An essay upon single vision with two eyes : together with experiments and observations on several other subjects in optics / by William Charles Wells, M.D.

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AN

ESSAY

UPON

SINGLE VISION WITH TWO EYES:

TOGETHER WITH

EXPERIMENTS

AND

OBSERVATIONS

ON

SEVERAL OTHER SUBJECTS IN OPTICS.

By WILLIAM CHARLES WELLS, M. D.

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To the Revd. Mr. Walker

from the Author.

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ESSAY

UPON

SINGLE VISION WITH TWO EYES.

PART I.

Of the different Opinions concerning ſingle Viſion with two Eyes; and principally of thoſe of Dr. Smith and Dr. Reid.

THE end I have chiefly in view, in this Eſſay, being to offer a new ſolution of the queſtion, why objects are perceived ſingle with two eyes, I think it incumbent upon me, in the firſt place, to ſhow, that none of the opinions I have met with upon this ſubject, can be admitted as juſt.

Theſe opinions, or ſuch of them at leaſt as have gained any conſiderable reputation,

may be reduced into two claſſes. The firſt comprehends thoſe of Galen, Alhazen, Rohault, Dr. Briggs, and Sir Iſaac Newton, all of whom have regarded the queſtion I have mentioned as equivalent to the following one: Whence comes it, that the mind ſhould be affected with only one perception from two impreſſions upon the external organs of ſight, ſince either of thoſe impreſſions is, of itſelf, ſufficient to produce a ſimilar perception? Their univerſal anſwer has been: Becauſe the two impreſſions are united before they are communicated to the mind. And the only difference among theſe authors, has been with reſpect to the manner in which ſuch an union takes place. To the ſecond claſs are to be referred the opinions of thoſe, who hold it as certain, that an object is ſeen ſingle by both eyes, becauſe it is ſeen by each of them in the ſame external place; and who profeſs to point out ſome law, or conſtant rule of viſion, from which this ſameneſs of place is to be derived as a neceſſary conſequence. Aguilonius, I believe, firſt gave

this

this view of the queſtion, which has ſince been adopted by Dechales, Dr. Porterfield, Dr. Smith of Cambridge, and Dr. Reid of Glaſgow.

In oppoſition to [redacted] of the opinions of the firſt claſs, more eſpecially as they have been repeatedly examined by others, I think I need only ſay, that they muſt all be conſidered as mere conjectures, founded upon certain ſuppoſed changes in the brain and nerves, the exiſtence of which it is impoſſible, from the nature of the parts, either to demonſtrate, or to refute by experiments; and that no one of them, though admitted to be true, is yet ſufficient to explain the phenomena on account of which it was framed.

The opinions of the ſecond claſs being built, as their authors think, upon experiments and obſervations, both allow and demand a more accurate inveſtigation. I ſhall proceed, therefore, to examine ſuch of them as I am acquainted with, beginning with that of Aguilonius; and what I ſhall obſerve concerning it will apply alſo

to thoſe of Dechales and Dr. Porterfield, who have done little more than copy what he has ſaid.

If a line be drawn through the point of the mutual interſection of the optic axes, parallel to the interval between the eyes, Aguilonius calls it, from its office, the *horopter*; and if through this line, a plane be made to paſs at right angles to that of the optic axes, he names it the *plane of the horopter*. After defining theſe terms he aſſerts, that, by a law of our conſtitution, all bodies which we ſee with one glance or look, whatever are their real places, appear to each eye to be ſituated in this plane.—And if this be granted to him, he eaſily and ſatisfactorily ſhows, why ſome ſhould be ſeen ſingle with two eyes, and others double. For ſince, according to a ſecond opinion maintained by him, and not contradicted, I believe, by any other writer upon viſion, the two lines of direction, in which an object is ſeen when we employ both eyes, can meet each other only in one point, it follows, that all bodies which are

really ſituated in the plane of the horopter, muſt neceſſarily appear ſingle, as the lines of direction in which any one of them is perceived by the two eyes, coincide in that plane, and no where elſe; and that all bodies, which are not ſituated in the plane of the horopter, muſt as neceſſarily appear double, ſince, in this caſe, the lines of their viſible directions interſect each other, either before or after they paſs through it.*

Againſt the truth of this explanation, only one argument need be offered.—Were the viſible places of all bodies to be contained in the plane of the horopter, theſe would appear of magnitudes proportional to the angles which they ſubtend at the eye. A finger, for inſtance, held near to the face, would ſeem as large as the part of a remote building it might conceal from the ſight. But as this is contrary to experience, the principle from which it is derived, muſt be rejected, together with all its conſequences. To Aguilonius, however, the merit is due of being the firſt, who ſo

* Aguilonii Optica, p. 110, 148, 331, 344.

ſo far generaliſed the phenomena of ſingle and double viſion, as to obſerve, that thoſe objects alone are ſeen ſingle, which are really ſituated in the plane of the horopter.

The opinion of Dr. Smith is the next in the order of time. *" If it " be aſked (ſays that author) why in " ſeeing with both eyes we do not al- " ways ſee double, becauſe of a double ſen- " ſation, I think it is ſufficient to ſay, " that in the ordinary uſe of our eyes, " in which the pictures of an object are " conſtantly painted upon † correſpond- " ing places of the retinas, the predomi- " nant

* Compleat Syſtem of Opties. Vol. I. p. 48.

† Dr. Smith gives the following definition of *correſponding points*. " When the optic axes are parallel or meet in a point, " the two middle points of the retinas, or any points which are " equally diſtant from them, and lie on the ſame ſides of them, " either towards the right hand or left hand, or upwards or " downwards, or in any oblique direction, are called *correſpond-* " *ing points.*" Vol. I. p. 46. According to this definition, points correſpond which have a certain agreement in ſituation. No contradiction is, therefore, implied in this ſyſtem by ſaying, that an object may appear ſingle, though its pictures ſhould fall upon points which do not correſpond. Dr. Reid's definition of the ſame term is very different.

" nant ſenſe of feeling has originally and
" conſtantly informed us that the object is
" ſingle. By this means our idea of its
" outward place is connected with both
" thoſe ſenſations, as is manifeſt by its
" appearing in two places when its pictures
" are not painted upon correſponding places
" of the retinas; which is only a direct
" conſequence ariſing from our general ha-
" bit of ſeeing." Should any one now enquire whence it is, that, to produce ſingle viſion, all men agree in directing their eyes toward the object in ſuch a manner, as to receive its pictures upon correſponding points of the retinas, ſince cuſtom might have connected the ſenſations of any other two points with the information of its unity from feeling ;* This anſwer may be given in the words of Dr. Smith : † " When we
" view an object ſteadily, we have acquired
" a habit of directing the optic axes to
" the

* This objection is made to Dr. Smith's theory by Dr. Reid, who ſeems to have overlooked the anſwer. Reid's Inquiry into the Human Mind, 8vo. p. 332.

† Vol. I. p. 46.

" the point in view; becauſe its pictures
" falling upon the middle points of the
" retinas, are then diſtincter than if they
" fell upon any other places; and ſince
" the pictures of the whole object are equal
" to one another, and are both inverted
" with reſpect to the optic axes, it follows
" that the pictures of any collateral point
" are painted upon correſponding points of
" the retinas."

Such is the ſolution which Dr. Smith has given of this celebrated queſtion, and ſuch the reply, which his general account of viſion furniſhes to one objection againſt it. But there are others which, in my opinion, cannot be ſo eaſily repelled. Before I offer theſe however, I beg leave to remark, that although it were proved, as I think it may be, that he is miſtaken in the fact of objects appearing ſingle, when their pictures fall upon the middle or other correſponding points of the retinas, ſtill the truth of what is peculiar to him * of the ſolution he

* Dr. Reid attributes to Biſhop Berkeley the opinion, that objects appear ſingle to two eyes, from an experienced connection between

he gives, might remain unmaken.— Objects, it may be ſaid, are conſtantly ſeen ſingle when we direct our eyes to them in a particular manner. Their pictures muſt, conſequently, in every ſuch caſe, fall upon the ſame places of the retinas; and whether theſe be correſponding or not, the unity of the viſible appearances will be owing to the connection, which has uniformly been obſerved between the ſenſations of thoſe places, and the information from feeling, that the objects which cauſe them are ſingle. What I ſhall ſay, therefore, upon his opinion, will tend to ſhow, that, admitting the fact reſpecting correſponding points to be true, his explanation of it ought however to be rejected.

For *firſt*, it may be obſerved, that, if we are taught by *feeling* to ſee objects ſingle,

between particular ſenſations of ſight, and the informations of touch. But I no where find it mentioned in the works of that author; and I even think it probable, that he purpoſely avoided treating of the queſtion, as he found, that the ſolution of it, which naturally flowed from his principles of viſion, was with difficulty to be reconciled to other concluſions he had derived from the ſame ſource.

notwithſtanding a ſenſation in each eye, the informations of the former ſenſe ought to be uniform, or elſe one ſet of viſual appearances would be aſſociated with different reports from feeling, and no certain mark afforded us which of them we ſhould truſt. Now Dr. Smith himſelf is obliged to confeſs, that we ſometimes *feel* double, " as " in the dark, when a button is preſſed " with two oppoſite ſides of two contiguous " fingers laid acroſs; for this reaſon, that " thoſe oppoſite ſides of the fingers have " never been *uſed* to feel one but always " two things at a time." * He adds, " We " have learned, therefore, by experience of " both ſenſes compared together, to make " their informations conſiſtent with each " other." Here then we find him to allow, that feeling is not always the predominant, but ſometimes the inferior ſenſe; that its informations are not conſtant and original, but changeful and derived; poſitions directly

* Vol. I. p. 48. Dr. Smith however has, from the influence of ſyſtem, I ſuppoſe, miſtaken this fact; for the button is *felt* double, when preſſed in the manner above mentioned, though we ſhould not be in the dark, and ſhould even *ſee* it to be ſingle.

rectly contrary to thoſe he had immediately before maintained. But in the firſt inſtance of difference between the informations of the two ſenſes, what rule had we for determining which was the moſt worthy of credit? How does a blind man correct his errors of touch? If the button be felt double, becauſe preſſed by two parts *not* accuſtomed to feel the ſame thing at the ſame time, there muſt have been a period in the life of every perſon, when a body preſſed by any two parts would have been felt double, by three parts triple, and ſo on. Nor could ſight have corrected thoſe deceptions, if they can be called ſuch; for every thing by the ſame hypotheſis muſt then have alſo been ſeen double. How came we therefore, both to feel and ſee things ſingle? Surely not by comparing the informations of the two ſenſes together.

But *ſecondly*; were we to grant, that the ſenſe of touch has originally and conſtantly informed us that objects are ſingle, it would not follow, that we are thence taught to *ſee* them alſo ſingle. For ſince the place,

which an object ſeems to either eye to poſſeſs, manifeſtly depends both upon its apparent diſtance, and its apparent direction from that eye, if viſible place be, in the language of Dr. Smith, only an *idea* of real or tangible place, viſible direction muſt bear the ſame relation to tangible direction; a conſequence of which is, that we can never have a more accurate knowledge of the direction, in which an object may lie from any part of our bodies, by ſight than by touch. Facts however prove the contrary. Let any perſon, for inſtance, taking a pin in his hand, endeavour, without looking, to bring its head upon a level with either of his eyes; and there are many chances to one but he will fail in the attempt, of which ſight will inform him, when he turns his eye to the object. This to me is a convincing argument, that external bodies are not ſeen in certain directions, becauſe they have been previouſly felt in them; and conſequently, that viſible place, of which viſible direction is a component part, is not merely a repreſentative of the place perceived

ceived by touch. But if the place, in which an object appears to each eye ſeparately, does not entirely depend upon any leſſon from feeling, the inference is, that when an object appears in one and the ſame place to both eyes together, neither is this effect to be attributed ſolely to the informations of that ſenſe.

Thirdly; in whatever direction an object may appear to either eye, it certainly cannot be ſeen in the ſame place by both, except at ſome point common to the two directions. Dr. Smith acknowledges this, and ſays, * that when an object is perceived ſingle with both eyes, it is ſeen at the mutual interſection of the two viſual rays; the viſible direction of any object coinciding, according to him, with the viſual ray, or the principal ray of the pencil which flows from it to the eye. Should we then even allow, that all we know by ſight of the places of bodies has been borrowed from feeling, it will ſtill be eaſy to ſhow, that the rule of viſion for each eye, which he has

* Vol. II. Remarks, p. 86.

has derived from ſuch experience, that of our ſeeing objects in the directions of their viſual rays, is inconſiſtent with many of the phenomena of ſight with two eyes; and conſequently, that he has left unremoved the chief difficulty of his ſubject, which was to explain the ſingle appearance of objects to both eyes, from thoſe laws, or rules of viſion, which affect each of them ſingly. For it is a well known fact, that if two bodies of the ſame ſhape, ſize, and colour, be placed, one in each optic axis, they appear but as one body, provided they be at equal diſtances from the eyes. Agreeably to the theory of our ſeeing objects in the direction of their viſual rays, this cannot happen, except the united body appear at the interſection of the optic axes. Dr. Smith accordingly, * maintains that it does. Now, in the firſt place, I appeal to experiment for a direct proof that it does not; and in the ſecond, I obſerve, that, as the two bodies in the optic axes appear as one, whether they be ſituated within or beyond the

* Vol. II. Remarks, p. 86.

the concurrence of thoſe lines; and as a right line joining the bodies, and extended both ways, appears at the ſame time to the ſight as a right line; it follows, upon admitting the fact which I have denied, that all objects in the plane of the optic axes which are ſeen in one poſition and ſtate of the eyes, however near to us, or however remote they may in reality be, muſt appear to be equally diſtant, or rather in a line drawn through the concourſe of the optic axes, parallel to the interval between the eyes, and named by opticians the *horopter*. Again, if a right line be made to paſs through any part of the plane of the optic axes, at right angles to it, the portions above and below this plane are perceived to be in the ſame right line with the point which is ſituated in it, and the whole appears perpendicular to the plane. But the point in the plane is ſeen, by the laſt article or propoſition, in the horopter; the whole, therefore, of the perpendicular line muſt be ſeen in a plane paſſing through the horopter at right angles to that of the optic axes; or in other

words, in the *plane of the horopter*, in which conſequently all bodies will have their viſible places. But this was the very opinion of Aguilonius, to which he was probably led by a ſimilar train of reaſoning; though, as a teacher, he might chooſe rather to ground it immediately upon an original law of our conſtitution.

It is probable, however, that Dr. Smith did not perceive the concluſions which might be drawn from his doctrine of objects being ſeen in the directions of their viſual rays, ſince he has no where ſpoken of them. At any rate, it is manifeſt he did not admit them, as he has mentioned the following circumſtance as a fact,* to which they cannot be reconciled; that, when an object is ſeen double, both its apparent places are ſituated between its real place, and the mark at which we look. For, if this were juſt, together with what he has elſewhere advanced, phenomena ought in many caſes to be obſerved, very different from thoſe which are in truth found to exiſt. Thus, for

* Vol. I. p. 48.

for example, if a right line be any where placed in the plane of the optic axes, it follows, from what he has ſaid in one part of his book, that thoſe points of it, through which the axes paſs, muſt be ſeen united at the mark we look at, the axes croſſing each other there; and from what I have juſt quoted, that every other point muſt be ſeen by each eye between its real place and that mark. The appearances, therefore, of all the points, if they do not lie disjoined, but are connected together in ſome orderly manner, will be arranged in the forms, either of two curves, both paſſing through the interſection of the optic axes, or of four right lines meeting one another at that point. If the right line be placed nearer to the face than the mark we look at, the apparent lines, whether curved or ſtraight, will approach toward us from their common point, but recede from us, if the real line be ſituated beyond the mark. Such are the phenomena which ought to follow upon the admiſſion of theſe two parts of Dr. Smith's theory of viſion with two eyes, but

which are not found to exiſt in nature.—Aguilonius was at leaſt conſiſtent when he maintained, that all objects are ſeen in the plane of the horopter; while Dr. Smith, by deſerting that opinion in part, ſeems only to have involved himſelf the more deeply in error.

Having now ſaid what, I hope, will be thought ſufficient to ſhow, that the reaſon given by Dr. Smith, for our ſeeing objects ſingle with both eyes, is neither grounded on well-atteſted facts, nor adequate to the explanation of the phenomena obſerved, I paſs to the examination of the opinion of Dr. Reid.

As this neither reſts upon, nor includes any new fact in viſion, I need only mention, in order to give an account of it,* that its author maintains with Dr. Smith, that an object is ſeen in the ſame place with both eyes, and conſequently ſingle, when its pictures fall upon the centres of the retinas, or upon points in them, which are ſimilarly ſituated with reſpect to the centres; but differs

* Inquiry into the Human Mind, c. v. ſect. 13.

differs from him in this, that he makes the property to be original, by which any two places in thoſe membranes exhibit only one object, while Dr. Smith derives it altogether from cuſtom.†

In my examination of the opinion of Dr. Smith, I took occaſion to remark, that the truth of what diſtinguiſhed it from all others might remain unſhaken, though it were proved, that objects do not appear ſingle, when their pictures occupy any of the correſponding points of the two retinas, ſince cuſtom might have aſſociated the perceptions of touch, with the ſenſations of any other parts whatſoever of thoſe membranes. The ſame obſervation will not apply with equal juſtice to the opinion of Dr. Reid. On the contrary, could it be ſhown, that the places of the two retinas, which repre-

† They differ alſo with reſpect to the meaning of a term; Dr. Smith calling *correſponding points*, ſuch as have the poſition juſt mentioned, whether they repreſent objects ſingle or not; whereas Dr. Reid ſays, that thoſe points correſpond, whatever their poſition may be, which repreſent objects ſingle; and he appears to me not always to attend to the double uſe of the ſame term, when he ſpeaks of the opinions of Dr. Smith.

ſent an object ſingle when each receives its picture, are not the centres, or ſuch others as are ſimilarly ſituated, an obvious inference would be, that the ſingle appearance of the object is not occaſioned by a property in thoſe places, beſtowed upon them for this ſpecial purpoſe by nature; it being reaſonable to expect, that ſuch a property ſhould be found, if any where, in thoſe parts of the retinas which are the moſt like to each other. I have, therefore, reſerved till now, the obſervations which have occurred to me upon this ſubject, and which, when ſtated, muſt, at leaſt, raiſe ſome doubt concerning what has been regarded as true by Dr. Smith and Dr. Reid, and by almoſt every other writer on viſion, ſince the time of Kepler.

Anatomiſts have commonly taught, that the centres of the ſpheres, to which the cornea, the ball of the eye, and the two portions of the cryſtaline belong, are all placed in the ſame right line, hence called the optic axis, and that this being produced both ways, paſſes through the centres of

the cornea and retina, conſidered as ſurfaces. Opticians, on their part, obſerve, that an object appears ſingle to both eyes, when the axis of each is accurately directed to it; from which they infer, that the centres of the retinas agree in ſuggeſting but one object, though each receives its picture.—Again; ſince it is known by experience, that, while any object is ſeen ſingle, to which the optic axes are turned, others at the ſame diſtance from the eyes likewiſe appear ſo; and ſince the pictures of theſe lateral objects fall upon points in the two retinas, equidiſtant from their centres, and both upon the ſame ſide, that is, both to the right or left of the centres, or both above or below them, opticians conclude, that every two places of the retinas, which are ſimilarly ſituated with reſpect to the centres, muſt alſo agree in exhibiting but one object, though pictures are received by both.

But the whole of this reaſoning is built upon a circumſtance in the fabric of the eye, which has been ſhown by ſome of the

moſt

moſt eminent anatomiſts not to have place. For Varolius * long ago obſerved, that the cryſtaline is not ſituated in the middle of the eye, but more *inwardly*; and the accurate Zinn † has more lately mentioned, that if the eye be divided into a right and left half, the centre of the cryſtaline will be found in the inner portion. Haller ‡ confirms this fact; and Winſlow's || obſervation, that the centres of the pupil and iris do not coincide, but that the former is nearer to the noſe than the latter, is connected with it; ſince both Zinn and Haller agree, that the centre of the pupil is placed in the axis of the cryſtaline, while that of the iris is evidently in the common axis of the cornea and globe. Now, a conſequence of this poſition of the cryſtaline is, that, contrary to what I believe is univerſally maintained, no ray of light whatſoever can paſs unbent to the retina from the atmoſphere, or any other medium differing in refractive

* Varolii Anatomia, 12mo. p. 16.

† De Oculo, 4to. p. 127.

‡ Elementa Phyſiologiæ, tom. v. p. 403.

|| Winſlow's Anatomy, vol. ii. p. 379. Engliſh edition, 8vo.

fractive power from the aqueous humour. If, then, the line joining the centres of the cornea and globe of the eye be what is called the optic axis, and if it be true, that objects appear ſingle when we direct both theſe axes to them, it muſt be evident, to ſuch as are acquainted with the common rules of optics, that the pictures of thoſe objects do not fall upon the centres of the retinas, but more internally; and, therefore, that the centres and all the other points of thoſe membranes, which by the preſent ſyſtem are ſuppoſed to repreſent objects ſingle, do in fact exhibit them double.

It will be ſaid here, perhaps, that the line * paſſing from each eye, which we turn to objects when we ſee them ſingle, is not a pro-

* I am of opinion, that this line, or at leaſt the line which we turn to objects when we ſee them moſt diſtinctly with one eye, is not the common axis of the globe and cornea. For I find, that, when I place the flame of a candle between either of my eyes, and a plane mirror, in ſuch a manner that it may conceal its own image in the mirror from the ſight of that eye, or rather that it may be a little below this image, but in the ſame vertical plane with it, the image of the flame, ſeen by reflection from the cornea, does not appear upon the middle point of this coat, but upon that point of it which is oppoſite to the centre of the pupil.

a production of the common axis of the cornea and globe, but ſome other, diſpoſed in ſuch a manner, that the pictures of thoſe objects are received by the centres of the retinas. I anſwer; I readily grant the poſſibility of the thing, but I aſſert at the ſame time, that we have no proof of it, which is a ſufficient reaſon for rejecting every concluſion that depends upon its truth.

Admitting, however, that objects are repreſented ſingle, when their pictures fall upon the centres of the retinas, or upon any other two points which are equally diſtant from the centres, and both upon the ſame ſide, it appears to me, notwithſtanding, to be in violation of all analogy, to aſcribe this effect, with reſpect to the points, at leaſt, on the right and left ſides of the centres, to any peculiar property which they poſſeſs from nature. For when anatomiſts find, in a new ſpecies of animals, organs ſimilar in ſtructure to thoſe of others they are already acquainted with, they immediately conclude, that they are alſo ſimi-

lar

lar in regard to their uſe. In animals of the ſame ſpecies, they believe with certainty, that the organs they ſee in one have the ſame properties, as the correſponding organs of another; and, if it be poſſible, they attribute with greater certainty the ſame properties to two organs of the like kind, which are found in the ſame individual. Such is the influence of the rule, that reſemblance of property is implied by reſemblance of ſtructure. Now it is an univerſal fact, that if an animal be divided into a right and left half, the correſponding parts of thoſe organs, which exiſt in pairs, are found at equal diſtances from the plane of partition. Thus, for inſtance, in reſpect to the eyes, the two optic nerves penetrate their outward coat at the ſame diſtance from this plane. Their muſcles, blood-veſſels, and every other of their component parts and appendages, are arranged in the like manner; thoſe neareſt to the dividing plane, or the innermoſt, in the one, being ſimilar in ſtructure to the innermoſt in the other, the outermoſt to the outermoſt, and the inter-

mediate to the intermediate, It is ſurely, therefore, natural to expect, that ſuch parts ſhould alſo be ſimilar in their properties; and we in fact find this ſimilarity to exiſt, wherever it can be clearly aſcertained what the properties are. Every perſon, for example, admits, that the internal ſtraight muſcle of the right eye performs the ſame office, with reſpect to that eye, as the other internal ſtraight muſcle does with reſpect to the left eye. What judgment are we then to form of the opinion of Dr. Reid, which attributes the ſame original properties, or rather the joint poſſeſſion of one original property, to places in the retinas ſituated at unequal diſtances from the general plane of partition; which makes an *external* point in one to correſpond, in uſe, with an *internal* point in the other, and this too by a principle implanted by nature? If ſuch things exiſt, they may, at leaſt, be ſaid to ſtand oppoſed to a moſt extenſive analogy.

To theſe arguments, *a priori*, againſt the opinion of Dr. Reid, I ſhall now add others, which

which are derived from a conſideration of its conſequences.

Firſt; Since viſible place, as was formerly obſerved, includes in it viſible diſtance, it is evident that, if both eyes, by virtue of an original property, ſee an object in the ſame place, diſtance muſt alſo be originally perceivable by ſight. Dr. Reid, * however, has himſelf ſo ably ſhown, that we would never have acquired, by means of our eyes, any knowledge of diſtance, unleſs they had been aſſiſted by the ſenſe of feeling, that I forbear to ſay any thing more upon this head, than that the exiſtence of no property can be admitted, which leads to the concluſion I have ſtated.

Secondly; If diſtance be not immediately perceivable by ſight, the only manner, in which an original property of the eyes can affect the viſible places of bodies, is by occaſioning them to appear in certain directions. Now Dr. Reid maintains, † that every external point is ſeen in the direction of a line

* Inquiry into the Human Mind, chap. 6. ſect. 3 & 20.

† Ibid. chap. 6. ſect. 12.

paſſing from its picture on the retina, through the centre of the eye. If, therefore, this direction be the ſame as that ſuggeſted by the original property ſo often mentioned, the latter law is merely another expreſſion for the former, and ought to be rejected as ſuperfluous. If it be different, and ſhould the two laws exiſt together, objects ſeen with both eyes might ſometimes appear quadruple, ſometimes triple, but never ſingle. Were they to exiſt ſucceſſively, one when we employ one eye, the other when both, an object, though at reſt, ſhould always appear to move when viewed alternately by one and by both eyes; neither of which concluſions is agreeable to experience.

Thirdly; To ſhow in a different way, and one perhaps more eaſily underſtood, that the opinion of Dr. Reid is not conſiſtent with the phenomena of viſion it ought to explain, I ſhall ſuppoſe an experiment to be made upon a perſon who ſquints. But I muſt premiſe, that it appears, from the

obſervations of Dr. Jurin * and himſelf, † that all ſuch perſons have one eye of a weaker ſight than the other; that when both eyes are open, the weaker is turned away from objects, which are attentively viewed; but that when the ſtrong eye is cloſed, the weaker is pointed to objects, exactly as the former would be in the ſame ſituation; and that it likewiſe perceives them in ſimilar directions. Let now the ordinary poſition of the perſon's eyes, upon whom the experiment is made, be ſuch, that the optic axes interſect each other about an inch or two from the face; and while the other is cloſed, let the flame of a candle be placed in the axis of the weak eye, which I ſhall call the left, at the diſtance of ſome feet from it, and on the right ſide of the body. The flame will conſequently appear in the ſame direction, as if his eye had no fault, and will be ſeen on his right, where it is in reality ſituated. Both eyes retaining the ſame poſition with reſpect to his head and each other, let the weak

* Smith's Optics. Vol. 2. Remarks, p. 30.

† Inquiry, chap. 6. ſect. 16.

weak eye be afterward ſhut, and the right opened, and let another object be placed in the axis of the latter, an opake body being at the ſame time ſo diſpoſed, as to hide from it the candle which is in the axis of the left eye. This object in the right axis will conſequently appear on the left ſide. Now, ſince the two objects, which have been thus viewed ſeparately, are ſituated, one to the right, and one to the left; and ſince they have been alſo *ſeen* in thoſe poſitions, their viſible places muſt be two, as well as their tangible, and muſt be remote from each other. How then ſhould theſe objects appear, if, inſtead of being viewed alternately, each by the eye in the axis of which it is placed, they were ſeen by the two together; the poſitions and internal ſtates of the eyes being in both caſes the ſame? Dr. Reid muſt anſwer; They will poſſeſs but one viſible place, ſince their pictures fall upon the centres of the two retinas, points endowed with the original property of repreſenting objects ſingle. But where is this one place

to

to be found? In the axis of the right eye, or in that of the left, or between the two? In any of theſe caſes, or in any other that can be imagined, the law of viſible direction, ſo much inſiſted upon by Dr. Reid, that objects appear in the perpendiculars to their pictures upon the retina, and in truth every other law of viſible direction hitherto publiſhed, muſt be ſuſpended with reſpect to one or both eyes; unleſs, indeed, the united object be referred to the interſection of the optic axes, about an inch or two from the face. This, I believe, Dr. Reid would not readily admit; but if he ſhould, another caſe of ſquinting may be imagined, in which the optic axes recede from each other, and where the ſame reaſoning will apply without the poſſibility of its force being thus eluded. It now remains for me to mention, that the experiment here ſtated by the way of ſuppoſition, in which the optic axes croſs each other near to the face, was actually made by Dr. Reid, with this reſult, that the two objects appeared in different places, when ſeen by both eyes together;

and

and that the other experiment, in which the optic axes are ſuppoſed to diverge, was made by myſelf, with a ſimilar event. Dr. Reid, however, inſtead of being led, by the termination of his experiment, to impute a fault to the principle from which he had expected a different one, concluded from it, that there was ſomething unnatural, beſide the ſquinting, in the perſon's eyes, upon whom it was made; though it had been previouſly aſcertained, that objects appeared in the ordinary manner to each of them, when ſeparately employed.

My examination of the ſecond claſs of opinions, reſpecting the cauſe of the ſingle appearance of objects to two eyes, being finiſhed, ſome perſon, perhaps, will now ſay; Granting that no error can, at firſt ſight, be ſhown in your arguments againſt thoſe of Dr. Smith and Dr. Reid, is it not a ſufficient reaſon for believing them fallacious, that they prove too much? If objects appear ſingle neither from cuſtom, nor an original property of the eyes, have we not an effect without a cauſe, and muſt there not

not be ſomething wrong in the facts or reaſoning which lead to ſuch a concluſion? The anſwer I make is as follows: Since viſible place contains in it both viſible diſtance and viſible direction, it is not neceſſary that the ſingle appearance of an object, to both eyes, ſhould depend altogether either upon cuſtom, or an original principle of our conſtitution; for its viſible diſtance to each eye may be learned from feeling, and its viſible direction be given by nature; in which caſe, the unity of its place to the two eyes, will be owing to neither of thoſe cauſes ſingly, but to a combination of both; and this I regard as a ſufficient reply.

THE END OF PART I.

AN

ESSAY

UPON

SINGLE VISION WITH TWO EYES.

PART II.

Of a new Theory reſpecting Viſible Direction, and of a Solution hence derived of the Queſtion, why Objects are ſeen ſingle with two Eyes.

I NOW proceed to offer a new opinion, why objects are ſeen ſingle with two eyes; or in other words, why they appear in the ſame place to both, this being the light in which I view the fact to be explained.

In every part of natural philoſophy, accidents often lead to diſcoveries, which reaſon alone might not eaſily have reached. Under this cover I hope to ſhelter myſelf from the charge of preſumption, in venturing

turing to give the ſolution of a problem, upon which the talents of many perſons of great learning and genius, have been unſucceſsfully employed; for ſhould I prove more fortunate than ſuch men have been, this muſt be attributed to the knowledge of a circumſtance I obſerved by chance, in repeating ſome very common experiments.

The viſible place of an object being compoſed, as I have already ſeveral times remarked, of its viſible diſtance and viſible direction, to ſhow how it may appear the ſame to both eyes, it will be neceſſary to explain, in what manner the diſtance and direction, which are perceived by one eye, may coincide with thoſe which are perceived by the other: and firſt with reſpect to the diſtance.

In judging of diſtance by ſight, we frequently make conſiderable miſtakes, even when the objects are not very remote; but no perſon, I believe, has ever obſerved, that while an object ſeemed to one of his eyes at a certain diſtance, it has appeared to the other to be at a different diſtance, and from this circumſtance alone has been ſeen double;

or, to expreſs the ſame thing in another way, that while the viſible appearance of an object to one eye, covered the viſible appearance of the ſame object to the other eye, the two appearances did not ſeem entirely to coincide, and make one, but were ſeen ſeparate by the two eyes. I do not ſtop to give the reaſon of this fact, which muſt be plain to thoſe who are acquainted with Biſhop Berkeley's theory of viſible diſtance; but proceed to mention, that the difficulty in finding a true and ſufficient cauſe for the union of the two viſible places of one or two objects to two eyes, muſt therefore conſiſt altogether in ſhowing, in what manner the two apparent directions may coincide, conſiſtently with the attending phenomena.

Since Kepler's great diſcovery of the ſeat and manner of viſion, there have been, as far as I know, only two theories offered reſpecting the apparent directions of objects. One is, that they are perceived in the direction of lines paſſing from their pictures on the retina, through the centre of the

eye; * the other, that their apparent directions coincide with their viſual rays. But both of theſe theories are inconſiſtent with the phenomena of ſingle viſion with two eyes. For according to neither of them can an object, placed at the concourſe of the optic axes, be ſeen ſingle, unleſs we have a moſt accurate knowledge of its diſtance; nor will either admit two objects to be ſeen as one, which are ſituated in the optic axes, whether on this ſide, or beyond where they meet, unleſs the united object be referred by ſight to their very point of interſection; both of which concluſions are contradicted by experience. It is evident, therefore, that ſome other theory of viſible direction is required, which ſhall not be liable to theſe objections; and ſuch a theory, I hope, I ſhall bring forward in the following propoſitions,

* Mr. D'Alembert has ſaid (Opuſcules Mathematiques, Tom. I. p. 265) that all optical writers before him had regarded it as an axiom, that every viſual point is ſeen in the direction of its viſual ray. But the aſſertion is not well founded. For Kepler long ago taught (Paralipomena in Vitellionem, p. 173) that objects are perceived in lines paſſing from their pictures upon the retina, through the centre of the eye; in which he was followed by Dechales and Doctor Porterfield; to the latter of whom Dr. Reid improperly attributes the diſcovery of the ſame ſuppoſed law.

ſitions, after mentioning the meanings which I affix to ſeveral terms I ſhall frequently employ.

EXPLANATION OF TERMS.

I. When a ſmall object is ſo placed with reſpect to either eye, as to be ſeen more diſtinctly than in any other ſituation, I ſay it is then in the *optic axis*, or the axis of that eye; and if another ſmall body be interpoſed between the former and the eye, ſo as to conceal it, and if a line joining the two be produced till it falls upon the cornea, I call this line the *optic axis*, or the axis of the eye; leaving for future determination the preciſe point of the cornea it falls upon, or what part of the retina receives the picture of an object which is placed in it.

II. When the two optic axes are directed to a ſmall object not very diſtant, they may be conceived to form two ſides of a triangle, the baſe of which is the interval between the points of the corneas, where the axes enter the eyes; but if the object be

be very diſtant, then they may be ſuppoſed to be two ſides of a parallelogram, whoſe baſe is the ſame interval. To avoid circumlocution, I ſhall call this interval the *viſual baſe*.

III. If there be drawn a line from the middle of the viſual baſe, through the point of interſection of the optic axes, or parallel to them, if they be parallel to each other, I name it the *common axis*. * This term I believe was invented by Alhazen; but with him it ſignified a line drawn from the centre of the junction of the optic nerves, through the middle of the interval between the centres of the retinas. Such

* It may be ſaid, perhaps, that as I do not define the points of the corneas, upon which the optic axes fall, I cannot, with propriety, define the line which connects them to be divided. To this I anſwer, that it is not neceſſary for the purpoſe I have mentioned, that they ſhould be defined; if it be granted to me, and I think it cannot be refuſed, that upon whatever point of the right cornea the right axis falls, the left axis will fall upon a ſimilarly ſituated point of the left cornea; that is, if this point of the right cornea be at any given diſtance from its middle, and upon the inſide of it, the correſponding point of the left cornea will be at the ſame diſtance from the middle of this, and alſo upon its inſide. Whatever extent, therefore, the line connecting theſe places of the corneas may have, its middle point will be the ſame.

Such a line was conſequently immoveable. As the term, however, is not in modern uſe, no miſtake can ariſe from confounding the two meanings, and the reaſon will ſoon be ſeen, why I employ it in the ſenſe I have mentioned. Thoſe who are acquainted with the writings of the older opticians will perceive, that I give it nearly the ſame ſignification as they did to their *common radius*.

PROPOSITION I.

Objects ſituated in the Optic Axis, do not appear to be in that Line, but in the Common Axis.

EVERY perſon knows, that, if an object be viewed through two ſmall holes, one applied to each eye, the two holes appear but as one. The theories hitherto invented afford two explanations of this fact. According to Aguilonius, Dechales, Dr. Porterfield and Dr. Smith, the two holes,

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or rather their borders, will be ſeen in the ſame place as the object viewed through them, and will conſequently appear united, for the ſame reaſon, that the object itſelf is ſeen ſingle. But whoever makes the experiment will diſtinctly perceive, that the united hole is much nearer to him than the object; not to mention, that any fallacy on this head might be corrected by the information from the ſenſe of touch, that the card, or other ſubſtance, in which the holes have been made, is within an inch or leſs of our face. The other explanation is that furniſhed by the theory of Dr. Reid. According to it, the centres of the retinas, which in this experiment receive the pictures of the holes, will, by an original property, repreſent but one. This theory, however, though it makes the two holes to appear one, does not determine where this one is to be ſeen. It cannot be ſeen in only one of the perpendiculars to the images upon the retinas, for no reaſon can be given why this law of viſible direction, which Dr. Reid thinks eſtabliſhed beyond diſpute, if it ope-

rates at all, ſhould not operate upon both eyes at the ſame time ; and if it be ſeen by both eyes in ſuch lines, it muſt appear where thoſe lines croſs each other, that is, in the ſame place with the object viewed through the holes, which, as I have already mentioned, is contrary to experience. Nor is it ſeen in any direction, the conſequence of a law affecting both eyes conſidered as one organ, but ſuſpended when each eye is uſed ſeparately. For when the two holes appear one, if we pay attention to its ſituation, and then cloſe one eye, the truly ſingle hole will be ſeen by the eye remaining open, in exactly the ſame direction as the apparently ſingle hole was by both eyes.

Hitherto I have ſuppoſed the holes almoſt touching the face. But they have the ſame unity of appearance, in whatever parts of the optic axes they are placed ; whether both be at the ſame diſtance from the eyes, or one be cloſe to the eye in the axis of which it is, and the other almoſt contiguous to the object ſeen through them. If a line, therefore, be drawn from the object to one of the eyes, it will repreſent all

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the real or tangible poſitions of the hole, which allow the object to be ſeen by that eye, and the whole of it will coincide with the optic axis. Let a ſimilar line be drawn to the other eye, and the two muſt appear but as one line; for if they do not, the two holes in the optic axes will not, at every diſtance, appear one, whereas experiments prove that they do. This united line will, therefore, repreſent the viſible direction of every object ſituated in either of the optic axes. But the end of it, which is toward the face, is ſeen by the right eye to the left, and by the left eye as much to the right. It muſt be ſeen then in the middle between the two, and, conſequently, in the *common axis*. And as its other extremity coincides with the point where the optic axes interſect each other, the whole of it muſt lie in the common axis. Hence the truth of the propoſition is evident, that *objects, ſituated in the optic axis, do not appear to be in that line, but in the common axis.*

Many other experiments might be mentioned which demonſtrate the ſame thing. If, for example, the head of a pin, or of a needle, be interpoſed between each eye, and any ſmall object to which both the optic axes are directed, the heads of the two pins or needles will conſtantly appear as one in the common axis. When the heads, however, are near to the eyes, this experiment is not ſo ſatisfactory as the former, ſince, in theſe poſitions, they ſeem as broad tranſparent ſhadows, for reaſons known to every perſon a little converſant in optics; whereas the holes appear well defined, though almoſt touching us. Again; if we hold two thin rulers in ſuch a manner, that their ſharp edges ſhall be in the optic axes, one in each, or rather a little below them, the two edges will be ſeen united in the common axis, and this apparent edge will ſeem of the ſame length with that of either of the real edges, when ſeen alone by the eye in the axis of which it is placed. If inſtead of two rulers we employ two ſtrings of different colours, as red and green, the like unity of appearance will

will be obſerved. But in this experiment it frequently happens, that, contrary to what we might naturally expect, only one of the ſtrings is ſeen at a time. When, however, only one is ſeen, its apparent ſituation is exactly the ſame as that of the ſtring, compounded, if I may ſo expreſs myſelf, of the two when ſeen together; and hence we have a convincing proof, if any were wanted, that the ſingle appearances of objects muſt depend upon ſome law of viſible direction affecting each eye, when employed by itſelf, in the ſame manner as when it is uſed conjointly with the other.*

PRO-

* Du Tour expected, that if two objects of different colours were ſeen in the ſame place by both eyes, which however he ſays, he was never able to obſerve, the colour of the apparently united object would be compounded of thoſe of the two really ſingle objects. Memoires des Savans Etrangers, Tom. iv. p. 500. And Dr. Reid mentions expreſsly that it is ſo compounded. Inquiry, p. 293. But in all my experiments upon this ſubject I have remarked, that, when the two objects appeared united, each was ſeen, notwithſtanding, in its proper colour; the red, for example, appearing as it were through a tranſparent green, and the green, in the ſame experiment, as through a tranſparent red. Nor is there any thing in this inconſiſtent with the received doctrine of the compoſition of colours. For in every inſtance of the production of a new colour, from rays of different colours being at

ſame

PROPOSITION II.

Objects, ſituated in the Common Axis, do not appear to be in that Line, but in the Axis of the Eye, by which they are not ſeen.

THE facts which demonſtrate the truth of this propoſition, are both numerous and common.

ſame time ſent to the eye, theſe rays fall upon the ſame ſentient extremities of the ſame nerve. But, in the caſe before us, the differently-coloured rays fall upon the ſentient extremities of two different nerves, which have no communication with each other, except through the medium of the brain. We have greater reaſon, therefore, for expecting, that the colours impreſſed upon the two eyes, ſhould be perceived uncompounded, than there is for two colours being perceived ſeparately, which are impreſſed upon two different parts of the ſame eye.

From the fact of the two colours being thus perceived diſtinct from each other, I would infer, by analogy, a mode of argument indeed often fallacious, that if it were poſſible for us to hear any one ſound with one ear only, and another ſound with the other ear only, ſuch ſounds would in no caſe coaleſce either wholly or in part, as two ſounds frequently do, when heard at the ſame time by one ear; that conſequently, if the ſounds of one muſical inſtrument were to be heard by one ear only, and thoſe of another, by the other ear only, we could have little or no perception of harmony from ſuch ſounds; and that, if in any ſucceſſion of ſounds emitted by one inſtrument, we were to hear the 1ſt, 3d, 5th, and ſo on, by one ear only, and the 2d, 4th, 6th, and ſo on, by the other ear only, we would be deprived, in a conſiderable degree, of the melody of ſuch ſounds, as this

ſeems

common. If a piece of wire, or any other ſubſtance, repreſenting a phyſical line, be placed in the common axis, with one of its extremities near to the viſual baſe, and if both the optic axes be directed to its farther or diſtant extremity, inſtead of one, two wires will be ſeen, meeting each other at their farther ends, and gradually diverging as they approach the face, till they apparently terminate at the eyes. If the right eye be cloſed, the wire which ſeemed to terminate at the left eye, diſappears; and if the left eye be cloſed, then the other wire diſappears; whoſe termination was at the right eye. The real wire, therefore, in the common axis, appears to the right eye to be ſituated in the axis of the left, and to the left eye to be ſituated in the axis of the right, agreeably to what the propoſition aſſerts.

The following experiments will illuſtrate and confirm both this and the preceding propoſition.

ſeems to depend in a great meaſure upon a new impreſſion being made upon the auditory nerve by one ſound, before the impreſſion of the ſound immediately preceding has paſſed away.

propoſition. Through a piece of card, or paſteboard, let two ſmall holes be made, the interval between which is ſuch, that while a very remote object is ſeen through one of them by the right eye, the ſame object may be ſeen through the other by the left eye. Make afterward another hole in the card, or paſteboard, exactly in the middle between the two former; and let the object be viewed through them as before. Theſe, or the outer holes, will now appear one, preciſely where the ſenſe of feeling indicates the middle hole to be; while the middle hole will appear as two, which ſeemingly occupy the places of the real outer ones. The two appearances of the middle hole, which is placed by conſtruction in the common axis, are therefore ſeen in the optic axes; and as the left is not ſeen when the right eye is ſhut, nor the right when the left eye is ſhut, each appearance is obſerved in the axis of the eye, by which it is not ſeen. As I have ſuppoſed the diſtance between the outer holes to be adapted to the interval of the

eyes when they are directed to a very remote object, the optic axes may, in this caſe, be regarded as parallel to each other. The object, therefore, will ſtill be ſeen through thoſe holes, though the diſtance of the card from the eyes be conſiderably varied; and at all the different diſtances, the ſame appearances will be obſerved, as thoſe which have been mentioned.

Again; take three ſtrings of different colours, as red, yellow, and green, and faſten, by means of a pin, one end of each to the ſame point of a table. Place now their looſe ends in ſuch a manner, that when you look at the pin with both eyes, the viſual baſe being parallel to the edge of the table, the red ſtring may lie in the axis of the right eye, the green in that of the left, and the yellow in the common axis. When things are thus diſpoſed, and both eyes are directed to the pin, the red and green ſtrings, inſtead of appearing ſeparate, each in one of the optic axes, and inclined to the viſual baſe or edge of the table, will now be ſeen occupying but one place, either

 together

together or ſucceſſively, as was formerly mentioned, and at right angles to the viſual baſe, or edge of the table; in ſhort, exactly in the ſituation, which the yellow ſtring in reality poſſeſſes; and the yellow ſtring, inſtead of appearing ſingle in the common axis, and perpendicular to the viſual baſe, will now be ſeen as two, each inclined to the baſe; that ſeen by the right eye, apparently occupying the place in reality poſſeſſed by the green ſtring, and that ſeen by the left eye, the place of the red ſtring.

PROPOSITION III.

Objects, ſituated in any Line drawn through the mutual Interſection of the Optic Axes to the Viſual Baſe, do not appear to be in that Line, but in another, drawn through the ſame Interſection, to a Point in the Viſual Baſe diſtant half this Baſe from the ſimilar Extremity of the former Line, towards the left, if the Objects be ſeen by the Right Eye, but towards the right, if ſeen by the Left Eye.

TWO caſes of this propoſition have already been proved. For it has been ſhown by

by the firſt propoſition, that objects, placed in the axis of either eye, appear to it to be ſituated in the common axis. But the common axis is a line drawn thro' the mutual interſection of the optic axes to the viſual baſe, and its termination there is diſtant, by conſtruction, half that baſe, from the ſimilar terminations of the axes of both eyes, to the left of the right axis, and to the right of the left. Again, it has been ſhown by the ſecond propoſition, that objects, placed in the common axis, appear to each eye to be ſituated in the axis of the other; and the terminations of both optic axes, at the viſual baſe, are diſtant half this baſe, from the ſimilar termination of the common axis, the left being to its right, and the right to its left.

Let it now be ſuppoſed that two objects, one placed in the axis of either eye, th right, for inſtance, and the other in common axis, be viewed at the ſame by that eye, it is evident that the directions of both will be equally to the left, from their real poſ

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ſuch an alteration of viſible direction, from real poſition, cannot be imagined to happen, with reſpect to objects placed in the optic and common axes, unleſs a ſimilar effect be, at the ſame time, produced upon ſuch as are ſituated any where between thoſe lines, or in their vicinity. Facts confirm this: If a line, for example, be drawn through the interſection of the optic axes to a point in the viſual baſe, exactly in the middle between the terminations there of the right and common axes, its apparent ſituation, to the right eye, will be found to have the ſame relation to the apparent ſituations of lines placed in the right and common axes, as its real ſituation has to the real ſituations of ſuch lines. And the like will be found, by obſervation, to be true of every other line, which may be drawn through the point of interſection of the optic axes to the viſual baſe.

The whole of what has here been ſaid may be illuſtrated and confirmed, by having again recourſe to the experiments with ſtrings of different colours. In formerly deſcribing

deſcribing thoſe experiments, I did not mention all the appearances which occurred upon making them, but only ſuch, as had immediate reference to the points then under conſideration. When, for inſtance, a red ſtring was placed in the axis of the right eye, and a green one in that of the left, I ſaid that they both appeared in the common axis. But this is not the only phenomenon to be obſerved with reſpect to their apparent number in this experiment. For as the red ſtring is alſo ſeen by the left eye, and the green by the right, two other ſtrings become viſible, beſide that in the common axis, the apparent poſitions of both of which will be found to be the ſame with thoſe, which ought to follow from the preſent propoſition. Should now a yellow ſtring be placed between the two former, as in the proof of the ſecond propoſition, its appearance to the right eye will biſect the ſpace between the appearances of the red and green ſtrings to that eye; and the like will be true with reſpect to the appearances of the three ſtrings to the left

eye, agreeably to what the ſame propoſition teaches us to expect.

I believe I need ſcarcely remark, that, although in moſt of the proofs and illuſtrations of theſe propoſitions, I have confined myſelf to the viſible appearances of lines *between* the interſection of the optic axes and the viſual baſe, the ſame things, however, muſt be equally true of thoſe lines, when they are produced beyond the interſection, with this difference only, that, while the portions within, ſeem, to the right eye, to be farther ſituated to the left than they really are, but to the left eye farther to the right, the portions beyond the interſection will ſeem to the right eye to the right of their real poſitions, but to the left eye to the left of them. For it is manifeſt, that, if a line be ſeen by one eye in a certain direction, a prolongation of it muſt be ſeen in the ſame direction; and that, if a line be made to turn upon any point in itſelf, the two extremities muſt move contrary ways.

Should the optic axes be parallel to each other, the ſame proofs and illuſtrations will ſtill

ſtill apply, ſince we may here ſuppoſe them to meet at an infinite diſtance from the viſual baſe. In this caſe, the viſible appearances of lines, drawn from this ſuppoſed point of interſection to the viſual baſe, will be parallel to the real lines, and diſtant half this baſe from them, through their whole extent.

AS I have thus, I think, ſufficiently proved, that the apparent directions of objects are governed by a law, different from any which has hitherto been thought to exiſt, I ſhall now proceed to ſtate, in a few words, in what manner the phenomena of ſingle and double viſion with two eyes are dependant upon it.

I formerly mentioned, that, ſince an object is never ſeen double, merely from its being ſeen at different diſtances by the two eyes, the only difficulty in explaining its ſingle appearance conſiſts in ſhowing how its two viſible directions may coincide, conſiſtently with the attending phenomena. But we are enabled to do this, with the utmoſt

moſt eaſe, by the theory I have endeavoured to eſtabliſh. For, if the queſtion be concerning an object at the concourſe of the optic axes, I ſay it is ſeen ſingle, becauſe its two ſimilar appearances, in regard to ſize, ſhape, and colour, are ſeen by both eyes in one and the ſame direction, or, if you will, in two directions, which coincide with each other through the whole of their extent. It therefore matters not, whether the diſtance be truly or falſely eſtimated; whether the object be thought to touch our eyes, or to be infinitely remote. And hence we have a reaſon, which no other theory of viſible direction affords, why objects appeared ſingle to the young gentleman mentioned by Mr. Cheſelden, immediately after his being couched, and before he could have learned to judge of diſtance by ſight.

When two ſimilar objects are placed in the optic axes, one in each, at equal diſtances from the eyes, they will appear in the ſame place, and therefore one, for the ſame reaſon that a truly ſingle object, in the concourſe of the optic axes, is ſeen ſingle.

Here

Here again, as the two viſible directions coincide in every point, it is not neceſſary that the united appearance ſhould be judged to be at any particular diſtance; that it ſhould be referred, for inſtance, to the concourſe of the optic axes, where the two other theories of viſible direction are obliged to place it, in oppoſition to the plaineſt obſervations.

Objects, any where in the horopter, will be ſeen ſingle, becauſe their apparent directions to the two eyes will then completely coincide. And for a contrary reaſon, thoſe placed in any other part of the plane of the optic axes will appear double. To make theſe things evident, let a line paſs through the point of interſection of the optic axes, and any given object, to the viſual baſe, which is to be produced, if neceſſary; and let it be called the line of the object's real poſition. Take afterward, in the viſual baſe, or its production, two points, one on each ſide of the line of real poſition, and both diſtant from its termination there, half the viſual baſe. Lines drawn from theſe

points, through the point of interſection of the optic axes, muſt conſequently contain the two viſible poſitions of the object. But when this is ſituated in the horopter, the line of real poſition will coincide with the horopter, and will not therefore reach the viſual baſe, unleſs at an infinite diſtance from the eyes. For which reaſon, the two lines, containing the viſible poſitions of the object, muſt fall upon the viſual baſe at a like diſtance, and muſt conſequently be regarded as coinciding with each other. When the object is not in the horopter, the two lines of viſible direction will be found, by the ſame means, not to coincide.

That I might ſimplify a matter, which under my management, muſt, I fear, ſtill be of difficult apprehenſion, I have, in expreſſing the law of viſion, ſo frequently mentioned, purpoſely confined it to objects ſituated in the plane of the optic axes. But in perſons who do not ſquint, or whoſe eyes are not diſtorted by external violence, the two appearances of an object, ſeen double, are always, either in that plane,

or in ſome one parallel to it; ſo that, if the viſual baſe be parallel to the horizon, a line joining the two appearances will, in every caſe, be alſo parallel to the horizon. Whoever then is able to explain, why objects in the plane of the optic axes appear either ſingle or double, may readily give a reaſon for the like appearances of ſuch as are placed any where elſe. Not to ſpend much time, therefore, upon this part of the ſubject, I ſhall ſhortly obſerve, that if planes be ſuppoſed to paſs through the two optic and common axes, perpendicular to that in which they all lie, and if two lines be drawn from any point of the common interſection of the former planes to the viſual baſe, one along each of the perpendicular planes which paſs through the optic axes, theſe two lines will appear as one, in the perpendicular plane of the common axis; the ſingle viſible line, however, poſſeſſing the ſame elevation, in regard to the horizon, as the two real lines: And again, that, if a line, drawn from any point of the ſame interſection to the viſual

baſe, along the perpendicular plane of the common axis, it will appear as two, one in each of the planes which paſs through the optic axes; the two viſible lines having the ſame inclination to the horizon in their progreſs to the viſual baſe, as the real ſingle one. In this manner, every thing may be ſhown to be true, with reſpect to the ſingle and double appearances of objects without the plane of the optic axes, which has already been done with regard to thoſe placed in it. But farther; ſince any point, taken at pleaſure, in the common interſection of the three perpendicular planes, appears ſingle, the whole of the line of interſection muſt appear ſo, and likewiſe every point of a plane made to paſs through it, parallel to the viſual baſe. Such a plane neceſſarily includes the horopter, and is the ſame as that, which is called by Aguilonius the plane of the horopter.

To exemplify the principal property of this plane, I ſhall mention an experiment, which at firſt I did not underſtand, though the

the reſult was a direct conſequence of my own principles. I ſuſpended a fine chord at right angles to the horizon, and retreating a ſtep or two, I looked ſteadily at a point in it, which was upon a level with my eyes. The chord, in theſe circumſtances, appeared ſingle; but whenever I directed my eyes to any other point of it, either above or below the former, two chords would appear, croſſing each other at the part, to which the eyes were directed. In the firſt caſe, the whole chord was in the plane of the horopter, but in every other, only that point of it to which both eyes happened to be turned. A concluſion from this experiment is, that no object, which is truly perpendicular to the horizon, will appear to be ſo, while our bodies are erect, unleſs we direct our eyes to a point in it exactly upon a level with themſelves.

It was once my intention to ſubjoin here ſeveral inſtances, from the moſt approved authors, of inaccurate deſcriptions of the ſingle and double appearances of objects; in order to ſhow, that the theory of viſible

viſible direction, which I have advanced, is not only conſiſtent with the univerſally received facts, but that it alſo diſcovers to us, ſome minute errors, which unguided ſenſe has committed upon this ſubject; it being, perhaps, one of the ſureſt teſts of the ſoundneſs, as well as one of the chief uſes, of theories in philoſophy, that they lead to the knowledge of what, otherwiſe, might have remained for ever hidden. But fearing I have already proved tireſome, I give up this deſign, and haſten to the conſideration of ſome conſequences from my theory, which ſeem to me both curious and important, and which, when firſt mentioned, may appear to carry with them their own refutation.

END OF PART II.

AN

AN

ESSAY

UPON

SINGLE VISION WITH TWO EYES.

PART III.

Of ſome Conſequences from the foregoing Theory of Objects being ſeen ſingle with two Eyes, together with the Explanation of ſeveral other Phenomena of Viſion.

IT has hitherto, I believe, been thought by opticians, that, if the poſition of the eye be unchanged, the viſible direction of an object will be the ſame, as long as its picture occupies any one point of the retina; and that, in every different poſition of the eye, a picture, which continues to occupy the ſame point of the retina, will repreſent its

object in a different direction. But if the theory be juſt, which I have advanced in the preceding part of this Eſſay, neither of thoſe opinions can be univerſally true. For it follows, from what was there mentioned, that if one of the optic axes be kept fixed, and the other be at different times variouſly bent toward it, objects, though ſituated in the fixed axis, will nevertheleſs change their viſible directions, with every variation of the moveable axis; ſince they muſt always appear in the common axis, which alters its poſition with every change of the moveable axis: And again, that, if the two optic axes ſhould vary their inclinations to each other in ſuch a manner, that the common axis, may, notwithſtanding, remain fixed, an object placed in either optic axis, and following it in every motion, will poſſeſs but one viſible direction, in all this variety of real poſitions. That theſe concluſions from my theory, or rather parts of it, are true in fact, I can aſſert upon the authority of obſervations, and I ſhall now attempt to trace

trace them both to a common principle, by means of ſome experiments, which were inſtituted with a very different view.

When we have looked ſteadily for ſome time at the flame of a candle, or any other luminous body, a coloured ſpot will appear upon every object, to which we ſhortly after direct our eyes, accompanying them in all their motions, and exactly covering the point, which we deſire to ſee the moſt accurately. Whatever therefore can be proved concerning the apparent direction of ſuch a ſpot, in any given poſition of the eyes, muſt likewiſe be true in the ſame poſition of the eyes, with regard to the apparent direction of an object, ſituated at the concurrence of the optic axes; as its pictures muſt occupy, in this caſe, the very parts of the retinas, upon the affections of which the illuſion of the ſpot depends. This being premiſed, I ſhall now relate one or two obſervations, reſpecting the apparent directions of the ſpot, and conſequently upon thoſe of external objects,

which, as far as I know, have not been mentioned by any other perſon.

1. The ſpot is always ſeen ſingle, whether the ſurface, upon which it is projected, be touching the face, or at the greateſt diſtance from us; and the reaſon is plain. For the parts of the retinas, by whoſe affections from the luminous body it is occaſioned, are thoſe likewiſe which receive the pictures of objects, placed at the interſection of the optic axes; and as ſuch objects always appear ſingle, ſo muſt alſo the ſpot. The fact indeed is ſo open to obſervation, and its cauſe ſo eaſily ſhown, that I ſhould ſcarcely have thought of mentioning it, had not Dr. Darwin* lately told us, that the ſpot is ſeen double, as often as the eyes are directed to an object more or leſs diſtant than the luminous body which gave riſe to it. With reſpect to our different aſſertions upon this point, I ſhall only ſay, that I have made the experiment, I believe, upward of an hundred times, uniformly with

* Philoſoph. Tranſact. for 1786, p. 318. Dr. Darwin indeed, ſays, p. 341, that Buffon had obſerved the ſame fact; but it is evident he has miſtaken that author's meaning.

with the ſame reſult; and that, if the ſpot ever appears double, this muſt be from ſome cauſe very wide of a change in the mutual inclination of the optic axes, to which he attributes it.*

2. The ſpot not only appears ſingle in every ordinary poſition of the optic axes, but cannot even be made to appear double, by any means whatſoever. If it be projected, for example, upon a piece of white paper, whoever makes the trial will find, that, although, on preſſing one eye upward or downward, or to either ſide, the paper will be ſeen double, yet the ſpot

* The only way, in which I think it poſſible for the ſpot to appear double, conſiſtently with the univerſally acknowledged fact, that an object at the interſection of the optic axes is always ſeen ſingle, is this, that, when the interſection is near to the face, an object placed in it ſhall not ſend its pictures to the ſame points of the two retinas, as it does, when the interſection is more remote. And ſuch I once hoped to find to be the caſe; for I had formed, upon the ſuppoſition of its truth, a more plauſible account of the manner in which the eyes are fitted to receive, ſucceſſively, pictures equally diſtinct from objects at different diſtances, than any I had met with. But, after many experiments to aſcertain the matter, I was obliged to return to the common opinion, that the picture of an object in the optic axis, whatever be its diſtance from the eye, is always received upon the ſame point of the retina.

will always appear ſingle, and to poſſeſs its former place on the paper, as ſeen by the eye, which is not diſturbed. Before I knew the reſult of this experiment, I had imagined, that, the poſition of one eye being forcibly altered, the external ſituation of the ſpot, which was ſuggeſted by the affection of that eye, would likewiſe be altered, and the ſpot by conſequence be ſeen double. As the event, however, was contrary to my expectation, I began to ſuſpect ſome cauſe of fallacy had been overlooked, which at length I thought might be this, that the ſpot had been ſeen by that eye only whoſe poſition was not diſturbed, the violence, ſuffered by the other, interrupting the due exerciſe of its functions. To determine, therefore, whether my conjecture was well founded or not, I made another experiment, which is mentioned in the following article :

3. Having looked ſteadily for ſome time at the flame of a candle, with *one* eye only, I directed afterward, with both eyes open, my attention to the middle

of

of a ſheet of paper, a few feet diſtant ; the conſequence of which was, that a ſpot appeared upon it in the ſame manner, as if I had viewed the flame with both eyes, though ſomewhat fainter. My attention remaining fixed upon the ſheet, I now puſhed the eye, by which the ſpot was ſeen, ſucceſſively upward and downward, to the right and to the left, and in every oblique direction ; the ſpot however never altered its poſition, but kept conſtantly upon the middle of the appearance of the paper, perceived by the undiſtorted eye, though the appearance of the paper to the diſtorted eye, was always ſeparate from the former, and the ſheet conſequently ſeen double. My conjecture, therefore, was proved to be ill grounded, and all ſuſpicion of fallacy in the former experiment ceaſed.

Now it is evident, from theſe two laſt experiments, that the ſituation of the ſpot does not depend upon the bare poſition of the eyes, or elſe, in the former of them, it would have appeared double, and in the latter,

latter, it would have been moved from the middle of the paper, when the only eye by which it was ſeen was puſhed from its place. Neither can it depend upon the bare poſition of the muſcles of the eye, as theſe were alſo moved in the ſame experiments; nor upon any affection whatever of the optic nerve. For ſince this laſt ſubſtance is altogether paſſive, even in thoſe motions of the eyes which do occaſion a change of the ſpot's ſituation, every alteration, induced upon the nerve by thoſe motions, muſt be ultimately aſcribed to a change of its poſition; and we have ſeen, that ſimilar changes of its poſition have been produced by external violence, without any alteration of the ſpot's ſituation. The apparent ſituation of the ſpot being, therefore, dependant upon none of theſe circumſtances, and being at the ſame time affected by the *voluntary* motions of the eye, it muſt, I think, be neceſſarily owing to the *action* of the muſcles, by which theſe motions are performed. Aſſuming then as true, that the apparent direction of an object, which ſends its picture

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to any given point of the retina, depends upon the ſtate of action exiſting at the ſame time in the muſcles of the eye, and conſequently that it cannot be altered, except by a change in the ſtate of that action, I ſhall proceed to trace to this principle, ſeveral phenomena of viſion, particularly the uniform ſingleneſs of the ſpot already deſcribed, and the two facts reſpecting the viſible directions of objects in the optic axis, which were mentioned in the beginning of this part of my Eſſay.

The thing itſelf is univerſally acknowledged, though a diſpute has ariſen whether cuſtom or an original property be the cauſe, that every voluntary motion of one eye, in perſons who do not ſquint, is attended with a correſponding motion in the other. Now as all voluntary motions are produced by muſcular action, it follows, that every ſtate of action, in the muſcles of one eye, has its correſponding ſtate in thoſe of the other, and that the two are conſtantly conjoined. When, therefore, the ſpot appears ſingle to both eyes in their

free poſitions, the ſtates of action in the muſcles muſt be ſuch, that the direction, in which it is ſeen by one eye, coincides with that in which it is ſeen by the other. But, if we puſh one eye from its place, no change is hereby made in the action of its muſcles; for the ſtate of action in thoſe of the free eye is confeſſedly the ſame as it was; and it will be attended with a correſponding ſtate in thoſe of the diſtorted eye; in proof of which it may be obſerved, that, whenever the preſſure is removed, the diſtorted eye immediately returns to its former poſition, without the aid of any new muſcular effort. The concluſion then is, that, ſince there has been no alteration in the action of its muſcles, neither ought there to be any in the direction of the ſpot ſeen by it, which is the fact to be explained.

Hence alſo is to be derived the true reaſon, why objects appear double, when one eye is puſhed from its place. For as their pictures muſt fall upon points of the retina in this eye, different from what they formerly

formerly poſſeſſed; and as no change is made, by the diſtortion, upon the viſible direction, ſuggeſted by any part of the retina, the objects will be ſeen by the preſſed eye, exactly in the ſame directions as they would have been, *before* it was preſſed, had the pictures *then* fallen upon the points of the retina, which they *now* occupy. They muſt therefore be *now* ſeen in different directions by the two eyes, and conſequently double. An experiment with a contrary event will confirm this explanation, and likewiſe ſhow more clearly, in what I differ from thoſe who have endeavoured to account for the ſame fact. Both eyes being open, let one of them be puſhed from its ſituation, and let two ſimilar objects, ſuch as two pieces of money of the ſame metal and ſtamp, be afterward ſo placed, that one ſhall lie in each optic axis; theſe two objects will now appear to be one, and the object ſo compounded will be ſeen in the place, to which the undiſturbed eye refers the truly ſingle object lying in its axis.

Another inference from this doctrine is, that, if the eyes are in any very unuſual poſition with reſpect to each other from the action of their own muſcles, as in perſons who ſquint, two objects placed in the optic axes, one in each, will not appear as one object; for each will be ſeen in the direction, which is determined by the ſtate of action in the muſcles of the eye, upon whoſe retina its picture falls; and as this ſtate, in one eye, does not correſpond with that in the other, the directions cannot coincide. This concluſion is verified by the reſult of an experiment of Dr. Reid upon a perſon, affected with ſtrabiſmus, and by that of another, made by myſelf, both of which have been already related.

To explain, therefore, why an object in the optic axis appears at different times in different directions, though the axis be kept fixed, it is only neceſſary to ſhow, that, whenever this happens, a change, notwithſtanding, occurs in the actions of the muſcles which move the eye. With this view, I obſerve, that the motions of that organ

organ may be divided into two ſets; the firſt, conſiſting of thoſe, by which one eye is carried along with the other, upward and downward, to the right and to the left, and in every oblique direction, the interval between the pupils remaining conſtantly the ſame; the ſecond, of the motions of the pupils, or the anterior parts of the eyes, to and from each other. Suppoſing now, that both the optic axes are perpendicular to the viſual baſe; ſhould the left axis be afterward inclined to the right ſide, the natural tendency of the right axis is to incline equally to the ſame ſide, ſo as to preſerve its former parallelism to the left. This tendency, however, in the right axis to follow the left, may be counteracted by an effort of the muſcles, which regulate the interval of the pupils, until the two axes interſect each other within two or three inches of the face. But it is evident, that the ſame degree of muſcular force will be required to retain the right eye in its original poſition, as is neceſſary to give to the left eye its motion toward the right; and

hence, that, in every different inclination of the left axis to the right, an object placed in the latter, though its real position be unchanged, will, nevertheleſs, appear in a different direction, in conſequence of the different ſtate of action in the muſcles of the right eye, which accompanies every new degree of inclination of the axes to each other. As the object muſt always appear in the common axis, the alteration, in this example, of its viſible direction, from an *increaſe* of the mutual inclinations of the optic axes, will be from left to right; but when the inclination decreaſes, from right to left. If the right axis be the one which is moved, and the left fixed, the alterations of viſible direction in an object placed in the latter, from ſimilar changes in their inclinations, will be contrary to thoſe which have juſt been mentioned.

The reaſon alſo can now be made to appear, why an object, preſerving conſtantly its place in the optic axis, may, in a conſiderable variety of its real poſitions, poſſeſs but one viſible direction. For, in ſuch caſes,

caſes, the change of its viſible direction, which might be expected to accompany the motion of the eye in the axis of which it is ſituated, is prevented from occuring, by a tendency to a change of its viſible direction the contrary way, produced by the muſcular actions which regulate the mutual diſtance of the pupils. To know how this happens, ſuppoſe the two optic axes to be parallel to each other, and perpendicular to the viſual baſe; and let a phyſical line be placed in either of them, ſo as entirely to coincide with it. This line will, therefore, not only be in reality perpendicular to the viſual baſe, but will, in the preſent ſtate of things, likewiſe appear ſo.—Incline afterward both the axes equally to the left ſide, and it is manifeſt that the line coinciding, ſay, with the right axis, muſt appear equally inclined. Let now the right axis be kept fixed, and the left be carried back again, and its motion continued, until it be as much inclined toward the right ſide, as itſelf was juſt before, and as the right axis is ſtill to the left ſide; the conſe-

quence

quence will be, that the line in the right axis muſt again be ſeen perpendicular to the viſual baſe; for ſuch is the preſent poſition of the common axis. Here then we have had two oppoſite cauſes of change of apparent direction acting in ſucceſſion. The muſcular actions, producing the joint motions of the eyes, firſt bent the viſible poſition of a line, in the right optic axis, from a perpendicular to the viſual baſe toward the left; and the muſcular actions, which regulate the mutual diſtances of the pupils, by *increaſing* the inclinations of the axes to each other, moved it afterward, from the left to the right, back again to a perpendicular to the viſual baſe. Let theſe two cauſes act together, and it is plain, that no obſervable effect will be produced by either, as long as they are thus proportioned. When they are not ſo, only the difference of their forces will be exhibited by the phenomena.

But farther; to ſhow the extent of this theory of viſible direction being dependant upon the actions of the muſcles of the eyes,

I ſhall

I ſhall now apply it to the explanation of an inſtance of apparent motion, which at firſt may be thought to furniſh an argument againſt it. Look with one eye, the other being cloſed, at any remote object through a ſmall hole in a card. If you ſhould afterward ſuddenly attempt to view the hole itſelf accurately, with the ſame eye, you will obſerve both it and the diſtant object, particularly the latter, to move from left to right, if the right eye be uſed ; but if the left eye be the one employed, then from right to left. Shift now your attention as ſuddenly back from the hole to the object ſeen through it, and both will return to the places they formerly occupied. In this experiment, no real change can be ſuppoſed to have occurred in the poſition of the diſtant object; and had any happened, with reſpect to either the eye or the hole, the object would not have been ſeen through the latter. No other fallacy, therefore, exiſts here, than that things, which are truly at reſt, appear, notwithſtanding, to be in motion.

The

The argument, which I have mentioned may hence be derived againſt my theory, is this : The viſible directions of objects, in the optic axis which remained fixed, were formerly ſaid to be altered, becauſe a new ſtate of muſcular exertion was required to keep it ſo, in every different degree of the inclination to it of the moveable axis. But in the laſt experiment, there ſeems no good reaſon for ſuppoſing any change in the inclination of the moveable axis to the other ; for, as one eye is cloſed, the obvious intention of directing the two axes to the ſame object, which is, that we may ſee it ſingle, no longer exiſts. If then an apparent lateral motion be, in one inſtance, obſerved in objects truly at reſt, without any change of the interval of the pupils, may not every other motion of the like kind be alſo independant of the muſcular actions, which regulate that interval ?

It is evident, that this argument reſts altogether upon the ſuppoſition, that in the experiment juſt mentioned, no alteration occurs in the interval of the pupils. Now,

we may be eaſily convinced, that ſome alteration does occur, by applying a finger to the cloſed eye, which will, by this means, be felt to move toward the noſe, when we endeavour to view the hole accurately, and from the noſe, when we carry our attention back again to the remote object. Were, indeed, the opinion of Aguilonius * juſt, that the mind perceives only thoſe objects diſtinctly, which are ſituated at the concourſe of the optic axes, whether they are ſeen with one or with two eyes, both the neceſſity and the degree of the alteration would be clearly aſcertained. But as this opinion is not juſt, which I mean to prove from experiments in a ſucceeding part of this volume, I ſhall proceed to give another reaſon, and I think the true one, why the interval of the pupils ſhould be as much altered, when we look with one eye at objects ſucceſſively, which are placed at different diſtances, as if we were to view them with both.

It is a fact, for which I have the authority of experiments almoſt without number,

* Optica, p. 84.

though I do not recollect to have ſeen it mentioned by any author beſide Dr. Porterfield, that every change of the mutual poſitions of the optic axes is conjoined, in perſons who do not ſquint, with a change of the power, in both eyes, to refract the rays of light which fall upon them.—When the axes are parallel to each other, the eyes are in their loweſt refracting ſtate; but in their higheſt, when the axes are mutually interſected within two or three inches of the face; every intermediate inclination being alſo conjoined with an intermediate degree of refracting power. Now, ſince thoſe objects are ſeen moſt diſtinctly, the radious pencils from which are accurately brought to points in the retina, it follows, that, although we employ one eye only, the ſame reaſon exiſts for adjuſting its refractive power to their diſtances, as if we ſaw with both. When, therefore, we view a remote object with one eye, we uſe it in its loweſt refracting ſtate, which, I have obſerved, is conjoined with the wideſt interval of the pupils. Should we afterward attempt to

ſee accurately a very near object, the eye will aſſume its higheſt refractive ſtate, and the interval of the pupils be leſſened; the conſequence of which muſt be, that both the objects lying in the optic axes will appear to move in the manner already related.

To finiſh this part of my ſubject, it ſeems only neceſſary to determine, whether the dependance of viſible direction upon the actions of the muſcles of the eyes be eſtabliſhed by nature, or by cuſtom. But facts are here wanting. As far as they go, however, they ſerve to prove, that it ariſes from an original principle of our conſtitution. For Mr. Cheſelden's patient ſaw objects ſingle, and conſequently in the ſame directions with both eyes, immediately after he was couched; and perſons affected with ſquinting from their earlieſt infancy, ſee objects in the ſame directions with the eye they have never been *accuſtomed* to employ, as they do with the other they have conſtantly uſed.

Having thus ſhown in what directions external bodies are ſeen, when their ſitua-

tion with reſpect to the eye is given, and upon what circumſtance the various directions depend, in which a picture upon any one place of the retina can exhibit the object producing it; I ſhould render the theory of viſible direction complete, were I now to point out the relative poſitions of the two lines of direction, in which any two different parts of the retina repreſent their objects. To aſcertain this, the firſt ſtep muſt be, to find the place of the retina which receives the picture of an object, whoſe ſituation with reſpect to the external eye is known; and if two ſuch points of the retina were determined, I think the chief difficulty in this matter would then be overcome. But as it appears to me, that the ſtructure of the eye has not yet been ſufficiently explained, to enable any perſon to take this firſt ſtep, I forbear ſaying any thing more upon the ſubject.

END OF THE ESSAY UPON SINGLE VISION WITH TWO EYES.

EXPE-

EXPERIMENTS *and* OBSERVATIONS

ON

SEVERAL SUBJECTS IN OPTICS.

ARTICLE I.

On Viſible Poſition, and Viſible Motion.

IN the eſtimates we make by ſight of the ſituation of external objects, we have always ſome ſecret reference to the poſition of our own bodies, with reſpect to the plane of the horizon; and from this cauſe, we often judge ſuch to be at reſt, whoſe relative places to us are continually changing; and others to be in motion, though they may conſtantly preſerve, in regard to us, the ſame diſtance and direction. To give an

an inſtance, let us ſuppoſe our eyes firſt directed to a ſtar near to the horizon; ſhould we afterward, by a mere motion of the head, point them to another, ſome degrees above the former, this ſecond ſtar will appear higher than the firſt did. Were we now, while the eyes are kept fixed in relation to the head, and the head in relation to the ſhoulders, to incline the trunk of the body backward, until we bring the optic axes to a third ſtar, this will appear ſtill higher than the ſecond was perceived to be. If inſtead of directing the eyes ſucceſſively to different objects, the ſame object be ſuffered to remain at the concurrence of the optic axes in all theſe different poſitions of the body, it is evident, that it muſt be ſeen to move, during the change from one poſition to another.

The facts I have mentioned are ſo obvious, that I ſhould not have ſpoken of them, had I not intended they ſhould introduce the following queſtion: What is there within us, to indicate theſe poſitions of the body? To me it appears evident, that, ſince they are occaſioned and preſerved by combinations

combinations of the actions of various voluntary muſcles, ſome feeling muſt attend every ſuch combination, which ſuggeſts, from experience perhaps, the particular poſition produced by it. But in almoſt all the poſitions of the body, the chief part of our muſcular efforts is directed toward ſuſtaining it againſt the influence of its own gravity. Each poſition, therefore, in which this takes place, muſt be attended with a feeling, which ſerves to indicate its relation to the horizontal plane of the earth; and conſequently, if our bodies poſſeſſed no gravity, or, if the thing were poſſible, had we been created unembodied ſpirits, but with the ſame faculties of perception as we enjoy at preſent, we could no more have judged one line to be perpendicular, and another to be parallel to the horizon, than we can at preſent determine, without ſome external aid, which is the eaſtern, and which the weſtern point of the heavens. I ſhall now draw from theſe principles, the explanation of a fact, which was firſt mentioned by one of the moſt ingenious authors

that

that have written upon viſion, but left by him ſtill to be juſtly accounted for.

"I have frequently (ſays Mr. Melvill)* "obſerved, when at ſea, that, though I "preſſed my body and head firmly to a cor- "ner of the cabin, ſo as to be at reſt in "reſpect to every object about me, the diffe- "rent irregular motions of the ſhip, in roll- "ing and pitching, were ſtill diſcernible by "ſight. How is this fact to be recon- "ciled to optical principles? Shall "we conclude that the eye, by the "ſudden motions of the veſſel, is rolled "out of its due poſition? Or, if it retains a "fixed ſituation in the head, is the percep- "tion of the ſhip's motion, owing to a ver- "tigo in the brain, a deception of the ima- "gination, or to what other cauſe?"

I need not, I believe, offer to ſhow, that the fact here ſpoken of, is not owing to any of the cauſes Mr. Melvill has ſpecified. I ſhall therefore, in a few words, point out its dependance upon the principles which have juſt been mentioned.

It is generally known, I ſuppoſe, that when a veſſel at ſea, in the language of ſailors, is

* Edinburgh Phyſical Eſſays, vol. ii. p. 89.

is ſaid to pitch, its two extremities turn upon its ſhorter axis, and that the term of rolling is confined by them to its motions upon the longer axis. In both pitching and rolling then, the relative poſition of a veſſel to a horizontal plane is neceſſarily changed. Conſequently, though, in the above-mentioned experiment, Mr. Melvill's body and head were at reſt with reſpect to every object about him, ſtill a different degree of muſcular effort was required to keep them ſo, in every ſuch different poſition of the veſſel. But each degree of muſcular effort, to ſuſtain his body againſt the operation of its gravity, would ſuggeſt to him its concomitant poſition with regard to the plane of the horizon ; each deviation, therefore, of the veſſel from its former ſituation, relatively to the ſame plane, would be perceived, and the veſſel itſelf be ſeen to move. In ſhort, nothing more takes place in this, than in the following experiment : Let a pole be placed upon firm ground, at right angles to the horizon. If, while we are ſtanding erect, it be inclined upon its

lower extremity, ſucceſſively backward and forward, to the right and to the left, theſe motions muſt, without contradiction, be perceived. Suppoſe now, our bodies to be ſimilarly inclined with the pole, during its different poſitions, ſo as to be conſtantly parallel to it; it is evident, that its motions will be as readily perceived in this caſe, as they were, when our bodies were erect; and this is all that happens in the experiment of Mr. Melvill.

Should the neceſſity of ſupporting the body againſt its gravity, by the actions of our voluntary muſcles, be ſuſpended in whole, or in part, our judgments of the ſituation of objects, with reſpect to the horizon, muſt become irregular and uncertain, notwithſtanding any general habit we may have acquired from experience. An inſtance of this, I think, I have obſerved; for I have frequently remarked during a ſea voyage, that, when the wind blew ſo ſtrongly, and in ſuch a direction, as to occaſion the veſſel to heel, or lean much to one ſide, chords freely ſuſpended from the

roof of the cabin, and kept ſtretched by heavy bodies attached to them, have appeared to me, as long as I lay in bed, though they were neceſſarily perpendicular to the horizon, to decline conſiderably from that poſition; while the ſides of the cabin ſeemed, if not perpendicular, at leaſt much leſs inclined to the horizon than they were in reality. My body being here ſupported by the bed, I was conſequently without thoſe feelings, which indicate its poſition with reſpect to the horizon. Objects therefore appeared to me in thoſe ſituations, in which I had been accuſtomed to ſee them. In confirmation of which I may mention, that, when I got up, and ſtood upon the floor of the cabin, the chords ſeemed perpendicular, or nearly ſo, and the ſides of the cabin inclined; for I was now obliged to exert a proper degree of muſcular force, to keep myſelf upright. What I here ſay, however, is from the recollection of things obſerved ſome years ago, when I had no thought of making the uſe of them I now do; for which reaſon,

I may poſſibly have committed ſome trifling error in ſtating them; but none, I believe, ſufficient to affect the theory they are brought to ſupport.

It being my intention to treat, in the preſent article, of ſeveral facts relative to viſible poſition and motion, which ſeem to me to need explanation, without regarding whether or not they depend upon any common cauſe; I paſs to the conſideration of the apparent rotation of objects, when we have become giddy, by turning ourſelves quickly and frequently round.

Some of the older writers upon optics imagined the viſive ſpirits to be contained in the head, as water is in a veſſel, which therefore, when once put in motion by the rotation of our bodies, muſt continue in it for ſome time after this has ceaſed; and to this real circular movement of the viſive ſpirits, while the body is at reſt, they attributed the apparent motions of objects in giddineſs. Dechales* ſaw the weakneſs of this hypotheſis, and conjectured, that

* Curſus Mathemat. Tom. ii. p. 422.

that the phenomenon might be owing to a real movement of the eyes, but produced no fact in proof of his opinion. Dr. Porterfield,* on the contrary, ſuppoſed the difficulty of explaining it to conſiſt in ſhowing, why objects at reſt appear in motion to an eye which is alſo at reſt. The ſolution he offered of this repreſentation of the phenomenon, is not only extremely ingenious, but is, I believe, the only probable one which can be given. It does not apply, however, to the fact which truly exiſts; for I ſhall immediately ſhow, that the eye is not at reſt, as he imagined. The laſt author, I know of, who has touched upon this ſubject, is Dr. Darwin.† His words are, "When any one turns round ra-
"pidly on one foot till he becomes dizzy,
"and falls upon the ground, the ſpectra of
"the ambient objects continue to preſent
"themſelves in rotation, or appear to li-
"brate, and he ſeems to behold them for
"ſome time in motion." I do not indeed pretend

* Treatiſe on the eye, Vol. ii. p. 426.

† Philoſoph. Tranſact. Vol. lxxvi. p. 315.

pretend to underſtand his opinion fully; but this much ſeems clear, that, if ſuch an apparent motion of the ſurrounding objects depends, in any way, upon their ſpectra, or the illuſive repreſentations of thoſe objects, occaſioned by their former impreſſions upon the retinas, no ſimilar motion would be obſerved, were we to turn ourſelves round with our eyes ſhut, and not to open them till we became giddy; for in this caſe, as the ſurrounding objects could not ſend their pictures to the retinas, there would, conſequently, be no ſpectra to preſent themſelves afterward in rotation. But whoever will make the experiment, will find, that objects about him appear to be equally in motion, when he has become giddy by turning himſelf round, whether this has been done with his eyes open or ſhut. I ſhall now venture to propoſe my own opinion upon this ſubject.

If the eye be at reſt, we judge an object to be in motion when its picture falls in ſucceeding times upon different parts of the retina; and if the eye be in motion, we judge

judge an object to be at reſt, as long as the change in the place of its picture upon the retina, holds a certain correſpondence with the change of the eye's poſition. Let us now ſuppoſe the eye to be in motion, while, from ſome diſorder in the ſyſtem of ſenſation, we are either without thoſe feelings, which indicate the various poſitions of the eye, or are not able to attend to them. It is evident, that, in ſuch a ſtate of things, an object at reſt muſt appear to be in motion, ſince it ſends in ſucceeding times its picture to different parts of the retina. And this ſeems to be what happens in giddineſs. I was firſt led to think ſo from obſerving, that, during a ſlight fit of giddineſs I was accidentally ſeized with, a coloured ſpot, occaſioned by looking ſteadily at a luminous body, and upon which I happened at that moment to be making an experiment, was moved in a manner altogether independant of the poſitions I conceived my eyes to poſſeſs. To determine this point, I again produced the ſpot, by looking ſome time at the flame of a candle;

then

then turning myſelf round till I became giddy, I ſuddenly diſcontinued this motion, and directed my eyes to the middle of a ſheet of paper, fixed upon the wall of my chamber. The ſpot now appeared upon the paper, but only for a moment; for it immediately after ſeemed to move to one ſide, and the paper to the other, notwithſtanding I conceived the poſition of my eyes to be in the mean while unchanged. To go on with the experiment, when the paper and ſpot had proceeded to a certain diſtance from each other, they ſuddenly came together again; and this ſeparation and conjunction were alternately repeated a number of times; the limits of the ſeparation gradually becoming leſs, till, at length, the paper and ſpot both appeared to be at reſt, and the latter to be projected upon the middle of the former. I found alſo, upon repeating and varying the experiment a little, that when I had turned myſelf from left to right, the paper moved from right to left, and the ſpot conſequently the contrary way; but that when I had turned from right to left, the paper would then move from left to right.

Theſe

Theſe were the appearances obſerved while I ſtood erect. When I inclined, however, my head in ſuch a manner, as to bring the ſide of my face parallel to the horizon, the ſpot and paper would then move from each other, one upward and the other downward. But all theſe phenomena demonſtrate, that there was a real motion in my eyes at the time I imagined them to be at reſt; for the apparent ſituation of the ſpot, with reſpect to the paper, could not poſſibly have been altered, without a real change of the poſition of thoſe organs. To have the ſame thing proved in another way, I deſired a perſon to turn quickly round, till he became very giddy; then to ſtop himſelf and look ſteadfaſtly at me. He did ſo, and I could plainly ſee, that, although he thought his eyes were fixed, they were in reality moving in their ſockets, firſt toward one ſide, and then toward the other.

The laſt inſtance of viſible motion I ſhall notice, is one which has been mentioned by Mr. Le Cat, in the following words:*

* Traitè des Sens, p. 419.

 " Place

" Place a lighted candle at a moderate dif-
" tance from a poliſhed body of conſiderable
" convexity, ſo that the image of the flame,
" which is ſeen by reflection from it, may
" appear as a ſmall luminous point. The
" experiment will ſucceed better, if the di-
" rect rays of the flame be intercepted
" from the ſight. Cloſe after this, one
" eye, and view the luminous point in a
" careleſs way, *(en revant)* that is to ſay,
" with the eye in a relaxed or dilated ſtate.
" The point will then be ſeen enlarged and
" radiated. If you bring now your fin-
" ger to the right of the eye which is open,
" and gradually move it toward the left,
" in order to conceal the luminous point
" from this eye, you will diſtinctly perceive
" the ſhadow of your finger to proceed from
" left to right, and to paſs over the point
" in a direction, contrary to that which
" you gave it. Should you, afterward,
" move your finger back from right to left,
" and in like manner, if your finger be
" moved from above downward, or from
" below upward, the ſhadow will always

proceed

" proceed the contrary way. It is there-
" fore manifeſt, that the ſoul muſt here
" ſee objects inverted, as their images
" in the eye truly are; and that it refers
" impreſſions to thoſe parts of the eye
" where it feels them, and not to the places
" from which the rays are emitted, as
" it does when it poſſeſſes the means
" of rectifying its judgment. Whence
" does this happen? Doubtleſs, becauſe
" the luminous point has neither a high
" nor a low, neither a right nor left ſide,
" nor any well-enlightened object in its
" vicinity, to awaken the attention of the
" ſoul; in ſhort, nothing which can de-
" termine its judgment."

I ſhould ſcarcely have mentioned this experiment, from any reſpect for the authority of its author in optics; but as Haller* ſeems to aſſent to the concluſion he draws from it, that the ſoul ſometimes ſees objects inverted; and as the Abbot Derochon,† a member of that learned body, the Aca-

* Elementa Phyſiologiæ, Tom. v. p. 479.
† Memoires de Phyſique, p. 66.

demy of Sciences of Paris, has lately, but in my opinion unſucceſsfully, attempted to reconcile it to the commonly-received principles of viſion, I think it worth while to ſhow, in a few words, that it is a direct conſequence of the very doctrine Mr. Le Cat means to overthrow by its means.

It would be proper, indeed, to mention before hand, the opinion of the Abbot Derochon; but this I muſt, notwithſtanding, omit doing, as it could not be underſtood without the figure by which he has illuſtrated it. I ſhall obſerve, however, reſpecting it, *firſt*, that it requires the ſide of the finger next to the eye, to be without the leaſt illumination; whereas the experiment will ſucceed, whether it be illuminated or not: *ſecondly*, that, according to it, the experiment ought to ſucceed equally well, whether the image of the flame in the mirror be ſeen as a point, or as a ſurface; though, in truth, it never does ſucceed, except in the latter caſe: *thirdly*, that the apparent ſhadow of the finger

finger is always much larger than it ought to be, were it ſeen by reflection, as the Abbot thinks: *fourthly*, that, while the eye, mirror, flame, and finger, remain in the ſame poſitions, the ſhadow ſeems at one time larger than at another, owing to the different degrees of relaxation in the eye; but that this, for the reaſon juſt mentioned, ought never to happen, according to his theory: *fifthly*, that, agreeably to his own reaſoning, the ſhadow ought to move in the ſame direction with the finger, which is the very reverſe of the fact to be explained. But as arguments againſt error may be infinitely extended, and as only one ſolution of a phenomenon can be true, the readieſt way of expoſing the inſufficiency of others, is to exhibit that which is juſt.

This, in the preſent caſe, ſeems to lie upon the very ſurface of optical knowledge, and has already been given by others, of various forms of the ſame fact. When the image of the flame is ſeen in the mirror as a point, its rays muſt be accurately

rately collected to a focus in the retina; but when ſeen as a ſurface, this muſt neceſſarily be attributed to their focus being either before or behind it; in either of which caſes, they will occupy a place upon that membrane of ſome aſſignable dimenſions. In the preſent inſtance, their diffuſion over a part of the retina, depends on the focus being behind it; for the eye is now, from a condition of the experiment, in a more relaxed ſtate than it was juſt before, when the rays of the ſame object were brought there accurately to a point. The rays, therefore, which go to the right ſide of the enlightened ſurface of the retina, or picture as I ſhall call it, are thoſe which enter the eye at the right ſide of the pupil, and its left ſide is formed of the rays entering at the left ſide of the pupil; and the like muſt be true of its upper and lower parts. Should we then begin to move a finger from right to left acroſs the eye, the rays forming the right ſide of the picture muſt be firſt intercepted. But from the known fact, that the points of an external object are always in an inverted poſition,

poſition, with reſpect to the parts of the retina, by the affections of which they are ſuggeſted, when the *right* ſide of the picture there is effaced, the *left* ſide of the external object it ſuggeſts muſt diſappear. And for the ſame reaſon, if the motion of the finger be continued from right to left acroſs the eye, the other parts of the luminous ſurface in the mirror will ſucceſſively vaniſh from left to right, and thereby furniſh the appearance of a ſhadow paſſing over it in that direction.—In like manner, it may be ſhown, that if the finger proceeds from left to right, from above downward, or from below upward, the ſhadow muſt move the oppoſite way.

That this is the true explanation of Mr. Le Cat's experiment, is, I think, plain, both from its intrinſic evidence, and the following conſiderations:—If the mirror be brought within four or five inches of the eye, and the candle be ſo placed, that the image of the flame muſt, from the laws of reflection, be regarded as a mere point;

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though we ſhould now view it with the utmoſt care, and though there ſhould be in its neighbourhood ſome well-enlightened object to awaken the attention of the ſoul, as Mr. Le Cat expreſſes it, ſtill the ſeeming ſhadow will move in a direction contrary to the finger. For the image is now ſo near to the eye, that no exertion we can make is ſufficient to bring its rays to a point upon the retina; the picture, therefore, upon that membrane will be formed of rays paſſing to a focus behind it, which is the only condition neceſſary for the ſucceſs of the experiment. Again, if a ſhort-ſighted perſon ſhould place the mirror at the diſtance of ſome feet from him, complying in other reſpects with Mr. Le Cat's inſtructions, he will conſtantly obſerve the ſhadow to move in the ſame direction with the finger. For, in his eye, the rays of the image, when at ſuch a diſtance, muſt meet before they fall upon the retina. The right ſide, therefore, of the picture upon that membrane, muſt be compoſed, in this caſe, of rays which

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enter the eye at the left ſide of the pupil. Conſequently, when theſe are cut off, the left ſide of the apparent luminous ſurface muſt diſappear, and the ſhadow be ſeen to move the ſame way as the finger, when this ſucceſſively intercepts the rays proceeding from the image to the eye.*

* Scheiner obſerved a fact of the like kind (Fundamentum Opticum, p. 33) namely, that, if a ſmall hole, made in any ſubſtance, be held near to the eye, and an opaque body be paſſed between them, from right to left, the left ſide of the hole will firſt diſappear. Mr. Grey afterward took notice (Philoſoph. Tranſact. Vol. xix. p. 286) that a needle he employed in this experiment was ſeen inverted; from which he ſuppoſed that the hole, or ſomething in it, produced the effect of a concave ſpeculum. Mr. Harris, however, ſays (Treatiſe of Optics, p. 141) that it is not the needle, but its ſhadow on the other ſide, which is ſeen, and is the cauſe of the inverted appearance. But the truth is, that the hole is to be regarded as a luminous point, the rays of which fall upon the retina before they are collected to a focus; and hence that the ſame appearances muſt be here obſerved as in the experiment of Mr. Le Cat. In proof of this it may be mentioned, that if the hole be placed at ſuch a diſtance, that the eye may refract its rays accurately to a point on the retina, no ſhadow or image of the needle will be ſeen; that if the hole be ſtill farther removed, and the eye be adapted to a leſs diſtance, the ſhadow or image will again appear, but its poſition will now be upright, and its motion the ſame way as that of the needle itſelf; and laſtly, that, at one given diſtance of the hole, either no ſhadow will appear, or it will be ſeen upright, or it will be ſeen inverted, according as the eye may be made to aſſume different ſtates with reſpect to its power of refraction.

ARTICLE II.

On a ſuppoſed Conſequence of the Duration of Impreſſions upon the Retina; and the Effects of accurate Viſion being confined to a ſingle Point of that Membrane.

FEW things, at firſt, appear more incredible to a perſon, not converſant in optics, than that he does not, at any one time, ſee diſtinctly a ſurface larger than the head of a pin. After he is convinced, by proper trials, of the truth of this, he naturally aſks, Whence comes it then, that, in ordinary viſion, I ſeem to view diſtinctly ſo many objects at once? I go into a crowded ſtreet, and I fancy I have an accurate perception by ſight, of men, houſes, carriages, and many other things, all at the ſame time; whence proceeds this illuſion?

Only one anſwer, as far as I know, has been given to this queſtion. The impreſſions made upon the retina by external objects, do not, it is ſaid, immediately ceaſe

ceaſe, along with the reception of the rays which flow from them; and, as in the ordinary mode of viſion, the eye is continually paſſing from object to object, the impreſſion left by a former one may be ſtill vivid, though the eye be directed to another; and hence we may imagine we ſee both of them diſtinctly, though the picture of only one occupies that place of the retina, which alone furniſhes us with accurate viſion.

There are, however, objections to this anſwer, which ſeem to me inſurmountable. For, *in the firſt place*, as the duration of impreſſions on the retina muſt be greater or leſs, according to the vivacity of the pictures which occaſion them, it follows, that, were this anſwer juſt, the apparent field of our diſtinct viſion ought to be in proportion to the quantity of light admitted by the eye; that it ſhould be contracted, therefore, by every cloud which paſſes over us, and be enlarged by every burſt of ſunſhine; that, at mid-day, it ſhould poſſeſs its greateſt extent, and

ought from that time gradually to decreaſe till the evening, when its limits ſhould be nearly the ſame with thoſe of the real field of accurate viſion. *Secondly*, ſince the coloured ſpot, which is produced by looking ſteadily for ſome time at a luminous body, appears projected upon every object to which we direct our eyes, during its continuance, and as ſuch a ſpot is neceſſarily the ſign and effect of the duration of an impreſſion upon the retina; every other viſible appearance from the ſame cauſe ought, in like manner, to have its ſituation determined by the poſition of the eye, as far as this may be occaſioned by the action of its muſcles. No object, therefore, ought to appear ſeparate and diſtinct from others, if the anſwer were true which I am combatting; but, on the contrary, all thoſe to which we ſucceſſively direct our eyes during the limits of the duration of an impreſſion upon the retina, ſhould ſeem crowded into one place; and, conſequently, none of them ſhould be perceived with any tolerable accuracy.—

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Such are the concluſions from the truth of this anſwer. I need ſcarcely mention, that they are contradicted by experience.

There is another form of the ſame fact, to which, it may be thought, an explanation taken from the duration of impreſſions on the retina will better apply; I mean the appearance of a fiery circle, when any red-hot body is moved quickly round. But it ſeems, to me, that ſuch an explanation cannot even here be admitted. For, if the circle depended upon the cauſe I have mentioned, it could only be obſerved as long as the impreſſions upon the retina were alſo diſpoſed in the form of a circle. Were this broken upon, which it muſt be by every movement of the eye, the appearance ſuggeſted by the laſt impreſſion would no longer be ſo arranged, with reſpect to the appearance ſuggeſted by the preſent impreſſion, as to lie with it in the circumference of a circle; and hence ſome very different figure would be obſerved. Every perſon, however, may eaſily convince himſelf, that the circular form of

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the fiery appearance is equally perceived, whether the eye be at reſt, or be moved in the moſt irregular manner.

If theſe arguments be thought ſufficient for the purpoſe I had in view, it muſt alſo follow from them, ſince the fact ſtill remains to be explained, why we apparently ſee ſo many objects with equal diſtinctneſs at once, that paſt impreſſions upon the retina are perceived as preſent, by means of ſome higher faculty than that of ſight. This faculty cannot, with propriety, be named *memory*, as it is eſſential to a thing's being remembered, that it be perceived as paſt. Nor can it be called *imagination*, ſince we believe in the preſent exiſtence of what it perceives. In one point of view it may ſeem rather a defect in our natures, that we ſhould not be able to diſtinguiſh between things paſt and preſent. However this may be, I am inclined to be of opinion, that many other phenomena, both of thought and external ſenſe, are partly to be reſolved into the ſame general fact. From the preſent inſtance of

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it, we learn, that ſeveral muſcular actions may be performed, in ſucceſſion, during the leaſt perceptible portion of time.

The queſtion I have juſt treated, naturally gives riſe to another: Would it have been more to our advantage, if accurate viſion, inſtead of being confined to one point of the retina, had been poſſeſſed by every part of that membrane? I anſwer, I think not, for the following reaſons.

Firſt; The diffuſion of ſuch a property over the whole retina would be of little uſe, unleſs our power of attention was alſo increaſed. For we would otherwiſe be ſtill unable to perceive more than one viſible object at once, with diſtinctneſs, ſince, by our preſent conſtitution, we are capable of attending accurately to only one thing at a time. The only benefit, indeed, I can ſee to ariſe from ſuch a condition of the retina, is this, that our attention might be ſhifted more quickly from picture to picture on that membrane, than our eyes can be turned from one external object to another.

another. This advantage, however, would be far out-weighed by an inconvenience accompanying it. For it is a well-known fact, with reſpect to perception, that we are capable of attending, more or leſs accurately, to any particular impreſſion upon the ſenſes, in proportion to the force of the other impreſſions, which are at the ſame time received. But in the ſuppoſed ſtate of the retina, there would be, almoſt always, ſeveral impreſſios of the ſame ſtrength as the one to which we might deſire particularly to attend; whereas, in its preſent ſtate, the vivacity of the impreſſion from the object, to which we turn the optic axis, moſt commonly ſurpaſſes, conſiderably, that of every other upon the ſame membrane; by which means our attention is rendered leſs liable to interruption.

Secondly; The extenſion of accurate viſion, to every part of the retina, would deprive us, in great meaſure, of the help, which we obtain, at preſent, from the eye, in learning the thoughts of other men.

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As far as I have been able to obſerve, the changes produced by our internal feelings, upon the ſtate of the eye itſelf, are very few, and relate only to the quantity of moiſture, which is diffuſed over its ſurface, and the degree of fulneſs in the blood-veſſels, which are ſpread upon its white and gliſtening part. Both of theſe circumſtances, however, are ſimilarly altered by oppoſite paſſions, and, conſequently, neither of them can be regarded as the appropriate expreſſion of any. The whole variety, then, of the expreſſions of feeling, which are juſtly attributed to the eye, muſt, I think, depend upon its motions. Some of theſe are the immediate effects of certain paſſions; the eye, for inſtance, being moved differently in anger and in grief; and ſuch may be eſteemed as directly expreſſive of the paſſions by which they are produced. But the far greater number of them do little more, than merely point out the external cauſe, or object of the ſentiment, which the changes of other parts of the countenance declare to

exiſt within us; or diſtinguiſh certain external appearances depending upon a mental cauſe, from ſimilar appearances ariſing from a different ſource. Thus, bluſhing is often diſtinguiſhed from an accidental fluſh of the cheek, by the eye being turned away from the perſon who occaſions it.

That many of the expreſſions, which we attribute to the eye, do in fact depend on changes in other parts of the countenance, is evident from the alterations we think induced upon it, by the eye-laſhes falling off from diſeaſe, by a ſlight inflammation of the edges of the eye-lids, without its being communicated to the eye itſelf, by artificially colouring the eye-brows, and by many other ſimilar circumſtances. And how eſſential to the right underſtanding of the expreſſions of the other features, are the motions of the eyes, when conducted with deſign, and properly directed, muſt be known to every one, who has attended in diſcourſe to the countenances of very ſhort-ſighted people, and more eſpecially to thoſe of perſons afflicted with

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blindneſs from a *gutta ſerena*, in which the eye, with reſpect to its external condition, ſeems without fault. But whatever is the aſſiſtance the motions of the eye afford, in expreſſing our internal feelings, the whole of it muſt ultimately be referred to the circumſtance of accurate viſion being confined to one point of the retina; ſince the intent of thoſe motions is, to bring the pictures of external objects upon the moſt ſenſible part of that membrane. Their neceſſity, therefore, would no longer exiſt, if the ſame property were extended, and the advantages we at preſent enjoy from them would, conſequently, ceaſe.

ARTICLE III.

On the Connection between the different refractive States of the Eyes, and the different Inclinations of the Optic Axes to each other.

I HAVE mentioned, in my Eſſay upon Single Viſion with Two Eyes,* that I had been convinced, by experiments almoſt without number, that every different degree of the mutual inclination of the optic axes, is attended by a different ſtate of the refracting power of each eye. The experiments I there alluded to were chiefly of this ſort. I placed a luminous point, moſt commonly the reflected image of the flame of a candle from the bulb of a ſmall thermometer, at ſuch a diſtance, that when both my eyes were accurately directed to it, its viſible appearance to one of them was likewiſe that of a point. Keeping then the axis of this eye fixed, and making the other to croſs it, ſometimes before and ſometimes behind the luminous point, I found that in both

* P. 82.

both cafes it appeared as a ſurface to the eye, in the axis of which it was ſituated; and that the more remote from it was the concurrence of the axes, the larger was the luminous ſurface. Now when the axes met before the point, the apparent ſurface muſt have been occaſioned by the rays coming to a focus, previouſly to their incidence upon the retina; becauſe, when I paſſed my finger acroſs the eye by which it was ſeen, its parts diſappeared, in an order correſponding to the direction in which the finger moved. The diſappearance of the parts was in an order, contrary to the motion of the finger, when my optic axes interſected each other beyond the point; which is an equal proof, that the rays, in that caſe, tended to a focus behind the retina.

One application of this fact has already been ſhown,* and I ſhall now proceed to mention ſeveral other phenomena in viſion, which it may ſerve either in whole, or in part, to explain.

1. It

* Eſſay upon Single Viſion, p. 83.

1. It accounts for the following beautiful obſervation made by Aguilonius,† that if we cloſe one eye, and look with the other at an object placed in its own axis, we will not be able to ſee this object diſtinctly, unleſs we alſo direct to it the axis of the cloſed eye. For in perſons, who are neither preſbytic nor myopic, the refractive ſtates of the eyes are ſo adapted to the mutual inclinations of the optic axes, that pencils of rays flowing from bodies at moderate diſtances are more accurately collected upon the retina, when they are ſituated at the interſection of thoſe lines, than if their poſition was, in any conſiderable degree, either nearer or more remote. The reaſon given by Aguilonius himſelf, is, that the mind perceives only thoſe objects diſtinctly, which are placed at the concourſe of the optic axes. But the following experiment proves that the ſolution is true no farther, than as it coincides with the one I have advanced. Hold, in the axis of either eye, a concave lens, at ſuch

† Aguilonii Optica, p. 84.

ſuch a diſtance, that the letters of a book, placed a little farther off, may appear through it very indiſtinct to that eye, when both axes are directed to any particular word. View afterward the lens itſelf with both eyes, and the letters will immediately become more diſtinct. In this experiment then, an object is more accurately perceived when diſtant from the concourſe of the optic axes, than when ſituated exactly in it.

It may be ſaid, perhaps, that the diſtinctneſs of the letters is here to be attributed to the contraction of the pupil, which is occaſioned by the eyes being directed to a nearer object than they were formerly. But that this is not the caſe, may be made evident by another experiment: Place a convex lens in ſuch a manner before one eye, that the flame of a candle, at the diſtance of two or three feet from the face, may appear indiſtinctly terminated to that eye, when both axes are pointed to it. The ſame eye being kept fixed, let the two axes afterward meet

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beyond the flame, and it will now be ſeen much better defined, though the pupil is at the ſame time become larger. The inſufficiency of the explanation of Aguilonius, is alſo proved, by a circumſtance frequently noticed in perſons who are very ſhort-ſighted; for ſuch are obſerved, when they deſire to view an object with much attention, to hold it cloſe to one eye, and to turn the other aſide; in this way occaſioning the two axes to meet very remotely from the object.

2. The reaſon commonly given, why ſhort-ſighted people view an object with one eye only in the manner above-mentioned, is, that by this means they avoid the uneaſy ſtraining of the muſcles, which muſt be employed to direct both axes to the ſame point. But it is evident they muſt derive from the practice this farther advantage, that, as their optic axes are now parallel to each other, or nearly ſo, they, conſequently, ſee the object in the leaſt refractive ſtate of their eyes. Pencils, therefore, will now have their focuſes in the retina,

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the rays of which would have croſſed each other, before they fell upon it, had both the axes been directed to the object.

3. Spectacles were long employed, before the manner in which they aſſiſted ſight was known. About the year 1601, this was propoſed as the ſubject of a queſtion to Kepler,* by his principal patron at that time, Ludovic L. B. a Dietrickſtein, a learned nobleman of Auſtria. The firſt anſwer he gave was, that convex glaſſes were of uſe, by occaſioning objects to appear larger. But his patron obſerved, that if objects were rendered by them more diſtinct, becauſe larger, no perſon would be benefited by concave glaſſes, ſince theſe diminiſh objects. It was not till three years after, that, in conſequence of finding out in what manner viſion is performed, he was able to give a juſt ſolution of this problem, though his attention had been directed to it during the whole of that interval. According to the diſcovery he then made, convex glaſſes were ſaid by him to aſſiſt the ſight of preſ-

* Paralipomena in Vitellionem, p. 200.

bytic perſons, by ſo altering the directions of rays diverging from a near object, that they ſhall afterward fall upon the eye, as if they had proceeded from a more remote one; and concave glaſſes to benefit the myopic, by producing a contrary effect upon rays which diverge from a diſtant object. Now it is manifeſt, that by this theory, to which I believe no addition has been made by any ſucceeding writer, preciſely the ſame effects are attributed to lenſes, whether they be employed ſingly, or in the form of ſpectacles. I am inclined, however, to think, that a difference, ſometimes at leaſt, exiſts here, which has hitherto eſcaped notice. For in regard to ſuch ſpectacles as I have tried upon myſelf, I have always found, that, when I looked with them at objects placed at moderate diſtances directly before me, my optic axes paſſed through the glaſſes, more inwardly than their centres. With reſpect, therefore, to ſpectacles for long-ſighted people, as the inner halves of their glaſſes may be regarded as two priſms, whoſe refracting angles face each other,

other, to have allowed both my eyes to receive through them pencils of rays from the ſame point of an object, the intervals of my pupils muſt have been leſs than was neceſſary for that purpoſe in naked viſion. The conſequence of which would be, an increaſe of the refractive power of my eyes. Again; as the like parts of glaſſes in ſpectacles for ſhort-ſighted perſons, may be eſteemed to be two priſms, the refracting angles of which are turned from each other, the interval of the pupils muſt have been increaſed, and the refracting power of my eyes by this means diminiſhed, when I looked at an object through them, which was directly before me. And effects ſimilar to what I have mentioned, muſt have followed my viewing objects placed obliquely, through glaſſes of both kinds. Here then is one advantage, which perſons, who ſee with both eyes, either do or may enjoy from ſpectacles, but which they cannot derive from uſing ſingle glaſſes. For if they are preſbytic, they can ſee an object by the means of them with a higher refractive ſtate

of the eyes, than if the optic axes met there, as in naked viſion ; and if myopic, with a leſs. It is alſo worthy of remark, that this advantage does not ultimately tend to increaſe the evil, which firſt gives occaſion for ſpectacles. On the contrary, if what every writer upon viſion aſſerts be true, that we are apt to become ſhort or long-ſighted, according as we are much accuſtomed to view near or diſtant objects, it muſt ſerve to diminiſh that evil. In ſupport of this opinion, I ſhall mention a fact, with which I have been made acquainted by Mr. George Adams,* of this place, who is not only well ſkilled in the theory of viſion, but, from his ſituation, as an artiſt, has better opportunities, than moſt perſons, of learning ſuch matters. The fact is this, that he does not know a ſhort-ſighted perſon, who has had occaſion to increaſe the depth of his glaſſes, if he began to uſe them in the form of ſpectacles ; whereas he can recollect ſeveral inſtances, where thoſe have been obliged to change their concave

* Mathematical Inſtrument Maker to the King.

concave glaſſes repeatedly, for others of higher powers, who had been accuſtomed to apply them to one eye only. This indeed may have happened by accident; but, at any rate, the fact is worthy of farther attention and inquiry.

It would ſeem, however, that the long-ſighted derive more benefit from the alteration in the mutual inclinations of the optic axes, which is produced by ſpectacles, than the ſhort-ſighted. For, as the inner halves of the convex glaſſes are to be regarded as priſms, with their refracting angles continually increaſing as we approach their edges, if two objects, ſituated at different diſtances, be viewed ſucceſſively through them, the inclination of the optic axes to each other, when the nearer object is ſeen, muſt bear a higher proportion to their inclination, when we look at the one more remote, than the different inclinations of the optic axes do to each other, when they are ſucceſſively directed to the ſame objects, without the intervention of ſuch glaſſes. Hence the nearer the object

is,

is, the greater will be the effect of the variation in the inclination of the axes produced by ſpectacles with convex glaſſes; which is the order of things, the beſt adapted to the wants of thoſe who uſe them. But with reſpect to ſhort-ſighted perſons, ſince the refracting angles of their glaſſes, conſidered as priſms, *decreaſe*, in proportion as the objects ſeen through them become more remote; they muſt, conſequently, derive the leaſt benefit from an alteration in the mutual inclinations of the optic axes occaſioned by their ſpectacles, at the time they moſt require it.

If it were aſked, then, what is the real foundation of the common reproach againſt ſpectacles for long-ſighted people? I ſhould anſwer, a very different one from that, which is, for the moſt part, aſſigned.—For the change, in the conformation of the eyes, which renders them uſeful, ſeems to be one of thoſe which nature has deſtined to take place at a particular age, and to which there is no gradual approach through the preceding courſe of life. A perſon, for inſtance,

inſtance, at forty, ſees an object diſtinctly, at the ſame diſtance that he did at twenty. When he draws near to fifty, the change I have ſpoken of commonly comes on, and obliges him in a ſhort time to wear ſpectacles. As it proceeds, he is under the neceſſity of uſing others with a higher power. But, inſtead of ſuppoſing that his ſight is thus gradually becoming worſe, from a natural proceſs, he attributes the increaſe of the defect in it to his too early and frequent uſe of glaſſes. Upon the whole, I ſhould draw this inference from what has been ſaid, that no perſon, whoſe ſight begins to grow long, ought to be, in the leaſt, prevented from enjoying the immediate advantage which ſpectacles will afford him, by the fear that they will ultimately injure his eyes; not that I think the convexity of each glaſs, conſidered by itſelf, can do no harm, but that I believe the benefit, ariſing from the combination of the two, to be at leaſt ſufficient to compenſate it. Whether thoſe, who have a tendency to ſhort-ſight, ſhould be alſo early in their employment of

ſpectacles,

ſpectacles, I ſhall not pretend to ſay; as there is not the ſame ground, from theory, for ſuppoſing, that the benefit ariſing from the combination of the two glaſſes is able to over-balance the injury, produced by the concavity of each conſidered ſeparately.

All that I have ſaid, however, upon the ſubject of ſpectacles, proceeds upon the ſuppoſition, that, when objects, placed directly before us, at moderate diſtances, are viewed through them, the optic axes penetrate the glaſſes more inwardly than their centres. But I can be by no means ſure, that the interval of the pupils of other perſons bears the ſame proportion, to the interval of the centres of the lenſes in ſpectacles, as that of mine does. It concerns thoſe, therefore, who are chooſing them, to have attention to this circumſtance. To me it appears proper, that the glaſſes in ſpectacles, both for long and ſhort-ſighted people, ſhould be ſo far aſunder, that, when we look at a very remote object directly before us, our optic axes may paſs exactly through their centres. For if the

centres

centres of convex glaſſes be nearer to each other, very remote objects will appear double; and if they are more diſtant, though the object viewed be infinitely far from us, the optic axes will, however, be inclined to one another, and the refractive power of the eyes increaſed, when this may be of diſſervice; ſince there are few eyes which are not able, even without the aid of the convexity of a glaſs, to bring parallel rays to a focus upon the retina. If the centres of lenſes in ſpectacles, for the ſhort-ſighted, be leſs diſtant than what I have mentioned, the optic axes muſt be bent toward each other, when very remote objects are ſeen, and the refractive ſtate of the eye, therefore, heightened, which is the very reverſe of what is here to be deſired. Should the interval of the centres of thoſe lenſes be greater, objects at very conſiderable diſtances will be ſeen double.

There are two other obſervations relative to glaſſes for the ſight, which I wiſh to add to what I have already ſaid upon this ſubject. The firſt is, that the ſingle con-

vex glaſſes with which ſome perſons read, muſt be very injurious, if they be ſufficiently large, to admit the ſame object to be ſeen with both eyes. For as both axes will then paſs through them, one on each ſide of the centre, the interval of the pupils will be widened, and the refracting power of the eyes, be diminiſhed; ſo that here a diſadvantage is to be added to the prejudice of the convexity of the glaſs, not a benefit to be placed againſt it, as in the caſe of common ſpectacles for the long-ſighted. If, indeed, the defect in ſight does not ariſe from the conformation of the eye, but from a want of tranſparency in its cornea or humours, then ſuch glaſſes, by magnifying objects, will be uſeful, for the ſame reaſon, that, in a very faint light, we can read a book of a large print, with more eaſe than one of a ſmaller. The ſecond obſervation is, that if flat-ſided priſms were fixed in ſpectacle-frames, with their refracting angles toward each other, they would aſſiſt the long-ſighted ſomewhat, without producing the evil which is ſaid to

ariſe

ariſe from the convexity of lenſes; and ſpectacles of this kind might, with more propriety, I think, than any others, be called *preſervers*. A like combination of ſuch priſms, but with their angles turned the other way, might, when the object was moderately diſtant, be of ſervice to the ſhort-ſighted. But objects, very remote, would be made by them to appear double.

ARTICLE IV.

On the Limits of perfect or diſtinct Viſion.

DR. Jurin,* I believe, was the firſt who diſtinguiſhed between *perfect* and *diſtinct* viſion; confining the former term to thoſe caſes, where the rays of a ſingle pencil are collected to a ſingle point of the retina; and marking, by the latter, the perception we have of viſible objects, when the rays of the pencils, diverging from them, though not collected to ſingle points of

* Eſſay on diſtinct and indiſtinct Viſion.

the retina, yet occupy ſo ſmall portions of it, as to allow the objects to be diſtinctly ſeen. But as few authors have adopted this diviſion, I ſhall, in the preſent article, uſe both terms in the ſenſe, which he has appropriated to the firſt. Neither of them is indeed free from objection, ſince bodies to be diſtinctly or perfectly ſeen, not only require, that their pictures ſhould be accurately formed upon the retina, but that they ſhould fall upon a particular part of it.

Although it has long been a ſubject of inquiry, within what limits of diſtance objects are diſtinctly perceived by ſight, yet the only experiments I have met with in books, which have been made, with any tolerable ſhow of accuracy, to determine this matter, are thoſe of Dr. Porterfield. I ſhall not here ſay what they were, as his Treatiſe is in every body's hands, but ſhall only mention, that the principal concluſions which he drew from them were, *firſt*, that objects could be diſtinctly ſeen by him, that is, the pencils of rays which

which came from them could be accurately collected to points upon the retina, when their diſtances from his eye did not exceed twenty-ſeven inches, and were not leſs than ſeven; and *ſecondly*, that, as often as the axes of both eyes were directed to any one point, ſituated within thoſe diſtances, the rays proceeding from it had their focus in each retina.

As the reſults of ſome experiments, which I have made upon the ſame ſubject, differ from theſe concluſions of Dr. Porterfield, I have read over what he has written upon the matter with more than ordinary attention, and I think I can thence ſhow reaſon, why they ſhould not be received without caution. For, *in the firſt place*, his experiments are related ſo circumſtantially, and with ſuch an appearance of accuracy in the making of them, that you would ſcarcely ſuppoſe he left the leaſt poſſible room for error. And yet, after finiſhing his account of them, he tells us, that he would have repeated them *with more care and exactneſs*,* had he not been

* Treatiſe on the Eye, Vol. I. p. 423.

been interrupted. *Secondly*, his experiments were made upon one eye only, though his concluſions apply to both eyes; an inaccuracy which gives occaſion to ſuſpect others. *Laſtly*, he ſays, that he could not ſee an object diſtinctly at the diſtance of ſeven inches, unleſs both axes were pointed to another object, at only half that diſtance. Had he then directed both axes to an object ſeven inches diſtant, which he does not mention he ever did, it muſt conſequently have been ſeen *indiſtinctly*; and yet one of his concluſions ſtates, that objects, diſtant from about ſeven, to about twenty-ſeven inches, were always *diſtinctly* ſeen, when the axes of both eyes were directed to them. Such are the reaſons which lead me to think, that the whole of the difference, between the reſults of the experiments of Dr. Porterfield and myſelf, is not to be attributed to a difference in the ſtructure of our eyes.

The experiments, which I made upon this ſubject, were with luminous points. They proved to me, *firſt*, that, when both

optic

optic axes are directed to any object, placed at a leſs diſtance from my eyes than about ſeventeen inches, my viſion of it by the left eye is indiſtinct, from the rays of light tending to focuſes behind the retina; *ſecondly*, that my viſion by the ſame eye is perfect, if the object ſeen, and to which both axes are turned, be from about ſeventeen to about nineteen inches diſtant; *thirdly*, that the viſion of my left eye becomes again imperfect, if the object be moved to a greater diſtance than that of nineteen inches, the rays being now collected to focuſes, previouſly to their falling upon the retina; and *fourthly*, that I have, by my right eye, imperfect viſion of all objects, to which I direct both axes, unleſs their diſtances be ſo great, that the rays of each pencil, proceeding from them, may be regarded as parallel.

A concluſion is furniſhed by theſe experiments, ſimilar to one, which was drawn by Mr. Delahire, * from ſome made by himſelf; namely, that each eye ſees objects dif-

* Memoires de Mathematique et de Phyſique, 4to. p. 298.

diſtinctly only at one diſtance; as I take for granted, that, in every caſe of ordinary viſion, both axes are directed to the object which is viewed. But Mr. Delahire drew a ſecond concluſion from his experiments, which he ſeems to have regarded only as another expreſſion of the firſt, but which, in truth, includes a very different fact. It was, that the refractive ſtate of the eye is always the ſame, whether we look at a very near or a very diſtant object. The following obſervations, however, will prove the contrary, at the ſame time that they ſhow, in what I farther differ from Dr. Porterfield.

1. Though an object, to which both axes are pointed, does not appear diſtinct to my left eye, unleſs it be from about ſeventeen to about nineteen inches diſtant; nor to my right eye, unleſs it be at a very conſiderable diſtance; yet I find, that when the axes are made to meet at a point, about two inches diſtant from a line connecting the two pupils, which however cannot be effected without much ſtraining, my left

eye

eye will now ſee an object diſtinctly, which is only about ſeven inches from it, and my right eye will at the ſame time ſee an object diſtinctly, the diſtance of which is about ten inches. I find alſo, that my left eye is made to ſee an object diſtinctly, though placed more than nineteen inches from it, if I direct both axes to a point ſtill more remote.

2. I formerly mentioned, that every degree of the mutual inclination of the optic axes is attended, by a particular ſtate of the refracting power of each eye. But I muſt now remark, that theſe ſtates are ſometimes ſubject to ſlight variations, while the inclinations of the optic axes to each other remain the ſame. For I find, that, when a luminous point, to which both axes are turned, is diſtinctly ſeen by my left eye, I can, by certain efforts not eaſily to be deſcribed, but without changing the poſition of either axis, make it afterward appear as a ſurface, and this too, at one time, from the rays coming to a focus too ſoon, and at another, too late, for perfect

viſion.* One inſtance of theſe variations deſerves to be minutely deſcribed, as it proves, that the refractive power of the eyes is ſubject to greater changes, than what are ſhown by any experiments I have met with in authors. When I look attentively at a bright ſtar, with the optic axes parallel to each other, it appears to my left eye a ſurface of ſome extent, and to my right eye, though not a point, yet a ſurface of very ſmall extent, as ſmall as the ſphericity of the cornea and cryſtaline, the various refrangibility of the different kinds of light, and the width of the pupil at night, can be ſuppoſed to allow; for I find, that, if I now paſs a needle acroſs the axis of the right eye, its ſhadow will not be ſeen. But ſhould I, after

* The variations, however, ſeem produced in ſuch a manner, that the middle of the ſet belonging to one degree of the mutual inclination of the optic axes, is always different from the middle of the ſet belonging to another degree of their inclination; and that, when no other effort is made, than to direct both axes to the ſame object, the eyes always aſſume the middle ſtate of the refractive power, which accompanies that particular inclination of the axes. No argument, therefore, can hence be derived againſt the applications I formerly made of the general fact, reſpecting the connection of the refractive ſtates of the eyes with the mutual inclinations of the optic axes.

after this, withdraw my accurate attention from the ſtar, and view it in the ſtate of ſight we have, when we are ſaid to be in a *reverie*, in which, though our eyes are open, we are yet ſcarcely conſcious of ſeeing ſurrounding objects, the appearance to the right eye expands itſelf, and if a needle be again paſſed before this eye, its ſhadow will be obſerved to move over the ſtar, in a direction contrary to that of the needle itſelf; a ſure indication that the rays of light now tend to a focus behind the retina. In the ſame ſtate of things, the appearance of the ſtar to the left eye contracts, and if a needle be held before the eye, no ſhadow is ſeen; a ſign that the rays are collected to a focus on the retina; whereas they had formerly croſſed one another before they reached that membrane.

Upon the whole then it is manifeſt, from the experiments I have related, that my left eye can collect to focuſes in the retina, rays which proceed from objects at every diſtance whatſoever, not leſs than ſeven inches; that my right eye can collect to fo-

cuſes in the retina, rays which proceed from objects at every diſtance whatſoever, not leſs than ten inches, and even ſuch as are ſomewhat convergent, ſince it can make thoſe, which are parallel, to meet before they fall upon the retina; and laſtly, that, while both the optic axes are directed to a point within the limits of diſtinct viſion, the rays proceeding from it are never accurately collected to focuſes in both retinas, and ſcarcely ever to a focus in either retina. Theſe are likewiſe the principal circumſtances, in which my experiments differ in their reſults from thoſe of Dr. Porterfield.

In making ſuch experiments with luminous points, one or other of two appearances very conſtantly occurs, neither of which, as far as I know, has been ſpoken of by any preceding author. The moſt proper way of mentioning what they are, is, perhaps, to ſhow what ought to happen in thoſe ſituations, in which they are obſerved.

When a beam of white light paſſes, obliquely, from one medium into another of different refractive power, its variouſly coloured rays muſt begin to diverge from each other, at the point of the beam's incidence upon the latter medium. In achromatic teleſcopes, the mutual ſeparation of theſe rays is checked, and its farther increaſe prevented, before it becomes perceptible to ſenſe, by the contrary refractions which they undergo, from paſſing, ſucceſſively, through the different parts of the object-glaſs. Hence, ſome have imagined, that, ſince objects, in ordinary viſion, are ſeen without colour, as far as this depends on the refractions of the eye, nature has furniſhed us with an inſtrument, conſtituted upon principles ſimilar to thoſe of the object-glaſs of an achromatic teleſcope. But every one, the leaſt acquainted with the ſtructure of the eye, muſt know, that this cannot be the caſe, as the refractions in it are all made one way.* And there are experimental proofs

* There are indeed ſome exceptions to this, but not of ſufficient conſequence to affect the preſent argument.

proofs, that compounded light is always ſeparated into its parts, by paſſing through the eye. For if we interpoſe any opake ſubſtance between us and a luminous body, ſo that only a very ſmall portion of this may remain viſible, it will appear to conſiſt of three differently coloured parts, red, yellow, and blue. The reaſon, therefore, of objects being, for the moſt part, ſeen colourleſs, muſt be elſewhere ſought.*

Now let us ſuppoſe, that a luminous point is the only object which is ſeen at any one time; ſhould the focus of its mean refrangible rays be anterior to the retina, the middle of its picture upon that membrane muſt be chiefly compoſed of the leſs refrangible rays; and this muſt be the reaſon, that, when I look attentively at a bright ſtar with my left eye, the centre of it always appears of a light orange colour. As the beams, however, from the luminous point, which enter the eye near to its axis, ſuffer but

* Dr. Maſkelyne has very learnedly treated this ſubject in the Philoſophical Tranſactions, Vol. lxxix. part 2.

but little refraction, the brightneſs of their white light, will, in great meaſure, overpower the colour given to the middle of the picture upon the retina, by the leſs refrangible rays of thoſe, which enter the eye at a diſtance from its axis. Were you then to intercept the former beams, the effect I have mentioned of the latter, muſt be more obſervable : and hence it is, that when I place a pin or needle between my eye and a luminous point, the rays of which come to a focus before they fall upon the retina, the ſhadow, inſtead of appearing black, is always of a red or deep orange colour ; which is one of the phenomena reſpecting luminous points, to which I have alluded.

On the other hand, ſhould the focus of the mean refrangible rays of a luminous point lie behind the retina, the middle of the picture there will be principally formed of the more refrangible rays ; and if the beams, which enter the eye near to its axis, be alſo in the preſent caſe intercepted, the effect of the latter rays, in giving colour

to the middle of the picture, will conſequently be rendered more evident. Hence it is, that, when a luminous point is not ſufficiently remote for diſtinct viſion, the ſeeming ſhadow upon it, occaſioned by any ſmall opake object held before my eye, is always blue; and this is the ſecond of the appearances, which I ſaid are frequently to be obſerved, in experiments upon luminous points.

THE END.