acta is certain by now. I myself will, according to Revesz and the publisher's wish, act as the secretary. We want to come out with a few papers as soon as possible. Revesz told me that you were willing to contribute with a study from your laboratory on the psychology of motor reactions.

.....
.....
.....

I would terribly like to see you again. I could perhaps pay you a visit in Louvain at the occasion of a trip to or from Germany. If you wished, I could tell you at this occasion even in a small circle, something about the investigations I recently tackled.

With best wishes and greetings and the request for a quick answer.

I am,

Yours heartily,

David Katz

From D. Katz

Stockholm Högskola
Institutet för
Psykologi och
Pedagogik
observatoriegatan, 8

Stockholm, August 26th, 1946

Dear Friend Michotte,

I have now read your book La perceptions de la causalité. It was for me not only unusually scientifically profitable, but also an authentic pleasure. I am convinced that your work will rank among the few classics in experimental psychology. When one is, as I am here, so isolated, when one is always asked at the outset which practical uses will come out of our science, to the point that one hesitates to devote oneself totally to theoretical questions which one holds so important and would like to study out of deep personal concern, a work like yours is truly heart-refreshing and encouraging. Your research is a masterpiece from every standpoint, in the experimental design, in the analysis of data and in the conclusions. What makes me specially like your book is the way it shows how we can adopt a truly fundamental attitude how we should tackle psychological problems in the conviction that Phenomenology and experiment should be closely related to each other. You did not formulate this yourself that way, but I assume that you would not object to saying that your experiments contribute to and justify the phenomenological method. Causality is a subjectively experienced phenomenon exactly as colours, sounds and tactile impressions are. In order to produce such a phenomenon, a primitive phenomenon so to say, some external stimuli or constellation of stimuli are needed. It is a wonder that so many, and first and foremost, Hume should have overlooked this and that all his disciples should have trusted their own subjective experience so little. Your impression of causality belongs qualitatively to the category of movement or "illumination", whose existence was also negated. I am inclined to think that animals do also have an experience of your causality and perhaps it will be possible to prove it some time. In case it were so, it would be an essential argument in favour of your thesis that causality can be subjectively experienced. The question also arises from your experiments, as to which psychological processes experienced causality corresponds. It is extremely difficult to build up adequate hypotheses in this field with the help of clinical physiology. I believe that the latter would need fundamental complements if one would wish to explain such phenomena and stick to the theory of psychophysical parallelism. I have encountered similar difficulties in other fields. In any case, many thanks again for what your work has brought to me.

.....
.....
.....

I believe that I have recently discovered some nice things. It seems that there exist in mental work Gestalt laws which correspond closely to the Gestalt laws of perception. I hope to publish these experiments shortly and will

... cont'd.

send you the reprint then. I don't know whether I wrote you that a small Gestalt Psychology which I have published in Swedish, has also appeared in German and Spanish and will now appear in French. You will receive a copy.

.....
.....
.....

With best greetings,

Your old friend,

David Katz

Psychologisches Institut
der Universität
Berlin C2 Schloss

November 11, 1923

Much honoured M. Michotte,

.....
.....
.....

I sent you recently a short presentation of the main facts and problems of the psychology of sounds and I hope that you received it safely. I did not send you another work in the Psychologische Forschung, because you intended to subscribe to the journal.

With best wishes,

Yours truly

Wolfgang Kohler

From W. Kohler

Koburg, June 8th 1930

Dear Michotte,

Many thanks for your kind invitation. I would so much like to see again and to speak to you. There are not many psychologists to which I feel so close as to you. But this time, I cannot come ...

.....
.....
.....

With heartiest greetings,

Yours very sincerely,

Wolfgang Kohler

SWARTHMORE COLLEGE
Swarthmore, Pa.

Department of
Psychology and Education

November 28, 1944

Dear friend,

..... Your American visitor did not find you in very good health. What a shame that I cannot come and try to cheer you up. My hope is that your troubles will be alleviated as soon as you can get more and better food. Surely, we must expect the situation in your country to improve almost at once when the resistance of the Germans collapses.

A few years ago I heard that you had hit upon something of fundamental importance in our field. The letter which I now had confirms this information. It also mentions the subject of your investigation. My dear man, I cannot think of any discovery that would be so apt to start things in a better direction as this finding is. Naturally, the repercussions will extend far beyond our special discipline. For, if I am not mistaken, the doctrine that we experience nothing but facts of concomitance and mere sequence is one of the ultimate factors behind the present scepticism and the corresponding crises in every field, including that of ethics and politics. The phenomenon that purely subjective values are being imposed by dictation seems to me to be a direct consequence of the fact that immediate awareness of the "nexus" of things has been for long negated in the name of science. I admit, the Positivists have meant no harm. They have none the less almost succeeded in destroying human culture. Am I mistaken in believing that you see your discovery in this wide context? I do hope that your health will soon allow you to complete your writing.

On our side there are a few things which will interest you, and nothing would give me greater pleasure than an opportunity to discuss your work and mine, and to see to what extent they fit each other. The more advanced part of my investigations refers to perception. I will send you something as soon as the mail accepts reprints. You will then see that in this field developments tend to take a turn which could not be expected even a few years ago. Otherwise I am beginning to be fascinated by problems in motivation. It is here that a more direct contact between your observations and mine is most likely to occur. Heavens, wouldn't it be fun if we could have a good talk about such matters tomorrow.

Of course, it will not be easy to arrange a meeting even when the European war is over. But there are possibilities. Recently Adrian was here. He seemed to see no reason why I should not soon be able to come to Cambridge.

I am writing this in a hurry in order to establish a first contact. You will soon hear more. And if it is at all possible I should be most happy to have a word from you.

Take care of yourself, and don't forget that you have a friend in America.

Yours,
W. Kohler.

8

//

Rehm

From W. Köbler

Upsala, July 23rd, 1947

My dear Friend Michotte

I was most happy to find in Upsala your very good letter, which contained Nuttin's information that you have quite recovered from your illness. I wish I could have been present at the celebration. If any psychologist deserved to be honoured at this time, you are the man. Your work on causation gives you an entirely unique position. To be sure, there will be difficulties with those Positivists who refuse to accept any phenomenological evidence which does not fit into their scheme and who hate the very concept of causation. This work represents the most serious danger to which their views have so far been exposed. In America, this is now the front at which all essential intellectual battles are being fought. My friends and I will make the best use of the help which has so suddenly come from Louvain.

α

.
.
.

... we will do our best to copy some of your most important observations at Swarthmore. The article which I wrote for your celebration volume is a bit primitive. I was ill for several weeks and had to write it in a hurry, if it was at all to be included in the volume. Let me hope that nevertheless the content is clear. As always in such cases, I now find that some relevant points have been overlooked, and that certain obscurities could probably be eliminated by a further experiment. But my own interest is now almost entirely concentrated on the direct (physiological) proof that "figure currents" pervade the brain as a continuum, so that the concepts of field physics become applicable. Naturally, if this is right, we ought to be able soon to say something from this point of view about the cortical correlates of the perceptual experience of causation. Your work would then have immediate consequences concerning the epistemology of causation in field physics not, of course, only in the brain but generally in physics.

Incidentally, have you seen Wertheimer's work on thinking? If not, I will send you a copy. As to your own book, people in U.S.A. seem to have difficulties in getting copies by ordering them in New York. If you could ask your publishers to send copies to a few addresses in America, the people in question would surely be glad to pay for their copies and the expenses, if the bill is added. I mention, first of all, Dr. Fritz Heider, Department of Psychology, Smith College, Northampton, Mass., U.S.A. His work is closely related to yours. Two others are: Dr. E. Newman, Department of Psychology, Memorial Hall, Harvard University, Cambridge 38, Mass., U.S.A. and Dr. K. Zener Department of Psychology, Duke University, Durham, N.C., U.S.A. These people would immediately make use of your discoveries in their own work

The Department of Psychology in Swarthmore ought also to have a copy. My students are reading my own copy so eagerly that in a short time not much will be left of it.

I am very much impressed by the fact that in spite of a second German invasion and occupation of Belgium you wrote your last letter in the German language. This is extremely generous of you. I wish I could write to you in French. But my French is now worse than ever because the Swedish, with which I am not really familiar, interferes with the other language. I am only glad that reading French has remained as easy as it was before.

.

Yours

Wolfgang Köhler

April 21, 1947

My dear friend:

Surely, I am not a "graphomane", and the fact that for such a long time you have not heard from me may have in part be caused by my being entirely free of this ailment.

..... Thus everything would be easy, if I had not begun experiments in which I am trying to prove by direct electrophysiological methods that the theory of after-effects as given in the paper by Wallach and me is right. This is most strenuous work, but also, as you can easily imagine, tremendously exciting. And what do you think? It looks exactly as though the theory were right: when a visual pattern appears in the field of the subject, you can register from his occipital skull, underneath of which part of the visual cortex is located, precisely such direct currents as the theory postulates. The facts seem clear, but I am not yet satisfied with the technique. We shall probably spend the coming year on the continuation of this work. I don't know whether I wrote you that in the meantime we have found that all figural after-effects known in the frontal plane can also be demonstrated in the third dimension of visual space. Nobody will be able to say any longer that depth perception is a matter of learning.

Yours,

W. Kohler.

November 13, 1947

My dear friend:

We are back in Swarthmore, and after the usual hectic weeks, with which a new academic year begins, I can at last write you.

..... I am most grateful for the second copy of your book. It will be most helpful when we try to copy your main experiments. I refuse to lend the students the copy which you sent me first. But they may use the second. The fact that the edition of the book is already almost exhausted is a most hopeful sign as to the receptivity of intellectual Europe for really important new developments.

Now, first of all about the resistance of my great and learned enemies, the Positivists. You cannot imagine how rapidly America is drifting away from certain valuable trends in European development. And the Positivists from Europe (Vienna), who are now here, do, in their blindness, whatever they can do to accelerate the process. Present Positivism here is no longer what Positivism was in Mach's time. His interest in phenomenal facts as starting materials for everything is entirely gone. Positivism has become formalistic and, paradoxically enough, negativistic. Phenomenal facts are hardly ever considered as such. "Propositions" are the things to study. And there is deep suspicion of phenomenal facts as soon as they threaten the negativistic bias of the school. This holds particularly for causation. In this respect, Hume is the Moses of Positivism, and any discussion of experiences of causation meets with a contempt which an orthodox Jew would show if the authority of Moses were doubted. Yes, my friend, philosophers have become quite narrow-minded in such matters. This is also the explanation of the fact that I am now working in electrophysiology. "If you do not listen to phenomenological arguments", I tell my Positivist opponents, "I will shift the discussion to a field for which you have more respect, and will try to defeat you there."

(cont)

November 13, 1947 (cont)

Naturally, my work does not immediately refer to experienced causation nor to the physiological counterparts of this experience. But the opponents refuse to accept whole provinces of human experience, particularly Gestalt experiences, so that, for a first beginning, it does not matter very much at what point they have to yield. You will hear more about this as soon as I know more myself.....

As ever,

Wolfgang K.

March 22, 1948

My dear friend:

It begins to look as though I could attend the International Congress at Edinburgh.

..... Your letter of February 4, which was so full of interesting remarks, makes me feel that you suspect me of being too strongly influenced by the Positivists. Let me assure you that in actual fact I regard these people as downright dangerous, although they do not realize themselves how great the danger is. I am far from believing that purely psychological work is less important than work in which physiological techniques are being used.

Yours,

W. Kohler.

THE INSTITUTE FOR ADVANCED STUDY
Princeton, New Jersey.

20 December, 1955

Dear Albert,

..... Langfeld told me recently that the next International Congress will be held in Brussels. What an excellent choice! We plan to go to Europe (Germany and Sweden) in 1956; but under the circumstances we see no reason why we should not also go in 1957. This would give us two chances of seeing you again in the near future. Nothing could please us more. So far as I am concerned, there is also the special reason that I have few opportunities to discuss psychological issues with people who share my general premises in science - as, I am sure, you do.

Yours,

Wolfgang K.

From W. Köhler

Upsala, September 1st 1956

Dear Albert,

During the past year, I made a discovery simply by reading important papers in neurophysiology. Matthews in Cambridge took records of direct currents in the central nervous system long before our work, namely in 1936 and 1938. To be sure, he took them from the spinal cord rather than from the cortex. Even so, after his success, our own observations might almost have been published in 1938. How then did it happen that none of the experts told us about Matthew's work when we first reported what we had found? The history of scientific developments is a curious affair. Apparently, Matthew's discovery was first as little popular among his colleagues as our findings ... as though only nerve impulses were direct physiological facts.

Just now I have begun a re-examination of some cortex work in Gestalt psychology, which seems to me to be in need of improvement. I hope to tell you about this enterprise in the near future perhaps in Brussels next year.

With heartiest greetings

Your old friend

Wolfgang Köhler

From O. Külpe

Bonn, April 20th, 1911

Dear and honoured friend,

My hearty thanks for the nice reading and my sincere congratulations for the completion of this book*, which made of you at one go one of the leaders among modern psychologists. I am very glad that we have it and I will have a discussion about it in next week's seminar. A very good student of mine who started studying experimentally choice-reactions during the last term, will comment on it. Of great importance is what you say on p.129 on realisation. You have indeed succeeded accordingly to test choice-reactions directly. The symbolic realisation is fine. I still do not understand all your distinctions. The word 'disjoint' for instance, raises difficulties for me. But I will certainly get through because you write so clearly. The analysis of motives, the fundamental distinction between value and feeling, the precise distinction of values, etc. seem to me to be of great importance. I cannot mention all points, but can say in general that I owe much to your book already and will still owe more to it.

I was in Bologna, where we missed you. These were nice but hard days. I found it very interesting to meet Bergson personally. He spoke in an insightful and stimulating fashion. Ach it seems to me, has answered to Selz in a conceited and dogmatic fashion without succeeding to refute him. Stumpf is of the same opinion. Selz will react and believes he is right on all points.

.....
.....
.....

Yours faithfully,

* Michotte and Prum (1910).

Oswald Külpe

Miscellaneous letters to Michotte 1911-1956

From Oswald Külpe

Bonn, October 20th, 1911

Dear Friend,

My hearty thanks for sending your work on logical memory* which I received on my way to Italy and read there and later on in Bozen with much interest and gratitude. Of great value to me are the fact that logical memory does not differ principally in any way from mechanical memory, further the enlightening explanation of individual differences, the simple explanations of the superiority of logical memory, etc. In the theory of the effect of reactions, I have missed the point concerning the explicit definition of tasks. The mere constellation of stimulus-word and relation is insufficient for the appearance of the reaction-word (c.f. p.75, end of page). But you have probably not specifically mentioned the joint action of the task because it was obvious to you. I have enjoyed the careful treatment both in the qualitative and the quantitative parts.

I have written an essay of psychology and medicine in order to recommend the independence of the former. You will receive it as soon as it is published.

Italy was interesting but hard. I am not starting the semester as fresh as I had wished. Work is going on with applications at the institute. You must have seen our last number. I hope you liked it! I particularly value the work of Westphal.

With hearty greetings and wishes,

Yours faithfully,

* Probably Michotte and Ransy (1912).

Oswald Külpe

from C.S. Myers

15 May 1914

Great Shelford
Cambridge

Dear Professor Michotte,

I have been asked to assemble a programme for the Psychology Section of the International Congress of Philosophy which is to be held in London from 31 August to 7 September 1915. Is it possible for you and any of your school to contribute papers which will show the philosophers the value of the experimental method applied to the 'higher' psychical processes and the principal contributions to our knowledge of those processes made by the school of Louvain? It would be an admirable opportunity of doing so, and I look forward to hearing that you are in sympathy with my proposal.

Yours very truly

Charles S. Myers.

From P. Guillaume

Faculté des Lettres
77 Boulevard Saint Marcel
Paris XIII

Paris, January 10th, 1946

Before thanking you for kindly sending the galley-proofs of your book, I wanted to read it and took it with me to the country where I spent the X-mas holiday and enjoyed some fresh air. I thus could study it closely at leisure near the open fire. The book has made me perceive still more clearly than the beautiful experiments you allowed me to watch in October, the far-reaching effect of your study, and the probability of new developments. And indeed, if there exists anything like a "phenomenon" of causality - in the way there exists a phenomenon of form or of movements movement - it is the whole psychology of intelligence which is being renovated thereby and is open to an experimental enquiry liable to replace the verbal explanation with which one has satisfied oneself up to now.

The accusation often made against the Gestalt conception to be only applicable to the field of perception ceases to make sense, since this latter domain finds a new extension and since it is necessary, for every notion, to define the "phenomenal" aspect under which it originally exists.

I am therefore very happy to be able to present some of your experiments to my students and to stress their significance. I moreover imagine what will be the reaction of those (students in philosophy and psychology) who will watch them. They will probably all object that their perception of this causality results from a projection - from an Einführung of the kind discovered by Th. Lipps - a thesis equally difficult to prove or disprove and which has little value to my eyes, but against which I would like to be armed because I know that it is from it that objections will come.

I will have some of the discs you describe made for me. In case I encounter some difficulties, ^{I then} to call on your kindness again.

Many thanks again for your kindness and best greetings for the New Year.

Paul Guillaume

from Kurt Lewin
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
Research Center for Group Dynamics
Department of Economics and Social Science
Cambridge 39. Mass.

19 March 1946

Dear Michotte:

It was wonderful to get the letter from you. I am glad to know that you and your family survived and that you were even able to work. I am very much interested in your experiments on "perception of causality". It happens that we too have become interested in this question in regard to social processes, but we are very much in the beginning and have no results yet so I am very eager to learn all I can.....

Your old and good friend,

Kurt Lewin

from E.L. Thorndike

Edward L Thorndike
501 West 120th Street
New York City

April 29, 1947

Dear Prof. Michotte:

I have read the "Perception of Causality" with interest and profit and congratulate you upon this very important research in the perception of events. I agree with you that the perception of events is as genuine a fact as the perception of objects. Your negative results concerning qualitative causality are very important. A beautiful piece of work! May you continue with more like it.

Yours truly,

E.L. Thorndike.

from R.H. Thouless

83 Newmarket Rd.
Cambridge

December 26th 1947

My dear Michotte,

.....I am very glad to have received the full volume of your Festschrift which I am studying with profit. It gives a good picture which has been lacking of what is going on in various psychological laboratories since communications were interrupted by the war.

I was also very glad to receive your own volume on perception of causation. Its line of thought is one that is very welcome to me since it fits in with the kind of idea of perception that I have formed although it goes beyond anything that I had previously had any idea of. It seems to me to strike out a new line of research in perception which is a very profitable one. It seems to me to make very difficult the somewhat mechanistic view of perception which Kohler has developed.

Yours very sincerely,

Robert H Thouless

from Fritz Heider

64 Kensington Avenue
Northampton
Massachusetts

March 28, 1947

Dear Professor Michotte:

Thank you very much for sending me your book on the perception of causality. I cannot tell you how interested I was in reading about your methods and results, and how greatly I enjoyed the tone of the whole book. It is a pioneering study in an important field.

I hope you have received the offprints I sent you. I had much fun with the moving picture which is described in one of them. Since in a moving picture the movements of the figures are more free the impression of people or animals moving about is stranger, and can be produced without your ingenious swimming or crawling motions. Thus one obtains phenomena belonging to social perception and phenomenal social causation, which in some cases is different from the more mechanical causation you investigated.

For instance, one can get the impression of causation even when the movement of the "patient" has not the same direction as the movement of the agent. That happens in the movie when the big triangle chases the circle in the "house".

Your theory of the "ampliation" of the motion is extremely interesting. I wonder whether it does not have rather far-reaching implications in regard to our ideas of unit formation.

I suppose you have heard that Kurt Lewin died of heart failure about a month ago. It was a great blow to all of us.

With many thanks,

cordially,

Fritz Heider

From G. Revesz

Amsterdam,
June 12th, 1955

Dear Friend,

I can hardly find any words to thank you for your great valuation of my book on the personal and social life of the blind, as appears from your letter. This rejoices me specially because, as you know, I value your work so highly.

I heard with interest that you are busy working on substantiality, as a counterpart of the perception of causality.

We are old intimate friends who appreciate our respective work from their objective value, On the other hand, we are - scientifically speaking - two totally different personalities. You are the classical type, I am more of a romantic. You stick to your problems, which you approach and value from all essential sides. I try to bring with the help of psychological methods various domains foreign to psychology within the sphere of psychology. Due to the passion we both have for our science, we have kept our youth. I often feel that we are as young as our young colleagues.

Our only mistake is that we meet so rarely and so briefly, although we are the best and well mutually understanding friends.

With best greetings
Yours old friend

Geza Revesz

from Edwin Boring

HARVARD UNIVERSITY
Psychological Laboratories

27 April 1956

Dear Michotte:

How delighted I am that you have been elected to the National Academy of Sciences in the United States. It is a recognition of your services to science and psychology that should have come long ago, but at least it has come now and many of us are extremely pleased. Your colleagues in psychology are Bartlett and Pieron. Your predecessors are Janet and Thomson and Burt, and, before them, Stumpf and G.E. Müller and still further back Wundt, of course. But I do not so much congratulate you as I do ourselves. We honor ourselves by such an election.

Sincerely yours,

Edwin G. Boring.

6

22

B: Pier. JAN

From Pierre Janet
Sanatorium Vert-Lapin
Avon-Fontainebleu
Seine et Marne
January 14th 1942

My dear friend,

After writing my last letter I restudied your article "La causalite physique ,est elle une donnee phenomenale?" which I had only gone through quickly. It is a remarkable study , as I have already told you ,important both for the philosopher and for the psychologist. As usual, one only assimilates a study by digesting it , by integrating it further with one's own ideas. I have been absorbed for years by the investigation of persecution deliria and I am writing a thick book , which requires much thought , on the feelings which go with these diseases. In my articles : in the Journal de Psychologie (1932), in the Annales Medico-Psychologie and , first and foremost , in my observations on "L'examen de conscience et les voix" in Scientia (1938) , I have tried to show that the essential feature of the disease is an illusion of objectivation. In his immediate feelings , and without inference , the patient attaches to society , integrates in his notion of the social , psychic phenomena which, to us, are part of his own personality. This is the theory I proposed for the peculiar hallucinations of the persecuted and for their feelings of anxiety .

After having read about your experiments , I am wondering whether the patient's illusion is not akin to the feelings of causality you illustrate. You describe experiments in which objects A and B fuse into a single object. There must be

cases where the objects fuse in B and others where they fuse in A. You must have found conditions that bring about either of these fusions. In my observations, feelings of effort or activity in the subject result in a confusion with the active subject, here with A, while feelings of anxiety, reaction to failure, the feeling of being defeated by a more active enemy, bring about a confusion with the social, thus with B. You must have experiments in which the swiftness of A, its shiny or coloured aspect, allow it to predominate over B; or again, cases in which these qualities in B bring about the inverse situation. The comparisons seem all the more justified when you frequently use expressions such as pushing, taking with, advancing, receding etc. all terms which pertain to social perception. You often seem to animate and to personify your two objects; you know that I draw a major distinction between the perception of material and social objects. In any case, in the chapter I am writing at present, I take the liberty to mention this apparent analogy between your experiments and the illusions of my patients. You see, I cling to you as A does to B, I hope you will not be bothered by this promiscuity with a simple alienist.

What a sad era ! Is it the fault of the times themselves or of the circumstances they contain ? Winter adds to it, I am trapped here by cold and snow, I am even prevented from going to Paris for appointments in an ice-cold apartment without water. Moreover, my sister just died at nearly 80, a warning for my 82.

Please excuse these sad reflections and believe in my faithful feelings:

P. JANET.

The Nativism - Empiricism Issue in Perception.

In 1942, Boring² referred to one of the "dreary topics" in the study of perception, namely, the nativism-empiricism controversy. Today the question of whether perceptual experience depends on innate processes and mechanisms (the nativistic position) or is the outcome of learning and experiential processes (the empiristic view) is often dismissed as meaningless and sterile. A fashionable resolution of the controversy is the view that all psychological functions result from an interaction of biological and experiential factors. The concept of interaction, however, is not at all new. As Boring indicated, nativists have admitted that learning plays some role in perception and empiricists have accepted the importance of innate structures. Moreover, the interactionist position does not achieve the scientific goal of understanding the nature and behavior of the perceptual system. This goal requires the analysis of the separate contributions of innate functions and learning processes to perception, in other words, a clarification of what it is that interacts.

The nativism-empiricism issue, like most problems in psychology, has its origins in philosophy. The present-day scientific student of perception cannot always be certain of the proper interpretation of a particular philosopher's thought-since philosophical commentators themselves disagree. For example, was Bishop Berkeley an absolute empiricist arguing that visual spatial perception is entirely learned on the basis of the tactile sense, or was his main interest the analysis of what it means to perceive an object as near or far? In general, psychologists have accepted the first interpretation of Berkeley's ~~thought~~.

To sketch briefly the origin of the issue in modern philosophy: Descartes position favored the nativistic position, that visual properties of objects

such as their shape, size, motion, etc., were based on innate ideas. British philosophers like Hobbes, Locke and Berkeley are described as empiricists; they stressed the role of experiential factors in perception, in particular, associative learning. No empirical evidence was available to these theorists, their discussions were based on logical argumentation and introspective observation. Locke did refer to one relevant empirical question which was raised in a letter to him by Molyneux. Molyneux asked whether a man blind from birth and given the power to see would be able to discriminate a sphere from a cube by vision alone. A number of such cases (congenital cataract patients) have occurred over the years and their experiences after surgery interpreted as support for empiricism. These data will be discussed below.

The scientific study of perception really began in the 19th century and proceeded at a rapid pace. In his Physiological Optics, the great Helmholtz summarized an enormous body of knowledge, much of which was the result of his own investigations. It was Helmholtz who first formulated clearly the theoretical dichotomy, on one side The Innate or Intuitive Theory of spatial experience and on the other, the Empirical Theory. The Innate Theory, he argued, ruled out any further investigation of the origin of spatial perceptions by regarding them simply as a function of hypothetical neural mechanisms assumed to be inborn. Helmholtz, a strong Empiricist, claimed that all aspects of perception were determined by experiential factors such as memory, judgment, expectation and reasoning. Sensations, he maintained, provide only a "sign" for external objects and events; we have to learn how to interpret these signs.

On the nativistic side, Hering, his theoretical rival, explained the experience of location and depth in the visual field on the basis of fixed retinal mechanisms and proposed particular neural processes as the basis for such phenomena as color contrast and constancy. However, he never denied the role of

memory factors in perception.

Helmholtz's influence on perceptual theory has remained a powerful one and his concept of "unconscious inference" has reappeared again and again. With the establishment of Wundt's laboratory in 1879, psychology officially became a science and an academic discipline. From that date until well into the 20th century the empiristic outlook remained dominant. Whether the psychologist was a structuralist, functionalist, behaviorist or Act theorist, he implicitly or explicitly accepted the view that the raw sensory data had to be elaborated into a percept by psychic functions based on past learning.

Of course, during this period a great deal of research in perception was undertaken by ophthalmologists, physicists and physiologists who, in many cases, favored nativism. In psychology, however, the main exponents of nativism (in a modified form) were the members of the Gestalt School. Since the fundamental concept of Gestalt Psychology is that experience is inherently organized, the idea that the experienced world is initially a mosaic or patchwork of sensations was completely unacceptable. The hundreds of pages devoted to problems in perception in Kurt Koffka's "Principles of Gestalt Psychology" constitute a relentless polemic against empiristic concepts.

Gestalt psychologists opposed the definition of perception as the interpretation of sensation; they did not consider sensations to be part of phenomenal experience but ^{perceived them} ~~rather~~ products of artificial analysis. They showed that the effective stimulus for a percept is relational in nature, thus making it unnecessary in many cases to refer to past experience. In place of the innate anatomical structures of the nineteenth century nativist they substituted the concept of dynamic self-distributing processes in the brain, i.e. sensory organization. These processes of organization are neither learned nor the products of inherited neural "wiring". Gestalt theorists did not deny that past

experience plays a role in perception. They insisted, however, that the empiricist provide a plausible account of how a particular perception could be learned.

Apart from the Gestalt school, perception was not a major research area in psychology for many years. In the 1940's there occurred a remarkable revival of interest in problems of perception, mainly from an empiricistic standpoint. A neo-functionalist approach called for bringing perceptual investigations from the laboratory darkroom into the "market-place". This so-called "New Look" psychology considered perceptual experience to be at the mercy of personality and learning variables such as needs, motives, emotions, reward and punishment etc. An enormous number of experiments was generated by this approach but most frequently, the effects of these variables were found to be extremely small. Moreover, most of the research did not deal with perception in the traditional sense of the term (i.e. the experience of spatial relations, movement, form, perceptual constancy, color etc.) but with the processes of recognition and interpretation. The results of this research, even if valid, are not, therefore, relevant to the nativism-empiricism issue.

A different theoretical current at the same time arose from the dramatic "demonstrations" of Adelbert Ames, Jr. His distorted room was perceived (from a particular viewing point) as a normal rectangular room, the rotating trapezoidal window was seen to oscillate, a stationary expanding and contracting luminous balloon was perceived to approach and recede in space. Discussions of these demonstrations and conferences with psychologists, educators and philosophers led to the development of what Ames believed to be a revolutionary new interpretation of the philosophical and psychological problems of perception. This approach came to be known as transactionalism and represented a

radical version of empiricism. Helmholtz's concept of unconscious inference was incorporated into the view that perception is based on assumptions developed in the course of our purposive action in the world. With one or two exceptions the transactionalists did not provide any direct evidence for the causal role of these assumptions, nor did they give a clear or coherent theory of purposive learning. No alternative to past experience was considered by them as viable. According to the transactionalists, the distorted room is seen as normal because most frequently in our world rooms are rectilinear, the trapezoid is seen to oscillate because of our assumption that windows are rectangular etc. At present little remains of transactionalist theory; the demonstrations, however, are valuable. The distorted room and the chair demonstration are useful for making clear to the student that perception is determined not by the physical object in the external world but by the retinal stimulation to which it gives rise. The trapezoidal oscillation can be explained without reference to learned assumptions about the rectangularity of windows.

An empiristic account of the origin and development of form perception was presented by Hebb in his book, "Organization of Behavior" (1949). Hebb was convinced of the validity of empiricism mainly through two lines of evidence. First was the work by Riesen (1950) of vision in dark reared chimpanzees and second, the analysis by von Senden (1960) of some 60 case reports describing visual experience in congenital cataract patients after surgery.

Riesen raised chimpanzees in complete darkness for many months beginning soon after birth. In later studies chimpanzees were reared in darkness with but 90 minutes of diffuse light stimulation each day. Tests in an illuminated environment revealed serious deficiencies in the visual behavior of the subjects. For example, they were significantly retarded in mastery of visual discrimination problems as compared with normal controls.

29

According to Hebb, form perception in these animals was deficient because there was no opportunity for perceptual learning. A different interpretation - that darkroom rearing interfered with the normal maturation of the visual system - was later confirmed in histological studies by Riesen and others. There is convincing evidence that light stimulation is necessary for the normal functioning of visual structures but this fact does not support an empiristic explanation of form perception nor deny the validity of a nativistic account.

Von Senden's book has had a curious history; his data have been cited by both nativists and empiricists as evidence in favor of their respective positions. Wertheimer pointed out in 1951, that it is necessary to distinguish between the first hand data in the case reports (usually written by the surgeon), von Senden's analysis of these reports and Hebb's interpretation of von Senden. According to Hebb, the data indicated that patients first experienced a kind of amorphous mass and gradually, with great difficulty, learned to see and discriminate shaped objects in their visual field. There is no unambiguous evidence, however, that patients could not discriminate forms. When shown a sphere and a cube and asked which was the cube and which the sphere, patients indicated that they were unable to say (but not always: see the Latta case- 1906). What is the source of their difficulty - that the objects are not perceived as different shapes or that the patients are unable to identify the two perceived shapes? This ambiguity is perhaps the explanation for the fact that nativists and empiricists alike have cited the cataract data. Actually neither side of the controversy should rely on this evidence. None of the case reports is based on careful, systematic observation and experimentation. There are serious problems with visual function such as muscle cramp, nystagmus, etc.; there are motivational and emotional factors which affect the ability of the patient to adjust to a newly experienced visual world; many patients had difficulty in

verbalizing their experiences and so on.

Thus the evidence adduced by Hebb as proof that form perception is learned is not at all convincing; the validity and fruitfulness of his cell-assembly model of perceptual learning, however, must be evaluated on other grounds.

Thus far in this rapid historical survey we have encountered no scientific (in the sense of strong observational and experimental data) resolution of the nativism-empiricism controversy. The middle 1950's saw a surge of research efforts utilizing new methods and new techniques; also it became possible to study perception in animal and human infants and to explore the flexibility and modifiability of perceptual functions in adult subjects.

Perhaps the most significant advance has been a growth of theoretical sophistication which is making possible a more fruitful formulation of this ancient controversy.

In the past nativists and empiricists argued about perception in general or about large areas of perceptual functioning. In recent years research questions have become more specific. For example, is a particular distance cue innately based? What is the origin of size constancy? How does form perception develop? A nativistic answer for one aspect of perception does not preclude an empiristic account of another.

Secondly, greater clarification has been achieved with respect to the concepts of learning and innateness. Traditional nativism assumed the existence of fixed structures and mechanisms which made it difficult to account for the flexibility and adaptiveness of the perceptual system. Empiricism, on the other hand, could not provide an adequate description of the learning process. A position which is emerging from the research and theorizing of the past decade can be characterized briefly as follows: What is "built-in" or innate is not fixed pre-wired structures but rather rules or principles which

guide the learning process along certain paths. For example, Wallach (1974) has shown that position constancy is quickly re-established when new relationships between retinal image displacements and head movements are imposed by the use of lenses. This flexibility can be understood by assuming the existence of a principle or rule which directs the perceptual system to discount image movement which covaries with head movement. A modified nativism of this kind has been proposed by Bower (1974). In order to explain how the perceptual system can adapt to the changes in proximal stimulus information produced during the course of the infant's growth.

It is clear, then, that the nativism-empiricism issue remains as an active theoretical and research problem.

REFERENCES

- Allport, E. H. Theories of Perception. Wiley 1955.
- Ames, A. Jr. An Interpretive Manual. Princeton University Press 1955.
- Boring, E.G. Sensation and Perception in the History of Experimental Psychology. Appleton Century 1942.
- Bower, T.G.R. Development in Infancy. W. H. Freeman, 1974.
- Hebb, O. Organization of Behavior, John Wiley, 1949.
- Wertheimer, M. American Journal of Psychology 1951.