EYELESS SIGHT

JULES ROMAINS

From the painting by Paul-Emile Bécat

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A STUDY OF EXTRA-RETINAL VISION AND THE PAROPTIC SENSE

BY

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TRANSLATOR'S NOTE

M. Jules Romains has very kindly supplied for the English edition of his *Vision Extra-Rétinienne* some additional material dealing with the origin and reception of his researches, and with tests recently held in Paris in the presence of M. Anatole France, Dr. Cantonnet and others. This appears in the form of two Appendices.

The translator also desires to thank Mr. J. H. Paxton of Yale University, for assistance in preparing the translation for the press.

C. K. O.

PREFATORY NOTE

No bibliography will be found in the pages which follow. The principal question with which I am dealing is new. I cannot consider as real sources the few brief passages in earlier works[1] where a presentiment of the phenomena we are going to study appears, nor as a tradition of scientific research a few observations collected in chance circumstances, noted without criticism and bearing witness at most to a superficial and easily satisfied curiosity.

The ideas of physics, physiology and psychophysiology already acquired by science and introduced in the course of the exposition, are sufficiently current for references not to be indispensable.

[1] When I had already finished this little work (February, 1919) a recent book by E. Boirac came into my hands: *l'Avenir des Sciences Psychiques*, which appeared in 1917. It is, I believe, the most recent general work which touches on these questions. I advise the reader to consult it. He will notice how the problem of "eyeless vision" presented itself in 1917 to so well informed and competent an author. Boirac, moreover, is the only authority who has given us, in my opinion, a few sensible and penetrating pages on problems of this kind. I refer to his *Psychologie inconnue.*

As for the questions of histological morphology which play an important part in this work, those of my readers who are not sufficiently familiar with them and who desire to consult the texts, can refer to the excellent bibliographies that Ruffini and Lefébure have appended to their original papers in the *Revue générale d'Histologie* (1905 and 1909). Nothing essential has appeared, to my knowledge, on these subjects since then.

I wish to thank here a distinguished physicist, M. Perrot, formerly of the École Normale Supérieure, professeur agrégé de physique, to whom I more than once submitted the physical hypotheses which I was led to consider, and to whom I owe most interesting objections and most judicious suggestions. The very fact that he found himself dealing with strictly psycho-physiological questions gave him a complete freedom in his critical attitude, and I did not consider my task accomplished until I had settled the special difficulties which he raised.

Some months before publication the present work was shown to two of my friends: to Dr. Georges Duhamel who not only enjoys

high intellectual distinction but has had long experience as a laboratory physiologist, and to Dr. André Nepveu, to whom we owe, among other works, a remarkable thesis on the *Photo-irritabilité de l'iris*. Both have suggested modifications and additions to the text. I thank them for these.

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CHAPTER I

REMARKS ON THE PRESENT RELATIONS OF MORPHOLOGY AND PHYSIOLOGY IN THE HISTOLOGY OF THE SKIN

"THE morphologist who bases his opinion on the study of topography and on comparative characters can say whether a nerve-ending is motor, sensory or vaso-motor. But his judgment, in certain cases, may be merely relative, especially when there is a lack of facts or of arguments drawn from physiological observation, from experiment and from pathological anatomy; further, when the morphologist, leaving the realm of generalities to attack particular cases, begins to propose or to discuss the attribution of this or that sensation to this or that nerve-ending, and when he attempts to fix functional

modalities, he constructs what is merely a house of cards. Too many houses of cards have been built and are still being built, particularly as regards the nervous system. . . . It is true that physiologists have shown a singular and inexplicable lack of interest in the face of the considerable progress of morphological analysis. This may serve in a certain degree to justify the invasion of the realm of physiology by morphologists, but it does not at all justify the inaction of the physiologists."

All who have paid attention to certain problems in contemporary histology and who have also been concerned to maintain the connection between histology and general biology will, I think, recognise the acumen and correctness of these words which are taken from the remarkable article on "Les Expansions nerveuses de la peau," that Prof. Ruffini published in 1905 in the *Revue générale d'Histologie.*

We find, in fact, once again to-day under a new form, the old discordance, or rather failure to give mutual support, of anatomy and physiology, of which such curious examples are provided by the sixteenth and even seventeenth centuries. Anatomy has become microscopic,

and provided with apparatus and with a truly admirable technique, has made rapid progress in the last sixty years and has assumed, on many points, an assured and almost definitive character which is highly satisfactory. But, however much morphologists may be desirous of avoiding adventures and of not endangering the fine economy and solidity of their science by the addition of doubtful material, it has been necessary for them from time to time to ask themselves what could be the function, in the whole system of the living being, of this or that arrangement, so carefully described by them. More than one histologist, consulting only his own personal taste, has doubtless avoided such questions and been content to make faultless diagrams just as many an astronomer has been satisfied with drawing up catalogues of stars or celestial maps and has mistrusted what he regarded as cosmogenic reveries. But it is even more difficult in biology than in astronomy to retain the purely descriptive standpoint. Thus it comes about that even the most cautious histological works find room for physiological hypotheses which attempt to assign function. The impartial reader is thus forced to recognise

how great is the contrast which prevails, for the most part, between the minuteness, the patience, the scrupulous severity of descriptive investigations, and the kind of levity with which functions are assigned. According to Ruffini, the physiologists are in this respect the chief offenders, since it is their failure which forces the morphologists to undertake a task which they do not at all desire. And in addition, as we shall show more explicitly later, morphologists do not possess, in general, either the training or the apparatus which would lead to success.

The reflections of Ruffini, quoted above, are expressed in connection with the histology of the skin and there is no instance, we believe, in which they are better justified. The structure of the skin is, on the whole, well known, particularly in the case of man. The discoveries which remain to be made on the morphological side can hardly be of first-class importance, although it is always rash to assign limits to the science of the future. On the other hand the physiology of the human skin has not, hitherto, gone much further than the data of common knowledge. To have established the protective rôle of the skin is no very meritorious achieve-

ment, and the first-comer had no difficulties in reaching a decision regarding the cutaneous secretions. More subtle and more mysterious questions arose. Some were formulated by morphology itself. Others came as the suggestions of general biology. It is impossible, for instance, to think of the special importance and extreme variety of the functions of the skin in the lower animals without being led to ask whether the human skin has not retained, in its ascent, unsuspected abilities. Doubtless all sorts of special organs have been formed in the course of animal development, have acquired considerable elaboration and perfection, and have become localised so as to occupy the safest and most advantageous position with regard to the external environment. They seem also to have taken over and monopolised many of the primitive functions of the skin. But we know that nature sometimes adopts very complex and highly ambiguous solutions and that she very rarely takes care to satisfy the demands for economy and simplification which our logic makes. Where we like to think of a clearly defined situation, it happens that nature appears to make the most of things as if by reversions,

substitutions and subterfuges. It is wise then to avoid over-simple conclusions and to continue to subject experience to ingenious and unexpected interrogations, even when the evidence we possess would seem to make this unnecessary. Would it not be safe to suppose, for example, that in an animal like man, provided with a respiratory apparatus so elaborately prepared, which has had full time to acquire the necessary size and complexity, and to adapt itself to relatively stable conditions in the environment, it would be superfluous to contemplate the persistence of the respiratory function in the skin? Would it not even seem probable that a skin liberated from this task would have had more freedom to specialise in its own function? Are not the sweat-glands found to perform without any sign of obsolescence, a sort of nephritic function, whilst it is impossible to consider the kidney as a recent or in any way tentative organ?

I shall be told that it was physiology which discovered these subtleties of nature and that it does not in the least deserve, therefore, the reproaches and reservations which we have suggested. But has it everywhere proceeded with

the same distrust of simple solutions? Have we been certain in all cases that we really possessed the method and the equipment necessary to bring to light unknown or misunderstood phenomena? In order to discover this or that function or to make its forms and its importance known, it has already proved necessary for the morphologist to extend in a remarkable fashion his habits and his methods of research, unless he was to leave the matter entirely to his colleagues the physiologists. The analysis of the structure of a gland under the microscope, is one thing; but it is a very different thing from the chemical analysis of its products and above all the appreciation of its place in the general economy. But here the passage from one attitude to another is still relatively easy, and if the two tasks are undertaken by two different investigators they will not have to make great efforts to remain in co-operation. They speak two dialects of the same tongue and there is a good chance that their labours will run parallel. In the histology of bone, for example, this co-operation can often be maintained. But the histologist who studies the skin finds himself in the presence of structures and arrangements the

deciphering and interpretation of which will require help of other kinds and much more daring extensions of method.

In the course of his analysis he meets with certain expansions of nervous tissue and even with various minute organs of definite types which have an exceptional interest through their containing nervous tissue. The morphologist might be content to describe them, leaving to others the responsibility of interpreting them physiologically. But the neglect of the problem by the physiologists which Ruffini mentioned, creates an additional responsibility for the morphologist. He holds himself bound to give a reply to a question which arises of its own accord, and so he starts assigning functions to the entities which he has isolated and described. We can well remark, with Ruffini, that the descriptions are incomparably more serious and better founded than the attributions. It is certainly no longer enough to add the resources of physiology to those of microscopic anatomy. A certain corpuscle is said to have a "tactile" function. This is of an altogether different importance and a different degree of rashness from affirming that a certain gland elaborates a certain

hormone. Whether you wish it or not, whether you perceive it or not, you are dealing with psychology, and it is to be feared that you are doing so without all the precautions or all the resources which are desirable. You would certainly smile at a psychologist who, having to allude to an anatomical idea, seemed to be unaware of the existence of the microscope, and had the same idea to-day as Descartes of the structure of the nerves. But perhaps psychology has also its microscope, and it is far from prudent to intrude on its domain without making oneself familiar with modern methods and apparatus.

But what do we actually find? The histologist who is so scrupulous in his descriptions, so careful to give his method the benefit of the latest technical innovations, so prompt to point out and denounce traditional errors in his own field, however much they may be sheltered by illustrious patronage, is much more easily contented where a psychological idea is concerned. Everyone knows that the skin is an organ of touch. We have our eyes for seeing, our ears for hearing, our skin for receiving impressions of touch. There is no need at all of psychological authority in order to discover this.

Common sense has never doubted it. As soon as the histologist finds in the skin an organ or an arrangement of any sort, where the nervous tissue does not visibly have a motor or vascular function, he attributes to it *a priori* a tactile function.[1] When the progress of analysis revealed the existence of several quite distinct types of organs, it became necessary to admit a corresponding diversity of function. But people have persisted in finding this diversity in the operations of touch itself, without any great display of ingenuity, it must be confessed. They have limited themselves to a literal translation of the topography: to the small organs nearest the surface the most "superficial" tactile impressions have been attributed; to the small organs of the second zone, slightly "deeper" impressions, and so on. This is an easy way of getting out of the difficulty, and one can well believe that Ruffini does not care greatly about the defence of such hypotheses.

One of those too rare interventions of psychophysiology in these questions has already rendered morphology the service of pointing out the

[1] Thus we find at this moment in works of pure description the words "Tastkörper," "Tactile corpuscles," "Corpuscles du tact," etc., as if the thing were a matter of course.

existence of the *thermal* sense and has stimulated it to look for the appropriate organs. It is true that here too common sense would suffice. We all know that as we put our hand near a stove we shall feel whether it is alight, and that a laundress estimates the temperature of an iron by putting it close to her cheek. But experimental psycho-physiology is perhaps able to bring to morphology more unforeseen and striking revelations.

When the morphologist is working on an animal species slightly removed from ours, he is willing enough to give a place to the unknown; and if he isolates an apparatus without a clear analogy to explain its purpose, he is content not to formulate a precise hypothesis. We admit for example that animals are able to feel certain external agents, or certain methods of action of these agents, which do not affect us; we admit also that they may be able to receive the same agents as we do in a totally different manner from ours. We are then ready to find, without too much astonishment, macroscopic or microscopic organs which, while they present the same appearance as sensory organs, have no close analogy with our own sensory organs, and

to hold in abeyance, in this case, the problem of attributing functions to them. The morphologist is well aware that this problem can only be formulated and treated ultimately in a laboratory of zoology or of animal psychology, by the aid of material processes and mental attitudes which have nothing in common with observations under the microscope of dead and stained sections of tissue.

But, when it is a question of man, then scruples ordinarily disappear. It seems to be agreed that man, at least man from the point of view of psychology, is not a mystery to man. The idea that there could be in man a completely unknown sense, a function foreign to ordinary consciousness, does not arise. The very histologist who conceives of the existence of unknown forms of karyokinesis, and puts in operation the most ingenious technique to detect them, seems to believe that he knows all about the mind and that it is useless to leave a place for the unknown in psychology.

I hasten to say that this observation is made without bitterness and is not a complaint. Histology could not entirely avoid a work of interpretation which nobody seemed willing to

take up, nor could it reach other results with the means at its disposal.

If, in many cases, a strongly specialised science is sufficient to itself and reaches discoveries without leaving its own method and technique, in other cases a discovery is only obtained by means of an unexpected combination of methods. Two scientific methods which seemed to be eternally unrelated, suddenly meet and the discovery flashes out from the encounter. This result occurs often enough between two neighbouring sciences. It is infinitely more rare in sciences far removed from one another, but when it occurs it is perhaps still more productive.

CHAPTER II

FROM HISTOLOGICAL PHYSIOLOGY TO EXPERIMENTAL PSYCHO-PHYSIOLOGY

I SHOULD like the reader to have finished the last chapter with this quite simple idea, that every problem of assigning a function in regard to those nervous expansions in the skin which are non-motor, or centripetal if the term is preferred, is at least, if it be not more, as much a problem of psycho-physiology as a problem of histology. Reduced to this brief formula, our remark has the appearance of a truism, and for this I must apologise. But in scientific investigation, it often happens that the most surprising, the most paradoxical discoveries, owe their origin to some very modest truth having been taken into consideration.

Without microscopic morphology we cannot hope to know anything precise as to the structure of any sensory organ situated in the skin.

And furthermore: we cannot even pretend to discover its existence, or to determine its exact position, the order of magnitudes in question being what it is.

Without psycho-physiological experimentation we can discover nothing and conclude nothing of value as to the function of any sense whatever. And furthermore: we shall lack all sorts of indispensable landmarks which would aid us in fixing the anatomical positions of the corresponding organs, or in identifying doubtful organs.

If, by an hypothesis already contemplated, the investigation is found to point, with or without the awareness of the inquirer, to a sense which is still to be discovered, morphology is radically incapable of bringing about or even of preparing this discovery. It can only indicate to the investigator anatomical entities still without physiological functions attributed to them, or entities provided with doubtfully acknowledged attributes.

So far as can be judged, in this hypothetical case the discovery of the function is only possible by methods peculiar to experimental psycho-physiology. As to the discovery of the

organ, or its identification, this can only follow later, thanks to a close collaboration of psycho-physiology and histology.

Doubtless the correspondence which we have just pointed out is not altogether complete. When it is a matter, for example, of the blood or of the lymph, a macroscopic physiology corresponds to a macroscopic anatomy, and on the other hand to a microscopic anatomy there corresponds a physiology also microscopic, although here too may be observed that same sort of failure to lend mutual support of which we spoke above. But, in the matter with which we are concerned, we should seek in vain for a physiology, and above all for a microscopic psycho-physiology corresponding to the analysis of the same order. It will be easily understood, and will appear clearly in what follows, that if we point out and deplore this gap, it is not in the least because it deprives us of a vain satisfaction in logical symmetry, but because it forbids certain crucial experiments and prevents certain strong probabilities from becoming, for our full contentment, certainties.

I do not wish, however, to imply that this microscopic physiology and psycho-physiology

could now spring into being and give results. The gap which I am indicating is due to the nature of things and not a lack in human initiative. If it were to disappear some day it would only be as a result of a considerable enrichment of our present means of approach.

In brief, to limit ourselves to the present, what can be achieved and what we have attempted to achieve is a collaboration of experimental psycho-physiology necessarily macroscopic, and histological analysis.

* * *

Having elucidated these questions of method, let us see briefly whence we start in histology.

Our knowledge of the human skin, we have said, is very advanced. During the last twenty years, above all, histology has succeeded in passing a critical point, I mean in making a synthesis of apparently contradictory indications furnished by diverse techniques.

In particular such works as those of Dogiel, Ruffini and Lefébure, have brought to an issue and completed what their predecessors had taught us concerning the topography and structure of the nerve-endings and small, more or less complex organs which occupy the skin. It is

here we should look for the anatomical substratum of every receptive function of the skin, whether this function be known or is still to be discovered. It is important, then, to recapitulate the almost unquestionable acquisitions.

In the deepest layer of the dermis we find the *arboriform terminations of Dogiel*, the *corpuscles of Ruffini*, the *corpuscles of Golgi-Mazzoni*, and the *corpuscles of Pacini*.

With the sub-papillary layer appear the *monolobar corpuscles of Meissner*.

In the papillary layer are found the corpuscles properly called *Meissner's* among which we can, if we wish, distinguish the *corpuscles of Dogiel*.

As for the epidermis it only contains two formations where the nervous tissue is involved: the *network of Langerhans* and the *menisci of Ranvier*, also called hederiform expansions or intra-epithelial baskets. We have here a quite considerable variety of organs and arrangements. Histology could have been limited to describing them, fixing their topography, making measurements and preparing statistics concerning them. But it has thought itself able to go further and to divide amongst them the modes of action of the single tactile function. With Lefébure, for ex-

ample, it has made conscientious efforts to justify a part of its inductions.

In this work of hypothesis, everything is not equally unsubstantial. To begin with there is no question that the skin is the organ or contains the organ of touch. I do not think it necessary to explain why. Consequently some at least of the nerve-endings of the skin should be playing a part in the tactile sensation. Further, some attributions rest on arguments of morphologic resemblance which are not negligible, and receive a fair amount of confirmation from psycho-physiological experiments. I admit, for example, that the considerations revived, developed and thoroughly investigated by Lefébure in his study of Meissner's corpuscles increase the conviction which can be entertained, that here there really are small tactile organs. It is perhaps imprudent to try to interpret all the details of their structure, including those whose interpretation remains contestable; but, on the whole, the hypothesis applies. In particular, the respective position of the nerve-bundles and the auxiliary nerve-cells, without having the fine simplicity the text-book diagrams offer, seems none the less clear enough. We are very

likely dealing with a distributive and amplificatory apparatus of pressures, and this conclusion is not after all much more audacious than those that an anatomist draws, by analogous processes of reasoning, from the arrangement of the middle ear. On the other hand what could be more gratuitous than the properties assigned to the hederiform expansions? They would be occupied with receiving the lightest tactile impressions, those of brushing and tickling. But are we sure that the corpuscles of the dermis are insufficient for this? The few rudimentary experiments which are appealed to in this connection are scarcely conclusive. Moreover the epidermis also contains the network of Langerhans. Why should we not limit to the network this function, which it seems morphologically very capable of filling, if we wish decidedly to give the epidermis a tactile function? I look in vain in the emplacement, distribution and structure of the hederiform expansions, for this clear use which I am willing to recognise in the case of Meissner's corpuscles. Let us go further. Are we sure of having eliminated, once for all, the hypothesis, formerly held, that one and the same nervous organ could, if need be,

assume more than one function? If I allow, and without any necessity, that a very light pressure applied to the epidermis is received by the menisci and transmitted to consciousness under the guise of a sensation of brushing or tickling, has it been proved that they are incapable of receiving and elaborating some stimulus of a totally different order?

Once more histology has had to be content with solutions which were too simple and too short. And let it not imagine that it has received from psycho-physiology all the assistance it can expect because some tables of tests with the æsthesiometer have been drawn up.

* * *

The method and the technique of psycho-physiology have been enriched by contemporary developments which are perhaps only comparable in importance to the introduction of the telescope in astronomy and the microscope in biology. Few people, however, have any accurate idea of them, even among those who claim to be specialists. The extraordinary productiveness of the new processes has not been fully perceived by most of the experimenters who use them, and *a fortiori* it is scarcely suspected by

those who have no practical experience of them.

I would say, with an apology for the inevitable crudeness of such general views, that it is possible to distinguish, up to the present, three periods in the development of psychology.

In the course of its first period which did not come to an end until the nineteenth century, psychology limited itself to presenting in a rigid and systematic form certain data of common experience, which we find with finer and more varied shades of difference and a more lively sense of reality in the literature and even in the conversation of men of the world. Thus at this stage the beginner can thoroughly appreciate a psychological work. He knows that he is going to learn nothing really new, nothing comparable to the discovery of a planet or a property of matter; but that an author is going to make an effort to give a personal turn to things which everyone knows, and this without making an appeal to any other procedure than the ordinary means of reflection.

In the second period, culminating in the years 1860–1880, psychology still made no discovery which was an intrinsically psychological dis-

covery, which notably increased what man could know of himself as a psychological being. But it made an entirely new departure and inaugurated a method often valuable for eliciting connections, hitherto unknown or only slightly known, between psychological facts which everyone knew, on the one hand, and the conditions, either physiological or sociological, of these facts, on the other. We can never have too much respect for the investigators who thus founded psychology as a science. We shall always owe to them an orientation, methods and useful mental attitudes. But I am persuaded that the future will not give them credit for any of those decisive discoveries by which other sciences such as astronomy, physics and chemistry, have marked their entrance into the modern era. Works such as those of Ribot on the Imagination or on Psychological Heredity are quite representative in this respect. We cannot say that they are not new, that they do not vivify in the most meritorious fashion the questions they discuss. But can we say that they contain anything comparable to those monuments at the same time definite and definitive which are constituted by discoveries of the first

rank? And what is a science, what is the life of a science in human history, if it is not a more or less logical, a more or less capricious sequence of discoveries?

As for the third period, it would be difficult to indicate exactly the beginning of it. It has been prepared and has had a way made for it by many works of the second period.[1] It has had as precursors, almost as founders, authorities whose preoccupations were not actually psychological. It is more essential to describe this period than to point out its acknowledged representatives.[2] What characterises it, is that psychology becomes through it a science of discoveries and of *special* discoveries, such, that is to say, as do not merely consist in showing unexplained connections between known psychological facts and facts of another order. Not that this psychology refrains in the very least from maintaining or from increasing the interrelation between psychological facts and facts of a physiological, sociological, or even physical order. But it considers that the principal

[1] By those same works of Ribot, among others.

[2] I am, none the less, reluctant not to mention here names such as those of Charles Richet, Pierre Janet, Georges Dumas, Binet, etc.

task of a science is to discover the unknown in its own domain. The task of the physicist is, first of all, to discover forms of energy, states of matter, and relations between forms of energy and states of matter of which man had as yet no notion at all. The task of the psychologist is, first of all, to discover what man as yet knows nothing at all of, in the psychological order. And let us not play upon the words. Making a new discovery is not presenting in a new way common ideas; it is not, further, outlining or building up a general system of explanation of known things, insusceptible to experimentation and resulting only in a satisfaction of speculative needs of the mind; it is not, even, venturing some ingenious hypothesis, the result of meditation but incapable of undergoing the test of a laboratory. A discovery is an annexation. The explorer discovers a continent, an island, or a mere islet, depending on his means and his luck. In each case it is a discovery. He has revealed to other men lands they knew nothing of, other men can go there after him, they can make all the observations they wish and even establish themselves there. The discovery becomes something impersonal, independent of its author, which

exists and subsists of itself, and which develops by itself. But having impressions from journeying in an old country is not a form of exploration or discovery at all, in the true sense.

The psychology of discovery needs a method and a technique of discovery. This method and this technique have not been improvised. As has occurred many times in the history of the sciences, the credit of having made the first rough sketch and dared to use it has not always come to the science which was in the end to benefit from it. In this case, the method of discovery which we propose to call the *method of detection*, in order that the originality of the process should be stressed by the use of a special term, had as originators the doctors and also certain empiricists. It has had difficulty in freeing itself from its origins and it still possesses some of their faults. From its medical origins it has retained what I will call the *pathological prejudice*. On the other hand its empirical origins have earned it the mistrust of certain good people, who tremble at the idea of being misled by the authority of quacks. It is then not without good cause that we stress this somewhat.

If we were to try to sum up in a word the essence of the method of *detection*, we might say that it consisted in bringing to light the idea of *regimes of consciousness* and in deriving experimental profit from it.

What should we understand by this?

A great many conditions, which, however, need not be specified here, have brought about in man as he appears in our civilisation and societies and in the present state of the physical world, a certain *regime* of psychological consciousness stable enough for it to have been regarded right up to the most recent times as the only possible regime of consciousness. Actually, however, innumerable exceptions to this regime have always been occurring, in spite of the fact that the conditions of social life tend more and more to make them infrequent. But the theorists have taken no notice of this and have continued to describe as an absolute type the form, arrangement, and mode of disposition and limitation of the personality which constitutes the ordinary modern regime of consciousness.

Now it is found that the ordinary modern regime of consciousness, although consolidated

by a long and almost universal usage, has in no way made man incapable of assuming another regime. The contrary would have been likely. It might have been supposed that a mode of behaviour which had lasted for ages would strongly resist any attempt at modification or substitution. A first discovery was the precise statement, that a rapid and, it seems, light intervention was sufficient for the sudden substitution of an unsuspected regime in place of a known regime. It is probable that the empiricists made this discovery long ago, and used it for ends which were not in the least scientific. It is even probable that the empiricists went no further than to preserve from oblivion certain very ancient traditions which modern science has all sorts of reasons for knowing nothing about or not accepting.

It is easy to explain how, among the reflective forms of knowledge, medicine was the first to direct its attention to this aspect. In the first place medicine, although severe on empiricists, has never entirely broken relations with them, and has always recognised the right—on which we must congratulate it—of taking a good thing where it was to be found. And then, from the

very nature of its social function, medicine deals with a super-abundant amount of human material; all the cases which are a little remarkable reach its clinics and its hospitals, automatically, as it were. However distracted or occupied by other concerns, it is compelled some day or other to take a glance at the most voluminous phenomena, or as we would rather say, the most crying phenomena. What was the documentation of the ancient arm-chair psychologist, in comparison?

Thus, without at first seeing too clearly in the matter, medicine has had to recognise that the known mode of behaviour of the personality easily enough gives place to another. The modification occurs, in certain cases, with so little difficulty that the impression is given of a mere unloosing. This very facility might well have suggested the idea that we were here in the presence of phenomena of a very general nature and of far reaching significance. Physicists did not reason very differently from this when confronted by the first sporadic manifestation of electricity or of radio-activity. But there intervened at this point the terrible *pathological prejudice* which cannot be too vigorously de-

nounced, so much harm has it done to the advancement of certain studies. The doctor, by profession, is concerned with people who are ill. He is bound to see illnesses and sick people to some extent everywhere. Every new or exceptional phenomenon—and what is newly seen cannot but be exceptional at first—seems morbid to him. Imagine, for instance, a civilisation where musical studies had little by little fallen into discredit, where the aptitude and habit of singing, of playing instruments and of experiencing the musical emotions, had little by little vanished from the society and was only to be found in a few wandering and uncertain elements of the population. Suppose that one of these Bohemians, suffering from some form of insanity, were admitted to a hospital for mental cases, or an asylum; it might very well happen that one of the doctors in charge, more curious than his colleagues, should take the trouble to investigate the strange aptitude of his patient; he asks him to sing and observes with amazement all that such a man manages to produce out of his throat, and the odd emotions which he undergoes. But how would you have him not establish some link of cause and effect between

the illness of his subject and dispositions become so exceptional? "An ordinary man, or normal man," he will say to himself—so few people are able to appreciate the difference between "ordinary" and "normal"—"does not possess such powers and is not overcome by such emotions. I am confronted by a pathological manifestation still imperfectly studied."

If the *pathological prejudice* occasioned only an error in classifying perceived phenomena, the disadvantage would not be very serious or very lasting. But quite another thing occurs. The pathological prejudice becomes an *argument for laziness*. How does this happen?

The physicist brought, by chance or by his investigation, into the presence of the strangest, the most surprising facts, makes certain, to begin with, that he is not being deceived by some mere appearance. But, in what ensues, he will not expect to be able to avoid this dilemma; either he must connect the fact with other known facts, with established laws, or else he must modify his acquired ideas, his established laws, so that they will admit the new fact. Whether the physicist is called Torricelli, or Hertz, or Curie, he never thinks of avoiding the necessity

by saying: "It is a freak of Nature." But, in the presence of entirely new psycho-physiological phenomena, medicine, without being able to deny them, is too often content to say: "It is pathological," meaning thereby, "Clearly if these phenomena were produced daily in the *ordinary* man, we should be obliged to seek some explanation for them in natural causal laws and the permanent structure of man; we should feel ourselves faced with the dilemma—either connect this with accepted ideas, or upset the accepted ideas. But, thank God, it's pathological."

"If smoke goes up instead of falling down like other bodies," the physicists of former days might likewise have said, "do not let that disturb us. It is pathological."

It is pathological, therefore it can break all known laws and be as absurd as it likes. Neither the laws nor our reason will be any the worse for it.

However great the admiration and respect which must be felt for the work of Charcot and his school, it must be admitted that these great investigators made use of the argument for laziness, or at least did not expressly enough declare that if the office of the pathologist could,

strictly speaking, be limited to the description of morbid phenomena and the quest of means of cure, it was indispensable that other investigators should undertake the task of connecting these phenomena with the laws of nature.

If then the method of *detection* in psychology has borrowed a number of methods from medicine, and owes to it the discovery of certain groups of facts, it has none the less entirely broken with the habits of thinking which we have just been pointing out. Experimental psychology, like physics, will only allow natural facts and natural laws. It has no use in thought for the category "pathological." Everything is normal in its eyes, in the sense that everything is subject to the general laws of nature. It is not anxious to form a collection of curios. On the contrary, it is only interested in what is general. But it is persuaded that here, as elsewhere, the general, before being recognised, takes the appearance of the exceptional.[1]

But pathological prejudice is not the only inconvenient legacy left us in these matters by

[1] It gives me pleasure to point out that, in his justly famous works, Boirac has expressed analogous ideas with striking force and clearness.

nineteenth century medicine. It put into currency a nomenclature which is very difficult to eliminate completely to-day although its vices are indisputable. For instance, the term "hypnosis" encourages the mistake of associating with states of sleep certain regimes of consciousness, which actually imply as regards the external world, an equal or an even greater degree of vigilance than does the ordinary regime. Further, through having been handled by doctors and in hospitals, it has now acquired an indelible pathological sense. But all that can be said on this point is that different individuals show unequal aptitudes for passing from the ordinary regime of consciousness to other regimes. With some, a slight unloosing is enough, as though the installation of the ordinary regime had never been more than highly precarious, and as though other regimes, organised and ready, were only waiting for a signal in order to appear. With others, the first change of regime or *mutation* requires prolonged efforts and the employment of a well chosen technique. In all cases there remains no difficulty, no appreciable resistance to *mutation* when it has been obtained a number of times. Often the subjects which at

first present least resistance are nervous cases. But there is no fixed rule; it is rash to suppose that aptitude in changing regime is *in itself* pathological. It may happen that a nervous case, or even a half mad person, feels musical emotions with much greater intensity than a great hearty fellow. Musical emotions do not become for that reason pathological.

We consider then that there is a case for the abandonment of terms like "hypnosis," which no longer correspond with the state of our information, and that we should respect the facts more by holding to the idea of *regimes* and that of *mutation* of regime.

It remains to point out in a few words to those readers who are not familiar with these questions, why the idea of regimes of consciousness is of such value in the eyes of the laboratory psychologist, and why the experimental employment of these regimes forms the chosen instrument of the method of *detection* and is to some extent the microscope of the new psychology.

Man in the ordinary regime of consciousness perceives more or less clearly that he possesses a number of mental functions. A careful study

allows a clear idea of these functions to be formed, allows them to be arranged in a hierarchy from the standpoint of complexity and importance, and psycho-physiological experimentation attempts to connect these mental functions with their organic conditions. But it is clear that a psychology limited to the knowledge supplied by the ordinary regime will have no chance of discovering those mental functions which, by hypothesis, only arrive at their full activity in other regimes of consciousness, and remain latent in the ordinary regime. In this case the objective methods of psycho-physiology will perhaps succeed in revealing this or that organic manifestation which does not correspond to any known mental function.

Psycho-physiology is then reduced to assuming the existence of some unconscious or subconscious psychological phenomenon. But it can know nothing, strictly speaking, of the real nature of this phenomenon. Indeed, in the majority of cases psycho-physiology will even pass by, without noticing them, manifestations or organic conditions of unknown mental functions.

But a regime β of consciousness is distinguished from a regime α, by the fact that certain

functions latent in regime α are exercised in regime β.

More generally, comparison of two regimes shows:—

1. That certain functions are common to the two regimes, and present in both the same characteristics.

2. That certain functions, although fundamentally common to the two regimes, assume in each certain more or less different ways of working.

3. That certain functions are manifested in one regime and not in the other.

We could agree to say, in this latter case, that a function conscious in one regime is unconscious in the other. But this way of talking has the disadvantage of concealing a theory within it, and of encouraging a confusion which may be serious between *latent function* and *unconscious function*.

However it may be on this last point, the value of the idea of a *regime* for psychology will be understood.

Setting out from a regime α, to provoke the establishment of a regime β, or γ, is to provide an opportunity for the appearance in full

activity of a mental function which it was impossible to perceive in a regime α.

This mental function once revealed it becomes possible to seek for its connection with organic conditions, by the methods of psychophysiology.

For—and this is a point which must be made quite clear—if a mental function remains latent, or virtual, in regime α, it is possible that in the organism also, the physiological processes which correspond to this mental function, themselves remain virtual or at least singularly attenuated, so long as regime α lasts. They will not be clearly distinguishable until consciousness has taken another regime. But *at any rate* the *anatomical* arrangements remain the same whatever the regime. For it cannot be imagined, in our present knowledge of anatomical analysis, that a *structure* can appear and disappear from one moment to another, according as a function is active or in abeyance. If variations of structure are produced in correlation with functional variations, they can only be of a molecular order; in other words we cannot admit that microscopic organs, for instance, are constructed and absorbed as wholes, as a functional process de-

velops or remains suspended. To take a larger instance, we cannot imagine the organ of Corti disappearing in the hours when a man is fast asleep, and reappearing when he wakes up.

If then the psychological method of detection brings to light a mental function not before known, and succeeds in fixing its modes of action with precision, there is reason to look without delay for the permanent anatomical arrangements with which it is connected. As to what concerns the central nervous arrangements, research there would meet with really insuperable difficulties. But if the function implies to some degree the collaboration of the periphery, if in particular it implies a sensory process, research into the corresponding structures has every chance of success.

But every inquiry into anatomical arrangements necessarily appeals to histology; immediately, if it is a matter of an arrangement of microscopic order, sooner or later, if it is a matter of an arrangement which can be macroscopically isolated.

We thus see clearly—and this was the purpose of the present chapter—in what conditions,

under the pressure of what necessities, and to what ends the most modern psycho-physiological method and histological method can be united.

CHAPTER III

EXPERIMENT REVEALS THE EXISTENCE OF PHENOMENA OF PAROPTIC PERCEPTION

VARIOUS authors have pointed out what they call "sensory" disturbances in subjects in states of "somnambulism," natural or induced. These disturbances are enumerated, as a rule, without much system, or much criticism. Facts of very different natures and very unequal significance have been miscellaneously mentioned. Some of these have no more interest than any ordinary clinical observation. Others, more mysterious, and such as to require an effort, are furnished with a question mark, and with that the author feels he has done his duty.

In particular, investigators have observed or supposed themselves to have observed, again and again, that certain somnambulistic subjects guide themselves with a remarkable ease,

with their eyes closed or even that with eyes bandaged they recognise with precision objects, people and written signs. The fact is confirmed by serious students, and seems to have been long suspected. How placid those who recount the fact remain is not the least curious thing to be observed about it. Some—the doctors—escape the difficulty by invoking "pathology"—that handy notion which is so frivolously abused.

Others look more closely for a reassuring explanation and supply it in a few words. "Somnambulistic" subjects acquire a prodigious delicacy of sensation, and know how to make use unconsciously of a thousand signs which a man in a waking state passes by without notice. Their hearing, touch, and smell undergo "hyperæsthesic" change and manage sometimes to take the place of sight. For hyperæsthesia is another of these convenient ideas, which does not, strictly speaking, perhaps mean anything at all, but which for that reason has an illimitable usefulness. Others again declare with confidence that there is a "transference" of vision to some part of the body. Vision wanders about like the demon in the bodies of the possessed. It is a little surprising no doubt, but "transferences"

since the time of Charcot have been naturalised in psychology and the word is neither more nor less empty than "hyperæsthesia." And besides, cases are so rare and even so dubious; somnambulists are such subtle pretenders that it is sufficient to give a name to one of these manifestations without becoming further involved.

Others, finally, leap with a single bound into the supernatural, and sparing themselves the tedium of scientific research, explain altogether and once for all the most diverse and the most embarrassing facts. "The truth is," says Lombroso upon this point in his *Hypnotisme et spiritisme*, "that no truly scientific explanation can be given of these facts which are on the threshold of a world that can rightly be called occult, because unexplained"; and later on he propounds the view that the existence of the "double" and the "fluid atmosphere" accounts for vision at a distance, transference of senses, transmission of thought, bilocation, and a hundred other marvels.

Far be it from us to doubt the validity of such an explanation! But in an age when six months of tedious research are cheerfully devoted to ascertaining whether a second-rate

historical personage was born on the fifth or the sixth of a certain month, it seems to us that Lombroso is going rather too fast.

My own observations had early made me suspect that there was hidden here a great problem, a problem which I may call *ripe*, capable of yielding positive results, and sufficiently "elementary" for there to be nowadays no rashness in attacking it. On the other hand, the physiological and histological considerations which I have set forth in the preceding chapters, appeared to me to make any investigation, even a somewhat adventurous investigation, into phenomena of a sensorial order, highly opportune.

The problem was precise and presented itself in a simple form: what exactly should be thought of these acts of vision or of pseudo-vision, which seem, in a few exceptional subjects, to take place without the help of the eyes? Can a "laboratory fact" be obtained from these instances, and what fact is it?

Let us notice that observation of the phenomena of so-called "somnambulism" is only a rudimentary and semi-empirical form of the method of *detection*. There was, then, in these cases a special reason to make use of the re-

sources of this method. In other words, it was desirable to examine whether facts of this kind, whose existence nothing authorises us to suspect in the *ordinary regime* of consciousness, are not capable of appearing in another regime; whether they have not also a place in the ordinary regime could be asked later.

We cannot attempt here to treat the question of regimes of consciousness itself. How many really distinct regimes does experiment enable us to recognise? What are their connections, relative importance, and degrees of stability? This is not the place in which to discuss these questions; and the method of detection, in order to be effective, does not require that we should have finally completed its theory.

Let us be content to note that outside the ordinary regime of consciousness, which we propose to call *regime* α there exists another *regime* remarkably stable and rich in ways of action, which we shall call *regime* δ. Many of the phenomena traditionally grouped under the heading of somnambulism really belong to this regime.

* * *

The first point to be cleared up is evidently this; does this Eyeless Sight really take place?

Is it not affirmed merely by observers anxious to please, or in response to persuasion? When the phenomenon is closely examined will it not immediately be reduced to facts already known? Or does it really possess a distinct character of its own?

Experimental psychology, we have taken care to stress, is only concerned with *general facts*. What we wish to know is not whether some subject, famous for miraculous performances of "clairvoyance," possesses a part of the powers which rumour assigns to him, or whether he is merely an astute charlatan. In addition to the fact that such attempts at control encounter all sorts of snares and expose the scientist to innumerable chances of error, they never become more than mere *observations*. Even when the investigator applies his ingenuity to multiplying tests, he only attains a *pseudo-experiment*. In order that there should be a real experiment, in the strict sense, the phenomenon studied must be obtainable from any subject whatever and be really provoked by the experimenter. It must be the scientist who controls the phenomenon, not the phenomenon which commands the attention of the scientist.

In conformity with these principles of method, I began my experiments upon a subject picked at random. He presented no other peculiarities than an aptitude for changes of *regime*, decidedly above the average. On the other hand, when in *regime* δ he by no means possessed that ease, that freedom of mind and behaviour which is characteristic of gifted subjects. I had not at that time chanced upon an exceptionally favourable case.

The arrangement of the first experiments was dictated by the very considerations which governed it. Since in *regime* δ of consciousness obtained by mutation, mental functions, which do not appear in the ordinary regime, can be revealed, it was necessary first to place the subject in a regime δ, as stable as possible.

On the other hand, since what was under investigation was a hypothetical sensory function, capable of being translated into conscious phenomena more or less analogous to those which make up ordinary vision, without the intervention of the ordinary organ of vision, it was necessary: (1) to suppress the use of the eye; (2) to contrive a very simple operation of "visual" perception, but one which none the

less allowed of strict control; (3) granted that consciousness in regime δ really contained phenomena of this kind, to lead the subject little by little to recognise them, to form a clear idea of them, and to express them.

For a mental function can very well be not latent but *actually present*, and yet its manifestations escape the subject. The psychology of every-day life takes note of such facts. If I am walking with a friend in the country, and I say to him, "Listen to the grasshoppers!" he may reply, "I don't hear anything." But if I insist, if he bestirs himself to listen, he will suddenly declare "Oh! that incessant little noise? . . . That's all I heard. . . . I heard it perfectly at the very moment you asked me, but I was looking elsewhere." To apply the notion of *unconsciousness* to such phenomena is not a very exact procedure; it is at least important to distinguish between a function which is really latent, in a given regime of consciousness, and particular facts of consciousness which a passing distraction prevents our noticing, but which a slight effort will enable us to seize.

If the hypothetical function existed in regime δ, it was to be presumed that a subject having

never had either the notion or the use of it would not perceive it spontaneously, that it would be necessary to ask him to perceive it; that it would be necessary to deal with him as with the friend who was not hearing the grasshoppers.

I therefore put the subject a certain number of times into regime δ; then I kept him in it; I bandaged his eyes, and warned him that he would be using a faculty which he possessed beyond doubt, although he has never had occasion to discover it. I explained to him briefly that I was going to place a newspaper in his hands and that he should try to "see" and "read" some, at least, of the largest letters. I made it very clear that he was not to rely upon sensations of touch; that he was to "see," in the strict sense of the word, and furthermore that I was persuaded that he could do it. I showed him further, certain gestures which are familiar to the empirics, and professional "seers," and which if they have no virtue of their own—a matter of which I then knew nothing—are perhaps favourable to mental effort and the concentration of attention.

The subject entered in some agitation, made the gestures indicated, seemed to put forth an

intense effort, the outward signs of which were numerous, hesitated two or three minutes without succeeding in articulating anything and finally began to enunciate in jerks, but correctly, the title of the newspaper, printed in letters 30 mm. in height, the lines of which were 5 mm. in thickness. Having congratulated him, I told him to decipher the title of an article printed in letters 5 mm. high with 1 mm. lines. He increased his efforts and after a few moments, pronounced, not the exact words themselves of the title, but a very close equivalent. There was reading apparently, as in the first case, but for some unknown reason, a work of interpretation added to it. He finally pronounced the words themselves.

The fatigue of my subject was so great that I had to stop the experiment. He complained of exhaustion and refused to repeat the experiment.

I will limit myself to noting, for the moment, (1) that the subject had no previous knowledge of the object presented; (2) that there could be no question of his deciphering by an *ordinary* operation of touch, the fingers having only brushed over the page of the newspaper rapidly

and partially; (3) that no fraud was likely, since I had not in any way informed him, before putting on the bandage, of the experiment I was hoping to carry out, and when I did inform him it was too late for him to be able to prepare any trickery.[1]

The experiment, even if incomplete, seemed to me already very instructive.

Doubtless it was premature to say anything, but the following hypotheses took shape, and demanded an immediate verification as laboratory hypotheses do.

1. *Phenomena of perception sui generis* may occur in man, which seem equivalent, in a certain degree, to phenomena of visual perception, and into which the ordinary mechanism of vision does not appear to enter.

2. These phenomena are capable of a certain generality, since they occur, after a relatively short preparation, in a subject chosen roughly by chance.

These phenomena we can call provisionally, phenomena of *paroptic* perception, indicating by this word that they are concerned with a

[1] I will add that the experiment took place in broad daylight, and that I had the help of two assistants.

certain perception of the optical conditions of the external environment besides or parallel to the normal mechanism of perception.

I insist on this idea that it was strictly scientific to consider them, until proof to the contrary appeared, as *sui generis*. When phenomena are given us as *different* we are proceeding, however it may seem, contrary to the positive method if we affirm *à priori*, by a stupid fear of the unknown, that they are reducible to facts already catalogued. Their reduction will be considered when they are sufficiently known. In order to know them they must be taken as they come, that is to say, as *different*.

In this spirit, I planned various experiments and carried them out without delay.

They were made on the first five people who were willing to lend me their assistance, after the refusal of the first subject, and constituted *five series* of experiments which were made simultaneously or successively. These experiments are numbered by hundreds.[1] It will be observed from the following account to what a degree of precision it has been possible to carry them.

[1] Each particular result has been the object of dozens of verifications.

These subjects were strictly the *first to come along*. They knew nothing of what I was expecting from them. With one exception they possessed no remarkable aptitudes; moreover, in the case of the one whom I have specified, these aptitudes only appeared little by little, thanks to his docility, assiduity, and the intelligent interest he took in the exercises, so that it was perhaps less a question of his aptitudes than of his education.

I had no failure. I could then have renewed my attempts on an indefinite number of subjects, limiting myself to the first manifestations of the phenomenon.[1] But this sort of statistical verification seems to me the minor aspect of the question. It will be applied, moreover, at leisure by future investigators.[2] I judged it more important to study, with the maximum of precision and criticism, the conditions and the mechanism of the phenomenon, in a limited number of fully significant cases.

[1] Indeed, I need not show by calculus that *five successes in series* for five attempts give a high probability to the *universality* of the function.

[2] If we assume that it should be considered necessary. Since experiments like those that we have given accounts of in Chapters VI and VII should, we consider, be enough to convince opinion on this point.

These conditions and this mechanism will be the subject of the following chapters. But we are able now to indicate the first result.

Paroptic perception is a phenomenon *sui generis*, whose experimental existence is beyond all question. Its reality is of the same order of certainty as that of the respiratory phenomenon or of the phenomenon of fecundation in biology.

This phenomenon is of a certain *generality*. It is even likely that *every individual is capable of exhibiting it in certain conditions*.

I need not say that I have taken, in the course of these hundreds of experiments, all imaginable precautions to eliminate the smallest chance of illusion or trickery. Any qualified investigator can repeat them by taking the necessary trouble. The facts which I have obtained are, without exception or reservation, "laboratory facts."

CHAPTER IV

RESEARCH INTO THE CONDITIONS OF PAROPTIC PERCEPTION. THE LAWS OF EXTRA-RETINAL VISION

In a somewhat artificial manner we have separated from the whole of the results obtained the propositions which conclude the preceding chapter. The objective existence of the phenomena of paroptic perception has been proved not from one special category, but from all categories of the experiments performed. In fact, it early ceased to be doubtful, and we deliberately turned towards the study of its *conditions*.

I am going to present here not a chronological and detailed recital of experiments, which would require very lengthy treatment on the scale of a great work, but the general procedure which I adopted, the material equipment, the arrangement of the typical experiments, the results

reached—in a word, what must be known in order to be able to repeat the investigation.

The reasoning was as follows:—

The phenomenon is considered *a priori* as *natural.*

It is thus subject to conditions, and to conditions not in the least vague or capricious, but clearly satisfying the rules of Bacon and J. S. Mill.

First there are the physical conditions.

It appears in the main as an equivalent of vision. It is thus necessary to investigate, in the first place, how it behaves as regards *light;* whether light is its *essential physical agent;* whether the rôle of light is of the same *nature* and of the same *extent* as in vision. From this comes the first class of investigations.

What are the *optical conditions* of paroptic perception?

A. An Examination of Optical Conditions

1. Whatever hypothesis we adopt concerning the nature of light, we can say that, in the order of magnitudes in which human life moves, the propagation of light is *practically rectilinear and instantaneous.* If we have a point *a* and a

point *b*, and if the straight line *a b* is cut perpendicularly by a completely opaque screen, of a considerable height, a ray leaving *a* will not reach *b* at all (all possibility of reflection, refraction, etc., being, of course, excluded). And if we take away the screen, a ray leaving *a* will reach *b* without appreciable delay.

Thus in order that light may be recognised as the physical agent of paroptic perception, it is first of all necessary that the fact should appear of a practically rectilinear and instantaneous propagation from the stimulating agent.

All the experiments, on this point, may be reduced from the following schema: to induce and prove paroptic perception; then to arrange that between the surface of the visible object and the body of the subject, we could draw no straight line which would not be cut by a completely opaque screen.

The subject can be placed standing at one

side of a door; the object to be seen, well lighted, at the other side of the door.

But it is quite sufficient to mask by screens the unclothed parts of the body, the rest of the body being covered with sufficiently opaque clothes.

I constructed in particular, two small pieces of apparatus, which in fact proved rather convenient for this series of experiments as well as for those that followed. I called them the *Guignol* and *Bouclier*.

The *Guignol* consists essentially of the following:—

1. A back *A*, *completely opaque*, highly diffusing, *but not polished*.

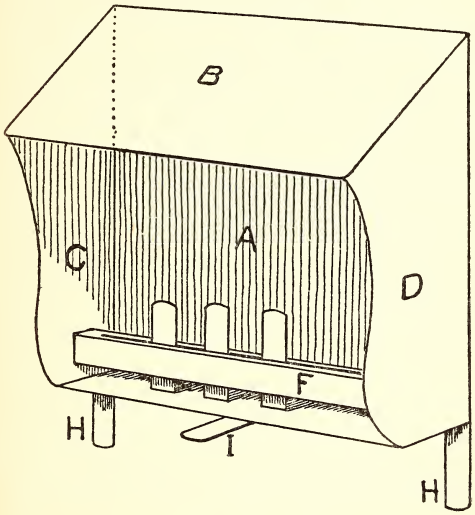
2. A roof *B*, with two *wings C* and *D*, somewhat *translucent*, but not at all *transparent*, that is to say preventing any rectilinear and distinct propagation of luminous rays.

3. A *slide*, *F*, completely covered by the roof and the wings, where the operator can insert and set up vertically cards bearing numbers, letters, designs of various sorts, colours, etc.

4. Two feet, *H*, *H*, and a handle *I*, which comes out in front; these feet and this handle

making it possible to hold the Guignol in various ways.

As can be easily conceived, the interior of this Guignol is not much less brightly illuminated

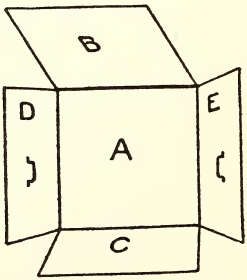


than the exterior; but if the cards are turned *face to the rear* no distinct ray can come from their surface to the holder of the Guignol in whatever position he holds it; and if the cards are turned *back to the rear*, the rays coming from

their face can only reach the holder of the Guignol when he holds it in certain positions.[1]

The *Bouclier* is a sort of articulated screen, composed of a central panel *A*, an upper panel *B*, a lower *C*, and two laterals *D* and *E* of unequal dimensions. The two side flaps are furnished with hand-grips.

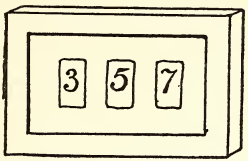
These panels or shutters, completely opaque, and movable on their hinges, are able to take the most diverse positions in regard to one another, as well as in regard to the body of the subject and exterior objects.



It can be imagined to how many varied experiments these two little pieces of apparatus lend themselves.

[1] And, depending on the position adopted, it will be this or that part of the body only, chosen and limited by the observer, which the rays will touch.

A third, still simpler, consists of a photographic printing-frame, with a plate of transparent glass, under which are placed the objects to be recognised: letters, various signs, etc.



Between the plate and the objects, screens of various thicknesses and unequal opaqueness can be placed.

I need hardly say that in all these experiments the operation of ordinary vision is prevented by the most meticulous precautions: perfected bandages of an opacity ten or twenty times greater than that of ordinary bandages, and where the slightest lateral filtration is prevented by pads of wadding, in addition to the sealing up of the eyelids—so that it can safely be said that no amount of light, even of an order such as would affect a very sensitive photographic plate in ten years, could reach the retina.

On the point with which we are concerned the

numerous experiments made have been closely in agreement.

If between the surface of the visible object and the surface of the body of the subject, no straight line could be drawn which would not be cut by a *completely opaque* screen, or by a screen which is *translucent* without being *transparent*, no paroptic perception takes place.

The perception begins when this condition is not completely satisfied (we will show this precisely later).

On the other hand no *delay in transmission* is observable (or at least there seems to be none due to the exterior agent).

Everything occurs then as if there were *rectilinear and instantaneous propagation*, and under normal dioptric conditions.

Let us note this, however:—

If the screen consists of a body of *discontinuous texture* (for example a thin or loosely woven material), placed against the surface of the *skin of the subject.* the perception can take place, more or less imperfectly, although the same screen is sufficient to prevent retinal vision (the same screen placed *against the object* prevents paroptic perception).

This peculiarity does not weaken the above conclusion, and we will attempt to account for it later.

Let us keep in mind that there is no *piercing nor avoiding the unbroken screens*, by the physical agent, and that everything happens as if this agent were light (in the ordinary and limited sense of the word).

* * *

2. It will be well next to examine how paroptic perception behaves, when the *intensity* of the light varies.

Here, again, numerous and carefully varied experiments have given results in agreement which we can sum up thus:—

Perception is absent in absolute darkness.

It is the more *sharp, precise and easy*, as the object to be seen is placed in a *brighter light* (up to the intensity of full sunlight).

This progression is easy to follow, and lends itself to an exact enough valuation (a decreasing number of errors in the reading of the numerals—increasing rapidity of the reading). It would be possible to give it a statistical and graphic form.

The *lower limits of brightness* are difficult to

fix, since they vary with the period of practice of the subject. However, they seem very close to what they are for the normal human eye. They are probably *slightly lower*, and we believe exhibit a *slight nyctopia* (night vision).

* * *

3. How does paroptic perception behave in relation to colours?

Here again we have been able to multiply the experiments.

Under normal illumination the *qualitative perception of colours is perfect.* Delicate differences in shades are well recognised and identified with assurance, whatever may be the actual material of the coloured object or the nature of the colouring substance (for example: "salmon pink," "carmine," "cerise," "cream yellow," etc., etc.). I have not been able to discover any error. When the subject has returned to the ordinary regime, he finds exactly the same terms for the same colours. (It is needless to say that any intervention of touch was carefully eliminated.) I have not noted paroptic "Daltonism" in any of my subjects.

When the light is very faint the perception of

colours becomes more hesitating, but here is a very remarkable thing:—

Paroptic perception of colours continues noticeably beyond the lowest illumination with which visual perception of colours can occur.

In a very dark room, where two people, endowed with excellent vision and taken as "witnesses," still distinguish certain shapes but do not recognise any colour, the subject still recognises colours, although with slight errors (for example pale green for pale yellow) and with complaints as to the difficulty. The colours which persist the longest are red and green. (Under the same illumination no numeral or letter was recognized.)

Thus "nyctopia" is more marked for colours than for shapes.

None the less, perception of colours ceases, as we should expect, in complete darkness.

* * *

4. It would be extremely desirable to fix with precision the *special limits* of paroptic perception.

I arranged, with the assistance of a physicist, the necessary experiments, in a good laboratory. But, at the last moment, the subjects refused. I am waiting to make another attempt.

Lacking exact conclusions, I will limit myself to the following brief notes, drawn from rough observations.

At the *infra-red* end the limits are probably the same as for the eye; in any case, they would be rather *over than under*.

At the *ultra-violet* end paroptic perception should go further than the eye.

(The hypothesis of light as the agent being admitted.)

* * *

5. I investigated how paroptic perception behaves in relation to *real and virtual images*.

Everything occurred as the hypothesis predicts.

In particular the phenomena of *perception of images reflected by mirrors* appear very distinctly.

* * *

6. Other verifications of an optic nature could be attempted. They would constitute superabundance of proofs.[1] All the preceding conclusions tend indubitably towards this:

LIGHT IS THE PHYSICAL AGENT OF PAROPTIC PERCEPTION.—We shall have occasion to con-

[1] The ordinary laws of *transparency* hold good, we have seen, for paroptic perception.

firm this result in the course of a discussion of the whole inquiry.

B. Investigation of the Psycho-physiological Conditions

We have provisionally considered the phenomenon as *sui generis*. The first part of the investigation of psycho-physiological conditions will consist of examining precisely whether paroptic perception is not reducible to sensorial operations already known.

* * *

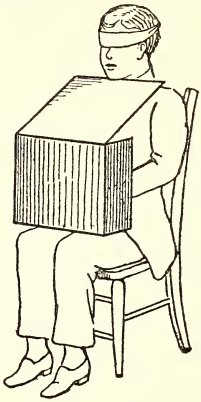
1. First, let us eliminate definitely normal or *retinal vision*. I recall what I said about the bandages used. I add, in order to close the question and not to return to it, that I placed between the object to be seen and the face of the subject, whose eyes were bandaged, various screens of an absolute opaqueness (plates of wood, metal, etc.). In particular I arranged the *Bouclier* in the position indicated in the sketch, the object to be seen being placed on the knees of the subject and the hands of the subject holding the interior grips.

The phenomenon did not cease.

Now every straight line connecting the object and the eyes of the subject was intercepted—

(i.) By the bandage.

(ii.) By the upper panel of the *Bouclier*.



Perception, therefore, really occurs *without the assistance of the retina. It is extra-retinal.*[1]

* * *

2. *The elimination of the tactile sense* is not less complete.

First Type of Experiment.—I place under the glass of the frame (see page 63 above) the

[1] We are content with citing here a single crucial experiment; but there are many others which we shall meet later; for example vision of an object situated outside the ocular field, vision gained exclusively by the hands, the back of the neck, etc.

object to be recognised (letters, numerals, colours, designs, etc.), without forbidding the subject to handle the frame. The perception is neither prevented nor even weakened.

SECOND TYPE OF EXPERIMENT.—The object is placed at a considerable distance from the subject, beyond any possibility of contact.

Perception occurs.

It could, then, only be a question of some "distant touch." But either this word is meaningless or it is synonymous with vision.

* * *

3. As regards *smell*, the question is more complex. Nevertheless, the experiments allow us to formulate the following conclusions:—

(1) The subjects asked to recognise colours, especially when delicate shades are concerned, or if the light is faint, *sniff spontaneously* as if they were making an appeal to their sense of smell. (They never sniff if asked to recognise shapes or signs.)

(2) If, the mouth remaining closed, the nostrils are carefully plugged, and if the supplementary precaution is taken of introducing a powerful odour by the previous insertion of a

plug of wadding strongly impregnated with perfume, the paroptic perception of *colours* seems a little hesitating; certain shades are less well appreciated, but the difficulty is neither deep nor lasting.

(3) If we leave the nostrils free, and place sheets of coloured paper, say, *under a thick glass*, in a tightly closed frame, so that the *visual rays* of colours pass easily through the plate of glass, but all emanation from colouring substances, that is to say all *odour* be stopped, perception of colours operates perfectly and the subject does not complain of any difficulty. But in a poorly lighted room he sniffs, and replies with assurance.

(4) The responses, moreover, of the subject do not vary with the *chemical* nature, that is to say, the olfactory properties of the colouring materials. He identifies two cerises of which one is derived from aniline and the other from carmine. He distinguishes further two neighbouring shades of aniline colours.

(5) The *actual odour* of a coloured object (for example the woolly odour of a cloth, the very peculiar odours of certain papers) does not em-

barrass or occupy the subject at all. If perception of odours were concerned, it would surely be otherwise.

(6) The use of small *screens*, placed at some distance from the open nostrils, in the line of the colour rays, seems to reduce slightly the ease of perception. But the effect is less marked than when the nostrils are plugged.

All these conclusions lead to this:—

The *nasal mucosa* is sensitive to light and to different coloured regions of the spectrum. This function *is sharply distinct from smell.* It is of the optic order. But paroptic perception of colour can occur without the nasal mucosa. Its rôle is important but not essential.

The fact that the screens do not seem sufficient to stop the participation of the mucosa is easily explicable: in contradistinction to a *shape*, a colour can be reflected, broken up, and diffused, without ceasing to be recognisable. The screen stops the direct rays, but not the diffusion of rays of colour, which, especially if it is intense, tints all the neighbouring objects by reflection.[1] The same reason explains how the rays of colour can reach the mucosa, in spite of

[1] Including perhaps the particles in suspension in the air.

the anatomical obstacles which can break up the direct rays.

* * *

4. A rapid examination makes it obvious that there can be no question of reduction to other known senses, taste or hearing. We shall not stress this, although we have been much concerned with it in our experiments.

To conclude concisely: paroptic perception is *sui generis*. It is an extra-retinal perception, and it is *vision*. We can name it now: EXTRA-RETINAL VISION.

But there remain many other conditions to analyse.

* * *

5. The part played by the nasal mucosa leads us to the following question: Is the unknown organ of extra-retinal vision situated in one part of the body? localised in a single one or diffused through many?

What are the parts of the body which seem to have a share in extra-retinal vision, besides the nasal mucosa?

My experiments have been numerous, various and conclusive. The *Guignol* and the

Bouclier[1] have done me good service. But I have had recourse to many other arrangements.

Let us set out the results:—

(1) No limited region of the body (I mean of the periphery) is *exclusively* responsible for the extra-retinal vision. Any region whatever (face, back of the neck, throat, chest, etc.) can be covered without the function disappearing (vision of shapes).

(2) *Any region* of the periphery of the body, provided that it is of a certain extent, *is capable of securing by itself* a certain degree of extra-retinal vision.

The minimal area varies according to the region considered: it is very difficult to determine exactly, it depends on conditions of brightness, the difficulty of the exercise of vision, the experience of the subject, and probably also on individual aptitudes.

We only indicate the approximate limits between several square centimetres and a square decimetre of surface.

[1] The Guignol allows the presentation of numerals, designs and colours for recognition, to the only part of the periphery which is bared. In the same way the various positions of the Bouclier allow the interception or the passage of the luminous rays between the object and the parts of the periphery whose paroptic power it is desired to study.

(3) *Vision is difficult, hesitating, and imperfect when only one region is exposed.* It even happens that the subject becomes discouraged and ceases to perceive. *The more regions engaged, the more easy and satisfying are the results.*

For example: if the hands are bare, the sleeves lifted to the elbows, the forehead clear, and the chest uncovered, the subject reads easily and at a normal speed, a page of a novel or an article in a newspaper, printed in ordinary print. (I am talking of an educated subject.)

If he exposes no more than the back of his head or his forearm he will have great difficulty in distinguishing a 6 from an 8, drawn in characters 3 cm. high with lines 1 mm. thick.

The same is true for colours.

(4) *The importance of the various regions is unequal,* which we can also state in this form: for a given area, perfection of vision varies greatly with the region.

The hierarchy of regions is difficult to establish. Experiments on the most diverse subjects should be collected. I indicate, with reservations, the following descending order:—

1. The right hand (among right-handed people);

2. The left hand;
3. The neck and throat;
4. The cheeks—the forehead, the chest;
5. The back of the neck—the arms—the thighs, etc.

The nostrils ought to be considered separately. They stand in the first rank in perception of colours. It is very difficult to know whether they play any part in the vision of shapes.

(5) The parts of the periphery *which move easily* (the fingers and the hand) seem to have *ipso facto* a superiority in perception (*even if they have no contact* with the object).

(6) It is possible, and even likely, that such regions would be more useful in vision *of a very near object* (up to contact through a plate of glass), other regions in vision of *distant objects*. The hands would enter into the first category; the cheeks and the forehead into the second. But in every case it is only a difference of degree, and the experiment allows of no positive statement.

* * *

6. Let us pass to more complex questions. What are the characteristics of paroptic perception of *space?* Or, if it is preferred, has paroptic space peculiar properties?

(1) Subjects who possess extra-retinal vision use and interpret the words *high* and *low*, *right* and *left*, *before* and *behind*, etc., as regards objects which they perceive only paroptically, exactly as does the normal man in ordinary conditions. Everything, then, seems as if paroptic space had the *three dimensions* and the general structure of our common tactile-visual space.

(2) Everything happens, besides, as if the *scale of sizes* were actually the same. It would, indeed, be not at all unlikely that printed characters, for example, which appear to our ordinary view as *large characters* should appear to extra-retinal vision as *very small*, even microscopic. It is not so. The subject sometimes complains of seeing confusedly, of being uncomfortable, but everything tends to show that he sees on the *same scale*. It will be said: the subject proceeds by comparison with known sizes; the characters seem to him large *in comparison* with dimensions with which his former experience has made him familiar. Perhaps. But it remains very surprising that this psychological readjustment takes place so quickly and with such certainty. Further, as we shall soon notice,

subjects who are in the first hours of their training, only discover objects one by one, and even bit by bit. For example: in an entire page they will only see one letter or one syllable at a time; so that even at this stage they seem in possession of their scale of sizes. They say, for example: "It is a very large letter, but I cannot distinguish whether it is a B or an R."

I am on my guard against drawing conclusions; but when we have dealt with a certain number of experiments of this nature, we cannot escape from the thought that some of the most celebrated theories of space have gained nothing by the production of these new facts.

(3) None the less, in all the first hours of learning rather curious facts of lack of spatial coordination appear. I present to the subject the number 492. He reads, with much hesitation, 249 (before declaring the number, he has named each digit). I tell him that he is wrong. He replies by announcing 924. To my question "Are you sure of it this time?" he replies, "I am almost sure of it."

These errors are neither frequent nor lasting. It seems to me that they are connected with psychological processes of learning with which

we shall be concerned later on, and are not such as to alter our opinion on the *essential identity of paroptic space and tactile-visual space.*

(4) From the fact that these two spaces seem identical, it does not follow that the subject can dispense with tentative efforts to identify them. Here we encounter the strange phenomenon of *angular deviation.* If I say to a subject who is at the beginning of his education: "Indicate with your hand the direction of this object which you see," his hand takes a direction which makes a certain angle with the real direction of the object. Or again, if, when three people have been arranged, side by side, facing him, I tell him to describe the one in the middle and point him out with his finger, he proceeds to a faithful and conclusive description of the person in the middle, but his hand will seem to point to one of the other two. It is unnecessary, then, to interpret this phenomenon as a sign that the subject sees badly, or that he guesses rather than sees. With the adult, in regime α, visual space and what might be called kinesthetic space coincide very exactly; my hand points of its own accord towards the object which I see; I have no need to concern myself with its movement or

to correct it. In the case of the adult with his eyes bandaged, who is seeing paroptically, the coincidence between the new paroptic space and the visual-tactile-kinesthetic space is not yet established. And the elegant experiment of the angular deviation allows us to appreciate this disagreement with all the desirable objectivity. If, in fact, the object moves (without any noise), the hand which is pointing at it moves with it, but the deviation remains constant. I tried to measure it. It seemed at first between 30° and 45°.[1]

(5) The *angular deviation* quickly diminishes and finally disappears. I think that this correction is performed by quite a simple mechanism dependent on learning—mechanisms that we will discuss later; the subject succeeds in seeing *simultaneously* the object and his extended hand. He corrects the error in direction himself. But, as we shall notice again, the problems of *perspective* are infinitely more complex for extra-retinal vision than for the eye. Indeed, if this function is exercised in a diffuse manner by the entire organic periphery, there is little chance

[1] Has this deviation a *definite orientation* in relation to the object and the body of the subject? I cannot yet reply categorically.

that a *perspective* as simple and well "centred" as our habitual perspective would be established easily. Consequently, *corrections*, like that under discussion, force themselves less quickly on the subject. It is not surprising that such a culmination should remain incomplete in subjects who already obtain results so brilliant in other respects.

Without wishing to engage in a purely theoretical discussion, I think I can state that these facts of learning do not, any more than those which preceded, imply the heterogeneity of paroptic and tactile-visual space.

* * *

7. We have passed gradually from the study of what might be called the "objective" properties of paroptic space to the examination of conditions where the subject has acquired the idea and the usage. The two questions are perhaps not fundamentally distinct.

Let us now place ourselves at the same point of view as *the subject who is learning*, that is to say, let us study the properly psychological conditions of the phenomenon of extra-retinal vision in space.

(1) In its first manifestation paroptic per-

ception seems to possess only in the lowest degree the *characteristic of simultaneity*. Objects and parts of objects are discovered one by one. In the middle of a page the subject sees a line, a word, or sometimes only a letter. He passes slowly from one discovery to another. He does not choose. He fumbles at random. He gives the impression of throwing on things a very limited circle of clearness, and that he has great difficulty in synthesising his successive finds. For example, he says: "I see an E— I see a G— GY — ENER — ENERGY — OF — RO — TATION." Now these four words are lost among the hundreds of others which compose the two pages of an open book. Nothing calls the attention to them. Other words in large characters should have been seen first, or indeed it would have been natural to start at the top of a page or at the beginning of a paragraph, after a very noticeable blank. But such a choice requires a preliminary *simultaneous* perception of the page and of its distribution. There is *successive* perception only.[1]

It is this probably which explains such errors as we have noticed above. The subject dis-

[1] At least as regards attentive perception.

covers a 2, then a 4, then a 9. As I ask him for the number as a whole he makes a sort of perceptive hypothesis and answers "249," then "924." In reality, he saw neither of these two numbers, in their spatial disposition. He sees successively and separately the three digits. A more advanced education is necessary for him to see *simultaneously* 492.

The progress goes on, however, with surprising rapidity. But even after weeks of learning a simultaneity of the same order as that of our ordinary visual perception is not yet attained.

(2) From this come certain remarkable gestures and attitudes. There is first the use of the hands, the right hand especially, not only for *his own visual power*, but as a *kinesthetic instrument of survey*, for marking positions. I slip a photograph under the glass of the frame, and place it twenty centimetres from the subject. "Look; what do you see?" The subject irresistibly stretches out his hand and wishes to touch, if only for a second, the edge of the object. In order to get information by *touch?* Not at all. As a matter of fact he does *touch* a corner of the wooden frame and declares: "Yes, it is a picture of the man who was here

yesterday morning." His muscular movements have allowed him to place the object in visual-muscular space which is familiar to us, and as paroptic space has the same properties, the object becomes placed therein at the same moment, at least in an approximate way (except for the angular deviation). The zone of possibilities is thus reduced in size. The subject knows almost where he must project his power of vision.

If I forbid him the use of his hands he is unhappy and irritated. It is as though someone were *focusing* a pair of opera glasses with difficulty.

Hence, too, the behaviour of the subjects in their first attempts to *walk* with eyes bandaged. They have not yet acquired that simultaneity of vision which allows us to walk boldly and absent-mindedly among the dozens of obstacles in a street. They are still under the necessity of *successively discovering* the obstacles, and are consequently *afraid of obstacles.*

(3) We have mentioned the term *focus.* Need we imagine an *accommodation* properly so called? We do not think so—but for other than experimental reasons, because, as will be stated later,

a mechanism of accommodation which would work for distances of this order is incompatible with the *psycho-physiological theory* of extra-retinal vision which seems to us the only one acceptable. We shall say, then, that there is a *pseudo-accommodation.* It is concerned, not with a relatively simple physiological mechanism such as that for the accommodation of the eye, but with an operation which is more complex and more "psychological" (or central).

(4) Vision of *distant objects* and *vision to infinity* seem to be connected with the same order of questions.

Extra-retinal vision at first seems to have a *weak range.* The subject reads a text placed on his knees, recognises an object held fifteen centimetres from his face, but everything occurs as if a fog covered all beyond. The *range* of vision increases little by little, from hour to hour, from day to day, and reaches "infinity." In our opinion it is concerned with a psychological mechanism of learning, a progressive utilisation of sensorial data, and not with perfecting the sensorial data themselves. Or if we prefer to keep to the language of physiology: it is concerned with a progressive adaptation of

central nervous processes, and not at all with a reflective adaptation of the sensory organ.

(5) The question of the *extra-retinal visual field* might be confused with the preceding question, but demands a special examination.

As regards *depth*, the extra-retinal field, we have just said, finally reaches the limits of the retinal field. The subject sees a building on the horizon or a boat moving in the sea.

But, as we know, the retinal field is roughly *cone-shaped*, and gives, in *horizontal section*,[1] a sector whose angle varies slightly according to the individual, without ever exceeding a maximum value which remains *below* 180°.

Now this horizontal section of the field in the case of extra-retinal vision reaches 360°. That is to say, the field is *circular* or even *spherical*. If I am in the centre of a room, and if I wish to see, with my eyes, all that surrounds me I must move about, or at least my head must pivot on my neck. To obtain the same result the subject who sees paroptically can remain *motionless*. Doubtless he could not give his *attention at the same time* to all the sectors of

[1] The subject is supposed to be in a standing position, his head straight, his eyes motionless and looking straight in front of him.

his visual field, any more than we can be fully attentive to all the objects in ours. But he only needs to "direct his attention." If I say to a subject: "What is there behind you?" he pauses a moment—time to turn his attention to the sector—and replies: "There is a person standing up who is holding his right hand to his hat." In the case of the well-educated subject, his change of sector is almost instantaneous, and we come to the problem of *simultaneity*, discussed above. To sum up, there are all degrees between the isolated vision of a letter in a page, which marks the beginnings of the function, and the simultaneous possession of all the elements of the circular visual field which is an ideal limit.

I insist on the primary importance of this idea of the *circular field*. Let the reader reflect well on this fact, that our *ocular* visual field is not only one of the peculiarities of our sensorial equipment, it seems to me, essential to our psychological structure, which it has helped to determine. In order to get a faint notion of it, let the reader attempt to imagine with some concrete precision, what would be his embarrassment if he were suddenly endowed with a circular vision field.

If, further, as appears from the experiments, extra-retinal vision is due to a collaboration of all the periphery of the body, we can conceive how difficult it is for the consciousness to *centre* this circular or even spherical field, and what new problems of *perspective* are thus open to us.

* * *

8. How is this collaboration of diverse regions of the periphery translated for the subject? Beyond doubt, he localises, as do we, the physical cause of his perception in the external object. The *objectivation* is no less advanced in his case than it is in ours (did he only owe it to the experience he has acquired in regime α). But has he any idea whatsoever of the *physiological processes* of his perception? Where does he localise the function in his own organism?

Any statement on this point ought to be very guarded. We are dealing with two methods of investigation which are by no means of the first rank: 1st, *Observation of the attitudes* of the subject; 2nd, Interrogation.

(1) The subject who sees *paroptically* has attitudes and a special mode of behaviour. Certain gestures reappear with regularity. The

experimenter has taught him some of them. But it is felt that the subject adapts and completes them; and he invents new ones.

A first point of interest is that the subject *does not search for the object with his eyes*. He does not place himself at all in such a position as to include the object in *the portion of space which would be his ocular field*, if his eyes were not closed and bandaged. I have carefully studied this fact, which seemed to me significant, and I found it convenient to call this attitude "the heterocentric attitude"; meaning by this that *paroptic sight* and ocular *sight* are in some way not *concentric*.[1] It would be possible, in fact, for the subject to feel the need of putting the object, in order to *see it distinctly*, in the same axis as his eyes, if only from old habit. This is not what occurs as a rule. I have attempted to estimate this sort of divergence. It is far from being constant, expecially if we pass from one subject to another. But it seems to be limited as an angular value between 30° and 90° (naturally, I eliminate the cases where the subject has received the order not to move at all, and to look at an object whose position he cannot alter. I keep

[1] Or, if it is preferred, do not have the same axis.

only those where the subject is free to take the attitude which seems to him most favourable).

This angular value is estimated by connecting the middle of the interval between the eyes to the object by a straight line, and by looking to see what angle this straight line makes with a line parallel to the antero-posterior ocular axes drawn approximately through this median point (the eyes being in their average position of convergence at infinity).

We have already met with an analogous idea in the angular deviation (see pp. 81–82), and the values in the two cases are not so very different. It would be tempting to reduce the two phenomena to the same cause. The hypothesis is seductive, and seems to promise some brilliant interpretation. I believe it should be mistrusted.

The *angular deviation* studied above is lessened with education. What I will call *declination*, to distinguish it, keeps its value.

We have said that the *orientation* of the angular deviation did not allow of measurement. In ordinary cases the *declination* offers a sufficiently apparent orientation. The angle of declination is situated in the plane of the symmetry of the body, the upper side of the angle

being constituted by the parallel to the ocular axes.

But the *heterocentric* attitude only manifests itself at certain moments. At others the subject makes various movements which constantly modify the respective relations of the body and the object.

If the object can be handled without detriment to the significance of the experiment (for example: if it concerns designs placed under glass) the subject touches it, time after time, with the palmar surface of his fingers and hands, with his forehead, the upper part of his chest and sometimes his epigastrium. These movements seem to have a real efficacy. If they are prevented, even in part, vision is less good, and comes into operation more slowly. But none of these is indispensable.

The subject does them with freedom and initiative, especially after several sittings. It is only a sort of ritual which he goes through mechanically.

(2) The *interrogatory* confirms the fact that the subject is conscious of his movements and of their fitness. But it is not at all necessary to conclude from his responses that he has a clear

idea of the processes of which he is the seat. It occurs to him, doubtless, to use words which seem to him especially significant. "I see better with my chest," etc. But let us be on our guard against taking for immediate intuition, for spontaneous localisation, what is probably only a reflective and intelligent interpretation.

If we collect the observations and the statements of the witnesses, we shall be led, I believe, to conclude as follows: all the work of consciousness tends to locate objects in space as correctly as possible; but the physiological processes are almost unperceived. The subject does not know in fact *how* he sees, by which organs, nor by what elaboration of peripheral data. He has the impression of being in direct contact with external realities, of *being present* at the spectacle of events by an immediate power, and the very difficulties of his perceptive work *become objective* themselves for him; it seems to him that the light is bad, that there is a mist, etc.

But does it operate in this respect very differently from our own visual perception?

* * *

9. *A time for elaboration* is necessary for a fact of extra-retinal perception to occur. There

is nothing more usual in the matter of sensorial activity. What is remarkable here is the length of time required for this elaboration at the beginning of the learning process.

For example, a number of three digits is placed under the glass of the frame, and the frame given to the subject who handles it freely. He adopts the attitudes and makes the gestures of which we have spoken. Clearly the work of perception commences immediately. We feel that vision is beginning, sometimes the lips move; the index finger traces something fumblingly, as by way of trial. Then vision asserts itself and the subject announces one of the three digits. Generally the other two are found more quickly; the previous work has had a bearing on them also, more or less. The announcement takes place, most likely, in the order of their *clear appearance.*

The time which elapses between the moment when the work begins and the moment when the first result appears I call specifically *time of elaboration.*

At the beginning of the period of training the time thus occupied is approximately a *minute.* It diminishes little by little. For example, a

subject after seven or eight exercises in vision, reads a number of three digits (30 mm. high, with lines 1 mm. thick) in a minute with almost equal intervals of 20 seconds between the digits. This refers to a subject of mediocre attainments, but generally in the case of subjects already educated, things present themselves differently. The *elaboration-time*, properly speaking (before the subject articulates anything whatever), remains clearly noticeable: 30 or 45 seconds, or even longer. But when release occurs the operation seems to continue with ease and rapidity.

When an exercise of *reading* is concerned (numerals or any sort of text) we are reduced to the statement of this fact of elaboration without any inkling of its mechanism or its phases. Other exercises make it a little less incomprehensible.

For example, I put at a distance of thirty centimetres from the subject and at the height of his face a very peculiar article: the bowl of an Arab pipe of a rounded shape, inlaid with copper and having copper attachments. The subject receives instructions to keep his hands motionless on his knees and to describe what

he sees. At the end of several seconds he begins to talk. "It is a small object," then with intervals of from three to five seconds, "it is a funny object . . . it is made of copper . . . no, it gleams, it has copper ornaments, but it is not made of copper . . . one would almost say a sort of tube . . . it is a pipe."

I show him an ivory statuette. Here are his replies: "It is a small object . . . it is white mastic . . . one would almost say a piece of india-rubber. . . ." Surprised, I allow myself to interrupt him. "Why do you say india-rubber?" "Oh, because of the colour, but I didn't see . . . it's a statuette."

If I ask the subject not to talk until sure of seeing well, he waits sometimes a minute or more, and replies without interruption, with precision and assurance.

When questioned as to what happened to him during the time of elaboration, the subject replies willingly. But I greatly mistrust results obtained by such a method—which too many people confuse with the objective method. However, I record this remark, which seems to be not without its value (in connection with a reading of figures). "I see first *lots of things which dance*

. . . then everything becomes gradually connected."

I note finally that the time of elaboration is shortened and vision facilitated, if, instead of keeping the object motionless, *it is slowly moved* and turned round without being shifted away from or towards the body.

All these particulars find a satisfactory explanation in the hypothesis which will be defended in the course of the following chapter. But apart from any hypothesis, these slow processes of elaboration agree well with the fact that all regions of the periphery contribute to extra-retinal vision (see p. 76 above).

* * *

10. What is the *acuteness* of extra-retinal vision? or if it is preferred, what angular *separating power* does it possess?

Sufficiently precise measurements are possible, although they meet with certain difficulties: the chief is in determining the *average* distance between the object and the body, and even in knowing whether it is legitimate in this sort of problem, to estimate *an average of distances*. The difficulty is reduced for vision of less distant objects.

But it remains for us to estimate the separating power of vision in *course of development*, and not the maximum acuteness of which an organ is capable by its very structure. Our figures indicate then the *resolving power* attained in our experiments. The values vary according to the subjects and circumstances, between $\frac{1}{100}$ and $\frac{1}{300}$.[1] In certain cases, the interpretation of which is difficult, it seemed to me still higher. (Let us recollect that for the normal eye it is $\frac{1}{3000}$, and that the separating power of an optical apparatus increases with the *denominator* of the fraction which is its conventional expression.)

* * *

11. It remains for us to consider two very general questions, which it would be important to elucidate, as much for the sake of future experimental researches as for eventual practical applications.

(1) What conditions should be combined for the extra-retinal function to occur?

(2) Is this function compatible with the ordinary regime of consciousness?

The first question is not to be confused with those which have been the subject of this chapter.

[1] The skin being bared over a large area.

We have shown under what *conditions* or *laws of functioning* extra-retinal vision works, *once it has been aroused* in a subject; but not at all under what conditions it is aroused.

This is a type of question which psychology is not accustomed to put, where elementary and ordinary functions of consciousness are concerned. It does not ask: In what cases, under what conditions, does the power of hearing, smelling, retaining images and associating them *appear* in a man? The possession and a certain spontaneous use of these functions seems inherent in the nature of man, and we are only curious to know how they are educated, how they are destroyed, or, again, how they have appeared in the animal kingdom. Psychology is only concerned with the *conditions of appearance in the individual* as regards those higher functions without which we think man can live, if necessary, and which seems to us the product of a recent evolution, like voluntary attention, logical reasoning, and mathematical invention. Our psychology has not yet acquired the idea of *latent function* and has taken from *general biology* a sort of axiom which that science would perhaps have difficulty in justifying: "A func-

tion only exists on condition of being used. A function, like an organ, which is not used by the living being, atrophies and disappears."

Now it is going to be necessary to give up such simple conceptions. At least in the psychological order, a function can persist indefinitely, in the *latent state;* it can sleep in some way, through thousands of generations and await, in order to awake with all its vigour and fullness, only a minimal irritation which possibly may never occur.

For it is essential to destroy a confusion that the word *acquire* is apt to occasion, as well as the word *training*. The subject *acquires* extra-retinal vision and undergoes the *training* for this vision; this means that he comes to *perceive*, to *take consciousness*, of a function which he possessed without suspecting it, which he learns to make use of. But it would be absurd to imagine that in a few hours there is *created* a function scarcely less perfect than that of the human eye. It would be a miracle of the same order as the spontaneous generation of a mammal.

So no comparison should be attempted between the *revelation* of extra-retinal vision, in the case of an individual, and the *acquisition* of some

higher function like algebraic reasoning or the art of talking in Chinese.

The *conditions* which we are now seeking to determine will be, then, in no way *formative causes*, but merely occasional causes or causes of release.

Now the *formative causes* of a fact may well be always the same, be *constant*, while the possibility of the *substitution of causes* remains in general very limited. But there is no reason why causes of *release* should be constant. The formative causes of a *névé* are finite in number and constant. But there are many ways of starting an avalanche and it is possible to imagine new ones without end.

The causes of *release* are for the most part irrespective of the main nature of the phenomenon, and it is only by exception that we learn anything from them. The way in which I persuade a friend to sing me a melody proves nothing about the mechanism or the art of singing.

Here are the conditions which are *all that need be combined* for the release of extra-retinal vision. But there is nothing to show that they are *necessary*, or that they might not advantageously be replaced by others:

(1) *To lead the subject into regime δ*, by mutation.

(2) *To reproduce this regime several times* for periods of short duration (five minutes for example) until the subject passes *in about a second* from the ordinary regime to regime δ, and by a single word or a single gesture. If the experiment is skilful this result should be obtained, on an average, at the fifth mutation.

(3) To make sure that in regime δ the subject attains the vivacity, the mental clearness which should characterise this regime, that he is *fully attentive*, that he is fully in possession of himself, that no parasitic process (spontaneous hallucination, etc.) crosses his consciousness.

(4) *To explain very clearly* to the subject what is expected from him. It is indeed an absurd prejudice to consider the subject of an experiment as a machine which one is concerned only to manipulate. All the superior functions, far from being paralysed, continue and even acquire a peculiar distinctness. Let us address ourselves with confidence to the intelligence, to the delicate feelings, to the most human forms of the will.

(5) To *indicate* to the subject, either at the

beginning or later, the mental and organic *attitudes* that experience shows to be the most useful, which we have studied in the course of this chapter.

(6) To hold the subject constantly *in hand*, never to *lose contact* with his consciousness, to divine his efforts and orient them in a way at the same time opportune and discreet.

In a word, to join to all the ordinary qualities of the experimenter those which all *education* and all handling of the human soul require.

How do the first results reveal themselves?

I advise that the exercise be directed to graphic signs at first (numbers, words, connected texts . . .) provided that the signs are of sufficiently large dimensions. Not that this is the easiest exercise, or the most elementary, but because it allows contact with the object, without disturbing the value of the experiment, the contact taking place only through a plate of glass, that is to say, without actual assistance from the touch—and also because it allows a very strict control.

It is usually the subject himself who gives warning of the awakening of the function. "It seems to me that I am going to begin to see

something." Here again let us dispose of a prejudice. The subject—if you are not involved with some corrupted professional—has a sincerity which reaches the point of scruple. There are unskilful experimenters—or men contaminated by quackery—who teach their subjects illusion, deceit or fraud. The subject, if he is persuaded that you value only sincerity, if you give him credit for some subtlety and if you have not ruined him by unwise suggestions, does not in the least seek to take advantage of you, nor to deceive himself. On the contrary, he takes pride in avoiding rash statements. He will say to you: "I have the impression of seeing an 8 rather than a 3; but I believe that it is only my mind which is guessing and I would prefer not to commit myself." The same subject some days later will read without hesitation, describe the details of an engraving, or a photograph, and if you question them will reply smiling: "You are trying to tease me, but it's no good. I see what I see."[1]

If, then, you have taught your subject good

[1] This does not in the least imply that it is necessary to use the *interrogatory* as a method of choice. Nothing is more likely, on the contrary, to disturb the natural processes, and to lead the experimenter astray. I have reduced its use to a minimum.

habits, you can become attentive when he begins: "I am beginning to distinguish something." Moreover, the phenomenon is quickly clarified. In the same sitting, or the following, he will declare, for example: "I see printed things . . . in large letters . . . but I do not yet distinguish clearly . . . wait . . . there is an M, there is an A, there is an N, but I do not know where they are." Verify it and say nothing. The subject will continue. "There is MA . . . MAN . . . " If the word is on the page it is needless for me to stress the importance of this indication.

If the experiment is made with a number of three digits, and if the subject announces the exact number, notice that there is only *one chance in a thousand* that the reply is due to luck. If you obtain three results in sequence, the probability of finding them fortuitously is only *one* in a *billion*. Many scientific truths have less guarantee.

I mention in this connection an advantage of *numbers* as objects of proof. A number is "unguessable," and its probability can be easily and accurately determined. On the other hand *words* allow the discovery of the first gleams of

the function. The subject who sees the number 689 "badly" does not dare to say anything, or replies at random. You can draw no conclusions from it. The subject who sees the word "constitution" "badly" will perhaps read "construction" or "institution." And if he sees "badly" the word "Napoleon" he will succeed perhaps, half-seeing, half-guessing, in reading "Napoleon." Your certainty will be less than if he had read a number, but you will be aware sooner of the awakening of the function, and you will follow its progress better.

I mention also that there is a value in the first exercises being *brief* and well *spaced*. Progress is generally noticed not during a sitting, but between one *sitting and another*.

* * *

12. The second question: "Is the extra-retinal function compatible with the ordinary state of consciousness?" will be taken up and gone into carefully later, in connection with other series of experiments. For the moment let us limit ourselves to these two indications:

(1) *There is nothing to show that mutation of regime is necessary to extra-retinal perception.*

It is perhaps a convenient, rapid, and, shall I say, *massive* way of releasing them.

(2) *It is possible, and even remarkably easy, to incorporate in ordinary consciousness the image-memories of paroptic perceptions experienced in regime δ.*

If at the beginning of the sitting, I say to the subject, even without insistence: "You will remember later everything that you are going to see," nothing is forgotten. The subject when he has returned to the ordinary regime gives a faithful account of all he has seen, recognises the objects, finds again a picture or a colour among others, in a word *incorporates* in his normal consciousness everything that he owes to the exercise of his new function.

CHAPTER V

THE MECHANISM OF EXTRA-RETINAL VISION. DISCUSSION OF IT AS A WHOLE AND HYPOTHESES

The Organs and Centres of the Paroptic Sense

THE phenomena of extra-retinal vision seem to us now closely conditioned, and it will be recognised that the results obtained are scarcely inferior in exactness to the ideas we possess with regard to other senses.

One point of primary importance remains to be cleared up. We know in what conditions the organism and consciousness exert the paroptic function: but by what means? by what mechanism? by the play of what organs? Even if we are not in a position to solve the problem, we cannot avoid it.

This investigation will necessitate a treatment of the problem of extra-retinal vision as a

whole. We will not for a moment lose contact with experimental reality, but it will be necessary to approach frankly a general discussion from which we have abstained until now. It will even seem that facts which we are accustomed to consider as accepted, are once again open to question. But from this the discussion will gain in breadth, and the care with which we shall weigh all the hypotheses, including the least probable, can only increase the validity of our conclusions.

To sum up, our duty is to *unify* and *explain* well-determined phenomena. Let us remark, however, that such an explanation is not in the least necessary for them to be the object of *scientific knowledge*. Many phenomena are considered as positively known, as an integral part of science, without its being possible to unify and explain them. It is, indeed, one of the characteristics of positive science to search for *laws*[1] at first and above all; to pass to explanations only with great caution, sometimes even with a sort of repugnance. There are many astronomers and physicists who consider it inoppor-

[1] A *law* does not unify in the sense we mean here; it is content with linking up.

tune to attempt an *explanation* of gravitation, and consider it more scientific to discuss, verify or correct Newton's formula. I apologise for insisting on such elementary ideas. But the best minds bring peculiar prejudices into the domain of psychical facts; more than one has said: "I will admit this when you have explained it to me," forgetting that he allows as perfect certainties a host of observations which no one has explained, and which *compel acceptance* as such.

We could, then, rest on the results of the last chapter, for the theory of extra-retinal vision and for the eventual practical applications as well.

* * *

In the presence of phenomena like those we are studying, the mind desirous of *explaining* can only, it seems, adopt one of the three following attitudes:

I.—*To attempt a reduction to facts already known;*

II.—*To have recourse to the supernatural.*

III.—*To seek a positive and natural explanation,* but in a new line, that is to say, *without reduction to the known.*

I. Here reduction to the known is impossible.

(*a*) It is not a question of keeping to the lazy attitude of radical scepticism (reduction to illusion, deception, etc.). It would be as easy to deny the liquefaction of gases.

(*b*) There can be no talk of *hypermnesia*. If I take out of my library a book by J. Loeb, and open it at p. 213, it will be admitted that a subject who is a shop-assistant will not know the text by heart in advance. In the same way the subject succeeds in giving me syllables, words of a foreign language which he does not understand; not, of course, without committing frequent errors in spelling and pronunciation.

(*c*) *Hyperæsthesia of touch* is easily eliminated by the arrangement of our experiments (see pp. 70-71).

(*d*) As for talking of *general hyperæsthesia*, I must confess that I find no meaning in this expression.

* * *

II. *Supernaturalism* could be *a priori* set aside from the point of view of sane method. But do not let us be afraid to examine anything that offers.

(*a*) The hypothesis of the *double*, of which

Lombroso and others are not afraid of making great use. *Another self* could escape in some way from our body, and consequently see exterior objects, without being embarrassed by an obstacle like a bandage which would only oppose our corporeal vision.

Such an hypothesis is slippery enough and discourages criticism. However we can say that the facts give it no shadow of *confirmation*. If I intercept all the luminous rays between the object and the body of the subject no vision occurs. And why should my screen embarrass the *double?*

(*b*) The theory of *immediate perception* has credentials which confer upon it some respectability. It is a discreet supernaturalism. Our soul perceives the external world directly, by itself. In ordinary life it is embarrassed by a material mediator, the body. But it is not surprising that it should manage to do without it. Our experiments, in short, would be on immediate perception *in vitro*.

But if I dress my subject in a thick cloth, from head to foot, he sees nothing. A strange immediate perception which two millimetres of material are enough to limit!

(*c*) It will be said that there is a *transference of sensation*. The visual power can leave the eyes to settle capriciously in the nose, the hand or the ear. It moves about like hysterical paralysis. Need I show that such a conception implies the purest ignorance of all the physics and physiology of vision? I much prefer an avowed supernaturalism to this shamefaced supernaturalism which plays on words and pretends to rest on precedents of an experimental nature. Besides there is not even the *appearance* of transfer. There is a substitution of a general function for a local function.

In brief all supernatural explanation breaks down on this fact: extra-retinal vision appears with the constancy, regularity, and determinism of the most natural phenomena. It is subject to all the limitations of space, matter, motion and time. It has not by any means the air of a capricious emancipation of the soul, but rather of a very well-behaved, well-regulated function of the living being. It is not a theoretical prejudice which persuades us of this, it is experience which dictates it.

III. We have, then, no choice. We must seek a natural explanation, but outside of known facts.

(*a*) Let us set aside, to begin with, any *telepathic hypothesis* (transmission of thought, mental suggestion, etc.). On the one hand this explanation would be no explanation, since it would explain a mystery by a greater mystery. On the other hand we have multiplied crucial experiments in this regard. The subject deciphers just as easily a text of which neither the experimenter nor anyone else has knowledge —or a number composed of digits which the experimenter has taken at random out of a hat, and which he has put under the glass with his eyes shut—or something equally untransmissible.[1]

(*b*) It will be said that this vision is not really *extra-retinal*. It is the retina that is the seat of the phenomenon—which is, however, itself new.

This hypothesis is serious and deserves consideration. How can it be put in a clear form?

[1] In a negative form experiment is not less conclusive. If I intercept all the luminous rays between the object and the body of the subject, the subject neither sees nor guesses anything about the object, which however is apparent to me in full light in all its detail.

(*a*) It cannot be light properly speaking which is propagated from the object to the retina. The very careful bandages which are used are of an absolute opacity, and the variation of their thickness has no influence on the phenomena. Put three bandages on top of one another; the subject is not at all at a loss; no more effect is produced by a screen.

But perhaps light is accompanied by *radiations as yet unknown*, very analogous to light in certain of their properties, but passing easily through the bandage and the eyelids and also through screens of wood or metal.

This first hypothesis does not hold.

(1) *It would explain*, if need be, *vision of shapes*, not *vision of colours*. How can it be allowed that a radiation *different* from red, emitted by a red object, could affect the retina like a red radiation? There is the question of *amplitude* which cannot be escaped.

(2) It is unlikely that a radiation, different from light, should always present itself in the same *proportions of intensity*, in the *same relations of amplitude* as light. It would be, on the contrary, more likely for inverse variations to occur, from the fact of equivalent transforma-

tions (at least from the point of view of intensity).

(3) But there is a crucial experiment. If I place the subject even without bandages, the eyes simply closed, behind a door or a curtain *only just opaque*[1] the subject sees nothing beyond the door or the curtain. Now the customary bandage is ten times as thick as the curtain. There is, then, no propagation of a radiation through opaque bodies.

(4) There is a second crucial experiment. The subject sees objects situated *outside his ocular visual field* (for example, behind him).

(*b*) But may not a sort of *indirect propagation* of light to the retina occur—something like a watch being heard when put on the forehead?

I mention this hypothesis because it presented itself for a moment to my mind. But it cannot be sustained.

In the case of the watch the propagation of sound travels across the solid and liquid media, which are very favourable. These media are opposed to the propagation of light. Further it is inconceivable that the retinal image could be formed if the rays did not pass through the

[1] On condition that the curtain be not against the body (see p. 64).

pupil. Let us not forget that there is not a simple *intensive perception* of brightnesses, but an *image*.

(*c*) We must return then to the idea of a frankly *extra-retinal* vision.

This vision the experiments give us as a general function of the periphery. Every explanation ought to take account of this fact, whether it be an appearance or a reality.

The problem thus becomes limited to the determination of the mechanism of a vision properly so-called, with formation of an image, in which all the periphery participates or seems to participate, without the intervention of the retina.

Let us state first the *a priori* physical and physiological conditions of this mechanism.

(1) The organ, whatever it is, must possess a separating power which is approximately known to us; or to put it otherwise, the luminous stimulus must be received in such a way that an element *a* of the sensory surface will be more specially affected by rays coming from an element *A* of the surface of the object. More briefly, there must be a *distribution of the stimulus*.

(2) There must be *transmission of the stimulus to the nervous centres.*

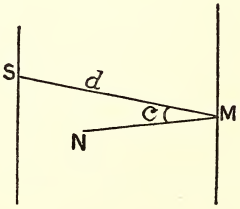
Let us first consider the formation of the *image.*

(*a*) Would it be possible to have an *image* in a new and enlarged sense of the term, that is to say not the image defined by the physicists produced with the aid of a circular hole, a lens or a zone plate; but an inequality in the division of the stimulus sufficient for an element *A* of the object to impress more especially an element *a* than an element *b* in the sensory organ?

The chances of success in this direction are weak. But let us neglect nothing.

First, what can be drawn from the classic formula—

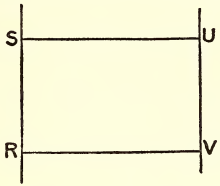
$$e = \frac{i}{d^2} \cos C?$$



Let us consider the object as the surface of emission, the organ as the receptive surface;

e is the illumination received from the point S by the point M; i the luminous intensity of the point S.

Let us assume that S is the most luminous point of the object. The two surfaces being considered practically parallel, the point S will correspond to a point U in the organ, which will be the most illuminated. And if the point R is the least luminous of the object, there will



be a corresponding point V in the organ which will be the least illuminated. But as soon as the distance d ceases to be negligible, an enormous difference in luminosity between S and R will give only a minimal difference in brightness between U and V.

Thus, for two points of a small object or for two neighbouring points of any object differing little in intensity—which is the usual case for points on the surface of an object in full daylight—there will occur only infinitesimal differ-

ences in illumination between the two corresponding points of the surface of the organ even if the distance *d* is very slight.

Further, in our experiments, the periphery of the body is flooded with light, coming perpetually from all around, which succeeds in making the illumination uniform. No vision is possible in this way.

I mention in the same connection that a likening of the skin to a sensitive paper which is affected by *contact with a negative* will not bear looking into. For we have here neither actual contact nor a transparent negative. Usually, indeed, we do not have contact of any sort.

Secondly, is a sort of image by *resonance* conceivable? I have closely examined this hypothesis, rendered plausible by acoustic analogies, but it is completely impossible thus to explain the *distribution of the stimulus*. Nor is there anything to be gained by way of analogy with *hertzian telephony* or *photo-telegraphy* (transmission of images by wires), as it is easy to convince ourselves by reflection.

I will not engage in the discussion of other still more daring hypotheses which I have

thought it my duty to form and examine, in order not to run the risk of missing the truth, and I will conclude with this, that there can be here only *an image in the narrow and classic sense of the word.*

(*b*) But our explanation should take account of:

(1) The fact that this image *is not visible* for the observer; or at least has not yet been seen.

(2) The fact that vision takes place and that thus at least *one* image is formed, as soon as a small area of skin is bared.

(3) The *physical* characteristics of extra-retinal vision.

We state, to begin with, that this image is necessarily of a *microscopic order.* Why?

A macroscopic image is inadmissible: since it would have to be formed in an organ itself macroscopic. Further as any part of the periphery can assume the function, a single organ would not be sufficient. But *n* macroscopic organs endowed with optical qualities could not escape detection by anatomy or physiology.

We should then assume an image of *microscopic* size forming in an organ itself *microscopic.* And these receptor organs must be

sufficiently numerous for local vision to be possible (vision by the hand, the back of the neck, etc.).

To these reasons of *anatomico-physiological* likelihood, are added reasons of a physical order.

Knowing the *resolving power* of an optical apparatus, we can at least locate them within certain limits, if we cannot determine their dimensions with strict precision.

If there is occasion, as here, to contemplate the psycho-physiological collaboration of *n* organs, indications of a physical order need to be corrected in a certain direction. It is probable, in fact, that the total separating power of the system will be higher than the separating power of an isolated apparatus; a hundred bad photographs of the same object, provided they are slightly different from one another, give us by synthesis as many details of the object as a good photograph. The total separating power of these organs will be noticeably higher than their individual separating power.

Now here what we know approximately is a total separating power, or resultant. It lies,

we have said, between $\frac{1}{100}$ and $\frac{1}{300}$. If we deduce from this the dimensions and characteristics of the single theoretical apparatus capable of such a power, we would have to admit that the *n real organs* are of a notably lower order, from the point of view of their dimensions as well as from that of their optical power.

What would be, approximately, the dimensions of a *single apparatus* capable of reaching a power of $\frac{1}{300}$?

If we assume it to be as simple as possible, that is to say, consisting of a dark chamber pierced by a circular hole, it should possess an opening of about 200 and a depth of about 6 centimetres. This would be then an apparatus of large dimensions. But:

(1) We must not forget that the human eye, if it were not equipped with its refracting media, in order to have a separating power of $\frac{1}{300}$ would have to be 6 metres deep. And in all the animal kingdom there is no example of an anatomical dark chamber where a considerable economy of space is not gained, thanks to *some refractive arrangement*. Thus, in order to remain within the limits of *biological likelihood*, let us

say that a *single apparatus* having a separating power of $\frac{1}{300}$ would have to be content with an opening of 200 μ and a depth of 300 to 500 μ. This would then be an organ just perceptible to the naked eye (of the size of a grain of sand).

(2) This single theoretical apparatus resolves itself into *n* actual organs, whose dimensions may be notably smaller than those we have just been indicating, because of the considerations mentioned; and thus clearly of a *microscopic* order.

* * *

Experiments confirm the results thus attained.

(1) If there is formed *on the skin itself*, by an unknown mechanism, an image of *large dimensions* of exterior objects, we should, by placing an area of the skin in the field of a photographic apparatus, observe that the subject perceives the *image* formed by the objective of the apparatus. In fact this image has every chance of being more *intense* and more *clear* than an image produced "in the large" at the level of the skin by one of the hypothetical mechanisms which we have imagined above (see pp. 118–119).

But if on the contrary, very small organs are concerned, opening more or less directly on the

surface of the skin, there should be produced, when we arrange the photographic apparatus as before, the equivalent of what is produced when we place the opening of the eye at the focus of a photographic apparatus, that is to say, at most, *vision of spots* coloured without assignable contours, or even of a single spot having the circular form of the objective.

Now, if we make the experiment, the replies of the subject are very clear; he does not perceive an image, he sees *coloured circles*, and the colour changes according as we direct the apparatus towards a mass of trees or a tiled roof.

A more complete agreement between the facts and the predictions of the theory could not be desired.[1]

(2) The experiments show that the separating power decreases rapidly when we reduce the uncovered area of the skin. When the minimal area is approached, this falls below $\frac{1}{10}$.

Now this single apparatus could, with even a rudimentary refracting arrangement, attain a power of $\frac{1}{10}$ with an opening of 6 μ, and a

[1] Let us add that the hypothesis of tiny microscopic organs alone takes account of the phenomenon of vision through a light material noted on p. 64 and more completely studied on p. 184.

depth of 8 or 10 μ. It would then be of the *order of magnitude of cells.*

* * *

We are, then, inevitably led to this conclusion: *extra-retinal vision* is brought about by *n* apparatus or *microscopic organs* of the magnitude of histological elements.

These organs are so distributed that a limited region of the periphery—a few square centimetres of surface—contains at least one and probably several of them.

They must, besides, have *multiple nervous relations* with one another, which make possible the co-ordination and the synthesis of their data.

Let us notice how much this hypothesis is in harmony with what we know of the processes of learning. The learning of extra-retinal vision greatly resembles, indeed, the progressive synthesis of data individually poor and incomplete.[1]

* * *

It remains to locate and identify these small organs. The enterprise is doubtless difficult, but we do not lack a starting point.

[1] The hypothesis of a *zone plate* taking the place of opening or objective does not essentially alter the data of the problem. It is, besides, beyond the range of physiological probability. So we limit ourselves to noting it.

(1) These small organs *can only be situated in the skin;* first for a physical reason: the thickness of the skin and the nature of the layers which compose it prevent most of the light from penetrating beyond, and let pass only a *confused* luminosity, absolutely useless for the formation of an image; then for various biological reasons: in the animal kingdom, every sensory function is a *skin* function, every sensory organ appears as a differentiation of the skin (coenæsthesis naturally excluded). If a single organ were concerned, it might be possible to admit a more or less pronounced internal situation; the thing is unlikely for this plurality of small organs. It is sufficient to look for another position for them to notice that we have no choice.

(2) *These small organs should be in relation with the central nervous system;* they should therefore be supported by nerve-terminations, or be composed of a complex combination of epithelial tissue and nerve-terminations.

(3) Each of them is probably *composed of a small number of histological elements* (in view of the total dimensions of the apparatus).

(4) Their structure, however rudimentary it

may be, should be compatible with their optical function; we ought to discover at this point the *essential characteristics of every visual organ which forms images.* Even their orientation is not unimportant: their sensory surface is necessarily turned towards the stimulus, that is to say, *approximately parallel to the actual surface of the skin.* If it were perpendicular to the surface how would the rays reach it?

(5) These small organs should be *as near as possible to the surface of the skin* so that a sufficiently clear and intense light may reach them: they should be separated from the exterior only by the minimum thickness indispensable for their protection.

(6) Let us note finally that we must not hope to discover a type of small organ which has entirely escaped histologists up to the present. The dimensions of what we are looking for prevent our doing this. Its total volume could scarcely be less than 1000 μ^3; it reaches, perhaps, 30,000 or 50,000 μ^3. The power of analysis of modern histology goes well beyond this order of magnitudes. But if the small organs, or at least their elements, could not fail to have been discovered, the presumption is that no impor-

tance has been attributed to them, or that they have been gratuitously assigned some other function. In brief, it is more a matter of a *physiological discovery* than of an *anatomical discovery*.

* * *

If we take some sections of the dermis and epidermis we notice how much the preceding considerations have cleared the ground. They have, indeed, cleared it so quickly that we feel a fear of premature conclusions and set ourselves to erect obstacles and introduce delays.

I will not linger here to enter, in detail, into the arguments and uncertainties through which I passed. They are easy to reconstruct, and the result alone is important.

The paroptic function can only be attributed to a group of microscopic organs situated in the epidermis which I call *ocelli*.

Each complete *ocellus* is morphologically constituted as follows:

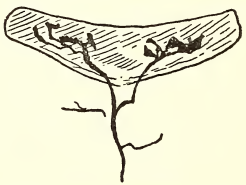
(1) Of a *nerve-ending*, already described by Ranvier under the name of *meniscus* or *hederiform termination* (*intra-epidermic expansion* of Prenant, *inter-epithelial baskets* of Dogiel).

(2) Of a coarse oval cell of finely granular

protoplasm, clearer than the protoplasm of the neighbouring cells and equipped with a voluminous nucleus of remarkable refractive powers. This cell, which rests on the hederiform expansion, has been noted and described by Ranvier under the name of *sensory* (tactile) *cell.*

(3) Of a nerve-fibre supporting the expansion and linking it with the system of ocelli.

The whole organ is strictly oriented outwards.



Physiologically, the ocellus is a microscopic eye, rudimentary, but complete. It includes:

(1) A *refractile body*, constituted by the oval cellule.

(2) An *ocellary retina*, constituted by the meniscus expansion.

(3) An *optic fibre*, constituted by the nervous fibre which supports the expansion.

The process of the ocellary function seems to us to be reducible to the following schema:

When the luminous rays have crossed the

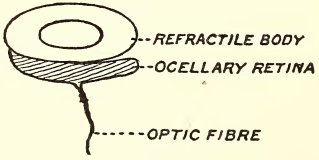
upper layers of the epidermis, whose transparence is sufficient for this, and strike the *refractile body*, they undergo there successive refractions, and come to form on the *ocellary retina* a more or less crude image. The refractile body, in fact, should behave not like a simple lens, but like a very complete *optical system*. The central part, at least, of the luminous pencil, is forced to pass successively through *several surfaces and several media:* the upper surface of the oval cell—the upper region of the cytoplasm—the upper surface of the nucleus—the thickness of the nucleus—the lower surface of the nucleus—the lower region of the cytoplasm—the lower surface of the cell.

Lacking precise measurements which will perhaps remain for a long time impracticable, it would be hazardous to attempt to imagine the detail of these successive refractions. But there is no difficulty in admitting that the *refractile body* as a whole behaves like the whole of the refractile media of the eye—*mutatis mutandis*—and comes to a focus on the *ocellary retina*. If we allow the isolated *ocellus* a separating power of the order of $\frac{1}{10}$, the depth of the "chamber" will be reduced, thanks to the inter-

vention of the refractile body from about 66 μ to a few μ.[1] The shortening produced by the media of the eye is much more considerable. We meet then with no serious difficulty in this. (A power of $\frac{1}{10}$ for an isolated ocellus is, moreover, a maximum.)

No further difficulties will arise, it seems to us, if we pass from these schematic considerations to the examination of some details and accessory arrangements.

(1) The *ocellary retina* is composed, in reality, not of a continuous nerve-plate, but of irregular terminal thicknesses, submerged in an interfibrillary substance.



(2) The ocelli are connected in some cases as it were by umbels or inflorescences of various sorts. Each of these branches occupies an interpapillary space. The fibres branching from the

[1] As for the greatest diameter of the *refractile body*, it corresponds with a striking exactness to the diameter of the opening of the chamber required by the theory.

ocelli unite in ramifications, which in their turn converge in the dermis. Further, fine ramifications seem to come from the periphery of the ocellary retina and join ramifications coming from other retina. Finally an ocellus may be supported by a fibre coming, not from the common stem, but from another ocellus. We could then distinguish, at least morphologically, between *primary* and *secondary* ocelli. For all these reasons a functional systematisation is extremely likely. Each *inflorescence* could be considered as a sort of *composite eye.*

(3) The *ocelli* are found very little distant from the basilar layer with the pigments it may contain. Does this layer have any part in the function? Do the depressions formed by the interpapillary spaces, and lined by this layer favour the action of the light on the bundles of small organs? This is not impossible.[1]

(4) The thickness of the epidermis which covers the zone of the ocelli varies with the regions of the body. It is from 50 to 100 μ in the greater part of the regions which seem

[1] How does perception of colours occur in the ocelli? Is a photochemical substance necessary, and can it be discovered? I will remind the impatient that the question is not yet settled in a final way for the eye.

precisely those that best perform extra-retinal vision. This thickness is surely not negligible, and may reduce the intensity and the clearness of the luminous rays.[1]

But the ocelli could scarcely be situated superficially without being involved in the phenomena of necrosis and desquamation of the epidermis. There is here doubtless only the indispensable minimum of protection. The eye, a privileged organ, only escapes this servitude by means of exceptional arrangements, like the lids.

(5) The ocelli are connected with a very fine nervous network which spreads through the epidermis; the network of Langerhans. Does this network have a share in the paroptic function? It could, in any case, only intervene as an accessory cause. Would it be concerned with modifying indirectly, with the aid of pressures exercised by the neighbouring elements, the

[1] Various experiments establish, nevertheless, the fact that a thickness of the epidermis double or triple that which we indicate is still transparent enough and does not notably change the composition of the light, on condition, naturally, that a strongly pigmented basilar layer does not intervene. On the other hand, the ocelli *above* this layer are not and cannot be embarrassed by it.

Dr. Georges Duhamel draws my attention, in this connection, to the fact that it would be interesting to verify whether an absorption of santonin by the subject, or even the presence of jaundice, would have a modifying effect on paroptic sensations.

curve of the *refractile body?* The hypothesis is unlikely.

* * *

To sum up, the ocelli present all the characteristics which we have been led to require *a priori* of organs of the paroptic sense.[1]

It seems impossible to hesitate long between them and other small organs or arrangements which we might be tempted to put forward.

Let us note, first, that the ocelli are, with the network of Langerhans, the only examples of nervous tissue in the epidermis.

Now the epidermis, including the basilar layer, is sufficiently thick and opaque for it to be very difficult, even impossible, to admit the existence of an optic organ *which forms images* more deeply seated. Further, the corpuscles of *Meissner*, *Pacini*, and *Ruffini*, which would be the only ones in question, have a form, structure and orientation which discourage any favourable hypothesis. Let us note besides that the basilar layer is the seat, in certain individuals and races, of a pigmentation which sometimes becomes very abundant, and which stops almost all penetration of light into the skin.

[1] To begin with the dimensions. The agreement, on this point, with the theoretical data, is remarkable.

The hederiform expansions have, as we have seen, been assigned tactile functions, without serious reason: they receive delicate tactile stimuli. In reality this attribution is due to the desire to let no anatomical entity be without its physiological use, and is also connected with the current idea that a sensorial function of the epidermis can only be assigned to touch.[1] The hypothesis of an optic function had not the slightest chance of arising while extra-retinal vision was unknown.

But what could not occur to histologists until now, what would necessarily remain for them a dead letter, acquires a full significance for us.

To sum up, short of admitting the existence of entirely unsuspected and invisible organs—which in the present state of histology seems to me pure nonsense—or refusing to give extra-retinal vision any physiological substratum at all—which borders on supernaturalism—I see no means of escaping the preceding conclusions.

[1] Moreover, it remains to be proved, as we said in Chapter II, that even a small organ is incapable of receiving two sorts of stimulus. The *nasal mucosa* seems well provided at the same time with an olfactory function and an optic function (*see* Chapter IV) and it is perhaps the same factors which operate in the two cases. We could then, if need be, allow the attribution of the tactile function to the menisci, without vitiating our hypothesis.

It can easily be imagined that I have been deeply concerned to discover a decisive experiment which would change these hypotheses to certainties. But the task is exceptionally arduous.

A heroic method, which was suggested to me by a physicist to whom I put my difficulty, would consist in attempting to *see* or even to *photograph* directly the *image* produced on the ocellary retina. The idea, perhaps, is not as chimeric as it seems. But for myself, I do not at all see what arrangement we should have to adopt in order to overcome the difficulties of this operation; and even if by some *tour de force* we should succeed in photographing the ocellary retina *in position and functioning*, would the evidence be clear enough to remove doubts? Everyone who has used the microscope will recognise that a photograph made under bad conditions of an image of a few μ^2 would be difficult to interpret.[1]

But let us be content with a less striking confirmation.

I naturally thought of the classic processes

[1] The more so as the surface to be photographed is far from being perfectly level.

of *stimulation and ablation* which are the application to physiology of the two first methods of J. S. Mill. There again the programme is simple, but he will be a clever man who shall achieve it.

Certainly we do not lack means of *stimulating* the epidermis; but whatever the irritant employed, I do not see how we can limit its action to our microscopic organs, or even to the epidermis alone. When we are dealing with this order of magnitudes, rough methods, which succeed for organs as large as the hand, are no longer of assistance.

As for the *ablation* of the ocelli, it would be fantastic to think of it. Removal of the epidermis is possible; but it is a somewhat difficult proposition—even were the subject very devoted to science. And what would this prove? Not even that the function has its seat in the epidermis; for, in this sort of operation, we do not know how far the effect of the resultant shock reaches, and we might be *inhibiting* a function in an organ which we have not directly destroyed.

I attempted, in order to ease my conscience, some experiments in *stimulation* and *anæsthesia*.

These experiments consisted in stimulating the only uncovered region of the skin, for example, by the aid of friction with alcohol, or in *anæsthetising* it by means of some opiate. I obtained some results, but I do not disguise the fact that they are almost valueless. Since, first, my method affected the entire epidermis and even the dermis; and secondly, it might have produced central parasitic phenomena, if only by suggestion.

The most elegant and illuminating procedure, in my opinion, would be to discover a substance which, when injected into the organism would settle in the ocelli, by a selective chemical action, and would temporarily paralyse them. Such a discovery is not quite out of the question—for curare exists—but it would only be the result of long trials, or chance.

Pathology will, in its turn, without yielding conclusive evidence, be able to furnish us with valuable confirmations. It would be very desirable, to observe, for example, if extended burns on the skin, or certain accidents connected with diseases of the skin, involve paroptic disorders, or *regional paroptic blindness*.

It will be valuable also, to examine what

occurs in certain anæsthesias and paralyses and in general each time that a motor, trophic, or sensorial disorder affects the skin.

Such investigations would be of a kind to put beyond doubt the paroptic function of the skin, in general.

I have thought of producing limited destructions of the skin by tincture of iodine. But these experiments have only been touched on. They deserve to be repeated. If they are conducted with many precautions, repeated a great many times and severely criticised, they might settle the question in favour of the epidermis.

Let us repeat in conclusion, and to reassure those whom our scruples and our reservations may have disturbed, that many functional attributions [1] which appear in all treatises on human or animal physiology [2] rest on probabilities less numerous even when relatively large organs are concerned, organs a billion times greater in volume than our ocelli—and that

[1] If we seek to accumulate probabilities, it would be easy to find them in the direction of comparative organogeny or in histological homologies. We note this direction without following it at the moment.

[2] The *menisci* are particularly abundant in the snout of the pig. Is it possible to use this fact for the verification of our hypothesis? I leave this to specialists in animal psychology.

most authors make fewer reservations than do we.

* * *

If the question of the *peripheral organ* can be considered as settled in an acceptable way by what precedes, it remains to clear up the question of *paroptic nervous centres*. What do experiment and reason contribute in this connection?

In the modern state of our means, experimentation, properly speaking, does not seem capable of giving us great assistance. It is enough to notice what difficulty the physiologists of the nervous system have had, up to the present, in establishing a few localisations of which many remain contestable. It is true that pathological observations can take the place of experimentation to some degree. Here observation of individuals wounded in the *cuneus* suggests itself.

As for the theoretical probabilities, they seem to be reducible to this:

(1) It is unlikely that a *single* centre controls the function. For the paroptic sense as for others, several nervous relays and several centres arranged in a hierarchy should exist.

(2) It is possible that the *cerebral* paroptic

centre is combined with the cerebral optic centre. Our experiments have shown, indeed, that the paroptic sense is educated with remarkable rapidity. It seems to benefit from acquisitions stored up by the visual sense. The subject *recognises* signs, colours, etc., to which his ordinary sight has accustomed him.

However, the phenomenon of *angular deviation* would allow the assumption of a certain duality of centres.

(3) It is, then, reasonable to hesitate between the two following hypotheses, which are not so very different from one another:

(*a*) There is only a single centre in the brain assigned to vision in general. In the case of an ordinary man this centre is in physiological relations only with those nervous relays which are assigned to ocular vision. It is, on the other hand, well linked anatomically to those relays or subordinate nervous centres which are concerned with the paroptic function; but physiologically, the relations are broken in the ordinary regime of the personality; and a change of regime is necessary to establish them.

(*b*) There is, in the brain, an optic centre and a paroptic centre. In the ordinary regime only

the first is conscious, and the two centres have no *functional* solidarity. The mutation of regime brings the paroptic centre into consciousness, and induces besides the functional solidarity of the two centres.

But it would be imprudent to exclude completely a third hypothesis.

(*c*) The paroptic function is foreign to the brain. It is situated in another region of the encephalus. The encephalic paroptic centre is divided into various inferior paroptic centres, some of which are perhaps medullary. The effect of the mutation of regime is to make us conscious of this paroptic centre, and to permit it, besides, to draw from the optical cerebral centre, such material, acquired by experience, as it may need.

CHAPTER VI

SUBJECTIVE EXPERIMENTS

At the point we have now reached, an idea automatically occurs to the mind: extra-retinal vision has the characteristics of a function common to all individuals of our species, at least as a latent function. Every individual possesses the small peripheral organs we have just been describing. It is probable that these small organs are always functioning and in all individuals: the elementary images form in the *ocelli:* the *fibres* transmit stimuli which are united and systematised gradually. But the ordinary consciousness of man does not receive them. Either they are lost in some nervous centre, without giving rise to any psychic event; or they may, perhaps, enrich some secondary consciousness, outside the traditional frontiers of our personality.

In any case, we are safe in assuming that *each of us is actually the seat of paroptic phenomena*, of which consciousness is not cognisant, and let us ask ourselves if it is not possible to bring them into the light of ordinary consciousness.

For the mutation of regime, experimentally induced, has no miraculous virtues. It is a massive, crude process, it changes suddenly the horizon of consciousness. But cannot what is obtained by a sort of violent discontinuity be obtained by a more continuous operation, by an extension or a deepening of ordinary consciousness, without break?

I therefore formed the design of "awakening" in myself the paroptic function, taking every precaution, also, to remain in the conditions of ordinary consciousness.

My experiments were long and wearying; I was only able to pursue them at the cost of a considerable expenditure of will, and sustained by the predictions of the theory. On the other hand, they have given unexpected results, and have revealed new aspects of the problem.

May I observe that it would be useless to undertake such a series of experiments without

having some guarantee of the lucidity, I would even say "objectivity," of one's own introspection. The experimenter ought to know with precision his degree of suggestibility. If he is in the least inclined to confound the perceived with the imagined, if he does not possess with integrity the *sense of reality*, it would be better for him to abstain. On the other hand, a super-acuteness of consciousness can be very advantageous.

* * *

I will not give a chronological record of these experiments, for that would necessarily be very extended. I will limit myself to noting the principal stages and results.

I. The Procedure of Research

By what procedure can the supposedly *latent* extra-retinal function be made manifest in one's own consciousness?—and this while deviating as little as possible from normal conditions, that is to say, in particular, without the *continuity of the memory and the sharpness of the critical reflection* being at all diminished.

In the absence of precedents and acquired experience we can proceed at first only by groping.

Practice in objective experiments—made on others—doubtless gives some indications, but rather as to results to be reached than the means to be used.

It is probable *a priori* that *voluntary attention* and concentration of consciousness are indispensable. But it is not easy to give attention to something not yet actual, to concentrate thought on an indefinite point. We are thus reduced to seeking a sort of silence of the consciousness and to catching in this silence the slightest indication.

Hence the man who sets out to discover will be faced by a preliminary period of waiting, with very little encouragement; and this can last for a considerable time.

The arrangement of the experiment is easy to imagine: the experimenter carefully bandages his own eyes, composes himself, places in front of his face, at a small distance, a very visible object—the cover of a book or of a magazine, a sheet of paper with any sort of signs, or a number in the frame, makes an intense effort of will to see the object, tries, if necessary, the gestures which are common to those gifted with paroptic clairvoyance; above all takes up

an attitude of extreme perceptive attention. Summed up thus in a few words, the programme seems simple. Its application is singularly complex and arduous.

At first we are led to observe that modern man, as he has been formed by our civilisation and our mental methods, has no habit of *attention*, nor even any idea of what it really is. We credit ourselves with an eminent faculty of attention, because we are capable of reading, without notable distraction, a hundred-page monograph on physics. We do not realise that these hundred pages are in reality a rapid succession of facts, images, and perspectives constantly new, stimuli constantly renewed and unforeseen. We are kept going by a phantasmagoric or cinematographic procession. We are likewise very proud of being able to meditate on a problem for hours at a stretch: we do not realise that the central idea of the problem is the basis for innumerable ramifications, and that our mind amuses itself by following now one, now another, of these divergent and capricious directions. But we have not the least suspicion of the truly *fixed* attention which grasps an immovable object and as it were squeezes it to

extract all its content. A great mathematician, a profound philosopher, is only an infant in this respect. And all sorts of ascetics, from the fakirs of India to certain modern empiricists, including the Christian ecstatics, could teach our most penetrating thinkers a great deal on this point.

Assuredly, our attention is *discursive*, in the sense in which discursion means *wandering*. We are skilful at following the flight of ideas. But if our quarry remains motionless, it escapes us, carried past as we are by our momentum.

First of all, then, we must learn to be attentive. Progress, moreover, is rapid enough. I myself did not get immediate results for two reasons.

(1) I imagined, I do not quite know why, that the first paroptic sensations would appear to me as states *internally* located, like some sort of *intra-cerebral* vision, and to discover them I took the mental attitude of a man who is seeking to clarify a memory or an image. I forced myself to see "within myself."

The result showed me that this attitude is a mistake. On the contrary we must force ourselves to see *outside ourselves*, to reach the

object at the place and the distance where it is; we must forget that we are wearing bandages, think nothing about the eyes, nor about any particular process of perception; we must act as if we had the power of entering into direct contact with the exterior things present, as if the surroundings and the objects of which they are made up came to us, declared themselves to us without intermediary. In a word everything occurs as if we had immediate perception.

(2) By a bad interpretation of what I had established in the case of my subjects, I thought I ought to direct my first attempts in vision on signs, printed words and numbers. But, as I became convinced later, such exercises assume an already developed function. It is quite legitimate to expect them from experimental subjects, because they allow a strict control, and also because the mutation of regime shortens the steps enormously. But if we seek to discover the first gleams of paroptic perception by introspection we must attempt to see large and more brilliant objects, such as a piece of furniture, a gold picture-frame, a crystal bowl, etc. The first thing to do, indeed, would be to try to perceive not any particular object but the *sur-*

roundings, the exterior light, space, however vague and confused this perception may be at first.

II. The Development of the Function

A dozen sittings, spread over about a month, none of which lasted an hour, passed without the faintest sign of vision appearing. These sittings were wearying, and disappointing, but not fruitless. In the first place, I thus learned the rudiments of *attention*. Then I satisfied myself how utterly impossible it is for a man of my nature—that is to say, normal—to suggest to himself anything, however slight. I often had in my hands a well-known object, the cover of a book of which I could reproduce the minutest details. I *imagined* the object without difficulty, but not for a second did I have the impression of seeing it.

I marvelled, indeed, at the calm clarity with which a normal consciousness marks the distinction between the imagined and the perceived, and at the ready and unhesitating assurance with which it refuses to take its desires for realities. I recommend this little experiment to theorists of certain idealistic tendencies.

The next sitting lasted several hours, with

short rests, and involved a great expenditure of energy which it would have been easy to observe from outside (respiratory acceleration, cardiac acceleration, muscular tension, etc.). I obtained a result. I saw, far from clearly, but with a striking objectivity and "exteriority," of which no idea can be formed without having experienced them, the following objects:—

The yellowish cover of a pamphlet, in the form of a brownish-yellow patch, without precise contour, and without any detail, which I saw move or change its dimensions when the object was moved.

A yellow travelling-bag with nickel catches, the bag itself as a very confused mass of a vague yellow-red, the catches as somewhat brighter.

In a still vaguer and more fleeting way, the floor of the room and the best lighted wall.

I had especially the impression of a whole, as the opaque darkness in which I had been enclosed during the earlier sittings gave place to a feeble and disturbed light, comparable to that found in the middle of a long tunnel, a light which scarcely suffices to reveal the most salient points of one or two objects.

I may add that this sight was wavering and

discontinuous. It lasted two or three minutes; then absolute blackness returned for a full quarter of an hour.

I again stress the point that in the entire course of this sitting, I did not abandon the most vigilant critical attitude—as if I were examining a section under a microscope. And though highly interested by this appearance of a result, I refrained from any conclusion.

After an interval of two days, I began my experiments again, devoting to them every day four, five, or even six hours. I hasten to say that they were much too long, and that sittings of one hour would have given the same results with less fatigue, but I could not resist the passion for research.

Nine sittings did no more than confirm the results of those which had preceded. I tested myself in other places and on other objects; but the function seemed to develop only imperceptibly.

A tenth sitting showed a sudden progress (and this from the beginning of the sitting). (1) I had the impression of a more intense general brightness. (2) I succeeded in discerning more numerous and more various objects, with a better defined shape and colour. In par-

ticular I distinguished for the first time small objects, or more exactly those objects which appeared, in a plane section, as lines of slight thickness (a key, scissors, etc.). (3) The periods of darkness appeared less frequent, shorter and to a certain degree I found myself able to end them by an act of will.

Two further sittings fully confirmed this progress, and allowed me to multiply observations which I noted, stage by stage, between the exercises.

At the next sitting appeared a sudden new increase of the function, but in an entirely unforeseen direction, and one which I had not expected. I found myself capable of *heterocentric* and especially of *sternal vision*. We will examine later these results, which are of extreme importance.

Eight sittings of long duration followed, in the course of which I found leisure for the most varied verifications, experiments, measurement, etc.

Then I suspended the series of subjective experiments, first, in order to give myself a rest, which I needed, and also to turn all my efforts towards the preparation and accomplishment of my experiments with the blind.

The subjective series is composed then of 31 sittings, representing a total of at least 150 hours of effective observations and experiments. Each of the results, which I will mention later, is founded not on some fleeting impression, on some observation which was not renewed, but on proofs and experiments repeated to satiety, by the dictates of reason which made a point of being as exacting, as meticulous, and as persistent as possible.

These 31 sittings distribute themselves, as regards results, into four periods.

A preparatory period of ten sittings, without apparent result.

A period of ten sittings, when the function appeared in a still rudimentary way.

A period of three sittings, when a somewhat more perfected function appeared.

A period of eight sittings, characterised by a remarkable extension of the function.

III. Results and Conclusions from the Subjective Experiments

The reader, doubtless, has difficulty in suppressing a certain annoyance. He is asking himself if he has not entered into a world of

phantasmagoria where we dream wide awake, and where there is no longer anything to give him solid support. I trust that this feeling will be resisted, since it owes nothing to sane critical reasoning, and arises fundamentally from our old terror of the supernatural. Qualified experimenters who wish to repeat for themselves these subjective experiments—I say *qualified* advisedly, since I would strongly advise anyone who has not had practice in experimental methods, and for whom the critical spirit has not "entered the blood," to abstain, since God knows what horned absurdity might not appear!—qualified experimenters who take the trouble will find that these experiments develop in the most reassuring way on a solid basis. They will perhaps be defeated by the monotony of the efforts and the slowness of the results, but they will feel no more disturbed, no more out of place, than if they were devoting themselves to experiments on their own visual acuity or on their aptitude for the appreciation of musical intervals.

I will, therefore, not delay to prove that I have not been the victim of auto-suggestion, illusion, etc. This is not a matter of knowledge founded on evidence. When a physicist has

made the selection and enumeration of the ions of a gas submitted to an electric field, he makes no attempt to prove that he was not dreaming. He indicates his procedure and results. Those who doubt may verify them.

I expressly oppose this tendency to treat certain psychological facts as "prodigies" for which it is well to adduce a sworn statement and substantiate a claim. The evidence of an individual witness has value when it is a question of knowing what Napoleon decided on the eve of Eylau. It has none when it is a question of recognising the properties of radium, the functions of adrenal capsules, or the mechanism of binocular vision.

* * *

Having said so much, I propose to classify and formulate briefly the results of *subjective* experiments, without omitting to compare them with the results of *objective experiments.*[1]

The agreements and divergences cannot fail to prove instructive. Moreover we will adopt an order different from that of Chapter IV, for reasons which are easily seen.

[1] Which we will call more briefly *Series O*, the subjective experiments being designated as *Series S*.

1. Under what Conditions does the Paroptic Function Appear? First it is necessary to secure a certain *intensity and fixity of attention*, and to learn to direct it by trial and error, in the course of a *preparatory period.* It is necessary next to take a mental attitude of *exterior perception*, to seek the object to be seen not inside *ourselves* but *outside ourselves;* and for this to make use of our former visual habits. For example, if we wish to try to see *paroptically* a wall about two metres distant, we must act exactly as if we were concerned with seeing the wall with our eyes; at the same time, it is certain that our mechanisms of *accommodation and convergence* will come into play under the bandage. Their part will not be negligible: they will put the central consciousness in a state appropriate to the perception of objects in space. Our eyes, although *physically* in repose, that is to say, being the seat of no optic phenomena, have a *psychological* share in the event and indicate convenient "postures" to consciousness.

This is what I call the *cerebro-visual attitude of accommodation;* although there is no occasion, in my opinion, to consider the existence of a

real physiological mechanism of accommodation in the ocelli. And in any case, this mechanism would not operate for distances of several metres (the dimensions of the ocelli make this superfluous).

It is then an entirely psychological effect or a *pseudo-accommodation.*

As for the gestures usual with the subjects, their subjective cause will only appear later.

Comparison with Series *O.* The *preparatory period* occurs in both series. It corresponds in series *O* to the first four or five mutations.

It seems incomparably shorter in series *O.* This is because in series *S* we attempt to produce by gradual degrees, what mutation accomplishes all at once. There is a continuity on the one hand and a discontinuity on the other.

The part played by attention is probably the same. But the subject, in series *O*, is benefited by a sudden concentration of mental energy, which the subject in series *S* is far from reaching, in spite of his efforts.

As for the *cerebro-visual attitude of accommodation*, the subject in series *O* attains it simply, not by round-about means. He is told to "see."

He pays no attention to his bandage, he puts no questions to himself: he *looks*.

* * *

2. How is the Training and the Development of Extra-retinal Vision Produced? There is at first confused vision of *voluminous objects*, with ridges, bosses and shining surfaces.

The attempt at vision of smaller objects occasions a truly remarkable phenomenon: *plurality of images;* that is to say, if I try to see a key, for example, I notice for several seconds a quivering, a dance of very fleeting, uncertain and incomplete images, which have neither the same localisation in space, nor exactly the same size and which are finally resolved into a single image, itself also quite unstable.

The range of vision seems to increase with its exercise. At first, everything occurs as if there were an impenetrable zone of shadow, two or three metres from the body. Then this zone moves back and the shadow little by little is dissipated.

Comparison with Series *O*. It is probable that a subject who begins to distinguish large numerals with difficulty has already an extra-retinal vision capable of grasping easily the

outlines of a voluminous object. But it is logical—if not psychological—to set the subject exercises in reading, which lend themselves to all sorts of control and measurement. If it is no longer a question of the speculative study of function, but of provoking its appearance *for practical ends*, and of helping education as much as possible, this remark should be taken account of.

The *plurality of images* is not easy to establish in the case of the subjects. But there is nothing improbable about it (let us recall the phrase cited on page 96). It would correspond, in this case, *to the elaboration-time.* It is impossible to avoid observing how much in harmony the subjective fact of plurality of images is with the hypothesis of the *ocelli.*

The increase of the *range* seems an identical phenomenon in both series. It is connected with the psychological mechanism of *pseudo-accommodation.*

* * *

3. A *time* for getting under way or *elaboration* is necessary for the perception to occur. It is about a *minute.*

This time seems to cover very diverse opera-

tions: synthesis of elementary images, pseudo-accommodation, etc. It lasts *the longer* according as the object or the detail to be seen is *smaller*.

Comparison with Series *O*. The phenomenon is alike in both series.

The connection between the duration of the elaboration and the size of the object escaped us in series *O*. It should be in the same direction there.

The order of this duration is the same in the two series.

* * *

4. Which are the Regions of the Body which are Concerned? (*a*) If there is a *complete screen* between the object and the body, the object is not seen.

We must, however, emphasise a very peculiar phenomenon. Relatively thick *woven fabrics*, enveloping the skin *closely enough*, do not arrest vision or only slightly embarrass it. At the same distance these same materials completely arrest vision by the eyes.

This phenomenon, although already indicated by several objective experiments, confused me a little and even for a moment seemed to call in

question some of the results of series *O*. Were the psycho-physiological and optical conditions of paroptic perception as clearly defined and as free from complications as we had supposed? Should we continue to exclude any other agent than light? etc.

On reflection, the paradoxical phenomenon seemed to us a further proof in favour of the theory of the *ocelli*. It is completely in accord with the laws of physics that a tissue, or in general a body, pierced with an *infinite number of microscopic holes*, should be completely opaque to the eye, which is an optical apparatus of large dimensions, but should cease to be so, at a short distance, for *microscopic* organs.

In an analogous way, a fence of closely placed stakes does not prevent us from seeing, with our eyes, the garden it encloses if we are very near to it. The same fence acts as a screen if we are a few paces from it, and would act as a screen at any distance for a gigantic eye.

Indeed, I consider that we have here an experiment which is *not far from being crucial* in favour of the *ocelli*.

Comparison with Series *O*. Let us recall a very analogous fact recorded on page 64. In

both cases *pseudo-vision through opaque bodies* occurred.

(*b*) *The participation of various regions of the periphery* is by no means the same before and after the acquisition of *heterocentric vision.*

Let us elaborate this.

At first, in series *S*, everything occurs as if the subject were seeing with his eyes. He directs his eyes automatically towards the object to be looked at and nothing outside the visual field is seen. Doubtless the subject is not thinking about his eyes, but he certainly feels that it is *his head* which is seeing, and this vision by the head is *centred* like the usual ocular vision. Everything occurs, in a word, as if extra-retinal vision and retinal vision had to have the same centre, as if the axis of the ocular visual field was *ipso facto* the axis of extra-retinal vision. It is this stage or aspect of the function that we call (extra-retinal) *homocentric vision.* And if we consider the more peculiarly organic conditions of the fact, we might call this *cephalic* vision.

And in fact at this stage the skin of the face alone seems to be concerned. If the experimenter covers over the other parts of the body,

including the hands, with very thick materials, vision seems scarcely affected. Inversely the complete *uncovering* of the body improves it only imperceptibly. Let us mask, on the other hand, a part of the face (forehead, cheek, etc.) with a wadded bandage and a sharp diminution of vision is produced, in particular of the *light perceived*. The subject has an impression as if a switch had been turned off.

This cephalic vision is also improved by constructing the bandage so that although it completely blocks the eyes, its attachments cover only an insignificant part of the nose and the cheeks.

These experiments in partial *obnubilation* show that the various parts of the face *collaborate* in vision. But in this collaboration the other parts of the skin do not seem to enter. In the language of the ocellary theory, consciousness, at this stage, receives only the data furnished by the ocelli of the face.

(*c*) *Heterocentric* vision appears, not by a gradual enlargement of cephalic vision, but suddenly, like a *new and distinct power*. One day the experimenter notices with surprise that when his head is raised he *sees with his*

chest an object at a slight distance in front of him. Nothing is more astonishing to feel than this phenomenon, but nothing is more difficult to describe. We here penetrate into an entirely new region of human psychology and personality. Our concepts, our language, our whole reflective thought, are based on an attitude and a *regime* of the ego, the personality, which assumes the exclusion of *all psychological data of the order of this sternal vision.*

We should not say, *seeing with the chest*, in order to describe the unprecedented impression received, but rather *perceiving that the chest is seeing*. And to perceive this, to fix and deepen this impression, at first exceedingly fugitive and slippery, we should have recourse to a sort of attention which seems to be suggested by instinct, but which has nothing in common with our classic attention.

(*d*) Ordinary attention is of a *cephalo-visual* nature.[1] It consists of a concentration and a projection of the consciousness of which the *head* is, so to speak, the projector; and the shape of the pencil projected is the very shape

[1] *Cerebro-visual*, if it is preferred, but the terms cephalo-visual and cephalic have the advantage of being vague and not designating prematurely any physiological theory.

of the visual field. Hence the extreme importance of this question of the field

For example: we can *imagine* two objects *at the same time* but on condition that we give them a situation such that they could be included at the same moment in our visual field (in imaginary vision). But we cannot imagine at the same time two objects, one of which would be in front of us and the other behind; or we would then have recourse to artifices; we would imagine ourselves outside our body or we would imagine a rapid turning of the head.

Abstract thought does not escape this law. Our concepts themselves we bring into this imaginary or ideal visual field, and if I had the intention of treating the question fully here, I would attempt to show that the processes of discursive thought spring from this essential condition.

Now the subject who *sees with his chest* has the impression that his attention goes down in some way from its usual position—the head—to establish itself, in a tiresome, inconvenient way, in the torso. If I might venture to employ this term, I should contrast *thoracic* attention with *cephalic* attention.

And let it be clearly understood, we can with our ordinary attention attend to this or that sensation which we receive from our chest: but this attention always has the head as seat and centre. We pay attention *from the point of view of our head.*

But in the other case the subject attends *from the point of view of his chest.*

I hope that the awkwardness of my expressions will be excused. A whole language has to be constructed to translate psychological realities so new; and not only a vocabulary of substantives and epithets. I doubt whether our verbs, prepositions, etc., are adequate for our new needs.

When the subject begins to *see with his chest,* he is unable to use his *cephalic vision* at the same time.

The result of this is a very curious state of consciousness. The subject who is holding his head straight has the impression that his face is stuck in an absolutely black hole, while there is a bright light at the level of his chest. If a small object is placed a few centimetres from his chest, this object seems bathed in light; the subject has the impression that if he could

succeed in making his attention *descend* enough, he would see the object with remarkable clearness, but he only succeeds in this to a slight degree, only a *glimpse* results.

This *glimpse* is very different from homocentric vision. That is vague and flowing, but like ordinary vision through the eyes, for example, in a room after dusk; the data are insufficient, but the attentive gaze can be turned fully on an object and can endeavour at leisure to decipher its form. On the other hand, in sternal vision, the difficulty comes from our *inability to change the centre or axis of our attention*. We feel that the object is well lighted and that the details are clearly marked, but somewhat as a man would perceive out of the corner of his eye a near and well-lighted object which he could not look at directly.

(*e*) The *heterocentricity* of this vision is clearly marked. And if the subject lowers his head slowly in such a way as to bring the object gradually into the region of the ordinary visual field, the moment the object enters it, *sternal vision ceases*. I repeated this experiment scores of times, with the most varied arrangements. It seems that, at least at the beginning of parop-

tic education, there is an incompatibility between the two visions. They cannot *superimpose* their effects. But after a while they seem to become *juxtaposed;* that is to say, it is possible to have *sternal vision* of an object without suspension of *cephalic vision of other objects* in another part of space.

It results from this *non-superposition* or *heterocentricity* that when the subject tries to obtain *sternal vision*, he ought to avoid leaning his head in the direction of the object. His head should remain straight or slightly uplifted.

We find then, but by a new way, and now including internal significance, that *declination* which we defined and described, at page 91, in connection with *series O*.

In series S, it seems to us that the value of the angle of declination could not go below 45°, and that it might attain 90° in the best position.

In series O, we found as extreme values 30° and 90°. The agreement is very remarkable.

(*j*) After *sternal vision* other regional visions reveal themselves. We have noticed a beginning of vision by the hand, the back of the neck, and the back.

It seems then that the collaboration of all

parts of the periphery would finally be acquired. But doubtless *cephalo-visual attention* must give place to *general attention.*[1]

Comparison with Series *O*. The correlations between the two series have been noticed as we went along.

Let us remark, as to the whole, that series *S* seems to gain slowly, step by step, the results which subjects in series *O* seem to reach at a bound. We find again here the massive action of the mutation.

Besides, this very slowness is valuable for the theory. For there results from it a disintegration and an articulation of the processes which peculiarly facilitate their study (something like Atwood's machine in relation to the free fall of weights).

* * *

5. The question of the *visual field* is intimately connected with the preceding question.

While vision remains exclusively homocentric the paroptic field is comparable to the bi-retinal field, or nearly so.

[1] Not that the attention could become *simultaneously* general. This seems to me contrary to the nature of all attention. But it could have generality in the sense of prompt and easy succession of regional attentions and synthesis of their data.

As the heterocentric regional visions appear, the field tends to become circular (or better, spherical).

But the subject who is in regime α has the greatest difficulty in becoming conscious of this circular field. More or less extended sectors of this field appear to him in glimpses, in a fugitive way, and preferably when he is *not thinking of it;* for example, while he is engaged in discerning an object placed in front of him, he will get a brief glimpse of an open window behind him, with the reflections of its panes, the dark bar of its sill, and the shining blue of the sky.

In series O, the subjects use this circular field with ease (see pages 87–88). I think that what embarrasses us in the ordinary state is our slavery to habits of cephalo-visual attention. The subjects perhaps do not acquire special power of attention; but at least their consciousness escapes the tyranny of cephalo-visual attention.

As regards the *depth* of the field, or the *range* of the vision, everything occurs in series *S* as in series *O* (see page 87), but more slowly.

But series *S* shows us a remarkable fact. Sternal vision seems to take place only for

objects at a short distance. This distance increased a little with practice but without exceeding 50 centimetres. This is doubtless caused by the fact that beyond this distance every object situated in the sternal field is situated also in the *cephalic field*, even for a high value of *the angle of declination*.

It seems also, *in series O*, that the subjects reserve their sternal vision for discerning objects at very close range. But we cannot be positive on this point.

* * *

6. In series *S*, paroptic space has, at the first attempt, the same characteristics and properties as our ordinary space. The experimenter cannot fail to be struck by them. We should expect a hesitating reconstruction of the accustomed space, and a slow process of adjustment and agreement. This at least is what I had envisaged under the influence of theories which are much in vogue on the nature of space.

Nothing of the kind. The subject sees, badly, confusedly, "obscurely," if I may use the word. But from the very first he sees objects in *their place*, at their *respective distances*, and in *their*

proper size. And it is impossible to detect any work of co-ordination, reduction to scale, etc. Now, as in series *S* all the processes are very well displayed, such work is extremely unlikely. Everything occurs as if we were entering suddenly, by a new door, *into an absolute space endowed with absolute properties.* I am no metaphysician, and I am on my guard against touching the metaphysical side of these questions, but I have to note an experimental fact accurately.

This sudden acquisition of paroptic space does not happen however without a peculiarity worthy of attention: if I extend my hand towards an object I have just discovered paroptically, my hand falls a little to one side, generally a little to the right. And if I move my hand slightly until it touches the object, I then have the curious impression of a *displacement as a whole* of paroptic space; as when we touch the screw of the stage-plate of a microscope and all the visual field seems to move at once.

Comparison with Series *O.*—The analogies are evident, for the properties of space (see pages 77–80) as well as for the phenomena of angular deviation (see pages 80–81). But in

series *S*, the average value of this deviation seemed to me lower. I do not believe that it exceeded a few degrees. It tends in both series to vanish in the course of training.

As for the probable cause of this phenomenon, I see nothing which needs modification in what I have said on pages 77–80.

* * *

7. The fact that paroptic space possesses its essential properties at the first attempt does not prevent their exploration from being *fumbling* or the recognition of special objects from being *successive*.

It may seem hardly reconcilable logically that the subject should have, from the beginning, an intuition as a whole of a three dimensional space, and of the general disposition of objects in that space; and that he should then be seen proceeding in an investigation full of fumbling and even of error.

This apparent contradiction, which may have already struck the reader in Chapter IV, is solved, I believe, by means of the subjective experiments.

The subject would have at first, for example, the intuition of the most general spatial relations

which existed in the room where he was. *Above, below, in front, behind, to the right, to the left,* are ideas which apply immediately and without miscalculation. And if he guesses that there is towards the right something dark and voluminous, then perhaps he has to make many efforts and trials to identify this object, to distinguish and classify its details; in this work *his attention,* which is still awkward, should proceed successively, should, as it were, *spell out* the data, and he may happen not to give a correct synthesis of his successive operations (as with the subject in series *O* who reads 249 for 492). But, and this is what seems to me to be essential, there is from the first a *given space* with *its structure.* Further training allows a more or less minute *inventory* of the contents of this space, and a more or less correct *synthesis* of the information acquired; but paroptic space itself does not in any way seem like the result of training.

I shall perhaps be told that after these explanations, it seems much less surprising that particular objects are seen from the first in their scale, since, in fact, their relation to the whole is, if not clearly appreciated, at least confusedly presented. I do not deny it, and it would per-

haps be well slightly to tone down some of the statements formulated on pages 77–80.[1]

The progressive *localisation* of objects in space has the same characteristics, and the same development in both series.

In particular the use of the hand as a *kinesthetic instrument of survey* is identical in both. The subject knows that there is an object before him and he wishes to see it. But as he does not know its exact distance he does not know where to *look*. And if the object is not large, this fumbling could be prolonged without any other result than a fatigue of attention. But let the right hand touch the object or its support and after a short delay for elaboration, vision takes place.

I have nothing to alter in what I said on pages 84–85 as to the interpretation of the phenomenon.

This work causes an *attitude of accommodation* in consciousness. As we remarked before, it is concerned with a psychological attitude (or a central mechanism) in all likelihood, and not with an accommodation properly speaking (see page 86).[2]

[1] I protest once more that I have no particular affection for, or aversion to, any general theory of space. I seek only to give facts their exact weight, shade, and significance.

[2] The *attempts to walk* in series *S* give place to the same observations as in series *O* (see p. 85). There is an overthrow of vision, a blinding.

The objective considerations agree in this again with the data of series *S* (see page 86).

* * *

8. As for the *optical conditions* in series *S*, we have had occasion to note several of them already: the part played by screens and pseudo-opaque bodies (see pages 162–164).

Let us note, still in connection with the phenomena of opaqueness and transparence, an effect which I have often observed: certain not very thick objects, especially sheets of paper and cardboard, lose all consistency for extra-retinal vision; they vanish and do not in the least hide any solid object behind them. Marks made on these sheets become, at the same time, visible only with difficulty, since the paroptic sense lacks a starting point for localising them which the surface of the sheet ought to give. The difficulty is somewhat reduced if the sheet is laid on something solid and thick.

Such an effect, which would be scarcely observable in series *O*, is not very readily explicable.

More solid bodies, placed beyond the sheet of paper, are not hidden by this sheet for all

regions of the skin; they then form ocellary images. And they attract attention perhaps at the expense of the sheet. Does the central pseudo-accommodation occur for them and on them? And is the sheet not seen?

THE PERCEPTION OF COLOURS appears as normal in series *S*, but it is lacking in clarity, because of the weakness of the light perceived. We might compare them with colours seen by twilight.

Here are the shades recognised with certainty:

White	Bluish white
Brick red	Gold
Reddish yellow	Brown
Azure blue	Black

This list, in spite of the gaps it shows, is sufficient to make it clear that paroptic vision of colours takes place throughout the whole range of the spectrum.

I would add that *sternal* vision of colour seems much clearer than homocentric vision and this because the light perceived is much brighter. But it is so difficult to fix the attention in this position and keep it there that I could not possibly draw up a list of shades unmistakably recognised.

The part played by the *nasal mucosa* is not clear in series *S*.

Comparison with Series *O*.—Except on the last point perception of colour shows the same characteristics in both series.

Considered as a whole, *the optical conditions seem the same*. Nevertheless, certain facts remain to be examined.

* * *

9. Series *S* leads us to particular considerations concerning *illumination*.

We will call the illumination which would be perceived or is perceived by a normal eye in any given case the *real brightness* of this case;

Perceived brightness, that of which the subject is conscious by the aid of the paroptic sense;

Rendition, the relation of the brightness perceived to the real brightness.

Or, what comes to the same thing, we will assume by convention that the *rendition* of the normal eye is equal to unity.

The paroptic rendition is very weak, at the beginning, in *homocentric* vision. In full light it is of the order of $\frac{1}{100,000}$. It improves with exercise, and reaches the order of $\frac{1}{10,000}$. Per-

haps it reaches $1.\frac{1}{000}$.[1] Naturally I am indicating orders of values, and do not pretend to furnish exact measurements.

The interpretation of these values is easy; to say that the rendition is $1.\frac{1}{000}$ in a fully lighted room, is to say that the subject *sees* things a thousand times less *clearly* than if he were using his eyes.

But experiment reveals a surprising peculiarity; in a room at night where there is only a very faint light, which is of the order of $100.\frac{1}{000}$ of average daylight, the brightness perceived is scarcely less than in full daylight, and the rendition reaches the value of about $\frac{1}{3}$. There would be then a most marked paroptic *nyctopia*.

The above estimates concern *homocentric* vision. For *sternal vision* the rendition seems incomparably higher in full daylight than for *homocentric vision*. It reaches probably $\frac{1}{3}$, $\frac{1}{2}$ or even unity.[2]

[1] There is even sometimes a strong increase above this value, but by a sort of *leap* so rapid that any serious estimate is impossible.

[2] The above estimates, in spite of the reservations which accompany them, will seem to certain readers ambitious and even arbitrary. It is well then to give an idea of the way in which they can be obtained or verified.

Let there be two rooms *A* and *B*, contiguous or very close together, and with an easy communication between them. It would be well that the general arrangement of the two rooms and even the objects

I do not attempt to find an explanation.

The curious phenomena of *dazzling* are connected with this question.

At the beginning of the training, in series *S*, the subject, when he turns *in the direction of the source of light*, for example towards a window opening on a fine clear sky, is suddenly enveloped in an absolute obscurity, and for several minutes he will have great difficulty in seeing anything at all paroptically. I called this unexpected effect *black dazzling*.

A little later, if the subject remains in a *sun-lit* place he is suddenly overcome by an impression of a confused, very intense, reddish light, which drowns all shapes and makes vision impossible. This I called *golden dazzling*.

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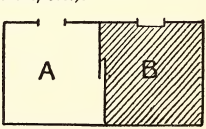
with which they are furnished should be as much alike as possible (for example, two bedrooms, or laboratory rooms, etc.). Room *A* is fully lighted by daylight. Room *B* is kept in a certain obscurity which is easy to diminish or increase by the most ordinary means (blinds, shutters, curtains, etc.).



The experimenter takes his place in *A*, makes an attempt at *paroptic* vision and turns all his attention to making an estimate of the light which appears to him. Then he passes rapidly into *B*,

In the same way I have never succeeded in seeing, even confusedly, a source of artificial illumination (electric light, candle, etc.)

Moreover, for artificial lights, at least those which are in ordinary use, the paroptic rendition is very bad. In a room lit by a 50 candle-power light, the *brightness perceived* is not felt to be higher than that which there is in a room where only moonlight penetrates.

Let us finally say a word on the *shadow phenomenon*. The experimenter, in series *S*, is constantly struck by the sharpness and strength of the shadow his own body throws on neighbouring surfaces, in particular on walls.

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takes off the bandage, looks *with his eyes* and asks himself whether the *real* brightness in *B* is higher or lower than the *paroptic* brightness in *A;* whether the whole effect in *B* is more or less dark than the whole effect in *A* perceived paroptically. The more similar the arrangement in *A* and *B* the easier the reply will be. Let us note well that it is not a matter of *measuring*, a quantitative operation which would be fantastic by such means, but of saying whether, qualitatively, one impression is more or less intense than another. A host of scientific measuring operations depend on such estimates.

According to the response, the brightness of *B* should be altered experimentally, and the test repeated, until the two impressions seem of the same intensity, or, if a negative formula, and one more modest, is preferred, until it is quite impossible to say in which room the impression of brightness is the stronger.

It will be enough to measure, by any ordinary photometric process, the relation between the *real* brightness of *B* and the *real* brightness of *A*. This relation will be simply the expression of the paroptic rendition in the case considered.

If he lifts his bandage, and tries to estimate the same shadow, in the same optic conditions, with his eyes, he finds that it is much less clearly detached, that it forms on the wall a silhouette so indefinite that he would not think of noticing it. The shadow of the body only becomes clear for the eyes towards dusk. I am, of course, assuming the most ordinary conditions, that is to say a room of average dimensions, conveniently lighted by daylight, and not the case where the body is directly accessible to the rays of the sun.

I can find no exact explanation of this phenomenon. Perhaps the analogy with what occurs, for the eyes, in a room at dusk, may provide an indication. Perhaps this peculiarity has some link with paroptic nyctopia.

COMPARISON WITH SERIES *O*.—The average *rendition* is certainly much higher than in series *O*. Without allowing us to estimate it, various observations on the increasing difficulty of the readings, etc., suggest that with an already educated subject, the rendition in full daylight, and in half light, approaches unity, and does not fall below $\frac{1}{10}$. In semi-obscurity the rendition seems to exceed unity (see pages 65–67).

Nyctopia, at least to a slight extent, seems common to both series.

I attribute the inequality of rendition to the fact that the subjects use regional vision infinitely better. In series *S*, it is only homocentric vision in operation.

As for nyctopia, if it exists, only a wider knowledge of the ocelli could account for it.

Without having been positively noticed in series *O*, the phenomena of black and golden *dazzling* are probably there. At any rate this is the best way of interpreting certain remarks with which subjects are apt to commence: "I don't know what's the matter, but I no longer see anything." "Wait a moment, everything is confused," etc.

The *rendition* of *artificial* light in series *O* seems of the same order as daylight, or of a very similar order. There is then, here, a certain disagreement between the two series.

The *shadow* phenomenon could scarcely be established in series *O*.

* * *

10. Of what order is the *resolving power* of extra-retinal vision in series *S?*

I have only been able to estimate it with

any precision in experiments on *homocentric* vision.

It is a remarkable fact that it seems unequal according to the nature of the objects looked at.

For signs drawn on paper it reaches with difficulty $\frac{1}{10}$.

If an object of three dimensions is concerned, made of metal say, it rises to about $\frac{1}{100}$.

For objects of a like nature, but larger, and seen at a distance of several metres, it does not seem to be much lower than $\frac{1}{150}$ or $\frac{1}{250}$.

To sum up, the separating power seems very weak as regards a small thin object seen at a very short distance. It becomes much greater if the object has thickness, consistency, and is seen from further away.

Comparison with Series *O*.—The separating power is not very different in the two series for vision at several metres. It is much higher in series *O* for objects seen from near at hand, in particular for graphic signs. Perhaps it would be well to attribute it to the use by the subjects of regional visions, and especially of *sternal vision*, when objects close by are concerned.

Let us notice again a subjective datum which I submit for what it may be worth:

the subject has the impression, not that the objects themselves are vague, or rough in outline, as in a very bad photograph, but that a sort of false and confused light prevents him from seeing them well. It seems to him that with an additional effort, or a better light, he would grasp details which escape him.

To sum up, if we except a few points of minor importance, the agreement between the two series is as complete as could be hoped for. But series *S* is not limited to confirming the other; it reveals intimately certain processes, or shows us an unexpected aspect of them.

Further, it allows us to reply affirmatively to this primary question: "Is the extra-retinal function *compatible with the ordinary state of consciousness?*" and to contemplate all the consequences of such a fact. It removes, finally, all doubts as to the *universality of the function.*

But why, it will be asked, has man never noticed his possession of this paroptic sense? While the exercise of it seemed connected with regime δ this ignorance was explicable. It now seems very strange.

I would reply: when has man made the necessary effort to discover this unknown power?

What precedes has established the fact that the effort is no small one, even for a subject who knows where he is going and possesses some indications of the route to be followed. The sort of attention required is exactly the sort least familiar to man in modern societies. And if men in some vanished society had suspected or even practised extra-retinal vision, what would we know of it? Every time that a piece of evidence from a far-distant past seems to affirm the existence of some mental function which we do not possess, have we not the convenient habit of rejecting it as absurd or legendary?

A more general question, and of greater range, is this: in the evolution of the living being and of consciousness, what part, what position is to be assigned to the paroptic function? Have our ancestors, human or animal, had the use of it? When and why did they cease? How is it that this desuetude has not involved the progressive disappearance of the organ itself and all possibility of function?

To reply with the necessary fullness, it would be necessary, I fear, to leave the positive ground which I have prudently held to, and outline some

ambitious theory of consciousness and the living being. We will not yield to this temptation.

But it is perhaps not too daring to indicate a few elements of the reply, to suggest a few reflections.

1. The law of atrophy from lack of exercise is perhaps not so general as is thought. Is not nature much more conservative than the people of the nineteenth century have commonly assumed? And I mean here by conservative, not that which is opposed to innovations, but that which tries to lose nothing it has acquired, even if it has fallen out of use, or seemed so.

2. It is not absurd to assume that, in animal evolution, the paroptic function *preceded* the retinal function. Vision would have been a general function of the skin, or to speak more accurately, the function of a diffuse organ, before that concrete and localised organ which is the eye was developed. By its very dimensions and complexity, the eye comes to have an optical superiority over the ocelli; this superiority, though slight at first, has become constantly more pronounced and has enabled it progressively to monopolise the visual function.

And, in fact, biology has suspected something of this kind for a long time. But we have never dared to think of *vision* by the skin, *properly speaking*, with the formation of *images* and *perception of objects in space.* We have been content to admit in the case of many of the lower animals a general sensibility of the epithelial elements to the light. We were far from guessing at the existence of the ocelli.[1]

Moreover the case of the ocelli has nothing very exceptional biologically about it. Is it not indeed a sufficiently general case in organogeny? Do we not see the *sweat-glands* persisting and functioning, beside the *kidneys*, and assuming an important part of the total work of elimination, in man, although the kidney is a very old, improved and perfected organ? Are there not a host of regional or diffuse functions which we only half know, or misunderstand, and which have been neither suppressed nor even perhaps weakened to any considerable extent by the appearance and development of a central organ? Have we not in reality diffused lungs, a diffused

[1] What animal types possess the paroptic function in *modern times?* This is a question that comparative psycho-physiology seems in a position to discuss usefully.

heart, perhaps a diffused liver? And do we not see on the other hand functions like lymphopoiesis which are still, in man, in a very inferior state of centralisation? Everywhere, in organisms, there can be found, without any mutual eviction, a certain centralisation and a certain regionalism.

3. The *subjective* experiments, in themselves, make it comprehensible, in the field of consciousness at least, that retinal vision could not fail to eclipse extra-retinal vision. If at the height of an experiment, when a good paroptic vision is being produced, I touch the bandage lightly, so that a small filtration of light reaches the lids, although they are *closed*, all vision immediately disappears. The slight stimulation of the retina is enough to *engage consciousness*.

This is why the subjects, to the great astonishment of sceptical people, sometimes ask to have their bandages readjusted during a sitting, and are very pleased to be given an especially thick and woolly bandage. For the paroptic sense to work, at least to work *consciously*, the retina should be in complete repose.[1]

It is this fact that has been carelessly observed

[1] On the other hand the paroptic sense is in no way embarrassed by the exercise of another *non-concurrent* sense, such as hearing or smell.

and misinterpreted by such doctors as have alluded in passing to the sensory disturbances of somnambulists. They talk freely of *a paralysis of the retina*. This is another of the improprieties or even errors that pathology inflicts on physiology. There is an absolute repose of the retina, or, if luminous stimuli still reach it, there is, on the part of consciousness, *a refusal to take account of them*. Is the ear of the sleeper paralysed because he does not hear the servant knocking on the door of his room?

But this *refusal of consciousness* seemed to me impossible in the ordinary state. It is, if not impossible, at least difficult, in the case of subjects in regime δ. Hence their desire to have the bandage not only perfectly opaque, but also perfectly *hermetic*.[1]

4. I said above "for the paroptic sense to

[1] In connection with this *refusal of consciousness*, Dr. André Nepveu points out to me that he had found something very analogous in cases of *amblyopia ex anopsia*. It is known that one eye, which through a disease or defect, however slight, is less convenient to use than the other, ceases little by little, in certain cases, to function and no longer conveys any impressions to consciousness. And, moreover, this eye preserves its essential qualities as an optical organ. The image is indeed formed there; but consciousness "refuses" it. This is what takes place, for example, in strabismus, where the amblyopia can be treated by orthoptic exercises. A re-education or a re-awakening of the function occurs which is comparable to what we have obtained for paroptic vision.

work, *at least to work consciously*." This reservation is made advisedly. I am not at all sure that even in the normal modern man, who is a long way from suspecting the paroptic function, this function really plays no part. Certainly, it does not seem to influence our voluntary activity in any way, or even our external motor reactions of a reflex order. It does not prevent our bumping against a piece of furniture unperceived by our eyes, or trying to sit down on a chair pushed out from under us. But it would be interesting to discover whether certain intra-organic processes are not controlled or released by sensations from the paroptic sense; or even whether, psychologically, affective states which our consciousness receives without in any way guessing why, do not result from it.

And even if the part played by the paroptic function were non-existent or negligible in *waking* life, while the eyes are open and working, would it remain so in the *nocturnal* and sleeping life?[1]

[1] In connection with sleep I may note this accessory hypothesis: Could not the rhythm of waking and sleeping among animals as well as man make the *normal unconsciousness* of the paroptic function advantageous? For we have our eyelids to protect our eyes almost completely from the light when we wish to sleep. There is nothing analogous to this mechanism for the ocelli.

I leave these questions to physiology and psychology, and I do not regard them as in the least idle.

In brief, the development of the eye naturally *excludes from normal consciousness* the paroptic data; without there being any necessity for admitting, at the same time, that these paroptic data play no effective part in the life as a whole of the existing organism.

CHAPTER VII

EXTRA-RETINAL VISION AMONG THE BLIND

Does our theory appear, at the end of all this experimental work, sufficiently well established for us to be able to contemplate an attempt at *synthesis* and to pass to practical applications?

We have thought so.

Now there is one application which must force itself on the mind: to evoke the paroptic function in the blind and thus to endow them with a vision, which, once educated, would be hardly inferior to ordinary vision in many of its uses.

The problem may be stated thus:

Series *O* shows us the *end* to be reached and some of the means to be employed.

Series *S* proves to us that this end can be reached without a departure from the normal

conditions of consciousness. It likewise helps us to determine the technique.

* * *

The present chapter will be very brief and very incomplete. It is intended to give *up to the present* an idea of the work still in progress and the results already obtained. Such a communication could only have gained by delay. But various considerations lead me to make it public at once.

My labours with the blind encountered obstacles, of which this is not the place nor the time to speak, and which have no real bearing on the matter, nor on the inadequacy of my technique.

It is difficult to realize what ill-will must be overcome or avoided to gain permission to do men a service whose value seems not to be called in question.

* * *

I will not enter into the details of the technique which I have developed. Indeed I do not consider that I need disclose it at all, so long as my labours on extra-retinal vision have not as a whole received the sanction of scientific opinion and so long as no means at all have

been given me of making an extended application of them.

I will only point out that this technique rests on a methodical training of attention by the most appropriate processes, without use of the mutation of regime.

Mutation, the *massive* method, would be such as to shorten the labour considerably and hasten the results. But it has the defects of its qualities. It produces a discontinuity in conscious life; it makes a wound which is later difficult to "sew up." And in particular it seemed to me still more instructive and more elegant to attain the result without using the most powerful methods at my disposal.

Besides a perspicacious reader will not fail to perceive in the preceding chapters the elements and the principles of the method I have built up. The rest is a matter of adaptation and application.[1]

* * *

[1] Owing to a lack of sufficient resources, I have been able to use only a small part of the means whose efficacy I know; I have had to be content with fortuitous arrangements which are clumsy and not highly productive. But I have a plan prepared for the establishment of an Institution for paroptic re-education, of considerable importance, and it is at the disposal of foreign scientists who may find in their countries the necessary support and assistance.

By means of this technique the first manifestations of the paroptic function appeared after the fourth or fifth sitting, *in a blind person taken absolutely at random and completely lacking all retinal sensibility.*

It would not be, in fact, very convincing to begin on the method with one of the *half blind* people who are very numerous, whose retina is far from being inert.

It should be attempted on *completely blind* people who have been the object of a double enucleation, or whose organ has been the seat of a *physiologically redhibitory* lesion.

It is not a question of one of those more or less fantastic utilisations of the retina or the optic nerve, which have been attempted again and again. *The more completely the ocular vision has been destroyed the more favourable will be the conditions.*[1]

Besides, in cases not sufficiently clear, the use of a thick bandage will remove all the difficulties of interpretation.

There is no decisive reason why those *born blind* should be incapable of paroptic vision. Nevertheless their education involves various

[1] The case of *central lesion* excepted.

difficulties and it is reasonable to await a perfection of technique before working on them. In fact the blind person *who was once able to see* is easy to guide and orientate in this tentative discovery of a new form of vision. All the *visual* expressions of our language have a definite signification for him. He talks the same language as do we. The individual *born blind* is a stranger whom we understand badly and who understands us not at all.

Men *blinded in the war* whose blindness is recent, and produced at a full adult age, are the best subjects to choose. They are unfortunately the most mistrustful[1] and the least controllable.

No sitting in the period of training should, it seems to me, exceed an hour, or an hour and a half. As in series *O* and series *S*, the progress appears, not during a sitting, but between sittings.

The characteristics of extra-retinal vision seem exactly the same with the blind as with normal men.

The progress is more rapid than in series *S*.

[1] Naturally this mistrust and this ill-will would quickly disappear if the experimenter were to present himself invested with an official prestige; and also, *a most important detail*, if those interested could believe that they were *definitely in possession of the poor advantages that their infirmity secures.*

I attribute it on the one hand to certain habits of attention formed by the blind, and on the other hand to the collaboration of the experimenter and the subject.

It is remarkable that the *heterocentric attitude* so difficult to maintain and utilise for the subject of series *S*, is quickly adopted by a blinded person. However recent his blindness may be, he already escapes the *tyranny of the retinal visual field and cerebro-visual attention.*

The part played by light,

Sensitivity to colours,

Successive perception of the content of space,

Errors in localisation, etc., etc.,

seem exactly of the same order as in series *O* and *S*.

As for the *separating power*, it would evolve rapidly enough, from what I have thought I noticed, but I hesitate to give it any approximation whatsoever.

Let us note, finally, in support of the ocellary theory, that the subject invariably refers to an impression analogous to that described on page 160.[1]

[1] Have not the blind in the past come to have recourse to extra-retinal vision, without anyone suspecting it and without their having been conscious themselves of the *originality* of their procedure? This is a question which has occupied me deeply, and which I shall

To sum up, the theory of extra-retinal vision seems to be about to solve the ancient and popular problem of the "vision of the blind" and to be able at the same time to add to the prestige of experimental psycho-physiology by providing one of its first practical applications.

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perhaps some day discuss. I would only suggest now that the thing is likely; but that the rare cases of blindness where this function has been exhibited, have been quite unaware how to develop it and remained in their first tentative stage.

Conclusions

It will perhaps be useful to review briefly the results of this work, and to classify them.

1. The human skin contains various arrangements and structures of a microscopic order, where nervous tissue is involved and which are morphologically well known. These have been provided by histologists with functions, in particular with *sensory* functions, some of which are very insecurely founded.

2. These attributions of function need to be revised from a closer study. But histological method cannot by itself fulfil this task. Every problem of assigning a sensory function needs the assistance of psycho-physiological experimentation.

3. It would be imprudent to declare *a priori* that in these attributions notice need only be taken of sensory functions already known and classified. The method of *detection* employed by contemporary experimental psycho-

logy shows us that there can exist in man higher or lower mental functions of which common experience gives no idea, and which remain *to be discovered.*

4. Our experiments place beyond doubt the existence in man of a *paroptic function*, that is of a function of visual perception of exterior objects (colours and form), without the intervention of the ordinary mechanism of vision through the eyes.

5. Light, in the usual sense of the word, is the exciting agent of paroptic perception or vision.

6. Opacity, transparence, translucence of objects, reflection of images in mirrors, and so forth are perceived and interpreted by paroptic vision in the same way as by ocular vision.

7. Variations in *intensity* of light have practically the same effect on paroptic vision as on ocular vision.

8. Paroptic vision shows, however, a certain degree of *nyctopia*, which is more marked for the vision of colours than that of forms.

9. Paroptic vision gives a *perception of the colours* of the spectrum qualitatively identical with the ordinary perception through the eyes.

10. It seems, however, to recognise *spectral limits* which are wider on the ultra-violet side.

11. Paroptic vision is certainly *extra-retinal;* that is, it takes place without an image being formed on the retina, and without the retina receiving any excitation.

12. *Touch* has no place in paroptic perception.

13. The *nasal mucosa* plays a part in the paroptic perception of *colours;* it seems to play none in the perception of *forms*.

14. Perception of colours occurs even when the nasal mucosa is not stimulated.

15. Perception of colours by the nasal mucosa is not of an olfactory order; that is, it does not consist in a recognition of *odours* belonging to the colouring substances. It is a perception specifically *optical*.

16. *Any region of the periphery* of the body, provided that it is of a certain size, can carry out *by itself* a certain degree of extra-retinal vision (perception of forms and of colours).

17. The *minimal area* necessary seems to lie between a few square centimetres and a square decimetre of tegumentary surface. It varies with the region considered.

18. Paroptic vision improves as more numerous and greater regions of the periphery come into play.

19. The functional importance of various regions is unequal.

20. The general characters of *paroptic space* are the same as those of visual space (ocular).

21. The *scale of magnitudes* is the same for paroptic vision as for ordinary vision.

22. *Paroptic localisations in space* show an *angular deviation* from ordinary visual localisations. This deviation tends to disappear with education.

23. Paroptic perception of the content of space is *successive* before being *simultaneous*.

24. The *range* of paroptic vision increases little by little with use, by a phenomenon of *pseudo-accommodation*.

25. The extra-retinal *visual field* is circular.

26. The *paroptic and ocular directions of gaze* have not the same axis. Their axes form a constant angle, whose value lies between 30° and 90° (the notion of *declination*).

27. An *elaboration-time*, relatively constant, is necessary for paroptic perception to be released.

It is, at the beginning of training, about a minute.

28. The *resolving power* of extra-retinal vision lies, according to our experiments, between $\frac{1}{100}$ and $\frac{1}{300}$, in normal exercise.

29. The *paroptic function* remains latent in the ordinary man. It is awakened through a special technique.

30. The paroptic sense has as organs the *ocelli*, microscopic organs situated in the epidermis.

31. The *ocellus* is a rudimentary but complete visual organ. It possesses a *refracting body*, an *ocellary retina*, and an *optic fibre*.

32. Each ocellus is able to form a coarse image corresponding to an individual separating power equal to or less than $\frac{1}{10}$.

33. The ocelli are grouped in umbels. Each unbel can be compared to a kind of composite eye.

34. The ocellary images are systematically received, thanks to the converging arrangement of the fibres. They tend to form by fusion a much richer central image corresponding to a theoretical separating power much higher than the individual separating power of ocelli.

35. The order of magnitude of the ocelli explains (1) that an apparatus for *accommodation* would be useless; (2) that certain unexpected phenomena such as *pseudo-vision through opaque bodies* (vision through cloths) are produced.

36. Without it being possible to determine the localisation of the *paroptic centre* or *centres* there is reason to suppose (1) a connection between the optic cerebral centre and the paroptic centre; (2) an extra-cerebral situation for the principal paroptic centre or at least for a secondary paroptic centre.

37. Subjective experiments confirm on the whole the data of objective experiments. But they supply a certain additional precision and certain interesting supplementary material:

(1) Paroptic perception is compatible with the ordinary state of consciousness.

(2) It requires, in order to arrive in consciousness, a new and methodical cultivation of attention.

(3) It can only take place in the absence of all ordinary visual perception.

(4) It is desirable to distinguish in extra-retinal vision, homocentric vision and heterocentric vision.

(5) Paroptic recognition and localisation of objects are accompanied by a *cerebro-visual attitude of accommodation.*

(6) The *paroptic rendition* (relation of perceived illumination to real illumination) is very feeble in homocentric vision, much greater in heterocentric vision.

38. Theory suggests and experience proves that blind persons (with the exception of cases of central lesion) are the most suitable subjects for the education of the paroptic sense.

39. A specially devised technique allows us to obtain, after a few preliminary sittings, the first signs of the function in the blind.

40. The paroptic function in the blind seems to present exactly the same characters as in the clairvoyant.

APPENDICES

APPENDIX A

THE ORIGIN OF THE THEORY AND ITS RECEPTION

At the beginning of July, 1918, I finished a number of experiments which I had earlier conceived in outline. Throughout I had the good fortune to succeed in producing and repeating the crucial experiment or experiments. The hypotheses remaining at the end of this work, which tended by their natural co-ordination to constitute *the ocellary theory of extra-retinal vision*, ceased one after another to be mere mental views, anticipations or approximations *on paper*. Submitted to exhaustive laboratory tests they now appear as the very foundation of the facts. On one point only, *the spectral limits of paroptic perception*, circumstances have not allowed me to attain the desirable precision. But this gap, which is only partial and can be filled in other ways, neither

affects the theory as a whole nor need it delay the theory as a whole nor need it delay the study of practical applications.

The principal, or at least the first, of the applications to emerge was the development of the paroptic sense among the blind. The war, which was then approaching its end, did not make this attempt at application less opportune or urgent.

But it seemed to me that, in order to determine the technique applicable to the blind, and to begin this very moving enterprise with a sufficient mastery of method and also enough confidence, I still lacked the knowledge of the phenomena from their internal or subjective side.

I therefore began, and continued from July 9th to September 8th, 1918, the subjective experiments with which I deal in Chapter VI of this work, and which occupied about one hundred and fifty hours of actual experimentation divided into thirty-one sittings.

I used the days following, from the 8th to the 15th September, to put on paper the method I was going to adopt with the blind. Let me copy these few lines from a note made at the time:

"The blind—general idea of investigations.

"Purpose: to give them *e-r* vision in conditions as nearly normal as possible, which permit its easy and daily use.

"Method proposed:

"To obtain the same results as in the *objective series;* but to use the data and indications of the *subjective series* so as to obtain the *awakening* of the paroptic sense without leaving the ordinary state, or with the power of returning to it."

As for the details of this technique, this is not the place to discuss them.

On the 16th of September, fatiguing attempts repeated daily for a week ended in my being entrusted with two blinded patients from the Centre de Nice (then located in the Villa des Colonnes), under all sorts of embarrassing reservations and, naturally, without my having committed the moral fault of divulging to these unfortunates the still theoretical hope which animated me.

On the 19th of September, a little before noon, Michel, a blinded soldier, who had been with the army at Salonika, recognised the figure 4, which was about eight centimetres high with lines one centimetre thick, placed under

glass in a printing frame; and succeeded in following with his finger the outline of the figure through the plate of glass.

I passed the rest of the day in a state of great emotion—persuaded that I had just been the witness of a fact both new and important for humanity; I thought of the discoverers of other times who in like circumstances vowed pilgrimages to Notre Dame. And I shall not be misunderstood, I hope, if I say that this particular figure 4, which I have kept, still moves me when I see it.

On the 20th of September, Baudoin, a Colonial Adjutant, who had been blinded, recognised, in the same conditions, the digit 7; a few minutes later the digit 8; a few minutes later the digit 2.

On the 21st of September, Baudoin again made several correct readings of numbers and recognised colours and objects.

On the 28th of September, after a week of interruption, the causes of which still remain obscure, Michel succeeded once more in reading several figures and capital letters, and described, in an incomplete but striking manner, an unusual object which was shown him at a distance.

From that moment, in spite of my precautions and the effort that I made to induce them not to exaggerate the range of these very modest results, Michel and Baudoin were convinced that their blindness would cease; and in spite of my distinct advice they announced to their comrades of the Centre, and perhaps to others, these facts, the possibility of which they had not considered ten days earlier.

I did not see them again. My most urgent representations did not even secure me the privilege of meeting them. I encountered polite but evasive intermediaries who talked of "fatigue" and "passing indisposition." I received from Baudoin a short letter which, I believe, talked about a "lasting recognition of my services." I was weak enough to yield to an impulse of anger and destroyed it.

At the beginning of October I fell ill. "Spanish influenza" which was raging then doubtless found me with sufficiently weakened resistance. But I did not die. My convalescence was protracted to the spring of 1919.

While this was still in progress I had sufficient strength once again to begin a few experiments and to write this book.

Various vicissitudes delayed its appearance until June, 1920. I had tried vainly in the interval to interest the Academy of Sciences, the Academy of Moral Sciences, and the Sorbonne.

I must here thank Gaston Gallimard who as soon as I expressed my desire to him, and although his firm was in no way prepared for the publication of works of this nature, occupied himself immediately with its production and even sacrificed previous arrangements to ensure rapid progress through the press; also Gustave Tronche who used every effort to support these benevolent intentions.

On its appearance, the work was received with great curiosity and, as was very natural, with reserve. Hundreds of articles, written or repeated on this subject by the press of the whole world, showed the only attitude which was reasonable at that time. "The discovery, if it is confirmed, is of the greatest importance. Moreover, it contains nothing that need shock the mind. It seems connected with the totality of our biological knowledge. It is for the specialists to examine the facts and to pronounce on them, which they cannot fail to do without delay."

This first reaction of opinion brings us to October, 1920. It was then that the ambush of the Sorbonne was organised, of which I shall some day give a detailed account.

Let it suffice for the public, for the moment, to know that I was simple enough to agree on a certain Thursday evening at 7 o'clock to prepare for the next day, Friday, at 2 o'clock, a test demonstration at the Sorbonne, though my apparatus and my trained subjects were hundreds of miles away. On my remarking that it was impossible for me to train a subject for 2 o'clock the next day, the prime mover in this *coup* replied with a pleasant smile: "But if you are unable to show us anything to-morrow we shall of course come to no conclusion whatever against your work; and if on the other hand you show us the slightest thing, the least bit of anything, *we will cry it from the house tops.*" (I quote verbatim.)

The next morning I succeeded in finding a person, who, more than two years before, had offered himself for two preliminary attempts at paroptic training of half an hour each; and I persuaded him to lend his assistance for the demonstration of the afternoon, if only to

show the specialists or pretended specialists how the paroptic experiments were made.

In the afternoon, at the Sorbonne, I had the luck—unbelievable when I look back on it—to secure several feats of paroptic perception, summary, but decisive. My adversaries—for such they were—seemed much disconcerted. But as at the beginning of the sitting I had not anticipated any conclusive result; as in my mind it was only a simple conference with fellow workers; and as, moreover, I suspected no machination, I omitted to demand that two clerks should draw up, experiment by experiment, an accurate report of the admitted facts. So, when two hours later my subject departed, Professor X—— of the Sorbonne was able, with a certain amount of encouragement, to present a parody of the experiments which had just taken place, a parody so grotesque that I replied only with a shrug of the shoulders. I attempted nevertheless to arouse on the part of those present a sign of indignation against such a proceeding. I appealed to the facts which had been established and checked, and to the experiments which had succeeded an hour earlier, while I was absent. (I had left the

subject in the hands of the professors in several instances.) But when the heart is at fault, the memory is also. The professors declared "that they no longer recalled anything clearly enough." And I had no accurate record with which to combat their cowardice.

On the next day Professor X—— began to spread in all quarters to which he had access—and they are many—the report that I had been convicted of "trickery," that my case had "broken down," and that in my collapse I had talked of "tearing up my book."

Of all calumnies which could be invented to "bowl over" a discovery of this nature, X—— had invented the most perfidious. "Trickery," "mystification"—who after this does not tremble for fear of passing as a fool? Who, in France, would not be drenched with cold sweat at the idea of having been "mystified"?

* * *

I endured this two years. My friends were astonished, for a while they almost doubted me. I had resolved to wait.

To wait, why? One thing among others: so that foreign authorities should do my work again and find, by chance or coincidence, what I found.

And besides, I would not have had the courage to be for so long that comic character, the "man of learning, scoffed at, misunderstood," if I had not had, in addition, my literary activity and certain human satisfactions it gave me in spite of everything.

* * *

In October, 1922, I decided that it was time to change the situation, since it had not yet changed of its own accord—since no one at Harvard, Vienna or Yokohama had taken upon himself to re-discover Extra-Retinal Vision, its laws and first applications.

Although, since October, I have only been able to devote a small part of my time to this task, the situation on January 15, 1923, is as follows:

All the test demonstrations which I either suggested or accepted have been completely successful. Some of them have taken place under peculiarly strict conditions and with unusual facilities of verification; among others the sitting of January 10th in the operating-theatre of the Service d'Ophtalmologie de l'Hôpital Cochin, under the presidency of Doctor Cantonnet, the head of the staff.

Of all the biologists, ophthalmologists, neurologists, psycho-physiologists, doctors, professors of the University, etc., who have been present at these various sittings, *not one* has hesitated to sign the verbal record giving the details of the experiments and implying formal recognition of the *authenticity of the facts*.

As for the explanation of the facts by the *ocellary theory*, it is natural that it should not appeal equally to all. Some, quite properly, came merely to acquaint themselves with the facts which it systematises, while others do not consider that they have the necessary qualifications to criticise an interpretation which implies an equal familiarity with psycho-physiological experimentation and histological analysis. Several consider it now *established;* which means, in scientific language, that it is the most coherent and probable explanation that the present state of the science has been able to furnish.

For some time, indeed, *Eyeless Sight* has even ceased to be a general question and has begun to become a group of special questions.

On the 12th of January, Doctor André Nepveu, specialist in Ophthalmology and Oto-Rhino-Laryngology, made before the Société de Laryn-

gologie, Rhinologie et Otologie de Paris, the first report on the first of these special researches, by experiments whose scope he himself fixed: *Sur la perception paroptique des couleurs* (*phénomène de Farigoule-Romains*) *par la peau des narines et la pituitaire*, by A. Nepveu. Two other series are going to be undertaken by Doctor Nepveu, on the *Rétinisation du tégument* and the *Convergence artificielle des ocelles*. I am going to undertake, with his collaboration, and I hope succeed in, the determination of the *spectral limits of paroptic perception*.

Doctor André Cantonnet has been kind enough, for his part, to ask me to collaborate with him on several pieces of research which his great ability and the apparatus he has at his disposal will permit us to carry far.[1] This work will have already begun by the time these words appear in print.

* * *

Before long a new phase of the question will probably open, the phase of: "I said that myself." From Harvard to Yokohama, many people are going to discover that they have dis-

[1] In particular on the laws of orientation and variation of the paroptic *angular deviation*.

covered Extra-Retinal Vision. And I am not certain that in a year they will still allow me a part of the merit, a shred of priority. Doubtless their claim would have had more weight and more elegance if they had produced it during the two years and six months when there was risk in so doing. But it is so natural to wait, to risk our boat on the water when we have seen how the wind blows! Besides, it all causes very little inconvenience; and thanks to the experience I have acquired, I anticipate it with good humour.

* * *

It remains for me to mention the names of the experts and variously qualified persons, who by the date of their intervention will be found to have played a decisive part in the recognition and authentication of the facts of extra-retinal vision. Although biased and embarrassed by the most perfidious campaign, although duly warned that they were going to "make fools of themselves," they were not afraid to be the first to undertake their scientific, or simply their human, responsibility. For my part I do not know how to thank them too much. And I dare to add that whatever the outcome of these dis-

coveries, and however humble we may imagine it to be, they will be honoured by their attitude.

I shall not be misunderstood if among names, many of which are eminent and all very dear to me, I allow myself to single out that of Anatole France.

The most illustrious of living Frenchmen learned, at the beginning of January, from an expert among his friends, who had been present at a series of experiments, what my researches were and what my tribulations had been. He said: "I ought to leave Paris next Saturday; but I will delay my trip for as many days as may be necessary if M. Romains cares to let me see some experiments. I am not an expert, but I can recognize whether three points are in a straight line. Let M. Romains establish in my presence what he affirms and I shall be glad to sign the report."

On the 11th of January, Anatole France signed the report of 18 experiments, whose every phase he continuously followed and discussed with ingenuity.

* * *

Here are the names:

BOUGLÉ, *professeur à la Sorbonne;* DOCTEUR

André Cantonnet, *Ophtalmologiste des Hôpitaux de Paris, chef de service à Cochin;* Docteur Paul Cantonnet; M. Cantonnet; Albert Cazes, *professeur agrégé de l'Université;* Mme. Albert Cazes, *professeur agrégé de l'Université;* Georges Chennevière; Docteur P. L. Couchoud, *ancien interne des Asiles de la Seine, agrégé de philosophie;* G. Cuisenier, *professeur agrégé de l'Université;* Docteur Fombeure, *assistant d'Ophtalmologie à Cochin;* Docteur Foubert, *assistant d'électro-radiologie des Hôpitaux de Paris;* Anatole France; H. Legrand, *professeur agrégé de l'Université;* G. Calmann-Lévy; A. Maublanc, *chef de travaux pratiques à l'Institut agronomique;* R. Maublanc, *professeur agrégé de philosophie;* Docteur A. Nepveu, *ophtalmologiste, oto-rhino-laryngologiste;* Docteur Charles-Robert, *ancien interne des Hôpitaux de Paris;* Docteur De Saint-Martin, *Ophtalmologiste des Hôpitaux de Toulouse;* F. Sartiaux, *biologiste;* S. Sévastos; Docteur Stévenard; Docteur Tastevin, *Psycho-physiologiste, ancien Directeur de la Revue des Sciences psychologiques;* Mmes. Joltrois and Lesueur, *externes du service du Docteur Cantonnet.*

I should add that several of the above were present at three or four test sittings, or even more; that is to say, at fifty, sixty, or even a hundred separate experiments. Some have experimented themselves, with or without my collaboration.

I add further that several authorities who do not appear in this list have made known to me the interest with which they have followed my work. Some have requested me to organise meetings which lack of time alone has prevented up to the present. Others informed in detail by colleagues of the progress of the experiments, consider themselves already convinced and only require direct observation of the facts for the personal satisfaction of their curiosity.

When he knew that I was continuing my experiments, the illustrious philosopher, Henri Bergson, expressed a desire to be present. Two demonstrations to which he was invited unfortunately coincided with obligations from which he could not escape. It is neither his fault nor mine, then, that this eminent judge, doubly qualified as philosopher and biologist, had not pronounced himself on the 15th of January on the facts of extra-retinal vision. When

these lines appear I hope that circumstances will have allowed him to give evidence.[1]

Finally I would like to be able to thank the various individuals who, for my recent demonstrations, have been good enough to expose themselves to the fatigue of repeated experiments and to the inevitable unpleasantness of test sittings; particularly one, a lady, whose devotion has been admirable. But I am not authorised to do it here.

JULES ROMAINS

January 15, 1923.

[1] A demonstration was actually arranged a few days later on January 24, 1923, at M. Bergson's house. M. Bergson and M. Leon Brunschvicq, both members of the Institute, were present, and the tests were completely successful. The record cannot be reproduced with the others in Appendix B, for M. Henri Bergson made it a condition *before the sitting*, that it should not in any case be made public. The demonstration, however, did not differ in any important respect from those which will be found set forth in detail in the following Appendix.

APPENDIX B

THREE TEST SITTINGS

I

Experiments at the house of Doctor P. L. Couchoud on the 24th of December, 1922, between 3 and 4.30 p.m.

THE subject, Madame X, is seated in the corner of a room lighted by two windows at right angles to one another.

Her eyes are blindfolded by Doctor Tastevin by means of:—

(1) Strips of adhesive tape stuck in the form of a cross over the eyelids from the superciliary arch to the cheekbone;

(2) Two rectangular pieces of black silk;

(3) A bandage made to measure, examined, and consisting of black plush, taffeta-lined, with the oval shape of the eyes and covering them entirely.

The blindfolding of the eyes is thus as complete as possible.

M. Romains asks the subject to concentrate and to say when she is ready. This pause lasts about two minutes. The first two experiments in recognising objects (a bronze statuette and a white water-jug) give erroneous results.

1st Experiment. Bronze Statuette. "Something shiny. . . . A lamp—a candlestick. . . . In copper . . . flat . . . a candleholder. (Are you sure?) Yes, it appears large and flat. (Of what colour?) Yellow."

2nd Experiment. Water jug. A long silence. Then: "It's a book."

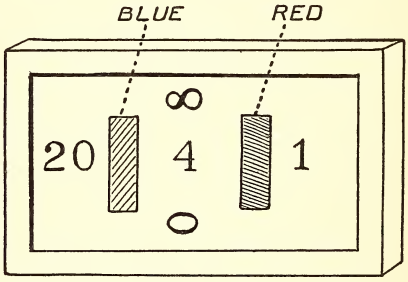
3rd Experiment. Three figures, 380, seven centimetres in height, with lines from 2 to 3 millimetres thick, placed under glass, in a photographic printing-frame, by Dr. Couchoud, the frame belonging to the doctor.

The subject holds the frame in her hand, brings it to her face, handles it, puts her finger to the glass, tracing the outlines of the figures, without contact with the glass. At the end of three minutes: "I see something black . . . an eight. . . ."

She brings her fingers into contact with the

glass: "A three," she says, after two more minutes. Then almost at once: "380."

4th Experiment. The same frame. Smaller figures of about 4 centimetres in height, with finer lines, arranged by M. Sartiaux, with strips of blue and red paper as below:



After a few moments: "I see little squares of paper, very little ones . . . a two . . . a six; no, a *naught.*" She brings the frame into contact with her nose, putting her nose above the blue strip and says: "There is something blue." Then in answer to a question from a third person asking the shade: "Pale blue." Then she turns the frame through an angle of 90°. "There is another naught." She brings it in contact with her nose: "There is a four."

She brings it close again: "I see black ink." She is asked what shade: "Mixed with red, I believe" (the line was in fact black with little transverse red lines). After a few moments: "There is still another colour between the four and the two." She touches the frame with her nose and lets her fingers stray over it: "It is neither blue, nor white; it's red." On being asked what shade it is: "Red with something of dark orange in it."

All these details are absolutely correct.

The subject finally turns the frame about and recognises the eight. She is asked to indicate with her finger the point where she locates the figures. Her finger points out the eight in an attempt to indicate the blue strip, it is placed a little above and to the right in order to point out the red strip. It is put directly over the naught to indicate that. She is asked where the centre of the naught is and she places her finger to the right and above that point.

To sum up: there was a displacement of about 1 centimetre towards the right and above, except for the naught.

The experiment lasted altogether ten minutes.

5th Experiment. An old silver vase placed on a chair at the level of the knees, at about 50 centimetres distance. Reflections in various yellows and blues:

Very rapidly the subject says: "It's a copper object." She turns her head towards the right seeming to wish to look at it with her left cheek-bone. "It is shiny as if it were of copper . . . it's a little vase. . . . You might call it a big glass."

6th Experiment. Chinese porcelain vase with blue ornaments on a white background, some

faded roses in the mouth, placed on the chair at about 50 centimetres distance.

Very rapidly: "It's a pot . . . there is some blue about it . . . and some white."

She was asked what there was above: "Something festooned . . . a bit of yellow stuff."

7th Experiment. A lady's black shoe with a silver buckle placed on a chair at a distance of about 50 centimetres.

Almost immediately: "It's black . . . this is not difficult . . . it's a lady's shoe, with a buckle . . . Louis XV heel."

8th Experiment. Silver bracelet, a dozen centimetres in diameter, placed flat on the chair, at the same distance.

After a few moments and a few gestures with the hands: "It's a little box." She is asked: "Is it really a little box?" Reply: "No, it's made of glass." She is asked: "Of what shape?" Reply: "It's round, shiny, made of silver, one would say to put cigarettes in. . . . You have lifted it up (incorrect). It's an ash tray."

The subject seemed fatigued and was given several minutes' rest.

9th Experiment. A fan placed on the chair.

A few movements of the hands and fingers and, without hesitation, in less than a minute: "It's a fan."

All these experiments were made in daylight. When the light became insufficient, the experiments were continued with electric light from a chandelier. The subject is placed on the left below the chandelier (she has the chandelier above her on her right).

10*th Experiment.* Three playing cards placed in the frame under glass by M. Bouglé, without his knowing them or any of those present having seen them.

The subject holds the frame in her hand, at about 50 centimetres distance from her face, without showing it to the observers, who are all grouped in the back of the room. The subject is asked to announce the final result only when she is sure. At the end of two minutes: "Ten of hearts, ten of clubs, and five of diamonds." Completely correct.

11*th Experiment.* Doubtful. Doctor Charles Robert's hand placed at about 50 centimetres from the subject's face, at the level of her eyes, is not recognised: "I am beginning to see a little object" (the doctor had a ring on his

finger). She asks him to move it; the doctor moves his fingers and hand. The hand is not recognised. The subject declares that she can see her own hand and her handkerchief, as well as the arms, hands and cuffs of Doctor Tastevin standing before her.

12th Experiment. Reading. A volume of Frazer, *duodecimo: Sur les traces de Pausanias.*

The subject moves her right index finger over the letters, a little to one side and says: "*Sur . . . les . . . traces . . . de . . .* (then a pause) *. . . Pau . . . Pausanias.*"

During all these experiments no communication was observed between the subject and M. Romains, who only spoke a few words at the beginning and remained at a distance.

When the bandage was withdrawn, the adhesive tape was still adhering to the eyes.

The subject remained constantly in a normal state.

Replies to some questions which were put:

"I feel that I am seeing by means of part of my skin, sometimes one part, sometimes another. At the present moment it is especially my right cheekbone. . . . My hands help me, especially the ends of my fingers. . . . I have

only seen once with the back of my neck. . . . The object is seen with its contours and localised in space. . . . There is something like a gauze which prevents me from seizing its contours and outlines directly; but by fixing my attention on them I succeed. It is clearer than in a mist. . . . I feel a sensation of burning and of activity; my cheekbone gives me the effect of having been rubbed and handled roughly. The image is in the direction in which the object lies."

Report prepared by MM. René Maublanc and Felix Sartiaux, and signed by them.

Signatures of those present: Dr. P. L. Couchoud, Dr. Charles Robert, Bouglé, Dr. Tastevin, A. Cuisenier, G. Chennevière, A. Maublanc, S. Sévastos.

II

Experiments on the 10th of January in the room of Dr. Cantonnet, Ophthalmologist of the Cochin Hospital.

The subject is Madame X, introduced by MM. Jules Romains, Georges Chennevière, and Henri Legrand.

There were present at the various experiments: Doctors Cantonnet, Stévenard, and Foubert.

Location: Operating-theatre of Dr. Cantonnet's Ophthalmological Department. The subject is seated with her back turned towards a great bay window.

At 10.45 the subject's eyes are blindfolded by Dr. Cantonnet and Dr. Foubert:—

By means of (1) A strip of adhesive tape stretching from the superciliary arch to the lower part of the socket and stuck on to the upper eyelid which is lowered;

(2) Another strip of adhesive tape, from the ridge of the nose to the external angle of the socket and stuck across over the first;

(3) An ellipse of pliant waterproof material covering the entire area of the socket;

(4) A bandage formed of several thicknesses of black plush lined with black satinette, fastened on to the eyes by means of two elastic bands going round the head. (This bandage was tried on by those controlling the experiment and found of itself a sufficient guarantee of blindfolding);

(5) Doctor Cantonnet slips under the band-

age at each side of the nose a certain quantity of absorbent cotton-wool in order to put the blindfolding of the eyes beyond any possible question.

At 11, M. Romains asks the subject to concentrate her attention and says that he is going to ask her to read certain "tests" in order to lead up to paroptic vision; more searching experiments may follow later.

He presents to her a photographic frame containing the symbols "3 III 3." The subject makes efforts to decipher it, without success. M. Romains takes away the sign "III"; the subject, obviously moved, is agitated and tremulous and patches of erythema appear on her neck. She makes fresh efforts but declares that she cannot see, that "it twinkles" and "it opens and shuts."

Doctor Cantonnet attempts an easier test; a piece of pasteboard 10 centimetres square, on which is marked the symbol C one centimetre high. The subject does not succeed in seeing.

She is allowed to rest and is talked to in order to restore her confidence; she becomes somewhat calmer, says that she sees Dr. Foubert who was talking to her. Dr. Foubert: "Is there anything peculiar about me?" "No,

except your blouse." Dr. Foubert: "What do you see to my left?" "Something dark." (Dr. Stévenard, doubtless, who is dressed up in dark grey.)

11.10. The subject seems ready to take up the course of the experiments.

1st Experiment. Dr. Cantonnet shows the symbol C to the subject again. The subject's finger follows the circle and stops at the break. She then explains that she sees an interrupted circle and shows where the break occurs. Other similar symbols are presented to the subject and she recognises them, although they are of different sizes, and indicates the break correctly.

2nd Experiment. Dr. Stévenard draws in pencil on a piece of white paper the symbols "3 A B." The subject: "The lines are very fine. There's a B, an A, another B"; then returning to the first symbol: "Oh, no, it's a 3." The recognition did not take a minute.

3rd Experiment. Dr. Cantonnet presents to the subject, who was standing up, one of his blue pamphlets, at the level of her eyes and holding it himself. The subject brings her fingers near the title and reads correctly, scanning her words: "Papillary stagnation."

4th Experiment. The subject reads correctly a line from a financial prospectus presented to her by Dr. Stévenard. Then a line from a newspaper article in small type is read with two mistakes, spontaneously corrected by the subject: "*Vieux doit*" instead of "*Mieux vaut.*"

5th Experiment. Dr. Cantonnet announces that he has arranged (without looking at them) under two photographic frames from twenty-four to thirty different symbols. None of those present know what the frames contain any more than he does. Everyone goes to the back of the room. The subject holds the frame on her knees, vertically, and is therefore the only person able to see what it contains. Her replies are taken straight down in writing. All at once she says, First frame: "I am going to be able to play cards. Here is the King of hearts, the 7 of clubs, the King of diamonds after the 2 of spades, and the 5 of diamonds. That's all." This was checked and found to be exactly right. Second frame: "I see a 10 of diamonds, a U, there is a good deal of black, a 3, and there's a King of clubs. I don't see any more, there is something black. There is a little white patch to the left of the frame;

it's a letter; it's an N." The reading is checked and found to be correct.

For all these experiments the subject, in order to see, let her fingers wander over the object, bringing it up to her face, her cheek-bones and her nostrils; the last, it seems, more particularly in order to recognise colours.

Vision of objects at a distance:

A chair is placed at a distance of one metre from the subject. She cannot reach it with her hand. On the cane seat is spread a piece of white cloth. Several different things are arranged upon it.

6th Experiment. Two objects are indicated. The subject: "I see something black. One would say a folded silk handkerchief; it's not a silk handkerchief—it's a portfolio. Beside it is a pencil." The subject's finger points out the direction of the pencil in space. The direction is correct. M. Romains asks the subject to point out carefully with a pencil which he gives to her, the direction of the middle of the portfolio. The subject begins at first with a considerable deviation towards the right, then corrects it rapidly and points out the right direction.

7th Experiment. Three objects are indicated. The subject says: "One is very shiny—it's a watch—a gold one; the thing next to it is also shiny but it's made of silver. Oh, it's another watch." "What is there peculiar about them?" someone asks. "One is right side up, the other is reversed." The only error consists in having taken the black enamel dial-plate of the second watch for a metallic cap.

8th Experiment. Two objects are indicated, but someone slips in also a one franc token of aluminium bronze.

The subject says: "Scissors and a garnet-coloured diary." "Is there nothing else?" After a moment the subject says: "A piece of money." Correct.

9th Experiment. The subject is told that a more difficult test is going to be made. A small object will be put on the chair which will then be pushed back to a distance of 3.50 metres. The chair was placed 3.50 metres distant and on it a hat of soft black felt, to deceive the subject as regards the dimensions announced. An assistant holds a white apron behind the chair and speaks in order to help the subject to locate the chair. The subject makes efforts to

see for a long time. It is obvious that she is embarrassed, that something bothers her. Finally she says: "Is this object of which you talk situated near the hat?" Congratulations and amusement.

10*th Experiment.* M. Romains, MM. Chennevière and Legrand who introduced the subject leave the room. A single object is placed on the chair at a distance of 1 metre. Almost at once: "Keys." Correct.

11*th Experiment.* Two objects. "The little spool I saw a while ago, and the 7 of clubs." Correct vision. The little spool was a reel of plaster seen by the subject before she was blindfolded.

12 o'clock. MM. Romains, Chennevière and Legrand are brought back, and the bandage is removed. The cotton-wool was in place, and the waterproof layer also. The strips were stuck on one another and on the eyelids which were still lowered. Some difficulty was experienced in getting them off, warm water being necessary as well as some courage on the part of the subject whose eyebrows and lashes stuck very strongly to the adhesive tape.

During the experiments various people came

into the room besides those already named, belonging either to Dr. Cantonnet's staff or his relations. These persons can sign this Report if they indicate the point at which they came in to witness the tests.

Drawn up in Paris, January 11, 1923, by Dr. Foubert, Assistant in Electro-radiology at the Cochin Hospital. Signed: Foubert.

Certified as conforming to the facts by us who were witnesses: Dr. Cantonnet, Ophthalmologist of the Cochin Hospital; Dr. Stévenard, Advocate at the Court of Appeal.

Continuation of the Report of experiments in Eyeless Sight.

Certified as conforming to the facts by us who were witnesses: Jules Romains, Georges Chennevière, author; Henri Legrand, Agrégé of the University.

The following were witnesses of the experiments from the second part of experiment number four: Mme. Lesueur, dresser on the Ophthalmological Staff of the Cochin Hospital; Mlle. Joltrois, dresser on the same staff and clinical assistant of the Quinze-Vingts; M. Cantonnet Père; Dr. Paul Cantonnet; Dr. Fombeure,

Assistant in Ophthalmology at Cochin; Dr. de St. Martin, Oculist at Toulouse, formerly Head of the Ophthalmological Clinic at Toulouse.

III

Demonstration on Thursday, January 11, 1923, *at the house of Monsieur Anatole France, 5, Villa Said.*

The subject is Madame X.

Her closed eyelids are stuck down with strips of gummed paper, arranged in the shape of a cross. This is done by M. Gaston Calmann-Lévy, who then puts the bandage on the subject's eyes.

A preliminary experiment is performed by M. Romains to make sure that the subject is in a suitable state.

This experiment is not subject to the control of those present. It succeeds in about five minutes.

1*st Experiment.* MM. Anatole France and Couchoud put under a photographic frame with a glass protection, a sheet of white paper on which is painted a blue square surrounded by a red band.

The subject says at the end of less than a minute: "It's a square." The subject brings the frame close to her nose. "It's blue. It has a red border."

2nd Experiment. MM. France and Couchoud put under the frame strips of red and blue paper and a number. The replies of the subject are as follows:

"There is a square of red paper. A figure 1. It is of pale blue. There are two of them. One of them is of pale blue, the other a little deeper. Natier blue."

The sheet of blue paper is, in fact, divided by a fold which causes a different brightness on the right and on the left, the part on the left being the brighter.

3rd Experiment. Doctor Couchoud arranges in the frame the numbers 5 and 3. The subject's replies are as follows: "It's a large 3, a 5, 5 and 3."

4th Experiment. M. Calmann-Lévy arranges, *without seeing them*, some figures in the frame. No one knows what these are (test experiment to avoid the hypothesis of thought-transference).

"It is there. There is a large 2 and then a little bottle." This is quite correct.

5th Experiment. Doctor Couchoud gives the subject a number with his hand. Reply: "It is an 8."

6th Experiment. The subject is given an admission card to the Opéra Comique belonging to M. Calmann-Lévy. Replies of the subject: "Permanent . . . admission. . . . National . . . Theatre . . . of the Opéra Comique."

7th Experiment. Madame Anatole France held the frame for the subject, while Mme. Couchoud held her head, in order to keep the object in a direction which would *eliminate any suspicion of an infiltration of light beneath the bandage.*

"There is a 7—a 9—a B—a 4—a line."

The line is a 1 seen horizontally.

8th Experiment. A picture cut from a catalogue is put into the frame.

The reply:

"It's a man wearing a tall hat; his features are clear cut and he has a high colour; a mannequin."

M. Romains asks the subject to put her finger on the nose of the person depicted.

The subject puts her finger high up and to the right of the nose, then, on request, corrects herself.

9th Experiment. A chair is placed before the subject. The subject replies that she sees a red chair.

Doctor Couchoud puts a pot of azaleas on the chair.

Reply:

"It's a pot of flowers. It's the pot of flowers which was on the little table—red flowers—dark rose."

10th Experiment. Doctor Couchoud puts on the chair a death mask in bronze of Napoleon I. "It is shiny and black." A long silence. "It's an animal . . . there is something like a gleaming ridge . . . beside this gleaming thing there is something like a little dog. . . . It's a head. . . . The head is like this." She indicates the head in a horizontal position.

11th Experiment. Mme. Couchoud sits on the chair. Replies of the subject: "Something black. . . . It's Mme. Couchoud."

12th Experiment. Mme. France replaces her. "It's Mme. France, because she has a light-coloured dress on, and I saw her come in."

13th Experiment. M. Calmann-Lévy replaces her. "Will you please move one hand? Yes, it's M. Calmann-Lévy."

14*th Experiment.* M. X. sits on the chair. Mme. X laughed and took him by the arm. "It's my husband."

15*th Experiment.* M. Romains uncovers the back of the subject's neck, then holds up to it behind, horizontally, a statuette. Replies of the subject: "I see the object moving. I don't see it moving any more."

The subject traces outlines with her right hand, in front of her.

"There is a little valise. What I see is square. I have lost it again. Move it now."

M. Romains presents the object to her in front. The subject recognises it and says that it was not that which she saw.

There was behind the subject an arm-chair whose back resembles a valise in shape, material and colour.

16*th Experiment.* Mme. Couchoud presents to the subject from behind a pair of white metal nail-scissors and moves them.

"I seem to see the object move. I see a little thing like an egg-cup. Something oval with a foot. This seems to me to glitter. . . . It's a glass because of the gleam. . . . Now I've lost it. Now it looks like a little silver statue."

The object when presented in front is immediately recognised. The subject shows the parts of the object that she had taken for the egg-cup.

17*th Experiment.* Experiment in nyctopia. Dr. Couchoud arranges pieces of coloured paper in the frame. All the lights in the room are turned out. The room is only illuminated by a lamp in a neighbouring room.

"Red and pale blue. It is yellow." This is quite correct.

18*th Experiment.* The illumination is still further reduced and the subject is given the frame in which is arranged a sheet of white paper with a rectangle in coloured border painted on it.

"A brick-red rectangle. A green border. Old rose. Green border."

The shade of the rectangle is correct. The border is blue-green.

The experiments are brought to an end at 6 o'clock. The eyes are found to be stuck as at the beginning of the demonstration.

This Report has been prepared during the actual course of the tests by René Maublanc.

It has been read by those present, who declare that it conforms to the facts.

Anatole France.	Emma France.
M. Ledoux-Lebard.	R. Maublanc.
B. L. Couchoud.	Gabrielle Romains.
A. Couchoud.	G. Chennevière.

Gaston Calmann-Lévy.

Paris, *January* 11, 1923.

Sunlight and Health

By

C. W. Saleeby

M.D., F.R.S.E.

With an Introduction by

Sir William Bayliss

K.B.E., D.Sc.

Since his student days at Edinburgh in the nineties the author has devoted himself to the investigation of the effect of sunlight on the tissues. Opinions vary as to the germicidal power of sunlight, but its tonic effect cannot be denied, and it is found that the condition of patients after treatment resembles that of a trained athlete, the muscles being firm and well developed and the skin supple. The ideal hygienic combination is, in Dr. Saleeby's opinion, cold and sunlight; the Canadians thrive under such conditions and are probably the healthiest people in the world. The author urges most forcibly the abolition of coal fires and the building of houses which will receive a maximum amount of sunlight; and the book should be of interest to all social workers and philanthropists as well as members of the medical profession.

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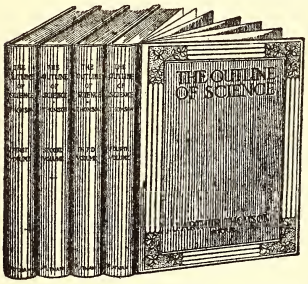

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