DISSERTATIONS

ON

SUBJECTS OF SCIENCE

CONNECTED WITH

NATURAL THEOLOGY:

BEING THE CONCLUDING VOLUMES OF

THE NEW EDITION

OF

PALEY'S WORK.

BY

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IN TWO VOLUMES.

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TO

THOMAS LORD DENMAN,

CHIEF JUSTICE OF ENGLAND,

THIS WORK IS INSCRIBED,

AS A TOKEN OF RESPECT

FOR HIS GREAT ENDOWMENTS,

AND ESTEEM

FOR HIS PUBLIC VIRTUE.

PREFACE.

The delay in the publication of these volumes has been, in part, owing to the bad health with which I was visited for some months after the former part of the work appeared, but chiefly to the Parliamentary and Judicial duties, which occupied by far the greater portion of the last two years, which in the peculiar circumstances of the country I did not consider myself at liberty to neglect or postpone, and with the performance of which such speculations as have generally occupied my leisure time could by no means be mixed up. This must be accepted by the reader as a reason why the admirable Appendix of Sir C. Bell has been so long in his hands unaccompanied with that which belonged to my department of the work, my Notes to this portion of it having alone been published in the former volumes. The delay may thus be explained; for the great inferiority of the execution, the only thing that can be urged, is that no one can be more sensible of it than the author.

The only further observations which appear to be required upon this occasion, relate to the two principal branches of the present publication, the Treatise on Instinct, and the Analytical View of the Principia; the form adopted in the one, and the appearance of the other in this place.

The form of Dialogue appears to me eminently suited to the thorough sifting of a subject confessedly extremely difficult, and on which there as yet can hardly be said to exist the means of laying down satisfactory, clear, and unquestionable doctrines. The whole arguments on all its parts are thus subjected to scrutiny; all possible objections are brought under consideration; and the ground is cleared for future discovery, even if no results shall for the present be obtained sufficiently free from doubt to rest upon. I do not certainly conceive that in the present case no progress has been made towards such results; but the doctrine is still encumbered with much difficulty; and there exists no work, to my knowledge, in which the subject has been fully investigated. In the writings of ancient philosophers this form of inquiry was very generally adopted. But it must be admitted that in almost every instance the form alone was observed. An excuse was thus given for making the discourse more desultory and less elaborate than a complete and systematic dissertation; but the prolocutors were very far from dividing the argumentation among them. One alone, as Socrates in Plato's Dialogues, performed nearly the whole, and the others were merely assenters. In the following Dialogues, the conflict of argument on either side is real throughout; so that the subject is fully sifted, the argument placed in all the lights in which it was found possible to view it. As for the fictitious nature of such Dialogues, Cicero has long ago observed, when writing to one of his prolocutors, —Puto fore, ut, cum legeris, mirere nos id locutos esse inter nos, quod nunquam locuti sumus. Sed nosti morem Dialogorum.* Nevertheless a good deal of discussion, both by letter and in conversation, had taken place between the persons of the present drama.

Upon the appearance of the Analytical View in its present place some observation will naturally arise, and its disproportion to the rest of the notes and dissertations is not denied. But the observation of Paley had always struck me as marked with his wonted sagacity, that Physical Astronomy, until

* *Ep. ad Fam.*, lib. ix. 8.

thoroughly understood, presents less striking proofs of design to the mind than any other branch of science, yet, when well apprehended, very far exceeds all the other evidences. Now there are two ways of apprehending a truth; the one is understanding merely the proposition which is stated; the other, and the only satisfactory one, is the comprehension of the proof upon which the proposition rests. Nor can any person be really said well to apprehend any science, or even to have a correct notion of its nature, who takes whatever is laid down regarding it merely upon trust. Now the great doctrines of Physical Astronomy are at present believed by the bulk of mankind (that is, by those who know anything of them) only upon the authority of others; and even among well-informed persons it is wonderful how few there are that know upon what proofs those most important truths rest. It seemed worth while to make the experiment how far, with a very slight knowledge of elementary mathematics, the Demonstration could be learnt, and thoroughly understood, by which Sir Isaac Newton has explained the Structure of the Universe. It seemed also very desirable that such as chose to give themselves a little more trouble should be enabled to understand the infinite merits of by far the greatest work ever produced by the human understanding; no longer to rely upon the accounts of others for the grounds of the praise universally lavished upon its singular excellence, but to see with their own eyes that those praises are in nowise exaggerated. The proofs of the leading doctrines may, it is hoped, be fully understood by carefully reading a few pages of this Analysis. To apprehend the whole structure of the work will require a more diligent perusal. But it is hoped that the great value of this knowledge will be considered by the student far to outweigh any such trouble. It is needless to add that this Analysis is not intended for those who wish to pursue mathematical studies fully and deeply. The able and learned commentaries of the Jesuits upon the whole work, and of Mr. Whewell* upon various portions of it, are in their hands; and Mr. Airey's excellent treatise on Gravity,

* The dynamics of Mr. Whewell and the publications by Mr. Wright upon the Cambridge Problems were only known to me as the latter part of this volume was passing through the press; otherwise I should have added some of the excellent Cambridge formulas.

as well as Sir J. Herschell's Astronomy, contain a more popular view of the subject. The present work is addressed to the great bulk of readers, whose vocations do not permit them to learn the science minutely, and who, having no wish to become expert mathematicians, may yet be desirous to examine the Evidence of the Newtonian Discoveries, and appreciate for themselves the grounds of the admiration and reverence in which a whole world holds the name of their immortal author. It must be added that, although in most of the important and fundamental doctrines a full demonstration is given, in several instances the nature of the proof only is explained, and the result stated.

The unexpected length to which the printing extended, especially in this kind of volume, has made it necessary to omit the similar view of the *Mécanique Céleste,* to which reference is made in the course of these volumes. This, with the analysis of the Second Book of the Principia and a portion of the Third, is reserved for a separate publication, as it was found that their insertion here would have extended the work to three volumes. The substance of the chief theorems upon disturbing force is, however, given in the account of the eleventh section of the Principia; and it is unnecessary to state that Mrs. Somerville's admirable treatise affords valuable help to the student, though it is too profound for the class of readers to whom the present work is addressed. A very learned and elaborate commentary on the *Mécanique Céleste* is given by the late Dr. Bowditch, of Boston (U. S.), in his translation, which through the kindness of Professor Whewell I have lately had an opportunity of seeing.

Although the omission of the above-mentioned portions of the Principia in this Analysis has thus been rendered necessary by the want of room, yet the course pursued, of connecting the fundamental doctrines of the Third Book with those of the First, as illustrations and corollaries, and which was adopted for the purpose of showing the student at each step how important are the consequences of the doctrines delivered, has also the advantage of giving this necessary curtailment of the Analytical View a very limited effect upon the completeness of the whole. The subjects of Resistance of Fluids and of the Tides are thus the only ones of which no explanation is given.

One observation remains to be added upon the references to the *Mécanique Céleste*. It must not be supposed that in every instance the matter thus cited is meant to be described as a discovery of Laplace. It is a criticism frequently made, and with some justice, upon this great work, that the author is too sparing in acknowledgments of the obligations which he owed to his predecessors; but the brilliant and truly original results of the illustrious Lagrange's researches, as well as of Euler's, are embodied in the *Mécanique,* and its pages are often referred to in the present work for these as well as for the author's own discoveries. To justify whatever has been at any time said of Laplace's genius and its effects upon the progress of the science, it is enough that we can point at the Third book, with the relative portions of the Second—at the theory of Jupiter and Saturn, and their satellites—and at the explanation of the moon's acceleration—and then add with truth, that even these great discoveries are far from exhausting Laplace's claims to be ranked with the most inventive and profound mathematicians that have appeared since the age of Sir Isaac Newton.*

* The Analysis of the Principia in a less abridged form, and comprising the parts now omitted, will be published in a separate work, and that of the *Mécanique Céleste* in another.

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OF INSTINCT.

BOOK, OR DIALOGUE I.

INSTINCT—Introduction; (Facts).

When the General Election of 1837 was near its close, and every day brought the accounts of those mighty boasts of our expected successes under the new reign, so idly made, being overthrown by the activity and resources of our adversaries and the listlessness of the people on our behalf, Lord A. came to me on his way to the North, where he was minded to diversify with field-sports his habitual life of farming. Those pursuits had never interfered with the duty he owed his country as long as he deemed that the sacrifice of all his domestic comforts could prove serviceable to his public principles; nor had they ever at any time prevented him from cultivating a sound philosophy, in the study of which much of his leisure is always consumed. When I passed a few days with him at Wiseton, the summer before, we had discussed together some of the more interesting topics which form the subject of these speculations, connected with Natural Theology, though of a substantive interest independent of the relation in which they stand to that sublime inquiry; and, while I remained at Harrington, we had corresponded constantly on the subject of Instinct, one of the most curious in its minute details and of the most interesting in its bearings upon the philosophy of mind, independent of its immediate connexion with theological speculations, but, it must at the same time be admitted, one of the most difficult, and upon which the labours of philosophers have cast a very imperfect light. It was natural then that we should renew these discussions when we afterwards met in Westmoreland. The weather being fine, we ranged somewhat among the lake scenery, and by the rivers and through the woods which variegate our northern country. There was not much to tempt us in the aspect of public affairs, which, if not gloomy for the country at large, was yet not very flattering for the liberal party, among whom the single object seemed now to be the retention of office, and who might say with the Roman patriot in the decline of liberty,—"Nostris enim vitiis, non casû aliquo, rempublicam verbo retinemus, reapse vero jampridem amisimus."* Nor, indeed, on these matters was there a perfect agreement between us two; for while we augured as little favourably the one as the other of our prospects, we ascribed to different causes the condition of affairs which gave rise to these forebodings: he, tracing it to the great natural weight and influence of the Tories throughout the country, both in church and state; I, relying more on the energies of an improved and active people, provided the government had acted so as to merit their support; but lamenting that no pains had been taken by them to shew any superiority of popular principles, or make the country feel itself better off under their rule than they would have been under the adverse faction, while I perceived sufficiently plain indications that the accession of the Court-favour in this new reign would have the effect of lessening rather than promoting any popular tendencies which might still exist. Altogether, therefore, the state of the commonwealth was a subject less suited to engage our conversation;

* Cic. Frag. de Rep., lib. v.

and we naturally dwelt little upon passing and unpleasing topics, as unsatisfactory, transitory, and fleeting—" ista quæ nec percunctari nec audire sine molestiâ possumus."* But upon those matters of permanent interest and universal importance, and which the follies or faults of men could not despoil of their dignity or deprive of their relish, we loved to expatiate: and coming to the island in the neighbouring river, found a convenient seat where the discussion might be carried on under the cool shade which the wood afforded against an autumnal sun: " Here," said I, " we may resume our Wiseton conversation."—" Ventum est in insulam. Hâc vero nihil est amænius; et enim hoc quasi rostro finditur Fibrenus et divisus equaliter in duas partes, latera hæc alluit, rapideque dilapsûs cito in unum confluit, et tantum complectitur quod satis sit modicæ palæstræ loci; quo effecto tanquam id habuerit operis ac muneris ut hanc nobis efficeret sedem ad disputandum, statim præcipitat in Lirem."†—" Here," said I, " we may resume our Wiseton conversation;" " si videtur considamus hic in umbrâ atque ad eam partem sermonis ex quâ egressi sumus revertamur.‡"

* Cic. Acad. Quæst., lib. ii.

† Cic. de Legg., lib. ii.

‡ Ibid.

A. Have you reconsidered my opinion, or rather the inclination of opinion, which I had last year, that it will be advisable, if not necessary, to begin with defining Instinct, in order that we may the more clearly understand what we are discussing?

B. I have indeed; and I remain of my own, as often happens through obstinacy and unwillingness to give up a preconceived notion; but here it is, I believe, from much reflection upon the subject, that I still regard the definition as rather the end of our inquiry than its commencement. Indeed, this may generally be observed of metaphysical, or rather psychological inquiries: they are not like those of the mathematician, who must begin by defining; but that is because his definition is, in fact, a statement of part of the hypothesis in each proposition. Thus, whoever enunciates any proposition respecting a property of the circle predicates that property of a figure whose radii are all equal; and it is as if he began by saying, "Let there be a curve line, such that all the straight lines drawn from its points to another point within it are equal, then I say that the rectangles are equal, which, &c." The general definition only saves the trouble of repeating this assumption, as part of the hypothesis in each proposition. But the nature of instinct, or of any other thing of which we discourse in psychology, is not the hypothesis we start from; it is the goal or conclusion we are seeking to arrive at. Indeed, so it is in physical science also; we do not begin, but end, by defining the qualities of bodies, or their action on one another.

A. I grant this. But if there be more things than one which men call by the same name, for example, of Instinct, must we not begin by ascertaining what we mean by the word, in order to avoid confusion? And this seems to bring on the necessity at least of some definition.

B. I agree that there must in this case be a definition; but it is only a definition of terms, and does not imply our stating the nature of the thing defined: it only implies that we must understand what the thing is to which the given word applies, and, if two things go under the same name, that we should be agreed in the outset which of the two things we mean when we use the word; perhaps, that we invest some second name, or give some qualifying addition to the given one, to express one of the two things, and keep the different meanings distinct.

A. The best way will be that we should come to particulars—give an example or two: perhaps it may suffice to mention the different kinds of Instinct, if, which I take for granted you do not doubt, there be more things than one going under that name.

B. Certainly; and there can here be no difficulty at all in our way; and, to shew you how little alarmed I am at defining, when it is clear that I am only called upon to define a word, and thereby make a distinct reference to a thing known or unknown in its own nature—not to pretend giving an account of that nature—I will at once begin by both inventing names and defining their meaning. There are some Instincts which may be called *physical,* and others *mental,* in the animal system; by physical I mean those actions or motions or states of body which are involuntary; as the action of the heart, and the peristaltic motion of the bowels, over which, generally speaking, we have no direct control by the operation of the will—for I put out of view such rare instances, almost monstrous, as Darwin has recorded of a person who could suspend the pulsations of his heart at pleasure, and another, still more rare, of one who could, at will, move his bowels by accelerating the peristaltic action.* Even if all men could acquire such control over

* Zoonomia.

those motions, they would still be involuntary; because they could still be carried on wholly without our will interfering, and without our minds necessarily having any knowledge whatever of them. So the secretions are all performed involuntarily, and may go on wholly without our knowledge; we can affect them as we can the involuntary motions of the heart and fluids, indirectly, because the passions and feelings of the mind have always an effect upon them; but still they exist and proceed, the parts perform their functions, and those functions serve the ends of their appointment, wholly independent of our will, or of any effort whatever on our part. We can affect them also immediately through the influence of physical agents, voluntarily applied as stimulants or sedatives, or the operation of voluntary motion, as well as mediately by the power which the mind derives from its union with the body; but they can go on of themselves, and, in all cases of healthy condition, go on better without any the least interruption on our part than with it.

A. This is certain: my only doubt is whether these can be justly or correctly termed instinctive operations at all. When I speak of Instinct, I mean something very different; namely, those voluntary movements, or that voluntary action of the mental faculties which is contradistinguished from reason. However, there is no harm, but much convenience, in beginning by defining and classifying, so as to leave on one side the physical and involuntary instincts—those things which may properly enough be called incidents of animal life, because there seems great difficulty in drawing a line between such motions and actions and those which subsist in vegetables.

B. There does certainly appear to be this difficulty. I hardly see how any line can be drawn between the motions of the lowest species of animal, the mollusca for instance, and those found in plants. There is in both organized form, a system of vessels, growth by extension not by apposition, a circulation of fluids and secretion of solids from those fluids, or of one fluid from another. There is also production of seed, and from the seed continuation of the species. But it is not only convenient that we should define in order to leave on one side what we are not to discuss, that it may not confound our inquiry; the definition and classification may also carry us on, some little way, in our argument with respect to the other class of Instincts, Instinct properly so called, the Mental Instincts; at least, it seems to furnish us at the very outset with an analogy.

A. I have a dread, at least a suspicion, of all analogies, and never more than when on the slippery heights of an obscure subject; when we are as it were *inter apices* of a metaphysical argument, and feeling, perhaps groping, our way in the dark or among the clouds. I then regard analogy as a dangerous light, a treacherous *ignis fatuus.*

B. It is even so, if we follow it beyond where we can see quite clear and find a firm footing. But all light is good, and the best way is not to despair, still less put out any glimmering we have, but rather to increase it by adding others, or make it available by using apt instruments. However, we are getting too metaphorical: only it is my comfort that you began, and that I am led astray by one who (as you said in your inimitable letter to your Lancashire antagonist) are not one of "the eloquent people." But to return from where your poetical imagery led us—analogy may sometimes illustrate, and it may often lead to useful and strict inquiry, by suggesting matters for comparison and investigation.

A. Then what comparison do you make between the two kinds of Instinct? or rather, as the question is of analogy, how do you state a relation of the mental Instinct, which we shall call Instinct simply if you please, similar to or identical with some relation of physical Instinct?

B. As thus—the physical Instincts are independent of will, or mind altogether, though they never are found except where animal life and consequently mind exists; but yet mind may influence them. Just so the mental Instincts are independent of reason altogether, though they are found in union with it and reason may influence them. It is a question if they are ever found without reason; for that depends on our solution of the *vexata quæstio*, "Whether the lower animals have reason at all or no?" Therefore, I will not say that here the analogy is complete, and will not affirm that, as physical Instinct is never found without animal life, so mental Instinct is never found without reason; but we may safely say that in this other respect the analogy is perfect, namely, that where mental Instinct is found with reason it can act without reason, though reason may also interfere with it; and in this respect, at least, reason seems to bear the same relation to mental

Instinct which animal life bears to physical Instinct. We may go further, and add, that as in plants, where the motions are without animal life, those motions are more perfect and more undisturbed, so if there be any animal wholly without reason, the operations of mental Instinct are the more regular and perfect; and, in any animal whatever, they are so in proportion as reason is dormant or inactive.

A. It may be as you say; but this will not carry us, as you seem to be aware, far on our road. However, it is well enough to remark it; for we thus gain perhaps a clearer and more steady view of the relation between Reason and Instinct, always supposing that there is any warrant for treating the two as different: because you are aware that some have considered them as identical; I mean not merely by denying that there is any specific difference, any difference in kind, between our faculties and those of brutes—though this denial is of course involved in their doctrine—but by going a step further, and holding that what we call our Reason, and are so proud of, is merely a bundle of Instincts, as some have termed it—a more acute and perfect degree of Instinct. Smellie, in his entertaining work on the Philosophy of Natural History, holds this opinion.—That is a book, by the way, much less esteemed than it deserves, even as a collection of facts and anecdotes; but I also think the honest printer (for such he was) had a good deal of the philosopher in him. I suppose, as the well-educated printers in the foreign university towns, and some of our own Oxford men, used to be critics and scholars, from the atmosphere of the place, so your Edinburgh printer, when well bred, is a metaphysician.

B. You are right as to Smellie at least, and I agree with you as to his book, though it is too long, and in parts loosely reasoned, as well as not over-accurate in his facts, according to what I have heard from naturalists. But he was a man of considerable merit; and lived a good deal in the literary and scientific circles of Edinburgh. I knew him, but slightly. He would have done much more had his habits been less convivial. But I rather fancy the somewhat pretending title of his book tended to make men disallow the merit which it unquestionably has.

A. But what do you hold of the dogma in question, and of which he is perhaps the most round asserter.

B. I entirely deny it; nor do I conceive that any part of the subject is more free from all doubt than this, unless indeed we come to the question of liberty and necessity, and resolve the whole into a mere dispute about terms.

A. Liberty and necessity! preserve us!—I am taken by surprise. Why I had no idea that we could ever have got among those heights and clouds already—"Apart set on a hill retired," and reasoning on "free-will," like the gentry more acute than amiable, who held their metaphysical disputations there.

B. Don't be alarmed—but the subjects in one single point do certainly touch. What I mean is this: if you say that, when a man reasons, one idea suggests another, and that he must follow the train, and can no more avoid drawing his conclusion, when he compares two ideas, than a bird can avoid building its nest in a particular fashion, or a bee can help making hexagonal cells, then you seem doubtless to liken Reason with Instinct. But this is true only on the supposition that a man's mind is mechanical, and that his faculties are placed beyond his control. Now, suppose it to be admitted that I cannot avoid drawing a certain conclusion from premises in mathematical matters—as that the three angles of a figure are equal to two right angles, if that figure have those three angles only—I am under no such necessity in any question of moral or probable evidence; and on a question like that different minds will differ, or the same mind at different times. Again, I am under no necessity—even if I admit that I have no choice on moral evidence—I am under *no necessity* of exercising my volition in one given way, unless indeed you deny that I have ever any free-will at all. If so, and if you contend that, the same motives being presented to my volition in the same circumstances, I must needs choose the same course, you may also contend that, the same circumstances being presented to my judgment in the same frame of the feelings, I must need draw the same conclusion; and this may seem to make out an identity of Reason with Instinct: but this is the dispute of liberty and necessity which every man's consciousness and hourly experience decides in favour of liberty, except in so far as it is a mere dispute about terms. But I really do think that, allowing the question to be disposed of either way, there is a specific difference between Reason and Instinct: for, even upon the principle of necessity, suppose the man and the bee to be equally under the entire control of the premises in reasoning, and the circumstances or motives in willing, whatever it is that each does, be it the necessary consequence of the circumstances or not, is different in the two cases. Suppose that if the bee reasoned she would be under the necessity of drawing the same conclusion, and that if she exercised an election, she could not avoid choosing one course, and that it is the same with the man—it still is not only not proved that the bee does reason or choose, while we know that the man does, but the contrary seems proved.

A. How so? Were I to maintain the contrary I should deny that we have any such proof. How do you prove the negative proposition, that the bee does not reason and will?

B. Observe, I do not say we have the proof of the negative as clearly as we have of the affirmative. But, beginning with laying aside those actions of animals which are either ambiguous or are referable properly to reason, and which, almost all philosophers allow, shew a glimmering of reason; and confining ourselves to what are purely instinctive, as the bee forming a hexagon without knowing what it is, or why she forms it; my prcof of this, not being reason, but something else, and something not only differing from reason in degree but in kind, is from a comparison of the facts—an examination of the phenomena in each case—in a word, from induction. I perceive a certain thing done by this insect, without any instruction, which we could not do without much instruction. I see her working most accurately without any experience, in that which we could only be able to do by the expertness gathered from much experience. I see her doing certain things which are manifestly to produce an effect she can know nothing about, for example, making a cell and furnishing it with carpets and with liquid, fit to hold and to cherish safely a tender grub, she never having seen any grub, and knowing nothing of course about grubs, or that any grub is ever to come, or that any such use, perhaps any use at all, is ever to be made of the work she is about. Indeed, I see another insect, the solitary wasp, bring a given number of small grubs and deposit them in a hole which she has made, over her egg, just grubs enough to maintain the worm that egg will produce when hatched—and yet this wasp never saw an egg produce a worm—nor ever saw a worm—nay, is to be dead long before the worm can be in existence—and moreover she never has in any way tasted or used these grubs, or used the hole she made, except for the prospective benefit of the unknown worm she is never to see. In all these cases, then, the animal works positively without knowledge, and in the dark. She also works without designing anything, and yet she works to a certain defined and important purpose. Lastly, she works to a perfection in her way, and yet she works without any teaching or experience. Now, in all this she differs entirely from man, who only works well, perhaps at all, after being taught—who works with knowledge of what he is about—and who works, intending and meaning, and, in a word, designing to do what he accomplishes. To all which may be added, though it is rather perhaps the consequence of this difference than a separate and substantive head of diversity, the animal works always uniformly and alike, and all his kind work alike—whereas no two men work alike, nor any man always, nay any two times, alike. Of all this I cannot indeed be quite certain as I am of what passes within my own mind, because it is barely possible that the insect may have some plan or notion in her head implanted as the intelligent faculties are: all I know is the extreme improbability of it being so; and that I see facts, as her necessary ignorance of the existence and nature of her worm, and her working without experience, and I know that if I did the same things I should be acting without having learnt mathematics, and should be planning in ignorance of unborn issue; and I therefore draw my inference accordingly as to her proceedings.

A. Come, come, Master B., I begin to surround you and drive you from your original position, maintained both now and last summer, about the impossibility of defining. Have you not as nearly as possible been furnishing a definition? At least, are not the materials of definition brought together which you deprecated, and would have us reserve to the last?

B. Patience, good man—patience! What is this to what you have gone through? Fancy yourself once more in the House of Commons, on the Treasury bench, listening to ——— ———

A. God forbid!

B. Or suppose yourself again in Downing Street, with Drummond announcing a succession of seven deputations or of seventeen suitors.

A. The bare possibility of it drives me wild. Why, to convert you to the most absurd doctrine I could fancy—to make you swallow all the Zoonomia whole, and believe that men derive their love of waving lines and admiration of finely-moulded forms from the habit of the infant in handling his mother's bosom, or even to drive you into a belief that the world was made by chance—would be an easy task compared to the persuading any one suitor at any one of the offices that you had any difficulty in giving him all he asks, or convincing any one of those seven deputations that there exists in the world another body but itself.

B. Or to convince any one man, who ever asked any one job to be done for him, that he had any one motive in his mind but the public good, to which he was sacrificing his private interest. I remember M. once drolly observing, when I said no man could tell how base men are till he came into office, "On the contrary, I never before had such an opinion of human virtue; for I now find that no man ever drops the least hint of any motive but disinterestedness and self-denial—and all idea of gain, or advantage, is the only thing that none seem ever to dream of." But now compose yourself to patience and discussion—take an extra pinch of snuff—walk about for five minutes, a distance of five yards and back, with your hands in your breeches pockets, and then return to the question with the same calmness with which you would have listened to a man abusing you by the hour in Parliament, or with which you looked an hour ago, in the Castle farm, at the beast you had bred, and which by your complacent aspect I saw you had sold pretty well.

A. But, indeed, I sometimes can't help fancying that it may be as well to take our observations upon Instinct from the operations and habits of such large animals as him you speak of—at least, not from insects; because it is possible that if we could see as accurately all the detail of the latter as we do of the former, much of the marvellous might disappear, and we might be as well able to account for their proceedings, which now seem to us so unintelligible, as we are to account for those of the greater animals, which are clumsy and cumbrous enough, and rather appear to proceed from an obscure glimmering of reason than from an inexplicable power guiding them unconsciously to work with the perfection which we ascribe to the bee. In a word, might not the cells be found to have as many imperfections, as great deviations from the true form, as any of the ox's operations have from perfect exactness, if either the bee were as large as the ox, or our senses as acute as the bee's? Has she not as great aberrations from the exact pattern in proportion to her own size and to the instruments, her feet and feelers, which she works with? I throw this out as a matter very fit to be settled in the outset, in order that our own reasoning may not proceed upon gratuitous assumption.

B. For the sake of ascertaining how far the working is as perfect as it appears, I admit the importance of your observation; but for nothing more. I deny that it affects the body of the argument at all; because that depends in no degree upon the perfection of the work. Thus the proceedings of the solitary wasp are just as good for my purpose as those of the bee. Nay, the instinctive operations of the greater animals furnish exactly the same materials for reasoning, though they may not be so striking. However, to the point of your comparison—you must keep in mind that we have applied the powers of the microscope to the operations of the bee. Now, without going to an instrument of the power of Torre's, which magnified the linear dimensions between 2000 and 3000 times, and consequently the surface above 6,000,000 of times, take the much more ordinary power of 400, which magnifies the surface 160,000-fold—nay, if you take a microscope of only a 90-times magnifying power, you will see the work of the bee in a straight line, exactly as you do that of a man with the naked eye. But, I need hardly add that, if you only saw it a quarter as well, or with a glass that magnified 20 times, it would be enough; for then you would examine it as you do the beaver's with your naked eye. But, further, all the difficulty you suggest proceeds upon a fallacy. The lines may not be exactly even which the bee forms; the surfaces may have inequalities to the bee's eye though to our sight they seem plane; and the angles, instead of being pointed, may be blunt or roundish: but the proportions are the same; the equality of the sides is maintained, and the angles are of the same size; that is, the inclination of the planes is just—in other words, all the inequalities do'nt affect the proportions of the parts; for they are common to each thing compared with another; the axis running through the inequalities (to speak more rigorously) is in the true direction, and the junction of the two axes forms the angle of 60° as accurately as if there were no inequalities. Now, then, the bee places a plane in such a position, whatever be the roughness of its surface, that its inclination to another plane is the true one required.

A. I suppose it is so; but, at any rate, the solitary wasp carrying the grubs in proper number and placing them in the hole over the egg, or the bee placing her egg in the liquor at the bottom of the cell, and making that cell of the length to which the worm when hatched will grow—she having never seen either the worm or the chrysalis—is sufficient for our purpose.

B. Not to mention the operations of the worm itself in spinning the cocoon, and making it precisely the size required to line or carpet the cell when expanded and applied to it—nay, the motions of the chick in the egg, which always begins at the same place, and moves itself on in the same direction, chipping away till it effects its own liberation—all of which must be prior to experience, and without the possibility of teaching.

A. You desired me last summer to examine, with a view to the same point, the ducklings hatched under a hen, and then taking the water, without the possibility of her teaching. They have the form, web-feet, &c., which enables them to swim, and which a chicken has not. Their manner of getting into the water I cannot say I well ascertained; but it is certain enough that the hen's proper brood would not have got in, and probably she would have succeeded in preventing them, though she might not be able to keep the ducklings out.

B. However, a more decisive case occurred to me afterwards: that of chickens hatched in the Egyptian ovens. I have lately seen an intelligent Bey and his Aide-de-camp, who gave me the whole process; and, as was to be expected, there is not the slightest difference between the conduct and motions, and habits generally, of these chickens, and of such as are hatched and brought up by hens. This fact, as well as the working of the chrysalis in spinning the cocoon, and of the chick in chipping with its bill-scale, renders it quite unnecessary to inquire whether or not the honey-bee or social wasp work by instruction from other bees or wasps. That, however, appears to be impossible, when we consider that as many as 30,000 young insects come from one nest, to teach whom there are not old ones anything like enough; and to teach whom in a few hours, or even days, to work as exactly as themselves seems wholly impossible. The observation of cases where such teaching is impossible, as in the chrysalis and unhatched chicken, at once removes all doubt, and precludes the possibility of supposing that the wasp's and the bee's architecture can be traditional, or handed down by teaching, from the first insects of the species that were created. Henceforward, therefore, we must assume as part of the fact that the cells of the bee are made without any instruction or any experience, and are as perfect at first as they ever are; which, by the way, explains another peculiarity of instinct—that it never improves in the progress of time. The bee, 6000 years ago, made its cells as accurately, and the wasp its paper as perfectly, as they now do.

A. Let us advert to one thing more, and, having settled it, the way may at least be said to be cleared for the argument, perhaps somewhat of progress even to be made in the inquiry. You have been speaking of Instincts in the plural; of course you do not mean to be taken literally, as admitting more kinds of mental Instinct than one.

B. Certainly not; any more than when speaking of the mental faculties I admit of more minds than one, or more parts than one of a single mind. This last form of speech has been so used, or rather abused, especially by the philosophers of the Scottish school, accurate and strict as they for the most part are, that they seem to treat the mind as divided into compartments, and to represent its faculties as so many members, like the parts of the body. But it is one thing or being perceiving, comparing, recollecting—not a being of parts, whereof perception is one, reasoning another, and recollection a third; so Instinct is one and indivisible, whatever we may hold it to be in its nature, or from whatever origin we may derive it. This thing, or being, is variously applied, and operates variously. There are not different Instincts, as of building, of collecting food for future worms, of emigrating to better climates—but one Instinct, which is variously employed or directed. I agree with you, however, that we have now done something more than merely clearing away the ground. We have taken a first step, or, if you will, laid a foundation. We have ascertained the peculiar or distinctive quality of Instinct, and that which distinguishes it from Reason. It acts without teaching, either from others, that is, instruction, or from the animal itself, that is, experience. This is generally given as the definition or description of Instinct. But we have added another peculiarity, which seems also a necessary part of the description—it acts without knowledge of consequences—it acts blindly, and accomplishes a purpose of which the animal is ignorant.

A. I pause here and doubt of this addition. I perfectly admit the fact that it produces an effect, manifestly the object of its operation, and yet without knowing it, consequently without intending it or designing it. But there seems reason to think that it always intends to produce some one effect, and does produce it—that it has some one purpose, and accomplishes it, and so designs something which it does. Thus animals are impelled by hunger to eat; their eating produces chyle, blood, and all that is secreted from the blood; yet they had no design to promote their own growth and preserve their own life. At least they ate long before they had any such design or any knowledge that such would be the consequence of gratifying hunger. So of continuing their species. May not the solitary wasp, for instance, have its organs and its senses so constructed as to receive an immediate gratification from collecting and burying grubs? If so, her knowledge extended to one, the first, event, and she had the design in view of producing this event; though wholly ignorant of any subsequent event. The desire of the first event, the fact of that event being a gratification to the insect, was the means taken by the creator of the insect for making her do that which was to produce the important consequence, forming the real object in view, though concealed from the animal. Thus we may conceive that the insect is endowed with an appetite for carrying grubs, and that this is so adjusted in point of intensity as to be satiated when just so many grubs are transported as will feed the next season's worm, which is endowed with the desire to eat these grubs, rejected as food by the parent insect. So the wasp's senses may make the flavour, or the smell (for that seems all she enjoys), of a living caterpillar more grateful than of a dead one; and hence she takes those that will keep sweet till her own grub is hatched.

B. I do not deny the possibility of all this; although there seems something gratuitous in it, and we possibly never can know the truth by any observations or experiments. I shall presently show why I do not think it would entitle us to erase this ignorance of what you would call the second event, or the object of the secondary design, from our list of the characteristics of instinct. But in the meantime I will mention what occurs to me on your objection in point of fact. The instant that a solitary wasp is hatched, or a bee can fly, away they go to the spot where the caterpillars or the wax-yielding substances are to be found. What guides them through the air to things they cannot descry or do not know the use of?

A. It costs me no more to suppose that there is some smell or other sensation to guide them—some odour, for example, which penetrates the air, and being grateful to them makes them desire to approach the odoriferous body. Thus the bee smells the nectary of flowers; she flies to them, she sips, and the wax is secreted in her stomach. I grant you that I have more difficulty with her operation in using it.

B. You clearly have; for what should be the special gratification of that? We are admitting that she has no kind of knowledge that the cell is to be used in hatching and rearing the brood, any more than that an hexagonal figure, with a certain inclination of its rhomboidal bottom, is to enable her and her associates to employ the space and the wax in the way of all others most economical of room and work and materials; and so as just to accommodate the size of the unknown and unseen worm, chrysalis, or young bee, and no more—and also to suit its form.

A. I think I could suppose also in this case that her desire of action—her love of motion—is gratified by the operation, and is satiated by continuing that motion to a certain extent, where she stops.

B. But allowing your right to make all these suppositions equally gratuitous, one after another, and to extend them as the argument proceeds, and to relieve the pressure as the fact pinches—see what it is that you must assume. The comb is constructed thus. Wax-making bees bring a small mass of this material and place it vertically to the plane from which the comb is to hang down. Then other bees begin to excavate, one on one side, another on the other, and they work with such perfect nicety, as never to penetrate through the thin layer of wax; also so equally that the plate is of equal thickness all throughout, its surfaces being parallel. You must, therefore, suppose some repugnance at once to a plate ever so little thicker, and to one ever so little thinner than the plate's given thickness. Indeed, this supposition, which some naturalists have made, is wholly unsatisfactory, and shows no accurate regard to the facts any more than their notion (a most crude one), that the hexagon cells arise from so many cylinders pressing on each other. The supposed instinct not to perforate wax, but to draw back when they come to a given thickness, is inconsistent with the fact; for the original plate they work on is uneven and of different thicknesses on both sides, and there is no bee in the world that ever made cylindrical cells. Huber has distinctly shewn, from having observed them at their work, that they make them in quite another way; nor indeed, if they did, could any pressure ever produce hexagons, and far less rhomboidal plates. The wax-worker's bringing plates of a given thickness is also wholly incapable of accounting for the angles, that is, the inclination of the plates—for supposing the bee to make a groove (as she does), and suppose she has some means of bisecting its arc by two chords, this only, with the thickness of the cake, would determine the depth of the rhomboid, and that can be easily shewn not to be the rhomboid actually made. She therefore makes angles wholly independent of the thickness, not to mention that were we to admit that the cake's thickness governs the whole, we do not solve the problem; the difficulty is only removed a step; for then how is that exact thickness obtained? But this will not do even to that extent; a great deal more is done by the bee, and a great deal more must be supposed to make it conceivable that she has any immediate or primary intention. She works so that the rhomboidal plate may have one particular diameter, and no other, and always the same length, and that its four angles may be always the same, the opposite ones equal to each other, but each two of different quantity from the other two; and then she inclines the plates at given angles to one another. Why is there such a gratification to the bee in a straight line—in a straight line at right angles to a plane—in rhomboids—in rhomboids with certain angles—any more than in lines or planes inclining at other angles to one another? Why is the bee, after working for half a quarter of a line in one direction, to go on, and not take delight in a change of direction? If she goes on, why is she to be pleased with stopping at one particular point? Nay, why is each bee to take delight in its own little part of the combined operation? Why is each to derive pleasure from doing exactly as much as is wanted, and in the direction wanted, in order that when added to what others have before done, and increased by what others are afterwards to do, a given effect, wholly unknown to her and to all the rest, her coadjutors, may be produced?

A. It certainly is difficult to say. I can barely imagine the different bees so formed that some inexplicable gratification may be the consequence of moving in one line, and making one angle, and that any other line or angle whatever may be disagreeable to them. The concert in the operation of animals seems to increase this difficulty much, always supposing there is real concert without any arrangement, communication, or knowledge. No man ever acted so as to make his operations chime in with another's, unless he either had previous concert with that other, or both acted under a common superior, and obeyed his direction; and then the joint operation was that of this superior. But suppose a man were compelled by some feeling he could not account for, and did not at all understand, to go at a given time, to a certain place, and with such speed as to arrive there at a given moment, and were to find another just arrived there, who came to meet him without the former previously knowing of this, —we should have a case similar to that of animals acting in concert, supposing them to do so. There is, however, some doubt of this as to the bees; for Huber has said that they all act in succession rather than co-operate contemporaneously.

B. I really can see no difference that this makes in the argument as to concert. One bee brings wax and does not sculpture; another sculptures and does not bring wax: but the wax-worker brings just as much as the sculpturing bee wants, and at the very time she wants it; also, one works on the face, and another on the back of the same rhomboidal plate; and all so work as never to interfere with, or jostle one another, which is the perfection of concert, and can only among men be effected by discipline, which refers the whole of the different purposes to one superintendent, and makes his unity of design the guiding rule and impulse, because concert among the different agents is otherwise unattainable. But I own, I can see no greater difficulty thrown in our way, by concert, than by blind agency—supposing it blind as to both the events, and not merely blind as to the secondary consequence—and your supposition of a first event known and designed, the secondary being hidden from the animal, would, I think, account for a case of concert, as much as for any other operation; for your hypothesis of sensations and impulses, would apply to concert. You might say that each bee was induced by the gratification of doing a certain thing, to take a certain line at such a time; that what it did, should answer to what some other bee was by the like means induced to do at the same time. I see no difference in the two applications of this hypothesis.

A. I rather think the time makes some difference; at least in rendering an addition to the hypothesis necessary. For though the gratification of bringing the caterpillars to its nest will account for the solitary wasp doing what is also to serve the purpose of feeding its young next season, something more is required than this motive to make one bee act in concert with another; it is necessary that there should be a gratification, not only in doing the thing required, but in doing it at the very moment required; so that both bees must be supposed to feel at the very same instant of time the desire of the gratification in question, and yet without any concert or communication. I hardly see how my supposition of sensations and pleasures or pains will explain this.

B. I all along have seen the greatest difficulty in your explanation; but does this consideration of time increase it materially?—or rather, is it not in all cases part of the riddle which instinctive operations present to us? Thus the solitary wasp acts, that is, according to your hypothesis, feels the given sensation, or derives the supposed gratification at such precise time that her acting upon it will suit the time required for the birth and growth of the worm. The bird breeds, but before laying her eggs, and without any knowledge when she is to lay them, makes her nest, and it is ready at the very time required. Therefore she feels the desire of nest-making at the proper moment. I will admit, however, that there is something still more extraordinary in two separate and independent insects feeling the same impulse at the same moment; and the difficulty is incalculably augmented, if twenty or thirty insects all have the impulse separately, but all at once, so as to act together. Indeed, I cannot help regarding your solution as not only a gratuitous hypothesis, for that it must needs be from the nature of the thing, but one hardly conceivable, and in truth as difficult to suppose possible as any other thing which we can fancy in order to explain the phenomenon—for instance, some invisible power or influence acting upon the animal, or upon the different animals at once. This is not at all more gratuitous, and it more easily explains the phenomenon.

A. Consider if there is really any such essential difference between the case of instinct which we have been considering, and any of the best known operations of men, as well as animals, where we are not wont to speak of instinct at all. Thus men eat from hunger, which they intend to satisfy; but the consequential effect, not intended, is chylification, sanguification, secretion, and growth or sustentation of the body, as well as the effect intended, and immediately produced, of satisfying hunger. The mother eats things which satisfy her appetite, and that is all she cares for; but those things also produce milk, which nourishes her infant, and that she never thought of. The time is also suited by the feeling. The hunger gives the supply when the system wants it; the eating produces the milk when the infant requires it. How does this differ from the other case?

B. Much every way. The difference is wide and marked. In the cases you put, the mental instinct is confined to produce the effect intended; and having produced it, the mind stops there and does nothing more. The powers of matter, its physical qualities, set in motion, do the rest, of course beyond our direct control, and unaided by us as unknown to us. But in the case of Instinct the mind performs both parts—both the things which it knows and intends, and the thing which it neither knows nor intends. The mother eats—nature produces the milk without the least action of hers. But the bee not only gratifies herself (if that is the cause of her architecture) by the structure of the cell, but by her art, by her work, she does the other thing also, that of providing a lodging for her young. It is as if the mother in your supposed case were both to eat intentionally for satisfying her hunger, and at the same time, without knowing or intending it, were to make milk by some process of internal churning. It is as if in eating we at once chewed and swallowed and also with our tongue or teeth or fingers made chyme, and then chyle, and then blood. It is as if the animal in pairing both gratified his sexual passion and voluntarily made the young by some process of manipulation, though without knowing what he was about, or intending to do it.

A. You must here distinguish a little, or rather you must take into your account a point of resemblance which you are passing over. How can any one even acting with design affect matter in fashioning it or moulding it, except by availing himself of the powers, mechanical and chemical, belonging to matter? If I distil, it is by availing myself of the process of fermentation and of evaporation, and of condensation. If I sow and reap, it is by availing myself of the prolific powers of heat and moisture in the process of vegetation. So even in processes where I seem to do more and nature to do less; if I build, or carve, or weave, it is by availing myself of the qualities of cohesion and gravitation, and of the powers of the wedge in hewing, or of friction in polishing. Do not the animals who eat, the mothers who give suck after eating and thereby secreting milk, in like manner do part themselves, and as to the rest avail themselves of the powers of nature in chylification, sanguification, and secretion? You perceive how much more nearly akin the cases are than you have stated.

B. I am well aware of it; indeed, we are now coming nearly into the controversy about productive labour, which you and I have often amused ourselves with as political economists; when I have always held that it was a far less easy thing than those who discussed the metaphysical parts of that science supposed, to draw the line between productive and unproductive labour, either by including manufactures, or only commerce in the latter—and agriculture alone, or with manufactures in the former, the productive class. Be it so: I am content, if there be as marked a distinction here as between the labour which produces or moulds matter into a new substance, and that which only exchanges one thing for another; or defends the community, or administers justice among its members. But, in truth, we have, in our present argument, a specific difference, admitting all that you have urged, as to the affections and properties of matter being used by the animal in both processes. The great and broad difference is this. In the one case, as in the wasp carrying the caterpillar to its nest, which she does and means to do, or, if you will, gratifying her senses with the carrying, whatever instruments she works with, she does the thing knowingly and intentionally; she does it by means of gravitation and cohesion, but still it is she, her action, her will, her mind that does it. In the other case, that of leaving the caterpillar in the nest for months, she has done; she quits the work; nothing she does is at all conducive to the operation then performed by nature; but what she did was all that could be done excepting by nature. So the mother eats the galactigenous matter, and then has done; nature does all the rest. But there is this material difference in what the bee or the wasp does, that she finishes the whole operation voluntarily; it is as if the mother were not only to become gravid, but to prepare the child's clothes and habitation herself, and yet to do this without knowing what she was about, and while she intended to do, and thought she was only doing, some perfectly different thing. If, indeed, you put the case of a person ploughing and sowing for the purpose of strengthening his limbs or amusing himself, and not meaning anything to grow, and also ignorant that anything will grow, and yet choosing the seed which will grow, and sowing it at the right time to make it grow—then you merely put the case of Instinct in other words; and the one thing will be as difficult to explain as the other. And if one man should, by mere blind chance, do this the first time, and some other man, equally ignorant of what the use of thrashed wheat was, should reap and thrash it, and garner it away—and if all men were to do so in two bodies, equally ignorant of what they were about, and yet both chiming in with each other in their operations, and both agreeing with the nature of things, then we should say this is the self-same case with Instinct—but we should add that this could not happen without some overruling power not only giving those men the desire to stretch their limbs, but guiding them immediately how to do it —for there, as here, two designs and only one designer appears, and therefore some non-apparent contriver must exist and work. We may again put it thus—When a man brews or tills, he does something himself, and leaves the rest to the powers of nature. So when a mother eats or drinks to gratify hunger or thirst, she has done; nature does the rest, namely, supports her body and secretes the milk for her young. But the bee, or the wasp, does the whole. They use the powers of matter, indeed, as the farmer and brewer do, and as the mother does, in the operation itself performed by them, namely, breaking the ground, throwing the seed, steeping the grain, eating the victuals—but the insects finish the operation, and leave nothing to be done. The solitary wasp has completed a cell and provided food; the young have only to eat it. The bee has completed a cell with food likewise. Neither mind nor matter on the part of either insect has anything more to do; the thing they intended and knew all about is done, and in doing that thing they did something else neither known to, nor intended by them. They only used the powers of matter in doing the thing they intended. They did not leave any natural powers to do the other thing not intended by them; but they did it also, though unintentionally. Man does what he intended, but he does nothing more—nature does the rest, both where he intended it, as in ploughing or brewing, and where he did not, as after eating to satisfy his hunger. In the bee it is like a whole manufacture completed by the animal, though unintentionally; as if a man were to make a skein of fine lace while he only meant to amuse himself with twirling the bobbins, or playing with his fingers among the flax or the threads.

A. I certainly think we do get to something like a specific difference. But compare the work of the insect with certain chemical processes. If you mix, or if any natural process mixes, certain salts, and the liquor is left to evaporate, there are formed crystals, say hexagons, as accurately as the bee forms her cells. Also certain bodies move in lines which have properties similar to the angles in the comb, as a heavy body falling through the shortest of all lines. There is no doubt a difference here, and a marked one; yet it is as well to consider it.

B. Doubtless there is a difference, and the greatest possible. These forms are assumed, and these motions performed: for instance, a stone falling to the ground in the shortest line, or the planets, all arranged respecting their masses, the direction of their motions, and the inclinations of the planes they move in, so as, according to Laplace's beautiful theorem, to preserve the system of the universe steady, by affixing limits, maxima and minima, between which the irregularities oscillate; all these things are the direct and uninterrupted agency of the property which the Deity has impressed on matter at its creation; perhaps, of the laws which His power perpetually maintains. But they are wholly unconnected with any animal workmanship of any kind; they have no subordinate mind to guide them; nor can any act of ours, or of any animal, affect them. On the contrary, in all our operations we must conform to them.

A. Unquestionably it is so; and this is the distinction, and the broad one. But then it follows from the preceding deductions, that we must consider in the works of Instinct the animal acting as an agent, though ignorantly and unintentionally,—a tool or instrument blindly used to do a certain thing without its own knowledge or design; and the tool being a living thing, the mind is the instrument. In the case of matter, the matter is the instrument blindly serving the purpose by obeying the physical law. In our case the mind is the instrument, and obeys the mental law as perfectly and as blindly.

B. There is one thing, however, always to be considered. We have hitherto been viewing Instinct alone, and arguing as if animals always acted by it, and never otherwise. Now this is quite impossible, at least in the sense in which we have taken the word Instinet. There may be some doubt if we are right in so limiting the term, though I have a very clear opinion that we are. Paley, and all, or almost all others, define Instinct to be a disposition or acting prior to experience, and independent of instruction. But among other objections, there is this one to the definition, that it amounts to saying "an acting without knowledge," and yet does not say it. There may be no experience, and yet no Instinct, *e. g.*, we may act on the information of others—but then what shall be said of the information given by reasoning; that is, by our inferences from our own thoughts? This is plainly not instruction. Is it experience? If so, the definition seems only to say, that Instinct is anything that is not reason, in other words, that Instinct is Instinct. But I apprehend when we speak of instinctive operations, we always have an eye to some end which is blindly served by the act—some act done by the animal, in which he does what he does not mean, and in doing which he is a blind instrument.

A. How is it when we speak of instinctive desires?

B. I should say we then mean something different from merely animal or natural desires, for that would make every thing instinctive. We mean desires which are subservient to some purpose towards which they move: some end beyond the doing the act seems always involved in our notion of Instinct.

We do not call mere moving, yawning, stretching, instinctive; and when we speak of sucking or eating, and the desire or power to suck or eat, as instinctive, it is surely with a regard to the subserviency of those operations to support life that we so term them. If they did nothing for our frame, we might call them natural, hardly instinctive.

A. But be this as it may, no one can doubt that animals, if we allow them to have these Instincts, and to act for ends unknown to themselves, have other actions of a kind resembling our own, and quite distinguishable from what we have been calling Instincts; therefore it signifies little whether or not we are right in giving the name to actions accomplishing undesigned and unknown purposes, provided we keep that definition in view. These animals also have other actions, where they both know and intend and accomplish their definite object.

B. Undoubtedly, they have many such in which their operations of mind and body cannot be distinguished from our own. Now whether these are under the guidance of faculties like ours; whether they have reason; whether they have faculties differing from our own in kind, or only in degree—

we need not at present stop to inquire. It is quite enough for us that they have two kinds of operations, one which we agree to call Instinctive, distinguished by the ignorance of the object, and want of intention; the other both knowingly and intentionally done: so man acting almost always rationally also acts in some rare cases unintentionally—chiefly in early infancy.

A. There may be instinctive acts with knowledge, and there may be acts not instinctive without knowledge. Does not this break in upon the definition which excludes knowledge as well as design? Many parts of human conduct seem to be guided by Instinct, and yet with knowledge.

B. This would no doubt overturn the definition, provided it be clear that "*knowledge,*" and the "*presence of knowledge,*" are here used in the same sense as in that definition. But we must make a distinction. There is a knowledge of some *end* or *object* in view, and a knowledge of the *means* whereby that end or object is to be attained; in other words, of the *mode* of *operating*—of the *process*. There is also a distinction to be taken between instinctive *desires,* and instinctive *operations.* The objection you have now made refers to the former—to desires;—the latter, the operations, are chiefly referable to the great question respecting the controlling mind, or actual interposition of the Deity, to which we are approaching; but it also refers, in some measure, to the objection which you raise. Knowledge of consequence comes within the description of object or end; and if there be no intention to attain an end actually pursued, there can be no knowledge of it; and conversely, if there be no knowledge of it, there can be no intention to attain it. Take any instance of what you call human instinct, as hunger, or the sexual passion—these are desires, and their gratification may be pursued without any knowledge of, and consequently without any view to, the consequences of making chyle and blood to support the individual, or offspring to continue the race. As far as the mere gratification of the desire, or supplying of the want goes, we may be said both to know what we are doing, and to intend or mean to do it. We are attracted by our senses, that is, by the effect of our senses on our minds, to do certain things; and this is called instinctive acting, I apprehend incorrectly. It is *natural* desire, but why instinctive? When we say Instinct, we do not mean something beyond this?

Desires may be subservient to Instincts; but are they all we mean by Instinct? They may lead to the attainment of a certain end; they may be the way in which Instincts operate; but are they themselves Instincts? If two foods are presented to an animal, a man for example, who knows nothing of either; and he is impelled, without knowing why, to take the one and reject the other, and the one is wholesome and the other a poison; we at once call this the operation of Instinct, which some define to be knowledge without instruction or experience, but which I have wished rather to call mental action without knowledge, or at least independent of knowledge. So in Galen's beautiful experiment on the kid just born, having been taken out of the mother, and which of course had never sucked, when, upon many shallow pans with different liquids being placed near it, the animal preferred at once the pan containing goat's milk. If the reason for the preference is some greater gratification of the senses, or that the one food is pleasing, for instance, in smell fragrant, and the other offensive, this may be the mode taken by nature to make Instinct operate according to your former hypothesis, which we have been discussing at large; and we certainly cannot tell that such may not, in all cases, be the mode taken by nature for working to the same end. It seems, however, eminently unlikely that the whole operations of bees, for example, should be owing to the pleasure their senses receive from one particular form and proportion alone, and a repugnance to all others, because of their being disagreeable to those senses. But do we not, in all cases, mean, by using the word Instinct, to point out the unknown connexion between the thing done and something else of which the animal—the agent—is not aware? I grant you that we speak of Instinct of hunger and Instinct of sex; but is not this only a way of saying, and do we not mean, merely desire of food or sex, the gratification of which is a natural propensity, and known and felt by us to be such? Thus it is an Instinct which makes animals propagate their kind while they merely mean to gratify their passions, and which enables them to prepare a nest, and have it quite ready at the very time they are to want it for laying their eggs in. We always seem to have the *motive*, the *end*, and the *blind instrumentality* in our view when we speak correctly of Instinct. I may intend to do a thing, and know both the object in view and that portion of the operation, or process, which depends on me—*e. g.*, to eat for the purpose of making chyle. My ignorance of that process, with which I have nothing do, would not make the operation of mine be called an Instinct. Indeed, even if I eat to satisfy hunger, without any design of supporting the system, this act is not instinctive, except in so far as doing and meaning one thing, I am doing another thing ignorantly and unintentionally.

A. I think we have got as far as we can in these preliminary discussions and observations of Facts, and may now proceed to Theorize and infer.

B. However, we are come, or coming, to a part of the subject where we should be among our books; for we shall now have to look at them in proceeding further. At least, it is as well we should observe what has been held on this matter by philosophers. So we had better adjourn for the present; and resume our conversation in the library, if indeed you, who are accustomed to Althorp and Spencer House, can condescend to call anything in this part of the world by that name. We commonly, from feeling this modesty, name it the Book-room.

A. And I dare swear, also from your love of the Saxon idiom.

B. Possibly; though I would that our good old English never suffered more havoc than by calling Book-rooms Libraries. I expect to outlive it, as Serjeant Maynard said he had nearly done the law, with the lawyers.

BOOK, OR DIALOGUE II.

INSTINCT.—(THEORY.)

HAVING thus far carried on our discussion in the open air, we removed, towards the afternoon, to the library—"cum satis ambulatum videretur, tum in bibliothecâ assederemus"*—and there conveniently pursued the subject which greatly interested us both.

B. The manifest difference between Instinct and Reason which we have been observing, and its regular and constant action, always the same, and never improved, but never different, indeed apparently incapable of improvement, was probably the consideration which induced Descartes to consider animals as machines.

A. I am aware that this is commonly said of him. But I know not how that great man could really have held so untenable a position. Did he really

* Cic. De Div. II.

consider them as mechanical contrivances—as mere physical substances, without any thing answering to what we call Mind?

B. He is always so represented; but when you examine his own statement closely, you really find that this is an exaggeration, and that his doctrine differs not very much from that commonly received. As has oftentimes happened to others, his sentiments are rather taken from the statement of them by those who were controverting them, than from his own words.

A. Where are they to be found?

B. Look here—you have them in the short treatise on Method, the introduction to his work on Dioptrics and Meteors. He dwells on brutes having no gift of speech, which yet requires very little reason, he says; and therefore he concludes not that they are less rational than man, "sed plane esse rationis expertia."* Thus far no doubt can exist; he only gives a very common opinion on the subject, though an opinion controverted by some, as I shall hereafter ask you to discuss: but it forms a head

* De Methodo 36.—"Istud autem non tantum indicat bruta minore vi pollere quam homines, sed illa plane esse rationis expertia. Videmus enim exiguâ admodum opus esse ad loquendum.

distinct from our present inquiry. But a little way further on he proceeds to illustrate his position in a manner which has given rise to the notion in question. "They do many things, even better than ourselves," he says, "but this does not prove them to be endowed with reason, for this would prove them to have more reason than we have, and that they should excel us in all other things also—but it rather proves them to be void of reason, and that nature acts in them according to the disposition of their members, as we see a clock, which is only composed of wheels and weights, can measure time better than we can with all our skill." He goes on to shew that the interests of virtue are greatly injured by the belief, not that brutes have souls, but that they have souls like our own—"brutorum animam ejusdem esse cum nostrâ naturæ,"—and that therefore we have nothing more to hope or fear in a future state than flies or ants; whereas he had shewn our souls to be by their nature independent of the body, and therefore not mortal like and with it. All this you perceive is anything rather than the doctrine, that brutes are mere machines.

A. But where do you find the adversary's representation of it which you mentioned?

B. Here, in this other and very curious volume, containing his Correspondence with many learned persons, and some less learned, as Christina, Queen of Sweden, and our Princess Elizabeth, the Electress Palatine, and stock of our present Royal family, to whom he writes, among other letters, one on her brother Charles the First's execution—which, to console her, he praises as more glorious than an ordinary death—pulchrior, felicior, et dulcior—(*Epist. Pars* I., *Ep.* xxvii.)

A. Does the Princess enter on the question of animals?

B. No; she seems to have been ailing with fever, and having been light-headed, she applies to the philosopher to explain to her how in the night she felt an irresistible desire to make verses: this he courteously explains (after saying it reminded him of a similar anecdote related by Plato, of Socrates), that it is owing to the agitation of the animal spirits, which in weak brains produces madness, but in strong ones, only a genial warmth, leading to poesy, and thereupon he holds her Serene Highness's case to be "ingenii solidioris et sublimioris indicium."

A. Upon my word, I shall begin to think a person, who could thus theorize as well as flatter about animal spirits and Serene Highnesses, was capable of shutting his eyes to the most ordinary facts, and believing brutes to be machines.

B. Do not undervalue this great man: he is the true author of all the modern discoveries in mathematics. He made the greatest step that ever man made since the discovery of algebra, which is lost in the obscurity of remote ages: I mean his application of algebra to geometry, the source of all that is most valuable and sublime in the stricter sciences and in natural philosophy. But assuredly his physical and psychological speculations are much less happy; although it was no mean fame to be the author of a treatise, the answer to which was the first work ever composed by man—Newton's Principia. But I was coming to the controversy on Instinct. An ingenious clergyman of Cambridge, Henry More, objected to the doctrine of the great philosopher, as laid down in that treatise to which we have been referring, on Method; and he began by describing the doctrine as denying sense and life to brutes. He speaks of Descartes's genius, "chalybis instar rigidum et crudele, quod uno quasi ictû omnium ferme animantium genus vitâ ausit sensûque spoliare in marmora atque machinas vertendo."—*Epist. Pars* I., *Ep.* lxvi This he repeats in various ways, and argues against, as the doctrine of Descartes.

A. Nothing in what we have read out of Descartes's own writings justifies this. Is there any other passage to which More can allude?

B. He refers expressly to the passage in the "Tractatus de Methodo," and discusses the argument there given from the want of speech. But there remains a letter of Descartes to a certain great personage (ad Magnatem quendam), in which he repeats the doctrine of the treatise at somewhat greater length, but using the same comparison of a clock, and using it as a comparison. His whole contention is, that they, the brutes, have not reason like us, which he terms sometimes "intellect," or thought—"intellectum vel cogitationem." But that he means reason, and does not mean to assert that brutes are machines, seems plain from this, that in the same passage he allows them natural cunning, or craft, as well as strength—"imo et puto non nullos (animantes) esse posse quæ naturalibus astutiis instructæ sunt quibus homines etiam astutissimos decipiant."—(*Ib.* p. 107.) This is anything rather than describing them as mere machines.*

* He afterwards, in the same letter, says, that although brutes do nothing to shew they can think, yet it may by some be supposed

A. But what does Descartes reply to his correspondent's letter, in which he represents that to be his doctrine? Does he object to Mr. More's statement?

B. Why, singularly enough, he does not in distinct terms repudiate it, though this may be owing to his supposing that, as he had used the comparison of the clock, Mr. More is also speaking in the same terms, especially as Mr. More had professedly used figurative language, and spoken of Descartes' cutting off all animals as with a sword. But he speaks certainly in this answer (*Pars* I. *Ep*. lxvii.,) more strongly than elsewhere. "I have diligently inquired," says he, "whether all the motions of animals came from two principles, or only from one; and as I find it clear that they arise from that principle alone which is corporeal and mechanical, I can by no means allow

that as they have limbs like our own, so thought (cogitatio) may be joined with those limbs, as we know it is with our own, although in them the thinking principle (cogitatio) may be less perfect than in us.—"Ad quod," says he, "nihil est quod respondeam nisi quod si illa cogitant ut nos, animam etiam ut et nos immortalem habent, quod non est verisimile;" and he proceeds to say that oysters, sponges, and other imperfect animals, can hardly be supposed immortal.

them to have a thinking soul. Nor am I at all hindered in this conclusion, by the cunning and sagacity of foxes and dogs, nor by those actions done by animals from lust, hunger, or fear; for I profess to be able easily to explain all these things by the sole conformation of their limbs." He adds, that though he sees no proof of the affirmative proposition (of their having a thinking principle), yet he also admits there is no proof of the negative; and he then comes back to his favourite topic of its "being less likely that worms should have immortal souls, than that they should move like machines;" and again refers to the want of speech.

A. How any man who ever saw dogs in a field pointing, or greyhounds chasing a hare, or still more, dogs sleeping and manifestly dreaming without any external object to excite their senses or motions, or who had observed birds taught tunes, could ever suppose them mere corporeal or material mechanism, things made of dead matter and without life, I cannot comprehend.

B. The best of it is that he positively affirms they have life. The letter I have just been reading from, and in which his doctrine, if anywhere, is stated the most explicitly, concludes by warning

Mr. More not to suppose he denies them life; and it is remarkable that he uses the very words *vita* and *sensus*, which Mr. More had represented him as refusing to brutes,—" Velim tamen notari me loqui de cogitatione non de vitâ vel sensû. Vitam enim nullo animali denego."

A. Then what does he mean by life and sense?

B. He goes on to tell you, " utpote quam in solo cordis calore consistere statuo,"—mistaking the indication or effect of life for life itself. He adds, " nec denego etiam sensum quâtenus ab organo corporis pendet." Now, can it be that Descartes really supposed he had taken a tenable distinction here between mind in man and in brutes? Or that there could be any perceptible difference between a machine endowed with life and sensation, and capable of imitation, of learning, and of much cunning—and a body animated by a mind? To speak of sensation as depending upon the corporeal organs is either unintelligible or it is a begging of the question, and the very same definition might be given of our own sensation—nay, is given of it by the materialists, who hold our mind to be the mere result of a physical organization. Yet with these Descartes differs more indeed than with all others.

A. I cannot help thinking, on the whole, that it is very possible this great man may have only meant to deny the brutes a reason, or mind like ours, a power of ratiocination, and not to consider them as mere machines. But I am clear of one thing, that if he did mean the latter, a more untenable doctrine never was broached upon this, or indeed upon any other subject.

B. We may therefore, I conceive, pass over this theory altogether. But another and a greater man has been so pressed with the difficulties of the subject, that he has recourse to a very different supposition, and instead of holding the Deity to have created brutes as machines without any mind at all, he considers their whole actions as the constant, direct, and immediate operation of the Deity himself. Such is the doctrine of Sir Isaac Newton, which is saying enough to prevent any one from hastily rejecting it, or rashly forming his opinion against it.

A. Does he not mean merely to derive the actions of brutes from a perpetually superintending and sustaining power of the Deity, as we ascribe the motions of the heavenly bodies to the same constantly existing influence? He probably only means that the brute mind, having been created, is as much under the Divine governance as the material powers, qualities, and motions are; in other words, that mind was created, and matter was created; and that still the actions and passions of both are constantly under the guidance of the Creator. So that Sir Isaac Newton would no more deny the separate existence of the minds of brutes, than he would the separate existence of their bodies, or of the heavenly bodies.

B. Here are his own words. The passage occurs in the famous 31st Query, or General Scholium to the Optics;* and you see that, after recounting the structure of animal bodies as proofs of design, he adds, "And the instinct of brutes and insects can be the effect of nothing else than the wisdom and skill of a powerful, ever-living agent, who, being in all places, is more able by his will to move the bodies within his boundless uniform sensorium, and thereby to

* There is nothing more admirable for extent and generalization of view than this 31st Query. The happy conjecture respecting the nature of the diamond in the 2nd Book, (*Part* II., *Prop.* 10,) does not surpass the wonderful sentence in the query, where Sir Isaac Newton classes together, as similar operations, respiration, oxydation, and combustion. These have since been discovered to be the same process. In Sir Isaac Newton's time, their diversity seemed as great as that between the diamond and charcoal.

form and reform the parts of the universe, than we are by our will to move the parts of our bodies." He proceeds to guard the reader against a supposition of the Deity being the soul of the world, or of brutes, or of His being composed of members or parts, stating that He only "governs and guides all matter by his prevailing power and will." So that you see he draws the distinction between the mind or will of men, which influences the motions of their bodies, and the influence which moves brutes; plainly enough referring the latter to the Deity himself, as the *primum mobile,* or actuating principle; for he allows that the kind of ubiquity or universal action to which you refer applies to our bodies, and I presume to our minds also, which were created and are sustained by Him. Of that no doubt can exist, because elsewhere he has laid down as clear this ubiquity, called, as you know, *essential* ubiquity, to contra-distinguish it from *potential* or *virtual.* You find this plainly stated in the Principia—here is the celebrated General Scholium: "Omnipresens est non per virtutem solam, sed etiam per substantiam"—"In ipso continentur et moventur universa, sed sine mutuâ passione." *

* Principia, Lib. iii., Sch. Gen.

Therefore it is quite manifest that, in here treating of Instinct, that is, of the operations of animals, he considers the Deity's action as different from that general direction which he ascribes to Him over matter and mind by His essential ubiquity. In other cases He acts on matter and mind, and in the case of mind, He acts on matter mediately or through the agency of mind, which mind He moves. But here He acts, according to Sir Isaac Newton, directly on matter, and is the moving and acting principle of animals; and such has generally been the construction put upon his words as you have them here in the 31st Query. It has been so stated by so popular a poet as Pope, and also, though with less precision, by Addison. The former takes the distinction in his Essay on Man, between brutes as only having volition, which in them acts for both willing and reasoning; while men have the double faculty. He expresses himself with his wonted felicity:—

> "See then the acting and comparing powers,
> One in *their* nature, which are two in ours;
> And Reason raise o'er Instinct as you can,
> In this 'tis God that acts, in that 'tis Man."
>
> *Essay*, *Ep.* iii.

Addison, in his 120th Spectator, after giving many instances in which he jumbles together Instinctive and Intelligent operations, concludes with the remark, that "they can no more be explained than gravitation can; and come not from any law of mechanism, but are an immediate impression from the first mover, and the Divine energy acting in the creature."

A. This dogma of Newton is certainly great authority—the greatest human authority. For it is the opinion—and, regard being had to the awful nature of the subject as well as the contemplative and religious nature of the man, it is probably the well-considered opinion of the greatest inquirer into nature that ever existed, and whose conjectures have been almost as happy, and are certainly quite as marvellous, as his complete discoveries.

B. Observe too, that it is the opinion of his maturer years. The Scholium to the Principia was added in the later editions—when written does not clearly appear, but the second edition was published in 1713, and the third as late as 1726. The 31st Query to the Optics was added at a time which can be fixed better. The first edition of the Optics, published in 1704, had not the queries. The second, published in 1717, had them; and the third edition was corrected by the author's own hand a short time before his death; from which corrected copy the one I am now citing was printed in the year 1730, after his decease. But as he first published this passage in 1717, and was born in 1642, he was then in his 75th year, and had long before made all his discoveries.

A. I quite agree that as far as mere authority goes, no opinion ever had so great a weight—nevertheless we have the same illustrious man's authority, and example too, to teach us that it is by our own reason alone that we ought to be guided in philosophizing, and we must bring to the test of that canon even HIS best considered opinions.

B. This I of course freely admit. Let us, then, examine a little this doctrine of immediate interposition—which regards the work of the bee, for instance, as the direct and immediate operation of Divine wisdom and power.

A. I need hardly warn you against being seduced by another bias, as powerful as Sir Isaac Newton's authority—the disposition we must have, if possible, to believe in a doctrine which, by exhibiting the finger of God as perpetually moving and working before our eyes, seems to bring us constantly into His presence, as if we saw a perpetual miracle wrought, and almost enables us to commune with the Deity, as the Patriachs did of old. The gratification to us, as men, of reaching this position, should not make us, as philosophers, open our ears the more readily to any unsound or inconsistent reasonings, assume facts on slight grounds, or, passing over flaws in the argument, receive easily erroneous conclusions from what we see.

B. Again I entirely agree with you. Far from making greater haste to reach a position so delightful, I should take the greater care of my steps, that I might not slip and fall by the way: for that the road is slippery, the light glimmering, and the route over high ground, leading through precipitous passes, must, I think, be admitted freely. But let us step on cautiously as we have hitherto done.

A. We left off with the deduction that brutes act from a principle, a thinking principle, a mental principle, something different from their bodies and from surrounding objects, but that they act towards an end of which they are ignorant, and accomplish that end without design, though very possibly they may also in so acting accomplish some intermediate end of which they are aware, and which they intend to attain.

B. We may add another thing to the proposition. The end which they accomplish blindly and instinctively is far the more important of the two, admitting that there is another and intermediate one. For, suppose your theory to be correct, that the solitary wasp gratifies some sense in carrying caterpillars, and the bee, in making hexagons and rhomboids, it is plain that this is a very trifling matter; it neither feeds, nor clothes, nor lodges her, nor her brood; whereas, the purposes to which those works are subservient are the continuation of the species of the insects respectively—the greatest and most favourite end in nature.

A. True; and you may add another thing, which I allow, even if my theory be ever so certainly correct—that the only possible use of the intermediate end is the accomplishment of the other end—for if you grant me that the wasp carries caterpillars, and the bee makes geometrical figures, to please themselves, or gratify some sense, it is of no importance that either should receive that gratification: its only use is the unknown and unintended consequence of providing for the unborn issue.

B. We are now then arrived at a very important height, from whence we may survey the subject correctly and advantageously.

A. Let us be quite sure that we have left no obstructions, or rather that we have passed over nothing material—that we have left no objections in our rear, which may rise up and mock any inference we now draw. For instance, are all our facts clear? As to the bee's architecture, some have questioned the theory. I have heard it said that what seems so perfect a structure, and so judicious a dividing out of the space, so as to save room and work and material, is only the necessary consequence of placing a number of cylindrical or globular bodies together; that if you blow many soap-bubbles in a basin they will, by their weight and pressure, settle into hexagons.

B. There never was anything more absurd than what some, calling themselves philosophers, have said without a moment's reflexion on this subject. No less a name than Buffon may be cited for such nonsense. There are two decisive answers:—*First,* the soap-bubbles will not make hexagons, although your eye may see straight lines formed by their intersections, but not one hexagon the least like the bee's will you find in all the foam; and *next*, there is not a single globe, or cylinder, or any figure like it ever made by any bee. Huber has seen them, or rather had them carefully observed, when at work; they first make a groove, and then form its walls into planes, and all the rest is a making of planes and angles one after the other without any circular figures at all. So some one finding the eye of the bee to be a net-work, when greatly magnified, and each mesh a hexagon, thought he had found out why the bee works in that figure. To which the answer was obvious, that men and other animals having circular pupils should, by parity of reason, work in circles. But another answer was just as decisive; that the light entering by a hexagon almost infinitely small no more helps the bee to that figure than if it entered by a circle or a square. Its paws and feelers are to work. Nay, suppose even it had a small pattern hexagon ready made, would its working a large one on that model be at all less wonderful? Not to mention that the hexagon is not the greatest wonder; the rhomboidal bottom of the cell, and the angles which its three plates form with each other, and with the walls, are the wonder, and no one pretends to account for that. I pass over the form of the limbs; nothing can possibly be deduced from them in the smallest degree fitted to aid the bee in her marvellous work.

A. Have not some sceptical inquirers thrown other doubts upon the mathematical part of this great wonder? I think I have heard something of the kind, as if Maclaurin, or whoever was the discoverer, had rather been fanciful, or over-refining, and that the bee had turned out to be not so good a geometrician as they had supposed.

B. Here is a sample of those doubts—though they are not indeed, like Newton's sound conjectures, stated with the modesty of doubts—but somewhat dogmatically. It was the celebrated Maraldi who first measured the angles, and found them to be 109° 28′ and 70° 32′ respectively. Reaumur afterwards set a young mathematician, pupil of Bernouilli, called Kœnig, to find what were the angles that made the greatest saving of wax, and the result was by his analysis 109° 26′ and 70° 34′ being within two minutes of his own measurement, which measurement he had not communicated to Kœnig. But it turns out that the bee was right and the analyst wrong: for by solving the problem in another way I find that he erred by two minutes; and other mathematicians, with whom I have communicated, distinctly find the same thing, and we have also found how the error crept in.*

A. These angles must have been very nicely measured; for the difference of two minutes, or the 2000th part of the lesser angle, is very small indeed. How were the angles first ascertained?

B. Maraldi was a most accurate observer, and he gives the angles, as I have stated, 109° 28′ and 70° 32′; and he gives them to differ with the result of Kœnig's calculus, which was made after Maraldi had measured—so he could not have fancied the amount. But I have reduced it from measuring an angle to the easier operation of measuring a small line. If those are the angles, then it follows that the breadth of the rhomboid is exactly equal to the side of the hexagon, and you find it appears to be so. Also, if those are the angles, the rhomboidal plates are inclined to one another at the angle of 120°, that of the hexagon; and you find they do not differ when you place them together, one within the

* See this fully explained in the experiments and demonstrations relating to the comb in this volume. There is some contradiction in Maraldi's statement, *Mém. Acad. des Sciences*, 1712, pp. 310-312; but the above measure has always been considered to be that which he intended to state as his result.

other. However, I admit that this is not a very close admeasurement of such small differences; and I presume Maraldi must have employed a micrometer. I have used one to compare the breadth of the plates and sides, and I certainly can find no inequality. At all events, the bee seems entitled to the benefit of Maraldi's previous measurement, which had been thought to put her in the wrong, now that the analyst and not she has been found in error. This, however, is nothing to what follows. A Berlin academician, thinking, I suppose, to do a kindness by Frederic II., objected to the bee, that though, if the dimensions of the cell be given, the saving is as I have stated, yet there is such a great waste of wax arising from those dimensions as proves the saving of wax to be no object. He sets himself the problem of what he calls a *minimum minimorum;* namely, to find the proportion between the length and breadth of the cell which saves most wax; and he finds it something quite wide of the actual proportions. Now, I went over this analysis, and again found the bee right, and the philosopher at fault; for he had wholly left out the hexagonal covering of the cell's mouth, which, whether for brood or honey, there always is; and I found the actual, or bee's, proportions to save more than the academician's, when this was taken into the calculation. I moreover found the sides to be so much thinner than the bottom, that a shallow and wide cell would have cost more, even independent of the covering at the mouth. Again, he admits the form chosen to suit the bee's shape, which the form he calls a true minimum never could; but I shew that it saves wax as well. Lastly, I have solved another problem of a like kind, namely, to find the angles that save most of the fine, or difficult work, which is the angular or corner-working evidently, and that also is the thickest part of the work necessarily. I find the solution gives the very same angles which the bee uses, and which also save wax in the other view. So that she has hit upon the very form which in every respect is the most advantageous, and turns out to be on all grounds right—as indeed we might well suppose when we recollect who is her Teacher.

A. All this is most satisfactory, and it was worth stopping to state it. However, as we have made a pause before our next advance, it may be just as well to stop for a moment longer in order to consider what the bee's operation really is. How we should go to work had we to build cells is plain enough. Suppose we had discovered, which we should do by mathematical investigation, the proper form, the due proportion of the width to the length, and the proper angles of the bottom or roof—then we should have drawings and plans; and by these we should either cut our planks, if the structure were of wood; or if it were of stone, which more resembles the bee's materials, and is, be it observed, much more difficult and complicated to work with, we should, by those plans and by models or frames, run our courses. It would be a nice and difficult work to make this masonry, and would require the builder, both in hewing the stones and in putting them up, to follow the details of the plan in its parts, and without any regard to the general figure or result. He would be wholly unable to succeed if he looked to that; all his building would be awry and out of the required figure; his only chance is to make his plan exact, and his model-frames suit it; and then he has instruments and tools, plumb-lines, squares and plumbs together, in order to raise his perpendiculars. By these he proceeds, for he cannot trust his eye or his hand a moment beyond the mere adjusting his work to his instrument and his plan. Now the bee confessedly has neither plan, except what is in her head; nor any model at all whereby to guide her hand; nor any instrument to adjust her work to the plan in her head; nor any tool to work with except her paw and her feeler, which is as her eye in doing the work. Then how does she work?

B. Certainly, this is a most important consideration. We cannot trust our eye or our hand an instant. We have no exact perception of the line, and no steadiness in pursuing it. We have recourse to plans and instruments because we cannot form our lines by volition, that is, by having a form in our mind and by making our hands follow that form. We therefore must first lay it down sensibly, and then guide our hands by material means. Thus we have no power of forming a dome, an arch, or a circle, or a perpendicular, or a level, or even a straight line at all, or any one line or form which we conceive in our mind. Far from being able to follow these lines in great works, as roofs and walls and excavations, we cannot even represent such forms on a sheet of paper by our handywork. If we could do this we should work like the insect, who acts immediately, and not through the instrumentality of means. Unable to execute any purpose of our minds, as she does, we have recourse to instruments. We endeavour, as far as we can, to reduce every thing to a physical or material process—to exclude mental operation or agency altogether—to make the whole a material, or as we call it, accurately enough, a mechanical operation. Reason no doubt has taught us to do so; but it has taught us a general rule; and there is little or no reason, little or no operation of the mind in its application to the particular cases. On the contrary, the use of the rule or method is that it precludes the operation of the mind as much as possible, and makes the whole physical, or nearly so. To take an instance—we reduce, by engraving or printing, the whole operation of drawing a picture, or writing a page, to turning a lever, which does the work for us. So in building, though there is less mechanical facility, we guide our hand by the instruments employed and the lines drawn, making the operation as mechanical, as little mental, as possible. The bee's operation is all mind together. She has no plans, no instruments, no tools. It is as if by waving our hands among plastic materials we formed walls, and domes, and columns, and never deviated a hair's breadth from the perfectly accurate plan. I am very decidedly of opinion that this essential difference between the works of Reason and Instinct is of the greatest importance to our inquiry: for nothing can more shew the peculiarity of the instinctive operation; or more prove that the mind of the agent is as it were the machine, and the instrument, to perform the work, and to perform it with an unerring certainty and with absolute perfection.

A. Does this, which appears to me as it does to you, a most important consideration, bring us at all back towards the ground of Descartes, which we had passed over as forming a position wholly untenable; I mean, that the insect is a mere machine, fashioned by a perfectly skilful mechanic, and wound up to perform the functions which he designed?

B. Certainly not. The proposition which we have just been deducing from the facts is rather of a kind the very reverse: it affirms that the insect's mind performs the whole operation; it makes the insect's mind the machine if I may so speak. But let us see to what it also leads or seems to lead us. We perceive there is mind at work, action exerted, effect produced; but we see that the mind is quite unconscious of the effect, and that the action works to a purpose which the mind never contemplated. There is a thing done, an important and rational thing done, but done by an agent who neither intends nor knows anything about it. Here there is design, but there is no designer—an action and an object no doubt; but that action performing, besides what the agent intended, knew, and did, something else (and that something the only important thing), which the agent neither knew nor intended, and cannot possibly be said to have done at all. This by no means leads us back to Descartes' position, but does it not lead us to Sir Isaac Newton's? The design is manifest; the action is perfectly and surely adapted to it; the purpose is with singular regularity effected; must there not be a designer, and who can that be but the Deity? There is none other that can be suggested even. Must it not be he?

A. Doubtless in one sense it must, as he is the designer of all we see. But how is he more the designer here than he is of the motions of the heavenly bodies, or the growth and germination of plants?

B. As thus. In those cases there is nothing but matter affected, or acting; whatever laws were originally imposed on matter are followed; whatever qualities first communicated to it are displayed: all is material. There was design in the original formation of it, in the prescribing those laws, and impressing those qualities. That design these bodies fulfil; they conform to the primæval and original intention of their being. But there is no renewed design, no repeated intention, no special and particular disposition in each case of action. The Deity made a stone, and made the earth, so that the stone falls to the ground by virtue of the general rule of their formation. He is not to be referred to; he needs not interfere each time the support is withdrawn from the stone, in order to direct the path it shall take. If on that support being withdrawn some interposition were required to decide how it should go—for instance, whether it should stand still or not—although it be admitted, that if it move it can but move in the straight line downwards, the case would more resemble Instinct, though even here it would be different; for it is as if each hair's breadth of the stone's motion required a new action to carry it on in its course.

A. The Deity created matter so as to obey in each case certain general laws: so he created mind in like manner to obey certain laws in each case.

Wherein do the two facts differ, the fact of material and the fact of mental action?

B. As thus. The moving power is wanting in the one case. The law is that matter shall act in a certain way, and mind in a certain way; but is it the mind of the insect that acts when the whole mental process is wanting, namely, the knowledge, thought, and will? Its mind acts, subject not only to a general law, but to a particular impulse each time. Who gives the impulse? Besides, your doctrine of the Deity creating the insect's mind such as to act so in given circumstances, applies quite as much to our Reason as to its Instinct. Let me, however, put a case: suppose we saw a man born blind, to our own knowledge, without any teaching, and without ever having tried it before, move his fingers in the design of giving them exercise, as to keep them warm, &c., but holding a pencil in them, and by the same act producing, unknown to himself, a beautiful and finished portrait, of perfect resemblance to the original: or suppose we saw a man who had been born and lived in a foreign country, and was utterly ignorant of our language, of which he had never heard a word, write a letter in correct English, or a beautiful copy of verses, while only meaning to try whether a pen was well cut, or the ink rightly made—these acts are quite analogous to the Instinct of bees. Nay, we may take a nearer case, and suppose a man who never had learnt mathematics, and did not know a line from an angle, to solve on a slate a problem of great difficulty with perfect and unerring accuracy, and this while he was only trying the pen and the slate; and suppose he then applied this solution to the combinations of a perfect time-keeper, while he thought he was only cutting off the superfluous pieces of two lumps of brass and steel of which he intended to make weights, he being wholly ignorant of what a time-keeper meant. There is nothing more strange in this than the bee's architecture. It is indeed exactly, and in all its parts, a parallel instance. In all such cases (the extra thing done, and not known or intended, being far more difficult and more important than the thing intended and known to be done), we should at once pronounce that there was a miracle, because of the thing done being without the possibility of the apparent agent doing it unassisted, according to the ordinary laws of nature. In other words, want of power in the immediate agent compels us to believe in the interposition of another agent having the power. There is *dignus vindice nodus,* and we call in the *vindex.* This is the foundation of all belief that there must be supernatural agency where the laws of nature are suspended. But in the cases put there is not only want of power, but of design. If want of power in the apparent agent drives us to suppose or infer the action of another unseen agent, want of intention or design should drive us to infer the intending of another designer, and want of both power and intention should make us infer the thinking of a planner who intends, and the action of an agent able to perform the work; in other words, to infer the interference of one who has both the will and the power, each of which is wanting in the immediate or apparent agent.

A. In the case you put of a miracle, there is a single instance, and because it is solitary, we say the laws of nature are suspended, and we call in supernatural aid. In the case of Instinct, it is the constant course; it would be a suspension of the law, and a miracle were it ever otherwise. It is as much part of the law of nature that the animal should do the thing in question without intending it, or knowing how he does it, nay, that he does it at all, as that man should do it knowingly and intentionally, or that the animal should knowingly and intentionally do those other things in which he acts rationally, and not instinctively. Therefore this case does not resemble a miracle.

B. The case of a miracle I did not put in this way or with this view at all. I do not say that the instinctive act of the animal, or of man when he acts merely from Instinct, as he does, though most rarely, are to be compared with miracles as being suspensions of natural law; but only that the same reason which makes us, when arguing from such suspension of natural laws, conclude that some power has interposed different from the powers acting under those laws, requires us, when arguing from the acts done by the animal without either design or power, to conclude that some agent has interposed of power sufficient, and some intending and designing being of will fitted, to do the acts in question. Suppose, to put again my first case with a variation, we saw a blind man draw a likeness as often as he stretched his fingers with a pencil in them, and every foreigner of a certain class write good English verses as often as he tried a pen, and every man of a particular description make excellent time-keepers as often as he cut away the parings of the metal balls he was forming into weights—we should in every such instance of these general laws (as they could now be) have a right to draw an inference of one and the same kind. What would that be? Manifestly that here the same thing was done without knowledge or intention, which in the other class of cases (those where reason and experience operated) was done by means of knowledge, and with intention. For the gist of the question and the whole difficulty is this—that we have two classes of cases—the same act done in the one class, knowingly and intentionally, and in the other, without knowledge or intention—and as in the vast majority of all acts taken together of all kinds of agents, we can see no such thing—indeed, cannot form the idea of such a thing—as an act without power and will to do it, or a thing resulting to all appearance from intention, because in itself such a thing as we should do if we intended a given thing, and yet without any Being to intend, so we are compelled to infer the power, that is, the knowledge of the intender.

A. Indeed, it must be observed, that when we speak of a miracle we mean, and commonly do mean, two things, not only the fact seen of the laws of nature being suspended, but the inference drawn of some power interposing capable of suspending them, and therefore above them, and having sway over them; and this inference arises from the necessity under which we feel of accounting for the phenomenon observed by supposing an adequate cause; in short, from our being unable to conceive anything done without a cause. The ordinary powers with which we are acquainted fail to account for this event, and we therefore infer another power to be in operation.

B. Certainly it is so; but then this is precisely the case with Instinct, as compared with the other phenomena, namely, those things done with both knowledge and design on the part of the agent, that is, things in doing which the agent is known to us, and intends, and knows what he does. Suppose, according to the case so well put by Paley, in the beginning of his book,—suppose you find on a common a watch going and producing manifestly an effect according to its construction; this would shew a design in its maker; but only a former, or bygone, a spent and executed design. Nothing would be seen designing or intending, as it were, before your eyes. Suppose, then, you saw the watch, or other machine, making a second and third machine, but not by mechanical contrivance—for that, too, like the case put by Paley, would still only be evidence of a former, or bygone, or executed design,—you must suppose a new watch to be made before your eyes without any material agency, or, which is the same thing, made by a machine wholly incapable of performing the operation itself. Then you would necessarily infer from these the existence of some being, some thinking and designing and skilful being, capable of doing what you saw, that is, of making the machine; and you would suppose this just as much if you saw an incapable body performing the operation, as if you saw the operation performed without any visible, or sensible material agent at all. Now, this is precisely the case of the bee: it is the incapable body or being.

A. May it not all be said to be only another inference of original and general design, as we deduce that conclusion from the structure of the limbs of animals, and the functions suited to that structure which those limbs perform?

B. Even if it were so, there is the broad distinction between mere mental and mere physical agency; and the difference between the inferences to which those agencies respectively lead. But I apprehend the difference is greater still than this. The two cases are not at all the same or alike, hardly even analogous. We never know of matter, or any combination of material parts, acting or affected but in one way. We have not matter with, and matter without, gravity, cohesion, impenetrability. But if the phenomena of instinct are to be regarded as only one class of mental phenomena, we have here two kinds of mind, endowed with wholly different qualities, and acting in wholly different ways; one kind such that the being possessed of it neither knows nor intends what he is doing, and yet all the while does exactly as if he both knew and intended. Nay, in one case, the agent possessing this mind is manifestly able to act; in the other he is as clearly incompetent in any way that we can conceive. If no being is here concerned except the apparent, and unconscious, and impotent agent, it is like matter gravitating to a centre which does not exist; and then to make the thing still more incomprehensible, and the difference between matter as subject to general laws and this case the more extreme, both these kinds of mind are found in the same individual; for he sometimes uses, as it were, the one, sometimes the other; he sometimes acts knowingly and intentionally; sometimes blindly, as an instrument to do he knows not what, nor cares —as if we had a piece of matter, a lump of metal, for instance, which at one time was heavy, and at another flew about in the air.

A. There is certainly a material difference; and I should not much wonder if we were, sooner or later, driven by the extraordinary nature of the case to some new conclusion. These things have really not been sifted as they deserved. Men have rested satisfied with general and vague statements, and I suppose their attention has been too much engaged by the great curiosity of the facts connected with the subject to let them closely reason upon the theory. However, I must again recur to my supposition, and refuse to quit this position where we now stand until we have examined it more accurately. There are two kinds of mind, I will say. Then the Deity created two kinds originally. As he created two kinds of substance or existence, mind and matter, and as he endowed these with different qualities, so did he endow the two kinds of mind with different qualities. As he made matter solid and heavy, and made mind imperceptible to the senses, but endowed it with consciousness, so he gave the two kinds of mind different qualities—both of course must have consciousness, which I take to be the essence of all mind, at least we cannot conceive mind to exist without it—but one he made such that it could act rationally, knowing and intending all it did—the other such that it acted without knowing or intending. This hypothesis, you perceive, gets rid of the necessity of supposing a constant interposition of the Deity, unless in the sense in which he is said to interfere for the purpose of maintaining and executing the general laws which he originally framed for the whole universe.

B. I perceive no such thing. I do not think your supposition at all meets the fact, or removes the difficulty, or dispenses with the other inference. In one sense I may grant your assumption, namely, if you only meant that the Deity originally willed the animal should act in a certain way for a purpose which He foreordained, and which He yet concealed from the animal itself, though foreknown to Him, the Creator. But in the same way all rational acts and intentions may be said to have been foreknown and foreordained by the Creator, which indeed seems, at least in the case of an intelligent agent, only to mean that with the Deity there is no such thing as present and future, but all things are seen as present. But then this resolves itself into saying that the Deity originally designed and ordered the animal's acts; and that this is the same thing as if He actually superintended and did each act of the animal at the moment of action—which is the same thing with saying that the Deity constantly acts and not the animal, and that is the theory in question. But, in any other sense, to what does your objection, or the hypothesis put by you in order to escape the conclusion, amount? Only to this, that the Deity created the instinctive mind such that it acts without knowledge or intention, exactly as the rational mind acts with both the one and the other. Now the theory of course never meant to deny that the instinctive mind was created by the Deity, and endowed with certain qualities. Sir Isaac Newton expressly excludes the supposition of the Deity being the *anima mundi*, or the soul of any part of nature, and clearly never intended to represent Him, as Himself the soul of animals, but only as constantly guiding that soul. But the theory holds that the mind being endowed with certain qualities originally and at its creation, those qualities are summed up in this one, namely, to act, and to act *quasi* mind, but without knowledge or design, and yet to produce all the effects of both, and, moreover, that this constitutes the whole of the qualities of instinctive mind. This mind therefore was created such that it must always be the blind instrument in the Creator's hands ; its knowledge and design, by the hypothesis, reside as it were out of itself and in some other intelligent being, that is, in the Deity, who is to supply at each instant, the knowledge and design wanting in the animal mind, or to know and intend for it—and whether the Deity performs this operation, exercises knowledge and intention, beforehand and once for all, or constantly and continually at all times, seems an immaterial distinction referable to the former head of the alternative. The question always recurs—Was a mind created of such a species that it could act *quasi* mind without knowing and intending? Is not that contrary to the nature and essence of mind? Nay, is it not a contradiction in terms? And is not your whole hypothesis of two kinds of mind grounded on a false position, which supposes a substratum to be endowed with various qualities, and then in order to make two kinds of that substratum, confounds the qualities with the essence? For what is mind but that which thinks, knows, wills? If there be no knowledge, will, intention, at all, mind is not concerned in the operation, and we come to the Cartesian hypothesis, that the animal is a machine. Therefore knowledge and design there must be; and it must either exist in the animal mind or in some other mind which uses or employs the animal as an instrument. Can this higher mind do so beforehand, or otherwise than by constant operation, that is, constant exertion of itself?

A. Then are we not getting either to the Deity being the soul of the animal, or to the mind of the animal having none of the qualities constituting mind?

B. We may suppose the mind to be the mere power of giving voluntary motion to the limbs, and to consist of no other quality, unless it thinks and intends. Then the Deity may have suffered it to have these powers, and to use them in some things, and there His own intelligence does not interfere; but not to use such powers in other things, and there His intelligence does interfere.

A. There *is* knowledge and intention in the animal. The bee, for instance, knows it is carrying wax to a given place, and placing it in a given direction. So far as the thing is done, the agent knows, and wills, and intends what it is doing, and this in every possible case of instinctive action.

B. But the whole question arises, not upon what the bee knows and intends, *e. g.*, putting particles of wax in a place, but upon what she cannot possibly know any thing about—the giving her work a peculiar form, most difficult to discover at first, most advantageous for a certain end, and still more difficult to follow and work by even when discovered. The question always is, who designs and knows these things unknown to the bee? And we cannot conceive the Deity acting thus originally through a future and non-existing animal; although we can easily enough imagine Him acting through an existing animal at the time. This is supposable on the theory of essential ubiquity, or indeed upon any theory of ubiquity, even virtual. It merely requires ubiquity—whether of essence, or of power—some ubiquity—which no one denies who believes in a Deity at all.

A. A child shall place together different lines and angles, or other parts of figures, so as to form certain diagrams. The figures he thus unwittingly makes have certain properties quite unknown to him. All he intends or knows is to put the parts together; the rest is consequential, arising from the necessary relations of number and figure: so in cases of physical or contingent truth: he may do, and mean to do, and know that he is doing, what will form a certain combination; but the laws of nature acting on that combination, produce, unknown to him, effects which he never intended, and knew nothing of; as, if he mixed sulphuric acid and oil of turpentine, and there was an explosion; or an acid and an alkali, and there was a neutral salt and a crystallization.

B. This, when examined, we shall find either to be a case wholly different from the one in question, or to be only *idem per idem*, as lawyers say when they have a case put which is like enough to the one in hand, but just as difficult to resolve; so, in either way, the argument will remain unaffected. If the child plays with the things at random, and they happen to fall into a certain shape once, or it may be twice, that is certainly not the case of the bee, which regularly, and without ever failing, always makes the figure required; and, upon being obstructed in her operations, varies her means till she can again attain the particular form. If, on the other hand, the child places the things always accurately in the same way, then the case not only resembles the one in question, but becomes identical with it; all the arguments and all the difficulties apply; it is exactly *idem per idem*. So again, if the child does a certain thing with knowledge and design to do that and no more, leaving the rest to be done by some law of matter unknown to it—this is not the case of Instinct; for the bee does all that is done by the operation of mental agency; the wall, the hexagon, the rhomboid, are all made by the bee's living power; she does not place wax and leave it to fall into hexagonal forms, as we mix salts and leave them to crystallize into cubes or hexagonal prisms; she forms the figures herself, and when she has done her work nothing remains to be done further by any law of nature. But if the child makes a combination constantly and correctly, say some useful substance not to be made by accident or random working, then the case becomes the same, and the argument is not affected by it in any way.

A. You often complain of my obstinacy; which I call sometimes caution, and sometimes slowness, according as I may be in a self-complacent or a modest humour.

B. Then as I do not remember ever to have seen you in the former state of mind, I am sure you must always call it slowness, which no one else ever called it; but I will call it caution, and ask what more it leads to?

A. To this—that I would again hanker after my doctrine of general laws, primarily impressed on matter and mind both. You argue, and argue justly, that the operations of matter and of mind are to be kept apart; you allow that the material operation is explicable by and referable to general laws; you allow too, that whatever is wrought by the operation of mind, acting as such, is explicable by and referable to general laws of mind, originally imposed, *e. g.*, to desire what is agreeable to it by its general constitution; to reject what is by the same constitution disagreeable. But you say that we see, in the case of instinctive actions, operations for which desires and aversions will not account, and operations carried on as if by the most refined and correct reason, and yet without any material or physical interposition; that is, without any instrumentality whatever, as if a cast were made without a mould, or a print without a plate. From hence you say it is difficult to understand how there should not be here an intelligent being, as well as mere desires connected with the senses—a cause connected with the understanding. Now, hankering as before, I still ask—though perhaps, after our long argumentation, with somewhat diminished confidence—may not this be accounted for by supposing a general law adapting and adjusting all the proportions beforehand? May not the Deity have originally appointed the taste or desire of carrying caterpillars in the solitary wasp, for instance, exactly to the very number required to feed the worm after born, when, by the laws of matter, the egg shall have been hatched and the grub produced? So may not the bee form her hexagons and her rhomboids, in consequence of a gratification felt by a foreordained law of her nature, in following those lines and angles, and no other?

B. That this is barely conceivable I may perhaps admit. But it is wholly unlike any other operation of the senses and desires of which we have any knowledge. It means this, that each desire is so nicely adjusted as to produce in the animal the effects of reason and intention in man, or of reason and intention in the same animal when acting with design and knowledge, and not instinctively. The bird is to have a pleasure in bringing sticks or moss to a certain place, just at a given time, and putting them in one position—the solitary wasp, in bringing, and only in bringing, for it never tastes, a certain number of caterpillars, and to have no gratification in bringing one more, but the strongest desire, because a sensible pleasure, in bringing the eleventh as much as the first—also no kind of gratification in carrying the eleventh to any other place than the same where all the other ten were put—also a like pleasure in forming the hole for them, without the least regard to the use she is to make of it, nay, ignorant beforehand of its being to have any use; and yet all the pleasure of carrying caterpillars is to consist in carrying them to that particular hole, and there is no gratification to be derived from carrying them to a place one hair's breadth on the right or the left. Still more—it means that the bee is to have such a gratification as proves irresistible, and occupies her whole life in tracing certain lines and angles; and yet this strong desire is so far under control, even of reason, that on obstacles being interposed, other lines and angles are to be made, reason suspending the desire for the moment. So that the law originally imposed, and the quality impressed on the mind, was not one and inflexible, to do a certain act in all circumstances, viz., to follow the impulse of the desires implanted, and which form the animal's nature; but it was a law or order coupled with a condition, and, as it were, giving a discretionary power provisionally, or a power to be used in certain circumstances; it was as thus—a law or order to do a certain thing, to obey the impulse of the desire, unless certain events shall happen; and then and in that case to cease following the impulse of the desire, and to follow another guide, or rather to use a faculty, namely, reason, and act according as it should direct, allow, or recommend in the circumstances. Now, in the mere union of desires with reason, while the desires act blindly by impulse and the reason with discrimination, there is nothing at all inconsistent or incomprehensible; it is the ordinary case of all mental operations. But the peculiarity of the case now supposed is that the desires act exactly like reason, producing the very same effects unknown to the agent which reason does with his knowledge. Are we not then calling different things by the same name, when we say that it is the influence of desires and appetites which makes the bee form her cell and the spider her web? Might not the same kind of argument be applied to the operations admitted on all hands to be those of reason, for example, the investigations of Newton or Lagrange? Might it not be said that they were influenced by an irresistible propensity, from deriving some gratification in drawing one line and using one divisor rather than another? But we know this not to be the fact. Why and how? Only from their statements and our own consciousness. But for this, the same argument might be used, and no one could refute it. So in the case of the animal we argue thus, because we cannot ask her and learn how she works. The impulse (it must all along be borne in mind) of which the argument speaks is a physical one, *i. e.*, the effect of some external object, or which is the same thing, some operation of the animal's body, on her senses; it is a gratification of this specific kind which the explanation assumes—if not, it explains nothing. Then how little resemblance does any such gratification which we can form any idea of (leading the bee to her lines or angles, and the solitary wasp to her carriages and deposits) bear to what we know and feel to be the ordinary nature of physical gratification, and the desires connected with it?

A. This consideration has much weight—I mean the way you put the question as to the mathematicians. It seems to shew that we have just the same right, in the case of the animal's instinct, to conclude in favour of design and reason, and an intelligent agent, and to conclude against its being animal impulse or the direct operation of the physical senses, as we should have, did we see the mathematicians at work, observe their process, and mark the result congruous with that process, before we spoke to them on the subject of how their working was conducted. Indeed, it is remarkable that we are in point of fact just as much without the evidence which the thus inquiring of them would afford, as we are in the case of the animal; for who ever asked the question of either Newton or Lagrange, and yet who doubts that both worked their problems from knowledge, with intelligence? The reason why we do not ask them is, that we have no kind of doubt in our minds; the view of the operation is enough for us. This is because we say to ourselves, "If I did so and so, I know it would be from knowing and meaning to do so and so, and not from any physical gratification." This inference we transfer to others, by saying, "Therefore I believe they act in like manner."

B. Certainly; and this, observe well, is the foundation of all our reasoning as to design. The only argument we ever have or can have in favour of any intelligent cause, from seeing the adaptation of means to ends, on surveying the works of nature, is, that, if we had done so and so, we should have had the design. All we see is the fact of an adaptation; the inference of a cause, or of a designing being, rests on the kind of reasoning you have just stated. So that in reality we have reached this important position, that our argument for the existence of a designing cause at all in the universe rests on no better, indeed on no other foundation, than our argument that instinctive action proves an interposition of the Deity at each moment.

A. I must further observe, however, that beside the great weight of this consideration as last presented, I feel the difficulty of the hypothesis of an original law generally imposed to be much aggravated by the consideration you adverted to at the same time, of a provisional and conditional law—a law to operate or not, according to circumstances, as if two implements had been given to the animal, Instinct and Reason; for I feel the very gratuitous nature of this assumption; and I know that there is not a greater proof of our reasoning being merely hypothetical on any question than when we find ourselves obliged to mould, refit, and modify our hypothesis, in order that we may adapt it to the new observations of fact.

B. But there remains a difficulty still more insuperable in your way, which you do not yet advert to. The supposition of a law, and a provisional or conditional law, is all along founded on the assumption of a person to obey it, to act instinctively, unless a certain thing happens, and then to use Reason till a certain other thing happens, and then to fall back upon Instinct again. What can be more gratuitous, not to say absurd? The supposition that the Instinct is to cease and the Reason to begin in a certain event, implies that the animal acting by Instinct all the while was reasonable and intelligent, else how could he know when to lay down his Instinct, and take up his Reason? If I send a man to go straight on till he meets a messenger, or sees a finger-post, he is just as much a rational agent all the while he does not deviate from the way, as he is when, meeting the messenger or seeing the guide-post, he does deviate. So that the theory involves here this absurdity, that the instinctive action is all the while an intelligent and rational operation, contrary to the supposition. I can really imagine nothing more decisive or demonstrative than this—and I purposely kept it to the last.

A. Perhaps the end is not yet come; you have said nothing of the known errors or mistakes of instinct—and thus I reserve also my strongest argument to the last. I own that it was this consideration which, always meeting me, drove me to deny the Newtonian doctrine, and to find any or every other escape from it; for surely if the Deity is always acting, there can be no mistake—every thing must be perfectly successful and quite certain. Yet how many cases of mistaken instinct do we see? Mules begotten; flies deceived by the smell of the stapelia to lay their eggs where they cannot breed the maggots, supposing the vegetable an animal substance putrifying; and many others. Now, if this was only the result of similar desires originally implanted, there is no difficulty; for the law would be to follow that smell, and this law is obeyed.

B. Now, I really think you have just yourself answered your strongest argument; for you admit there was that general law. Had it no design? Doubtless, and but one, to lead the animal towards its food, and the nest for its young—the two great objects of all nature, preserving the individual, and continuing the species. Yet here they fail in particular instances, and do neither. Then is not this a defect or imperfection in the general law, detracting, *pro tanto*, from its adaptation to work its undoubted purpose? The same being gave the general law whom the Newtonian theory supposes to be the particular agent. Then is it not just as inconsistent with His perfections, to believe He has made a faulty statute, as to suppose that He makes a mistake in particular cases? Can there be any difference at all here?

A. How do we get out of this in the general case?

B. You mean, how do we answer sceptical, or rather atheistical arguments, drawn from these supposed errors or imperfections? Only by saying, that as in the great majority of cases the design is perfect, and the wisdom complete, it is probable that further knowledge would remove all apparent anomalies, and reduce every thing to order, and to a consistency with perfect wisdom and skill. In truth, we always assume design, even where we cannot trace it. The physiologist never supposes any part which he sees produced, as the spleen, to have no use; but rests satisfied that there is a purpose, though he has failed to discover it; and he hopes that it will hereafter be revealed to his inquiring eye. So when he finds apparent imperfection, he has a right—nay, it is sound logical reasoning to suppose, that further knowledge would prove it to be perfect, as in the vast bulk of cases he has found perfection. The instances of erroneous or defective instinct are as mere nothing compared to those of true or perfect instinct.

A. We also approach here the argument on the Origin of Evil. There is something to be said, though perhaps not much, as to the irreverent nature of the supposition that the Deity acts, considering the meanness or impurity of some instinctive operations, and the trifling nature of others.

B. You may well say, not much in this; there is absolutely nothing at all. Our present argument only refers to physical and not to moral considerations. Moral feelings or actions are of course not instinctive at all. There is no blame where there is no choice—no knowledge—no intention—no reason. Then, as to indifferent acts; there is nothing small, or mean, or impure in the Deity's eye. There is nothing in this more than is sometimes, without due consideration, urged against the doctrine of Essential Ubiquity. It all proceeds upon a forgetfulness that the Deity cares as much for one creature as another; all are alike proofs of his wisdom; all alike objects of his favour. So as to matter; there is nothing impure or disgusting, except in relation to our weak and imperfect senses, which are, for wise purposes, so formed as to delight in some things, and to repudiate others. This is all relative, and relative to ourselves and our imperfect nature. To the Deity it can have no application. The structure and functions of the maggot, bred in the most filthy corruption that can disgust our senses, exhibits, even to the eye of the philosopher, how cumbered soever with the mortal coil, as marvellous a spectacle of Divine skill and benevolence as the sanguiferous or the nervous system of the human body, or the form of the most lovely and fragrant flower that blows.

A. I think the instinct of hunger has begun to operate upon my structure, whether stimulated by the operation of the gastric juice upon the coats of the stomach, or how otherwise, I do not stop to inquire. Nor do I apprehend that our good hostess's instinctive love of order and method would approve of our keeping dinner waiting.

B. Your own excellent mother was the pattern of that regularity, as of so many other admirable qualities; and the intercourse of society was in this, as in far more important particulars, greatly reformed by her example. Therefore let us adjourn our further discussion, of which not much remains, till to-morrow, at least not much that is difficult.

BOOK, OR DIALOGUE III.

ANIMAL INTELLIGENCE.—(Facts.)

A. It must be confessed, that for a subject so extremely amusing as well as interesting in a higher view, Instinct has been giving us but little matter of entertainment. I question if any persons ever talked upon it for so many hours without almost a single anecdote, or illustration of any kind from the facts, which are inexhaustible in variety, and every hour present new matter of wonder. Indeed, those ordinarily known are full of interest; and we have been going on with, I think, two, the bee and the solitary wasp, never even casting a look over the rest of this boundless and variegated field.

B. Why truly so; and the reason is plain enough. We had a problem to solve, and we set ourselves to try our hand at it. We assumed that the whole facts resembled those few to which we applied our arguments, or from which we drew our inferences; and our choosing two was quite right and safe—indeed, one rather than two, for we have dwelt more on the solitary wasp than even the bee, because no question could ever be made in her case of training or traditionary instruction. I do not at all repent of having pursued this course; it has prevented digressions and distractions, which would have ensued, had we gone upon the facts at large. We should have been perplexed, sometimes by questions of evidence, sometimes by minute differences of no importance to the argument, sometimes by analogies only calculated to mislead. Our way has been to pitch upon a good example or two, which in some sort embody the subject, as far as matter of fact is concerned—an abstraction of Instinct, as it were, without immaterial particulars—and to confine our reasonings and our illustrations to that. However, there can be no sort of reason, why we should not now reward ourselves with a little of the entertainment which, as you say, so amply belongs to this great subject.

A. The Instincts which we have been considering as our choice examples, especially that of the bee, are certainly the most wonderful of all the animal phenomena. But the cases where sagacity is shewn, and which seem really quite inconsistent with the doctrine that denies brutes all rational faculties, are most frequently cited to raise men's wonder; and as I take it for this reason that we set out with supposing the common animals to be wholly devoid of intelligence, and are astonished to find them sometimes acting as if they had it—while the operations of Instinct being in many brutes above what any degree of intellect can account for, we refer these to a totally different origin.

B. I quite agree with you. Perhaps one need not go much more now into examples of Instinct. None can exceed that of the bee, which has from the beginning of the creation been working, and all over the world working, in the same manner, upon the successful solution of a problem in the higher mathematics, which only the discovery of the differential calculus a century and a half ago could enable any one to solve without great difficulty at all; and which a celebrated mathematician, who was devoted to the ancient geometry, though an adept also in modern analysis, when he solved, conceived that he had gained no small victory for that favourite science by shewing that it could solve this question of maxima and minima.

A. Nevertheless, there are other wonders of a like kind, those which shew Instinct to be as great in manufactures as the honeycomb proves it to excel in architecture. The paper-making of the wasp is of this class. She makes a paper as excellent as any manufacturer at Maidstone; she has been for sixty centuries acquainted with what was only discovered by men between five and six centuries ago—for I think the question raised by Meerman confined the discovery to the years between 1270 and 1302, though afterwards a specimen was produced as early as 1243. Moreover, when some of the more recent improvements, as the lengthening and equalizing the fibres, are considered, it is found that the wasp was all along acquainted with these useful devices also.

B. I have observed, too, in examining her structures, that she makes two kinds of paper, white and brown, the former being fine cambric paper, and the two glued together by an excellent smooth and durable kind of cement. The white paper, I find, takes the ink as well as if it were sized.

A. When stories are told to excite wonder under the head of Instinct, they generally relate not to Instinct, but to the Reason or Intelligence which animals shew. However, there are other wonders of Instinct beside those we have been adverting to.

The uniformity of the operations of animals of the same species everywhere and at all times is remarkable; and the expertness they shew from the first clearly proves that instruction and experience has nothing at all to do with the matter. Bring up a crow under a hen or under any other bird, it makes as exact a crow's nest as if it were born and bred in a rookery.

B. So Maraldi found that a bee an hour old flew off to the proper flowers, and returned in a little time with two pellets of farina, then supposed to be the material for making wax, now known to be used only in making bees breed, since the capital discovery of our John Hunter shewed wax to be, like honey, a secretion of the animal. Nay, before birth too the animal works to an end, and with the same exact uniformity. The inimitable observations of the great Reaumur shew that the chick, in order to break the egg-shell, moves round, chipping with its bill-scale till it has cut off a segment from the shell. It always moves from right to left; and it always cuts off the segment from the big end. There is no such thing as a party of what Gulliver calls "little-endians" in nature. All these singular Instincts, however, regular and uniform though they be, are, when circumstances require it, interfered with by the rational process of adapting the means to the end, and varying those means where the end cannot otherwise be attained. But Instinct is regular and steady in all ordinary circumstances.

A. The vast extent of the works performed by animals, especially by insects, is no less wonderful than their instinctive skill. This arises from their immense numbers and the singular Instinct whereby they always work in concert when gregarious. What can be more astonishing than the work of the termites or white ants, which, in a night, will undermine and eat out into hollow galleries, a solid bed or table, leaving only the outside shell or rind, and soon will make that too disappear!

B. Or the ant-hills in tropical countries, twelve and fifteen feet high, as if men were to make a building the height of the Andes or Himalaya Mountains, when they are vain of having made the little pyramids? But let us go to instances of the other class—of Intelligence.

A. Had we better begin this new discussion by ascertaining whether or not the doctrine of a specific difference between man and the lower animals is well founded; or had we better begin with the facts?

B. I am upon the whole for beginning with the facts; and I should come at once, as we have just been speaking of concerted operations of Instinct, to the case of the beaver, which is, under the head of Intelligence, almost as wonderful as the proceedings of the bee and the ant are under that of Instinct.

A. But before quitting the bee, and the ant, and the wasp, let us just observe their rational acts. They are nearly as notable as their instinctive ones. The bee, upon being interrupted by Huber in her operations, shortened the length of her cells; diminished their diameter; gradually made them pass through a transition from one state to another, as if she was making the instinctive process subservient to the rational; and, in fine, adapted her building to the novel circumstances imposed upon her; making it, in relation to these, what it would have been in relation to the original circumstance if they had continued unaltered. It is found, too, that the ant, beside the wonderful works which she instinctively performs, has the cunning to keep aphides, which she nourishes for the sake of obtaining from them the honey-dew forming her favourite food, as men keep cows for their milk, or bees for their honey.

B. On this discovery of Huber some doubt has lately been thrown; and do not let us trouble ourselves with anything at all apocryphal when the great body of the text is so ample and so pure. But the expeditions of a predatory nature are by all admitted. They resemble some of the worst crimes of the human race; the ants undertake expeditions for the purpose of seizing and carrying off slaves, whom they afterwards hold in subjection to do their work—so that the least significant and the most important of all animals agree together in committing the greatest of crimes—slave-trading.

A. With this material difference, that the ant does not pharisaically pretend to religion and virtue, while we bring upon religion the shame of our crimes by our disgusting hypocrisy. But the wasp, too, shews no little sagacity as well as strength. Dr. Darwin relates an incident, to which he was an eye-witness, of a wasp having caught a fly almost of her own size; she cut off its head and tail and tried to fly away with the body, but finding that, owing to a breeze then blowing, the fly's wings were an impediment to her own flight, and turned her round in the air, she came to the ground and cut off the fly's wings one after the other with her mouth. She then flew away with the body unmolested by the wind.*

B. I have myself observed many instances of similar fertility of resource in bees. But perhaps the old anecdote of the Jackdaw is as good as any—who, when he found his beak could not reach the water he wanted to drink, threw into the pitcher pebble after pebble till he raised the surface of the liquid to the level of his beak. Lord Bacon tells it of a Raven filling up the hollows in a tree where water had settled.

A. Or the Crows of whom Darwin speaks in the north of Ireland, who rise in the air with limpets and muscles, to let them fall on the rocks and break them, that they may come at the fish. It is said that animals never use tools, and Franklin has defined man a tool-making animal; but this is as nearly using tools as may be—at least, it shews the same fertility of resources, the using means towards an end.

B. It does a little more. It shews the highest reach of ingenuity, the using the simplest means to gain your end—the very peculiarity for which Franklin's own genius was so remarkable. He

* Zoonomia, Sec. xvi. 16.

could make an experiment with less apparatus, and conduct his experimental inquiry to a discovery with more ordinary materials, than any other philosopher we ever saw. With an old key, a silk thread, some sealing-wax, and a sheet of paper, he discovered the identity of lightning and electricity. Here we are instituting a harmless comparison between the bird and the sage: but the crow's genius is said once to have come in collision with the head of a philosopher in a less agreeable manner, when, mistaking the bald skull of Anaxagoras for a rock, she let fall the oyster from such a height that it killed him.

A. But there certainly must be allowed to be even nearer approaches to tool-making, or, at least, to the use of tools, among animals. There are many insects which use hollow places, and some which use hollow reeds or stalks for their habitations.

B. Indeed they do; and perhaps the most remarkable of all proofs of animal intelligence is to be found in the nymphæ of Water Moths, which get into straws, and adjust the weight of their case so that it can always float—at least, Mr. Smellie says that when too heavy they add a piece of straw or wood, and when too light a bit of gravel.* If this

* Transactions of Royal Society of Edinburgh, Vol. I., p. 42.

be true, it is impossible to deny great intelligence to this insect.

A. Why should we doubt it? The crow in rising and letting the muscle fall shews as great knowledge of gravitation as the moth in this case.

B. But an old Monkey at Exeter Change, having lost its teeth, used, when nuts were given him, to take a stone in his paw and break them with it. This was a thing seen forty years ago by all who frequented Exeter Change, and Darwin relates it in his Zoonomia. But I must say that he would have shewn himself to be more of a philosopher had he asked the showman how the monkey learned this expedient. It is very possible he may have been taught it, as apes have oftentimes been taught human habits. Buffon, the great adversary of brute intelligence, allows that he had known an Ape who dressed himself in clothes to which he had become habituated, and slept in a bed, pulling up the sheets and blankets to cover him before going to sleep; and he mentions another which sat at table, drank wine out of a glass, used a knife and fork, and wiped them on a table-napkin. All these things, of course, were the consequence of training, and shewed no more sagacity than the feats of dancingdogs and bears, or of the learned pig—unless it were proved that the ape on being taught these manipulations became sensible of their convenience, and voluntarily, and by preference, practised them —a position which no experiments appear to support. Smellie, however, mentions a Cat which, being confined in a room, in order to get out and meet its mate of the other sex, learnt of itself to open the latch of a door; and I knew a Pony in the stable here, that used both to open the latch of the stable, and raise the lid of the corn-chest—things which must have been learnt by himself, from his own observation, for no one is likely to have taught them to him. Nay, it was only the other day that I observed one of the Horses taken in here to grass, in a field through which the avenue runs, open one of the wickets by pressing down the upright bar of the latch, and open it exactly as you or I do.

A. I have known, as most people living in the country have, similar instances, and especially in dogs.

B. But there is one instance of animals catching their prey in a way still more like the tool-making animal. I do not allude merely to the Spider's web, or to the Pelican's use of his large open pouch in fishing; but to an American bird, of which you find a curious account in the *Philadelphia Transactions.** It is called the *neun-tödter* by the Germans, as we should say, the *nine-killer,* and is found to catch grasshoppers and spear them when dead upon twigs where the small birds come on which it feeds; for the grasshoppers themselves it never touches. These are left, generally about nine in number (from whence its name), the whole winter, and they attract the birds of which the animal in question makes its prey. This is really using one creature as a bait, in order thereby to decoy and catch another.

A. It is certainly a singular and curious instance, whether of Instinct or Intelligence. Are there not stories told of apes using a cat or some other animal—I should suppose rather anything than a cat—to get chesnuts out of the fire?—or what else is the origin of the phrase *cat's paw?*

B. Fable, I presume. Many fables have a real origin in fact: this, I suspect, has not. Monkeys, on the contrary, have been used by men to obtain fruit or cocoa-nuts, by pelting them, and their defending themselves with a fire of nuts.

A. That, however, is a plain instance of sagacity

* Vol. IV.

and imitation. They used missiles, as missiles were used against them. Some of our own belligerent measures of retaliation have not always been nearly so judiciously contrived.

B. No: we once, by way of retaliating on Napoleon, helped him; as if the monkeys had pelted themselves, instead of throwing at us. However, an unexceptionable authority, Captain Cook, or at least Captain King, in Cook's last voyage, has a singular instance of sagacity in the use of means, and almost weapons, in Bears. Here you have his account of their mode of hunting: "The wild deer (*barein*) are far too swift for those lumbering sportsmen; so the bear perceives them at a distance by the scent; and, as they herd in low grounds, when he approaches them, he gets upon the adjoining eminence, from whence he rolls down pieces of rock; nor does he quit his ambush, and pursue, until he finds that some have been maimed."*

A. Certainly, such a well-attested fact as this is very important, and worth a thousand stories of lions and jackalls. But you spoke of coming at once to the Beaver, as the parallel to the Bee.

B. Certainly it is, and may be called, in respect of its works, the Bee of quadrupeds, or if you will,

* Cook's Third Voyage, Vol. III., p. 306.

of Intelligent animals, holding among them as high a place as does the Bee among Instinctive creatures. Nevertheless, there may be some doubt raised how far Instinct has a share in his operations. They are of great uniformity: all packs or companions of beavers, and at all times, build the sameshaped structure, and resemble one another closely in matters which are arbitrary, and therefore cannot be considered as the result of experience or reflection—cannot be dictated by circumstances. This, however, opens a question of some difficulty, which, according to the plan we are pursuing, may be left to the end of our discussion, after we shall have gone through the facts. In considering the beaver, I think we shall do well to follow Buffon, as we did upon the ape, because he purposely rejected everything marvellous or doubtful, in the accounts he had received from travellers, and these must have been numerous, for Canada was then a French colony. Those singular animals assemble in bodies of two to four hundred, and choose a convenient station in the lake or the river, having regard to the slope of its banks and their woodiness, but also, no doubt, to the frequency of floods in the water. If it is a lake, or a river that varies little in its level, they build their huts without any further structure, but if the level changes much, they construct a dam or dyke, what we call a breakwater, extending 80 or 100 feet across, and 10 or 12 broad: they thus keep the water nearly of the same height, at least they thus always obtain a sufficient depth of water. They then work in concert on the wood, gnawing the trees and branches to suit their operations. A tree the thickness of a man's body they will soon bring down by gnawing round its base, but on one side merely, and they know so exactly the operation of gravity on it, that they make it fall always across the stream, so as to require no land carriage. It must be observed, in passing, that if they do this the first time they have built, and without any previous experience of falling bodies, the operation must be taken as purely instinctive. They form their cabins so as to contain from 15 to 25 or 30 animals; each cabin has two doors, one to the land, and one to the water, in order that they may either go ashore, or bathe or swim, and sit in the water, which is part of their pleasure, or rather of their amphibious existence. They have in each cabin also a store-house for placing the parts of the shoots on which they feed (for that they make provision against winter is quite certain), and room enough for accommodating their young when brought forth. The cabins are built on piles, so as to be out of the water;

they are neatly plastered with cement, the animals flat and scaly tail being used as a trowel in this operation. They are of sufficient strength to resist, not only the stream and floods to which occasionally they may be exposed, but also severe storms of wind. The beavers choose to work with a kind of earth not soluble in water, and which they mix with clay. Such is the account of those very rational and intelligent proceedings which Buffon, sceptical beyond all men of stories respecting animal reason, sifted out of all he had heard, after rejecting everything that bore the appearance of exaggeration or fancy. He adds, that a single beaver which he had, shewed, in its solitary and domestic state, no signs of sagacity or resources; but rather appeared to be a stupid animal. According to his strange theory, that animals are degenerating in mind, and losing their faculties as man improves (a notion derived from confounding their loss of dominion, power, and numbers, in a wild state, with their loss of intellect),* he considers the beaver as the "only subsisting monument of the ancient intelligence of brutes."

A. They say doubts have of late been cast upon

* Vol. IV., p. 73, and V., p. 21.

the former accounts of the beaver. I am told, Hearne, one of the best North American travellers, is cited for this.

B. Here is what that excellent observer says upon the subject: you shall judge if he has in the least altered the case. The beavers select, he says, either in small lakes or in rivers, spots where the water is of such depths as not to freeze to the bottom, preferring, however, running water, because this helps them to convey the timber they require. They begin by forming a dyke across with fascines, stones, and mud, but without piles buried in the ground; this dyke, whose only use is to give them a convenient level of water, is convex on the upper side fronting the stream; and it becomes solid and strong by repeated repairs, so that the branches sprout, and birds build in the hedge which it forms. Each hut contains commonly one or two, but sometimes four, families; and sometimes each is separated from the others by a partition. The hut has a door opening on the water, and no connexion with the land. He then goes on to shew how they cut down and build, wherein he differs from the common accounts only in saying that no piles are used in the construction. They work, he says, only by night, and each season they cover the buildings with a new coat of mud-plaster, as soon as the frost sets in. In summer they make excursions in the woods, choosing the trees they mean to make use of, and marking the position of new settlements, when their increase of numbers require them to plant colonies. Their wood-cutting begins at the end of summer, and the building is carried on in autumn. They have also subterraneous retreats along the banks of the river or lake, to serve as a place of refuge when they may be attacked by the glutton. You perceive, then, that there is very little discrepancy between this account and Buffon's; indeed, there is one remarkable addition to the latter, if it can be relied upon, the precaution taken in summer to choose and to mark out the convenient stations where the new settlements are afterwards to be made.

A. There seems reason to suppose that other animals still preserve their sagacity and act in concert. No one can have observed a flock of pigeons without perceiving that they have sentinels posted to give the alarm. Indeed, wilder birds act in like manner. Fieldfares, when they are occupying a tree which you approach, remain steady and fear-less until one at the extremity rises on her wings and gives a loud and very peculiar note of alarm, when they all get up and fly, except one who continues till you get near, as if she remained to see that there really was occasion for the movement, and to call them back if the alarm proved a false one. She too at length flies off repeating the alarm-note.

B. In the forests of Tartary and of South America, where the Wild Horse is gregarious, there are herds of 500 or 600, which, being ill prepared for fighting, or indeed for any resistance, and knowing that their safety is in flight, when they sleep, appoint one in rotation who acts as sentinel, while the rest are asleep. If a man approaches, the sentinel walks towards him as if to reconnoitre or see whether he may be deterred from coming near—if the man continues, he neighs aloud and in a peculiar tone, which rouses the herd and all gallop away, the sentinel bringing up the rear. Nothing can be more judicious or rational than this arrangement, simple as it is. So a horse, belonging to a smuggler at Dover, used to be laden with run spirits and sent on the road unattended to reach the rendezvous. When he descried a soldier he would jump off the highway and hide himself in a ditch, and when discovered would fight for his load. The cunning of Foxes is proverbial; but I know not if it was ever more remarkably displayed than in the Duke of Beaufort's country; where Reynard, being hard pressed, disappeared suddenly, and was, after strict search, found immersed in a water-pool up to the very snout, by which he held a willow-bough hanging over the pond. The cunning of a Dog, which Serjeant Wilde tells me of, as known to him, is at least equal. He used to be tied up as a precaution against hunting sheep. At night he slipped his head out of the collar, and returning before dawn put on the collar again, in order to conceal his nocturnal excursion. Nobody has more familiarity with various animals (beside his great knowledge of his own species) than my excellent, learned, and ingenious friend, the Serjeant; and he possesses many curious ones himself. His anecdote of a drover's dog is striking, as he gave it me, when we happened, near this place, to meet a drove. The man had brought 17 out of 20 oxen from a field, leaving the remaining three there mixed with another herd. He then said to the dog "Go, fetch them;" and he went and singled out those very three. The Serjeant's brother, however, a highly respectable man, lately Sheriff of London, has a dog that distinguishes Saturday night, from the practice of tying him up for the Sunday, which he dislikes. He will escape on Saturday night and return on Monday morning. The Serjeant himself had a gander which was at a distance from the goose, and hearing her make an extraordinary noise, ran back and put his head into the cage—then brought back all the goslings one by one and put them into it with the mother, whose separation from her brood had occasioned her clamour. He then returned to the place whence her cries had called him. I must however add, that I often have conversed with Scotch shepherds coming up from the Border country to our great fairs, and have found them deny many of the stories of the miraculous feats of sheep-dogs. Alfred Montgomery and I, the other day, cross-questioned a Roxburghshire shepherd with this result.

A. Many of the feats which we are now ascribing to intellectual faculties may be instinctive operations. How shall we distinguish?

B. The rule seems simple. Where the act is done in ordinary and natural circumstances, it may be called instinctive or not, according as it is what our reason could, in the like circumstances, enable us to perform or not, and according as the animal is in a situation which enables him to act knowingly or not. Thus a bee's cell is made by a creature untaught; a solitary wasp provides food for an offspring it never can see, and knows nothing of. We set these things down to Instinct. If horses fearing danger, appoint a sentinel, it may be Instinct certainly, but there is here nothing to exclude Intelligence, for they do a thing which they may well do by design, and so differ from the bee; they are aware of the object in view, and mean to attain it, and so differ from the wasp. But these remarks apply to acts done in ordinary circumstances, and which I admit may or may not be instinctive. Another class is clearly rather to be called rational. I mean where the means are varied, adapted, and adjusted to a varying object, or where the animal acts in artificial circumstances in any way. For example, the horse opening a stable-door, the cat a room-door, the daw filling a pitcher with stones. So there is a singular story told by Dupont de Nemours in Autun's *Animaux Célèbres,* and which he says he witnessed himself. A Swallow had slipped its foot into the noose of a cord attached to a spout in the College des Quatre Nations at Paris, and by endeavouring to escape had drawn the knot tight. Its strength being exhausted in vain attempts to fly, it uttered piteous cries, which assembled a vast flock of other swallows from the large basin between the Tuilleries and Pont Neuf. They seemed to crowd and consult together for a little while, and then one of them darted at the string and struck at it with his beak as he flew past; and others following in quick succession did the same, striking at the same part, till after continuing this combined operation for half an hour, they succeeded in severing the cord and freeing their companion. They all continued flocking and hovering till night; only, instead of the tumult and agitation in which they had been at their first assembling, they were chattering as if without any anxiety at all, but conscious of having succeeded.

A. The means taken to escape from danger, and to provide for security, are certainly often of this description, the danger being often of a kind purely accidental, and solitary, and the operation of the animal varying in different and new circumstances. Some birds wholly change their mode of building to avoid snakes, hanging their nests to the end of branches, and making the exit in the bottom, in places where those reptiles abound.

B. So too, the Ants in Siam make no nests on the ground, as with us, but on trees, that country being much subject to inundations. But you find this change of habits in animals, upon circumstances changing, pretty general. The Dogs which the Spaniards left in the island of Juan Fernandez were found to have lost the habit of barking, when Juan and D'Ulloa visited that famous spot in the course of their journey in South America. Possibly they found that barking warned their prey, and enabled it to escape. But Dogs in Guinea howl and do not bark, and when European dogs are taken there they lose their bark in three or four generations. This fact, then, is somewhat equivocal.

A. The docility of some animals may, however, as it seems to me, be strictly ranged within the class of facts we are speaking of. Although children, as well as animals, learn through fear and kindness, both operating (and fear alone would suffice), yet it is an act of Intelligence to follow the dictates of both feelings: it implies this process of reasoning,—"If I do so and so, I shall have such a punishment or such a reward." Now the degree to which animals are teachable is wonderful. All Singing-Birds probably learn their whole notes.

B. Yes; Daines Barrington has demonstrated this by numerous experiments* on various birds; the young untaught birds, being placed in the nests of different species of birds, always had the song of those it nestled with; and we all know how a Piping Bullfinch can be taught almost any tune. They seem to have no notion of harmony or melody. I recollect a Green Linnet, which I had when a boy, or rather a mongrel between that and a goldfinch, being placed in a kitchen, and leaving its own fine and sweet notes, to take to an imitation, and a very good and exceedingly discordant one, of a jack which, being ill-constructed, generally squeaked as if it wanted oiling.

A. Dogs shew the greatest talents in learning. The feats of pointers, but still more of shepherds' dogs, after making all the deductions you have mentioned, are astonishing. It almost seems as if the shepherd could communicate, by sign or by speech, his meaning, when he desires to have a particular thing done. But assuredly the dog takes his precautions exactly as he ought, to prevent the sheep from scattering, and to bring back runaways. Indeed, Greyhounds and other dogs of chace, as well as Pointers backing one another, shew the adapta-

* Phil. Trans., 1776.

tion of, and variation in, the means used towards an end.

B. Retrievers exceed all other dogs in this respect. There was one died here a year or two ago that could be left to watch game, till the keeper went to a given place, and she would then join him after he had ranged the field; nay, could be sent to a spot where game had been left, and where she had not been before. Indeed, she did many other things which I have hardly courage to relate.

A. How were her pups? I have always found such extraordinary faculties hereditary.

B. My worthy, intelligent, and lamented friend, T. A. Knight (so long President of the Horticultural Society), has proved very clearly that the faculties of animals are hereditary to such a point as this. He shews that even their acquired faculties—the expertness they gain by teaching—descends in the race. His paper is exceedingly curious. But I think we need hardly go so far as to his minute details for proof of the fact. It is found that where man has not been, no animals are wild and run away from his approach. When Bougainville went to the Falkland Islands (or, as the French call them, the Malouines), he found himself and his men immediately surrounded by all kinds of beasts and birds, the latter settling on their shoulders. No navigators had ever been there before. Lord Monboddo says that the same thing had been related to him by navigators.* It seems clear, then, that the running away from man, which seems natural to all wild animals in, or bordering upon, inhabited countries, is an acquired propensity, transmitted to the descendants of those whose experience first taught it them as necessary for their safety.

A. Have you Knight's paper here? I know the accuracy of his observation to equal his great ingenuity.

B. To that I too can bear my testimony. Here is his principal paper, read lately before the Royal Society. It is given as the result of his observations and experiments, made for a period of 60 years; it is therefore most justly entitled to great respect. He chiefly dwells on the case of Springing Spaniels, and among other instances gives this, which is indeed very remarkable. He found the young and untaught ones as skilful as the old ones, not only in finding and raising the woodcocks, but in knowing the exact degree of frost which will drive

* Origin of Language, B. II., Ch. 2.

those birds to springs and rills of unfrozen water. He gives the instance, too, of a young retriever, bred from a clever and thoroughly-taught parent, which, being taken out at ten months old, with hardly any instruction at all, behaved as well and knowingly as the best taught spaniel, in rushing into the water for game that was shot, when pointed out to it, however small, bringing it, and depositing it, and then going again, and when none remained, seeking the sportsman and keeping by him. He imported some Norwegian ponies, mares, and had a breed from them. It was found that the produce "had no mouth" as the trainers say; and it was impossible to give it them; but they were otherwise perfectly docile. Now in Norway, draught horses, as I know, having travelled there and driven them, are all trained to go by the voice, and have no mouth.—Again, he observed that they could not be kept between hedges, but walked deliberately through them—there being, he supposes, none in the country from which their dams came.

A. Does he speak of any other animal?

B. Yes, he mentions his observation on Woodcocks, which he could remember having been far less wild half a century ago; for on its first arrival in autumn, it was tame and chuckled about if disturbed, making but a very short flight, whereas now, and for many years past, it is very wild, running in silence and flying far. He gives an instance of sagacity in a Dog, unconnected with hereditary intelligence. He one day had gone out with his gun and a servant, but no dog. Seeing a cock, he sent the servant who brought this spaniel. A month afterwards he again sent for the same dog from the same place. The servant was bringing him, when at twenty yards from the house, the spaniel left him, and ran away to the spot, though it was above a mile distant. This he often repeated and always with the same result; as if the animal knew what he was wanted for. Leonard Edmunds tells me of a dog (a Newfoundland spaniel) of Mr. Morritt's, at Rokeby, which has been known to take the shorter road to where he knew he was wanted, and leave the servant or keeper to go round about. You yourself told me of a dog that met you sporting by a short cut unknown to you.

A. The manner in which animals can find their way is very extraordinary. But though, in many cases, it may be through close observation, and observation the clearer and better remembered because, like the Indian woodsmen, they have so few ideas; yet, in other cases, it seems an Instinct very difficult to conceive in its workings. In truth, if the stories told be true, I question if any instance we have yet examined of Instinct be so truly unaccountable on any principles of intelligence. I have known of dogs sent to a distance, and coming home immediately, though taken in the dark.

B. That might be from smell or track, but stories are also told of dogs and cats taken in hampers, and finding their way back speedily. L. Edmunds had one that was carried from Ambleside to three miles on the other side of Burton, a distance of twenty-seven miles, in a close hamper, by a coach; and it found its way back next morning. Dr. Beattie's account of a dog which was carried in a basket thirty miles distance, through a country he never had seen, and returned home in a week, is less singular than this, even if it were as well authenticated. Dr. Hancock, in his excellent work on Instinct, which, however, contains fully as much upon the peculiar tenets of the Society of Friends as upon our subject, relates the story of a dog being conveyed from Scotland to London by sea, and finding his way back; of a Sheep returning from Yorkshire to Annandale, a distance of at least eighty miles; and of another Sheep returning from Perthshire to the neighbourhood of Edinburgh. Kirby and Spence, too, in their *Introduction to Entomology,* state, on the authority of a captain in the Navy, a strange anecdote of an Ass taken from Gibraltar to Cape de Gat on board of ship, and finding its way immediately back through Spain to the garrison, a distance of two hundred miles of very difficult country. The ass had swam on shore when the ship was stranded. This fact seems to be well-authenticated, for all the names are given, and the dates.

A. There is no end of such facts, and many of them seem sufficiently vouched. The *Letters on Instinct* mention a cat which had been taken to the West Indies, and on the ship returning to the Port of London she found her way through the city to Brompton, whence she had been brought.

B. That is a work I have often wished to see, and never been able to get. Dr. Hancock quotes it for one of the most remarkable proofs of sagacity and resource in the Goat, and this operation has been, it seems, observed more than once. When two Goats meet on a ledge bordering upon a precipice, and find there is no room either to pass each other or to return, after a pause, as if for reflection, one crouches down and the other walks gently over his back, when each continues his perilous journey along the narrow path.

A. In Rees's Cyclopædia a story is given as well vouched, of a cat that had been brought up in amity with a bird, and being one day observed to seize suddenly hold of the latter, which happened to be perched out of its cage, on examining, it was found that a stray cat had got into the room, and that this alarming step was a manœuvre to save the bird till the intruder should depart. But what do you make of carrier-pigeons? The facts are perhaps not well ascertained; there being a good deal of mystery and other quackery about the training of them.

B. I desired one of the trainers (they are Spitalfields weavers generally) to come, that I might examine him about his art, but he has never been with me. I have read and considered a report made to me on the subject. It is said the bird begins his flight by making circles, which increase more and more in diameter as he rises; and that he thus pilots himself towards his ground. But still this indicates an extraordinary power of observation; for they come from Brussels to London and return.

Nay, they have been known to fly from the Rhine to Paris. Serjeant Wilde took pigeons of the Rock kind to Hounslow, and they flew back to Guildford-street in an hour. They were taken in a bag, and could see or smell nothing by the way. On being let loose, they made two or three wide circles, and then flew straight to their dove-cot. The Serjeant also knew of a cat which a shopkeeper's apprentice in Fore-street had been desired to hang, and found he could not. He then took it in a bag to Blackfriars Bridge and threw it into the river—the cat was at home in Fore-street as soon as the apprentice. He might have made a circuit, but certainly the cat returned in an hour or two. The grocer's name was Gardner—the distance is certainly above a mile, and through the most crowded part of London. The case of bees is referable to Instinct clearly. Honey-finders in America trace their nests by catching two bees, carrying them to a distance, and letting them fly. Each takes the straight line towards the nest or hive, and by noting these two lines, and finding where they intersect each other, the hive is found. Now the bee is known to have a very confined sphere of vision, from the extremely convex form of her eye. She is supposed only to see a yard or so before her.

A. I fancy we must pass over the subject of migration for a like reason. It seems still involved in much obscurity and doubt, though I take for granted that no one now yields to Daines Barrington's theory, which denies it altogether.

B. Clearly no one; the facts are quite indisputable as far as negativing that goes; and indeed his reasonings are so full of prejudice, or preconceived opinion, and his suppositions for disposing of the facts so strained, that his argument never could have had much weight. One fact seems also not to be disputed, and is referable to Instinct alone. I mean the agitation which, without any cause, comes on upon a bird of any of the migratory classes at the appointed season of migration. It is, in all probability, connected with the sexual impulses.

A. The communication with each other, which animals have by sounds or signs, can, I think, hardly be doubted.

B. The observations of Huber clearly shew that ants have a kind of language by means of their feelers or antennæ; and every day's experience seems to shew this in other animals.

A. Some believe that they have a notion of what men are saying, and no doubt very strange and lucky guesses have sometimes been made, one of which I wrote you an account of. I had it from a most accurate and literal person, and it tends to prove that his shooting dogs had found out his intention of going into Nottinghamshire the day after. However, it is perfectly clear that these things are referable to minute and exact observation of things which escape us in the greater multitude of our ideas and concerns. All this, however, only illustrates the more how well animals can profit by experience, and draw correct inferences from things observed by them.

B. Among other instances referable plainly to intelligence must be ranked the devices which one animal is known to fall upon, for benefiting by another's operations. The ant enslaving workers is the most curious instance certainly. But the cuckoo laying in other birds' nests, and leaving her progeny to be brought up by them, is another. Nor can this be set down wholly to the score of Instinct; for there are abundant proofs of her also building when she cannot find a nest, and then she lays in her own, and hatches and rears her brood.

This curious and important fact, long disbelieved by vulgar prejudice, was known to that great observer, Aristotle, who says she sometimes builds among rocks, and on heights.* Darwin confirms this by the observations of two intelligent friends whom he cites.† The man-of-war bird is a still more singular instance of contrivance, for though its food is fish, it has not such a form as to be fit for catching any, and therefore it lives piratically on the prey made by other fishing birds; hence the name we have given it.

A. Only think of our never having all this while said a word, or more than a word, of either the Fox or the Elephant, proverbially the two wisest of animals. Of the former's cunning every day shews instances; but that the elephant should be left to take care of a child unable to walk, and should let it crawl as far as his own chain, and then gently lift it with his trunk and replace it in safety, seems really an extraordinary effect of both intelligence and care, and shews that fine animal's gentle nature, of which so many anecdotes are told by travellers in the East.

B. The amiable qualities of brutes are not quite

* Lib. VI., c. 1. † Zoonomia, Vol. XVI., p. 13.

within the scope of our discussion, unless indeed in so far as whatever things are lovely may also be said to betoken wisdom, or at least, reflection. The natural love of their offspring I should hardly cite in proof of this, because it seems rather an instinctive feeling. But the attachments formed between animals of different classes, a cat and a horse, a dog and a man, and often between two elderly birds, may be cited as interesting. One of these friends has been known to be unable to survive the other. I have heard this of two old parrots, upon the best authority.

A. We have said nothing of fishes, or of any marine animals.

B. Why of these our knowledge is necessarily very limited. That they have remarkable Instincts, some of them resembling those of land animals, is certain. The Sepia, or cuttle-fish, ejecting a black or dark-brown fluid to facilitate his escape, resembles the stratagem of some beasts emitting an intolerable effluvia in the face of their pursuers. The Whale, when attacked by the Sword-fish, diving to such a depth that his enemy cannot sustain the pressure of the water, is another well-known example of defensive action. I used to observe with interest the wary cunning of the old Carp in the ponds here: there was no decoying them with bait, which the younger and less experienced fish took at once. So little have men formerly undervalued the faculties of fishes, that Plutarch wrote an ingenious treatise in the form of a dialogue, on the question whether land or water animals have the most understanding.

A. How does he treat this odd question?

B. Here is his book; and certainly as far as the first portion of the subject goes, where the merits of land animals are concerned, he sails before the wind. To his first remark I willingly subscribe, that those hold the most stupid doctrine upon the subject (οἱ αβελτερως λεγοντες) who say that animals do not really fear, rejoice, remember, rage, &c., but only do something like fearing, rejoicing, &c. (ὡσανεῖ φοβεισθαι, &c.); and he asks what such reasoners would think were it also contended that animals do not see, but make as if they saw; nor hear, but make as if they heard; nor roar, but make as if they roared; and, finally, do not live, but only did something like living. He then relates a great variety of facts respecting the sagacity of animals, some of them evidently fabulous (as the love of a dragon for a young woman), and some, as the account of the ant laying in grain, now proved to be erroneous; but he gives others worthy of attending to. Thus, the contrivance of African crows, who, when the water was scarce, threw pebbles into deep cracks of the earth, so as to bring the fluid up towards the surface, and within their reach—the similar cunning of a dog on board of a vessel—the like device fallen upon by elephants to rescue one that had fallen into a pit—the astuteness of the fox, used by the Thracians as a kind of guide in crossing a river frozen over, to find out whether the ice is thick enough, which the animal does by stopping and listening to hear if the water is running near the surface—the judicious mode of flight in which cranes and other birds of passage marshal themselves, forming a wedge-like body, with the strongest birds at the front angle or point. But when he comes to the other side of the question, and is to state the case for the fishes, we find a great falling off both in his facts and in his evidence. Beside telling very absurd stories about crocodiles in Egypt obeying the call of the priests and submitting to their influence, he dwells upon the Sepia, whose escape in a black cloud of his own making he compares to the tactics of Homer's gods; upon the cunning shewn by fishes in gnawing lines to escape with the hook; nay, upon a story he tells of their helping one another to escape when caught, which is plainly groundless; upon the Torpedo, or electrical eel, giving shocks, which is clearly a mere physical quality, and no more indicates reason than the shark using his teeth; upon shoals of fishes, like flocks of birds, forming themselves into wedges when they move from one sea to another, which is certainly true; upon the dolphin loving music, which is purely fabulous, as well as the feats of wisdom and philanthropy that he ascribes to this fish (μονος γαρ ἀνθρωπον ἀσπαζεται καθο ἀνθρωπος εστι); finally, upon all the fables to be found in the poets respecting this fish. After reciting one of these, by way of proving his case in favour of marine animals, he innocently enough says that although he had promised to relate no fables, he now finds himself, he knows not how, in the company of Cæranus and Ulysses, and so he brings his notable argument to a close.

A. How does he ultimately decide the question propounded?

B. With a verse of Sophocles, intimating that both sides have gained some advantage towards a common purpose; but the victory is given to neither, the umpire pronouncing that both the arguments combined overthrow the doctrine of those who deny Reason and Intelligence to animals generally.

A. There are no modern books which fully discuss this subject systematically, either as regards Instinct or Intelligence. One is exceedingly disappointed in consulting our best writers, whether metaphysicians or naturalists, with this view; and the omission is the less to be excused because there are great opportunities of observing and comparing; this branch of knowledge is eminently suited to inductive reasoning; we live as it were among the facts, and have not only constant facilities for making our experiments, but are in some sort under a constant necessity of doing so.

B. Truly it is as you say. I have often felt this disappointment and this disapprobation. The works of metaphysical writers contain a few scattered suggestions, or dogmas, and with these they leave the subject. Naturalists, who could throw so much light upon it, confine themselves chiefly to the structure and functions of the organs, and leave the mental part of the subject out of view. Yet a physiologist, who also applied himself to this latter branch of the inquiry, would be the person best qualified to grapple with its difficulties and to throw light upon it. Therefore I learnt with extreme satisfaction that an able and learned professor of Natural History had given a course of lectures upon it at Paris, and was still more gratified to find that he soon afterwards published them. I speak of M. Virey's work; those two thick volumes lying there contain above a thousand pages on the Habits and Instinct of Animals; and to raise my expectation still higher, it professes by its title to deal in facts—for it is called *Histoire des Mœurs et de l'Instinct des Animaux.*

A. Well; I suppose you rushed upon it to slake your thirst?

B. As a traveller upon a delicious and copious spring, and found it a picture; or upon a luscious-looking large peach, and found my mouth filled with chalk. I have had these volumes here these two years, and I can barely now say I have been able to get through them. They are throughout not only written in the very worst style of French sentimental declamation, but they avoid all precision, all details, all facts, as something grovelling, common-place, and unimportant. The constant object is not to find out or illustrate some truth, to describe or arrange some phœnomenon, but to say something pretty, far-fetched, and figurative. And all this with an arrangement of the classes of animals so methodical, that on looking at the contents, and finding they proceed regularly from the structure of the globe and the general qualities of its different products, to mammalia, then to birds, reptiles, fishes, and so downwards through the invertebrated animals, ending with zoophytes and molluscæ, you naturally expect under each head to have what the title promises, a History of the Habits and Instincts; and find nothing of the kind from beginning to end, but only trope after trope, one piece of finery after another, nothing but vague declamation long drawn out, an endless succession of the most frivolous sentimentality. Truly such a work, from so learned a naturalist, one who could so well have instructed and entertained us, had he but chosen to be plain and didactic, instead of being brilliant and rhetorical, where all eloquence and ornament are absolutely misplaced, is no small offence in the literary world.

A. I'll assure you our French neighbours are not the only sinners in this particular. I have been somewhat mortified of late years, at perceiving a tendency to fine writing and declamation among our own men of science, and I ascribe it, in some degree, to the more general diffusion of scientific knowledge, which naturally introduces the more popular style of composition. Our Society of Useful Knowledge has no sins of this sort on its conscience, because we correct with unsparing severity all we publish; but you may perceive the tendency of popular explication to run in this bad direction, from the kind of matter that is often submitted to us for revision. I am sure I sometimes draw my pen through half a page of fine writing at a time.

B. I will engage for it you do inexorably whenever you find such outrages. My experience is precisely the same; and I am just as severe on those parts, evidently the prime favourites of the learned and very able writers. But we originally set out with firmly resolving to be most rigorous in matters of taste, being aware, as you say, of the tendencies of popular writers. In truth, however, that vile florid style darkens instead of illustrating; and while we never can write too clearly to the people, we never can write too simply, if our design be to write plainly and intelligibly. But though our Society is free from having any of this blame, I cannot quite acquit of all blame the meetings, however useful and praiseworthy in other respects, of an association which brings crowds of hundreds and thousands together, to hear mathematicians and chemists making declamatory speeches. I must say that those assemblages offer some violence to Science, at least they somewhat lower her by shewing her cultivators trying a trade they no more can, or even ought to excel in, than poets in solving questions of fluxions. It is since these meetings, otherwise useful and excellent, rose into eloquence, that I have seen a mathematical discussion, by a very able and learned man, in two consecutive pages of which I reckoned up above twenty metaphors—all tending to darken the subject—to say nothing of poetical quotations without any mercy. Formerly declamations were reckoned so little an accomplishment of scientific men, that when Bishop Horsley filled our Royal Society with a factious controversy, the ministerial side, Sir Joseph Bankes's party, had to send for assistance—and where think you they went for an orator?

A. I suppose to some Nisi Prius advocate.

B. Guess again.—No!—So humble were their views of oratory, that they went to the other side of the hall, as the lawyers say, and got for their champion, Mr. Anguish, who was Accountant-General, a Chancery man, and had perhaps made as few speeches as any one in that Court. But in the work which I have referred to, and even in those scientific meetings, there is at least much that is highly valuable, much good grain, and the trash may be rejected as chaff—whereas, in this piece of French declamation all is chaff, and hardly a grain can be gleaned out of the light and worthless matter.

A. Can you find nothing by sifting and bolting it? I generally find something even in the worst books.

B. I will not say that these heavy volumes of light matter contain absolutely nothing; but wondrous little assuredly they have to reward the pains of searching. What can be more hateful than a man of science unable to speak of granivorous animals without terming them Pythagoreans and Gymnosophists; calling the crying baboon of South America a wild Demosthenes, the lion a generous prince, the jackall a courtier; describing the nightingale as appealing to Heaven against the robber of her nest, and the crocodiles as the "sad orphans of nature," because hatched in the sand; nay, carrying his ridiculous fancies into actual practice, seriously explaining the mild temper of one animal by the sweetness of its humours, and the ferocity of another by the acrid juices of its system—all a pure fiction in fact as well as a gross absurdity in theory! Then mark the consistency of a philosopher—a consistency worthy of the veriest mob. He denounces, as the most atrocious of men, the experimenter on a living dog or rabbit, Fontana, or Majendie, I suppose, and afterwards speaks with the utmost composure of dividing a bee in two, in order to examine her honey-bag. Of the bee, indeed, he seems very moderately informed. He speaks of Aristarchus having devoted his life to the study of this insect, instead of Aristomachus; assumes to be true the notion long exploded of honey being collected from flowers, instead of a secretion in the stomach; will not believe that wax, too, is a secretion, though he refers unconsciously to Huber's experiment, of obtaining it from bees feeding upon sugar and water; and, to set off his modern natural history with a little false classical lore, must needs call the cells "their citadel, or the palladium of their republic."

A. Bad enough in all conscience. But now give us the grain or two of wheat in all this bushel of chaff.

B. First, and this makes it more provoking, the author writes clearly and admirably when he chooses to leave off declaiming. There is a long note upon vertebrated and invertebrated animals, shewing, with much clearness and precision, that in the former, which have a cerebral and nervous system, Intelligence prevails; in the latter Instinct. He maintains the specific difference of Instinct and Reason or Intelligence, with great force and clearness; indeed, there seems nothing to find fault with in his statements here except that he places the seat of Intelligence in the cerebral nervous system, and of Instinct in the ganglionic, and thus is forced to deny Intelligence altogether to insects, whereas we have seen that Huber's observations plainly shew the bee to have the capacity of varying its means in accomplishing the end in view, when the circumstances vary; and this surely cannot be distinguished from Intelligence. Also he discusses, with perfect strictness of reasoning, the hypothesis of a very celebrated naturalist, no less than M. Lamarck, and, I must say, refutes very satisfactorily the theory of my most learned and worthy colleague, for whom we all must feel the most profound respect. He had been induced to suppose that Instinct results from the habits originally acquired by animals adapted to the circumstances in which they found themselves placed at the beginning of the creation, and that these habits occasioned an adaptation of their structure to particular operations, as well as a constant capacity and desire to perform them. Now, my only objection to M. Virey's refutation of this theory, which is merely the exploded doctrine of appetencies in a new form, is, that it requires no such elaborate answer to overthrow it. For what do we see in all nature which in the least entitles us to suppose any animal at any period to have had the power of altering his bodily structure, creating one part and altering another according to his wants? Besides, if animals, at their first creation, had so much power and so much intelligence as this theory supposes, why should this all cease and leave them only possessed of blind Instincts now? The reasoning, however, of M. Virey is sound, and does much credit to his acuteness.

A. But have you found, in his volumes, no facts; nothing to place among the phenomena which we are collecting previous to resuming our discussion respecting the faculties of brutes?

B. Very little; and that so wrapped up in declamation, and so disfigured with figures (if I may thus speak), that there is no small difficulty in seizing hold of it. What he says of the architecture of squirrels, marmots, rats, and some other rodents, is new to me. I had only been aware of the beaver, as among this tribe, remarkable for ingenuity. But it seems these others excel all animals in digging subterranean dwellings; they make compartments or chambers, which they line with clay, and cover with a roof from the weather; in some of these chambers they stow vegetables, which they previously dry in the sun; others they use for the reception of their young; in others they sleep. He brings together some curious instances of swift and long-sustained flights of birds. Thus the smallest bird, he says, can fly several leagues in an hour; the hawk goes commonly at the rate of a league in four minutes, or above forty miles an hour. A falcon of Henry II. was flown from Fontainbleau and found, by its ring, at Malta next day. One, sent from the Canaries to Andalusia, returned to Teneriffe in sixteen hours, a distance of near seven hundred miles, which it must have gone at the average rate of twenty-four miles an hour. Gulls go seven hundred miles out to sea and return daily;

and Frigate birds have been found at twelve hundred miles from any land. Upon their migration he states, as a known fact, that Cranes go and return at the same date, without the least regard to the state of the weather, which shews no doubt, if true, a most peculiar instinct; but these, and, indeed, all facts which we find stated by a writer so addicted to painting and colouring, must be received with a degree of suspicion, for which no one but M. Virey is to be blamed. The accounts, however, of the swiftness of birds, I can well credit, from an experiment which I made when travelling on a railway. While going at the rate of thirty miles an hour, I let fly a bee; it made its circles as usual, and surrounded us easily. Now, if there was no current of air or draught to bear it along, this indicated a rate of ninety miles an hour; and even allowing for a current, the swiftness must have been great. I should, however, wish to repeat this experiment before being quite sure of so great a swiftness in so small an insect.

A. Have you given all your gleanings from this work?

B. I should, perhaps, add these two. We find in it a curious passage from an old Spanish author of the 17th century, giving a quaint and lively account of the sagacity of the beggars' dogs at Rome; and we also find the titles of some German works on the faculties of brutes, which are truly curious, and shew how great a degree of attention that laborious people have paid to the subject; but, at the same time, betray not a little of the characteristic boldness and enthusiasm of their speculations.

A. I conclude you have never seen more than these titles in this book?

B. Never; and I really should wish to see the works themselves. One is *Mayer de peccatis et pœnis Brutorum*, 1686, in quarto. Another, in 1725, *Hermanson de peccatis Brutorum;* this, however, is printed at Upsal. A third is *Schrœder de Simulaeris virtutum in Brutis Animantibus,* 1691; and a fourth, *Schrœder de Brutorum Religione,* 1702. Then, it appears that one Drechsler wrote, in 1672, a *Dissertation on the Speech of Animals,* and Meyer and Martin, not to be outdone, followed this up a few years after, the one with a *Treatise on the Logic of Animals,* and another with one, *De Animalium Syllogismo.*

A. Does the Spaniard give any curious particulars of dogs?

B. Not perhaps any that surpass what we have been stating from facts known among ourselves. But his account is diverting enough. "The blind man's dog," says he, "will take him to the places where he may best hope to get his alms, and bring him thither through the crowd by the shortest way and the safest; nay, he will take him out of the city some miles to the great church of St. Paul, as you go to Ostia. When in the town he cometh to a place where several ways meet, and with the sharpness of ear that the blind have, guided by some sound of a fountain, he gives the string a jerk by either hand, straightway will the poor dog turn and guide him to the very church where he knows his master would beg. In the street, too, knoweth he the charitably-disposed houses that be therein, and will lead thither the beggar-man, who, stopping at one, saith his pater-noster; then down lieth the dog till he hear the last word of the beadsman, when straight he riseth and away to another house. I have seen myself, to my great joy, mingled with admiration, when a piece of money was thrown down from some window, the dog would run and pick it up and fetch it to the master's hat; nor, when bread is flung down, will he touch it be he ever so hungry, but bring it to his master, and wait till he may have his share given him. A friend of mine was wont to come to my dwelling with a great mastiff, which he left by the door on entering; but he, seeing that his master had entered after drawing the string of the bell, would needs do likewise, and so made those within open the door, as though some one should have rung thereat."

A. Upon my word, you have been amusing yourself with making the old Castilian speak in old English.—But now, I think, we may be said to have gone at sufficient length into the facts, and to have gathered together a collection large enough for our purposes of speculation—nor have we perhaps much more to do with this in that way. For can any one rationally doubt, that they evince in these brutes some faculties at least approaching in kind to our own —nay, and to such of our own as we are wont to prize the most, and to be the proudest of? No blind impulse of a mechanical kind, no mere instinct, or feeling, or operative principle, apart from knowledge, experience, learning, even intention,—can surely account for the things we have just been considering as done by animals—and one example, and an ordinary one, is as good as a thousand. The cat opening a door from observing men do so before it; or the bird, from its own observation of the effect produced by solid bodies, sunk in water, raising the water by throwing in pebbles; or letting muscles fall to break the shells—these things surely argue a thinking and a reasoning process.

B. There seems little doubt of this; however, we may perhaps adjourn the further discussion, as we no longer require to be among our books, but may take our walk out in the sun, which is far from disagreeably hot to day.

A. I have no kind of objection, and will meet you on the Terrace as soon as I have written my letters.

OF INTELLIGENCE.

BOOK, OR DIALOGUE IV.

INTELLIGENCE.—(THEORY.)

WE accordingly finished our letters, and prepared to go out and walk about in the sunny exposure, which a north-west wind made agreeable, as in the north it often does, even at this season—"calceis et vestimentis sumptis, placitum est ut in aprico maxime patente loco conveneremus:"*—where, as we walked about, he began in continuation of his last remark.

A. I know not why so much unwillingness should be shown by some excellent philosophers to allow intelligent faculties, and a share of reason, to the lower animals, as if our own superiority was not quite sufficiently established, to leave all question

* Cic. De Repub. lib. i. cap. 12.

of jealousy out of view, by the immeasurably higher place which we occupy in the scale of being, even should we admit the difference to be in degree rather than in kind; because when the difference of degree becomes so vast, there is hardly any more chance of encroachment or confusion, hardly any more likeness or comparison, than if the difference were radical and in kind. Some writers, as D. Stewart, really seem to treat the question as one of an exciting nature, and almost to regard the purity of religious belief as involved in the controversy. How is this, and why should it be?

B. It is possible that the origin of the feelings shown by those good and able men, resembles that of Descartes' absurd theory, of brutes being like machines, which, as far as he holds it, he avows to have proceeded from the notion that unless they are so, their souls would be immortal. But another reason may be assigned. The sceptical, or free-thinking, philosophers always lowered human nature as much as possible. They regarded it as something gained to their arguments against religious belief, if they could show the difference to be slighter than is supposed between men and brutes; and that there is a chain of being from the plant, nay almost from inorganic matter, up to man. They seem to have had a confused idea that this helped them even to account for the constitution of the universe, "without the hypothesis of a Deity," as Laplace is said to have termed it when Napoleon questioned him on the remarkable omission in the "Mécanique Céleste." Thus much is certain in point of fact, that those philosophers, and especially the French school, were fond of lowering the human intellect by raising that of animals; and while the priests were lavish of their admission that our moral nature is utterly corrupt, but claimed for our intellectual capacity to be only a little lower than the angels, the society of the Encyclopédie, and the coterie of Baron d'Holbach were fond of levelling the intellectual distinction between immortal and confessedly mortal beings, though they denied the moral depravity of their race with perhaps no very strict regard either to the evidence of their consciousness or of their observation. It thus appears that this theory of a difference in kind is found in company with that of scepticism, just as some other theories are usually coupled with it also; for example, the selfish system,—philosophical necessity,—expediency,—materialism,—all of which are held by

Hume, Voltaire, Helvetius, Diderot, and other free-thinkers; yet all of which are also held by some as determined believers as any that are to be found in any church. Priestley, for instance, held all these doctrines, and Paley all but the last. Hume's opinion on the reason of brutes cannot be doubted from some accidental remarks interspersed in his writings. Helvetius, a materialist and sceptic both, has explicitly stated that if the arm of man had chanced to terminate in the foot of a horse, he would still have been found wandering about as the tenant of the woods.* The company in which the opinion has been found has thus greatly disinclined pious men towards it. Professor Robinson, in his attacks on the French school, is nowhere more severe upon them than where he impeaches them of endeavouring to lower the dignity of human nature,† and undoubtedly such attempts may be made in a manner to hurt the interests both of religion and of morals.

A. Has not Lord Monboddo given great offence of the same kind, and in the same quarters?

B. Possibly he has; although from his station

* De l'Esprit. † Proofs of a Conspiracy.

as a judge, and a man of most loyal political opinions, and also from his being an orthodox believer, at least as far as professions go, he has been less blamed than the rest. He was an admirable Grecian, such as in modern times Scotland has very rarely produced; and there is an infinite deal of ingenuity and subtlety as well as learning in his writings, with a constant display of most correct taste, in judging of the ancient controversies. But his theory has subjected him to great ridicule, not so much from his holding that there is a gradation in the whole scale of beings, and that the mental faculties of man are found in the minds of brutes, as from his denying any specific difference even in body; and holding that originally men were fashioned like monkeys, and lived like them wild and savage.

A. I could much more readily understand this doctrine giving offence and scandal as heterodox, than the other; for it seems not very easily reconcileable either to our religion, or indeed to almost any other received among civilized nations.

B. I consider it a thing just as little supported by the facts as it is repugnant to all known systems of theology. But my objection to it is really not founded upon its tendency to lower human nature.

On the contrary, I doubt if it does not rather exalt our faculties beyond all the ordinary doctrines, and draw a broader line of distinction between us and the lower animals, than that which it was intended to efface. For surely if we have not only by our intelligence made the great progress from a rude to a refined state—from the New Zealander to Laplace, and Newton, and Lagrange—but have also, by the help of the same faculties, made the progress from the state of monkeys and baboons, while all other animals are the same from one generation to another, and have made not a single step for sixty centuries, and never have attempted in a single instance to store up for after times the experience of a former age, our faculties must needs be immeasurably superior to theirs. In short the only question is as to the nature of the difference.

A. I can well suppose a difference merely in degree sufficient to explain any diversity of condition or result. We have only to compare individual men together to perceive this. It is admitted that reason, nay that the power of forming abstract ideas, as well as drawing inferences from premises, is possessed by persons whom yet you shall in vain attempt to teach the simplest mathematical demonstration. Then their faculties only differ in degree from those by which Pascal learnt geometry without a master or a book, and Newton discovered Fluxions, and Lagrange and Euler the Calculus of Variations. It may truly be said, that there is no difference in kind which could make a greater diversity in the result.

B. It may indeed be truly so said; but it may also be added, that there is not a greater difference, call it in kind or in degree, between the person whose obtuseness you have supposed, and a sagacious retriever, or a clever ape, than between the great mathematicians you have named, and that same person. Locke, whose calmness of understanding was equal to his sagacity, and never allowed his judgment to be warped by prejudice, or carried away by fancy and feelings, seems to have held this opinion, and indeed to have allowed some reason to animals. "There are some brutes," he observes, "that seem to have as much knowledge and reason as some that are called men;" and he goes on to say that there is such a connexion between the animal and the vegetable kingdom, as makes the difference scarce perceptible between the lowest of the one, and the highest of the other.

A. You quoted Addison's paper upon Instinct yesterday, in proof of his taking the Newtonian view of the subject. What does he say as to the Reason, and generally the Intelligent faculties, of animals?

B. He is, as you are aware, no very great reasoner; insomuch, indeed, that I have known persons made converts to Deism, or rather from Christianity, by reading his most feeble treatise on the Evidences. One man of great virtue, learning, and ability confessed as much to me. Accordingly, he is very wavering and inconsistent on this subject also, and encounters it with prejudice. At one place he says, reason cannot be the cause of brutes acting as they do; and then, after seeming to deny it, he only adds a kind of admission that they have reason: "for," says he, "were animals endued with it to as great a degree as man," &c. And again, in the same paper, he seems to deny it altogether. "One would wonder to hear," he says, "sceptical men disputing for the reason of animals, and telling us it is only our pride and prejudices that will not allow them the use of that faculty." This is exactly the notion to which I was a little while ago imputing the unwillingness of so many reasoners to allow brutes their fair share of intelligence. You see

Addison considers it the natural course of a sceptic; yet surely Locke was as firm a believer as himself, and certainly a far more reflecting and intelligent one.

A. Perhaps we had as well consider, before going into the question, by what kind of logic the argument is to be conducted, by what sort of evidence we are to try the cause.

B. I presume there can be no doubt here. We must examine it according to the rules of inductive science. The facts are before us. Some we gather from observation—those relating to animals; some, as those respecting the nature of the human mind, we ascertain by our own consciousness, or at least chiefly by that, though in some sort also by observing other men's conduct, and communicating with them; but having no means of communicating with animals, we are reduced to our observation merely; and then we naturally draw the inference that, because the same things done by ourselves would be known by us to be done from certain mental powers, therefore we ascribe those powers to the animals. This conclusion as to ourselves is certain, because we know and feel it to be so by our own consciousness. With respect to animals it is not nearly so certain, because we cannot either enter into their minds, as we do into our own, or communicate with them, as we do with our fellow-men. Nevertheless, by varying our observations on them, by making experiments on their faculties, by placing them in new and arbitrary combinations of circumstances, we can reduce the chances of error to a very small amount, and render our inferences as highly probable as most of the propositions of contingent truth are.

A. It is not, however, necessary that we should now go into an investigation of the nature of the human faculties. Our researches are in their nature comparative only.

B. Certainly; and therefore, agreeing with you, I would begin by laying down this position, that all we have to do is to grant or to deny the existence of certain mental faculties, and to ascertain the meaning of the terms which we employ in expressing these. Whatever those faculties may be in us, all we are now to consider is, whether or not the brutes have the same, or in any degree.

A. I think it quite right and really for our safety, in conducting the inquiry, to lay down a second preliminary principle or caution, namely, that we have no right to argue from the mere effects produced by certain endowments, or by any given combination or modification of these. Thus, when we see what has been achieved by man, and contemplate the extraordinary monuments raised by his industry, his activity, and his intelligence, and the power which he has acquired over the operations of nature, and of all other animals, profiting so largely by both, and when we compare this with the feeble state of those animals, their having no accumulation of either knowledge or possessions, and gained nothing upon man or by man, we are drawing a contrast which really proves nothing; because it is just as easily accounted for by supposing the two classes extremely different in degree, as by assuming that they differ in the kind of their faculties. Thus, to take a common instance, and one which Adam Smith himself gives as marking a great difference between us and the brutes, they have no appearance of barter; but if barter arises from comparing ideas together, and forming a conclusion from the premises, and if, from other facts, animals appear to possess that power, there being no positive barter only shows that their judgment or reasoning faculties are weaker than ours, or that for some other reason, it is immaterial to the argument what, they have not acquired that particular result of the reasoning faculty.

B. I entirely agree in this general position, holding that the neglect of it has been one main cause of the errors into which philosophers have fallen on this question; I must, however, doubt the correctness of the position, that the brutes are wholly ignorant of barter. No one, as Smith says, ever saw one dog barter a bone with another. But many of the operations of both dogs and horses in dividing their labour, and of insects, as ants, in helping each other, seem referable to a principle not to be easily distinguished from barter. The division of labour is clearly to be observed among them. Of course I do not mean that comminute division by which bees work together, and in which they incalculably excel ourselves; for that we have classed as instinctive and unintentional, and therefore it cannot enter into our present argument. But horses plainly help one another in drawing, and take different parts of the work: so do dogs in the chase. However, to leave no doubt about it, and allowing beavers to act instinctively, the wild horses sleeping and watching by turns is a clear and unequivocal instance of the division of labour. But I admit your position—that if anything which is the result of a faculty, proved already to be one of the animal mind, is not possessed by them, this is no argument against their having that faculty. It may lead us to be the more cautious in examining the proofs by which their possession of the faculty is established: but that is all. Indeed, such distinctions are taken upon no more philosophical ground than he would have for his classification who should make two divisions of metals or of water, one the solid, and another the fluid, accordingly as they had different temperatures.

A. I hold it to be a part of the same preliminary position, that if brutes are shown to possess any given simple faculties, their not having the power of doing things only to be accomplished by combinations of these simple powers, does not impeach the proposition, already established, of their having those simple powers. For it would only show that they have not the combination, though they may have the separate powers. Does any other proposition occur to you as convenient to be laid down in the outset?

B. I should say this, which is perhaps rather a corollary from the last, that we must carefully distinguish between simple and composite faculties, as they are called. Indeed, I deny the accuracy of this form of speech, and I believe it tends much to error in metaphysical speculations. No system of psychology, ancient or modern, sanctions it; neither those of Hartley, Priestly, Berkeley, nor that of Reid and Stewart, and Brown, although I think it has been much encouraged by the speculations of these last, and their separate treatment of our mental powers under distinct heads, how necessary soever this was for the elucidation of the subject. The mind being one, and entire, and invariable, without parts or composition, acts always as one being. It recollects, praises, judges, abstracts, imagines; and when you say that it exercises a compound, or complex, or composite faculty, as for example, the imagination, you only mean that it first exerts one faculty, then another, and then a third. We never should call the process by which chemists bleach vegetable substances a composite operation, because they first make oxymuriatic gas, then mix lime with water, then, by agitation of the water exposed to the gas, cause lime to combine, and then expose the vegetable fibre to this compound liquor; we say that these are so many successive operations performed, and not one complex operation. And so imagination is not one compound faculty, nor is imagining one complex operation of the mind. But that mind in succession remembers, abstracts, judges or compares ideas, and reasons or compares judgments—and the whole four successive operations form imagination; to which you may add the further operation of taste, which, rejecting one and selecting other results of imagination, produces the fruits of refined or purified fancy; if indeed this taste itself be anything but a sound exercise of judgment —a judgment refined by experience, that is, by constant attention to what is pleasing, and what disagreeable. The rapidity with which all these separate operations are performed by the mind, neither prevents them from being in succession and separately performed, nor at all shows the mind to have composition or parts. Giving names to certain combinations, or rather successions of operations, and not to others may be correct; but it must be admitted is somewhat capricious. We talk of imagination as if it were one operation, though it is many; and yet we give no separate name to several other successions as rapid of our mental operations. So as to our moral feelings. We speak of conscience as one; yet it is, as Smith describes it, a succession (he says a compound) of several, among which pity for the party injured, and fear of the consequences to ourselves, are the chief. Yet we give no name to the reflection on past enjoyments, which is as quick a succession of several emotions, —namely, recollection, comparison of the present, and sorrowing for the contrast. However, as regards our present purpose, the simplest part of the proposition is, that any given simple faculty or single operation of the mind being found to be possessed by animals, the circumstance of their not possessing the compound exclusively, or several combined, or a successive operation of different faculties, is no proof against their having the simple ones. Thus, if they have no fancy, it is no proof that they have no memory or judgment; because they may have these without having abstraction, which is one of the faculties that go to make the imaginative process. But it is also no proof of their being without abstraction, and all the other simple or single faculties; for it only proves that they have not the power of using these faculties together, or rather in quick succession, and for the same joint purpose. And should they have the simple or single, without having the compound faculties or processes, this would again argue no specific difference, but rather a diversity of degree.

A. I think these preliminary positions not only have cleared the ground for us, but helped us a good way on our journey. There appears hardly much more to reason about now. The subject has been a good deal enveloped in mist and smoke, from confusion of ideas, and from prejudice and high feeling. These being blown away, it seems pretty clear what the structure is that we are to examine.

B. Before going to the brute faculties, let us just cast a glance over the faculties which have been enumerated as belonging to ourselves, and see if they should not be a little simplified—Sensation, Perception, Consciousness, Memory, Abstraction, Imagination, Judgment, Reasoning, to which have been added Taste and the Moral sense; and Mr. Stewart thinks these not enough, adding among others, the power of connecting general or abstract signs with the things signified. Now suppose we admit the correctness of calling a state of mind in which it is purely passive an active power or faculty, as Sensation, which is merely the effect produced upon the mind by the operation of the senses, and involves nothing like an exertion of the mind itself, any more than receiving a hurt or a gratification passively is any exertion of the body, although the operation whereby that reaches the mind may be termed bodily exertion; then it will follow, and not otherwise, that Sensation is a faculty. But Perception is no doubt an active exertion of the mind. *Memory* differs from Recollection as Sensation does from Perception. The state of mind in which one idea calls up another, or a present state of mind influenced by a past state, is Memory. The exertion by which the mind voluntarily induces the present state from the past, is Recollection. The one is the *sensation,* the other the *perception* of the past, as sensation and perception are of the present.

A. Is not Perception an inference from Sensation? I have the sensation of solidity or of smell, and I perceive either the solid, resisting body, and the odorous body, or I perceive the solid or odorous quality, that is, I infer a being from the sensation; or I infer a quality; the former seems a simple inference, the latter an inference coupled with an abstraction.

B. I do not incline altogether to this opinion; but at any rate it will not apply to Memory and Recollection; for Recollection is not an inference from Memory; it is an effort by which the mind throws itself into the state into which it might have been brought by the former ideas recurring of themselves. In Perception we do not voluntarily throw the mind into the state of Sensation; we draw an inference from that sensation according to your theory. But I think it pretty clear that there is something between the sensation and the inference—the simple apprehension and the conclusion drawn. The latter is clearly an inference that an external being exists which created the sensation and the perception. But I think there is also a perception upon the sensation, and which cannot certainly exist without it. However, be this as it may, to our present purpose it makes no difference, except as far as there can be no doubt of the mind being in a much more passive state in the two conditions of feeling and remembering than in the other two of recollecting and perceiving.

A. Then of Imagination we have already disposed. It consists of the successive, though rapidly succeeding operations of other faculties whereby we create or combine new ideas that had no previous existence, abstracting the qualities of one object to clothe another with them. But abstraction we may allow to be a simple operation and one of the most important. What do you make of two that I do not remember you to have named, Attention and Conception?

B. I omitted them purposely. I can see really nothing in Attention but the degree in which certain other faculties operate. It is only the intensity with which I perceive. Possibly there may be some good from considering it as the difference between Perception and Sensation; in the latter case the mind passively receives the impression of the senses, in the former it fixes itself steadily upon those impressions, so as to feel them by a voluntary effort more acutely. As for Conception, which used formerly to be called Simple Apprehension, it is only the forming ideas of objects neither presented by the senses nor by the imagination; and I am unable to separate it from Memory and from Abstraction —from memory as far as it deals with former ideas, from abstraction as far as it deals with quality apart from the objects remembered or imagined.

A. Then Judgment being the comparison of ideas, and Reasoning the comparison of judgments, that is of the ideas arising from the former comparison, may be set down as one faculty—that of Comparing—and I conclude you make quick work with Taste and the Moral sense, of which the one gives us preferences among objects of mental gratification, and the other among objects of moral approbation?

B. They are both evidently exercises of the judging and reasoning powers,—say the comparing powers, according to two standards,—the one the sense of beauty or fitness, of what is pleasing or agreeable; the other the sense of what is just and right. But whether this last sense is natural or acquired, and how acquired, is a question that has long divided philosophers, and which will very certainly never be determined. Nor is it more easy to determine the other, which is quite a kindred one, how it is that our taste is formed, and whether it be natural or acquired. All that we can say on this subject is, to remark the little practical importance which belongs to either question, and to state that, as far as our present discussion is concerned, the only faculty involved in either the one or the other is that by which we compare different ideas.

A. Our enumeration then of mental faculties seems to resolve into Perception, active or passive;

Memory, active or passive; Consciousness, Abstraction, and Comparison; then how do we place animals as to the first?

B. Clearly no animal, nothing having life, can be conceived to exist, without Passive Perception at all events, and hardly any without Active Perception also. Consciousness too seems a necessary quality of every mind; it is the knowing one's own existence; so Memory of the passive kind must exist in every mind; without Consciousness and Memory no animal could know its own personal identity; and no acts could be done by it upon the supposition of that identity. With respect to Active Memory and Conception, if this is to be held a separate faculty, it is implied in Comparison, or in judgment and reasoning; so that our inquiries come to be confined within sufficiently narrow limits. Do the lower animals possess Abstraction and Comparison? I will at once begin with Abstraction, because it is the power most generally denied to brutes; and this arises, as I conceive, from an ill-grounded notion of its nature, and from a supposition that it is a faculty of a far more refined nature, subservient to operations of a much more difficult kind, than the truth will warrant us in affirming. The truth appears to be, that there are, if not two kinds of Abstraction, an active and a passive, yet certainly some degrees of Abstraction so easy and even unavoidable, that we can hardly conceive almost any mind incapable of forming them. But on the other hand, the very highest and most difficultly attained reach of human thought is connected with Abstraction. Observing this, philosophers have passed all under one name, and because the brutes could not conduct algebraical investigations or metaphysical reasonings, have denied them all power whatever of forming abstract ideas.

A. To a certain degree this is no doubt true. The abstraction by which we reason upon m and n or x as only numbers; deal with x the unknown quantity, multiplying it and speaking of m times x, or dividing it and speaking of one n^{th} part of x, is no doubt a high and refined reach of thought; but so is the forming to ourselves an idea of abstract qualities; indeed I know not if, when we reason about m and x, we do more than mechanically deal with the letters; whereas in reasoning of colour or smell as abstracted from the rose with which we always have seen them conjoined, and forming to ourselves the idea of something in the abstract which we have only ever seen in the concrete,—of some ideal existence of which in actual existence we have never known anything, nor can know,—we really appear to go a step further. Now do you maintain that Abstraction is ever otherwise than a difficult and painful operation?

B. First of all be pleased to observe that many philosophers altogether deny, even to man, the power of forming abstract ideas. The dispute of the *Nominalists* and *Realists,* so well ridiculed by Swift, or rather by Arbuthnot in Scriblerus, is as old as metaphysical inquiries, under one name or another. They consider it impossible for us really to form these abstractions, and hold that we only are using words and not dealing with ideas, just as you seem to think we do in algebraical language. Mr. Stewart is among those who conceive that we think in language. My opinion, if against such venerable authority I may venture to hold one, is different. I think we have ideas independent of language, and I do not see how otherwise a person born deaf and dumb and blind can have ideas at all; which I know they have, because I carefully examined the one of whom Mr. Stewart has given so interesting an account. Indeed he has recorded the experiment of the musical snuffbox which I then made upon this unhappy but singular boy. But next I am to show you that abstraction independent of algebra, or metaphysical reasoning altogether, is neither difficult nor painful. Without Abstraction we cannot classify in any way, or make any approach to classification. Now I venture to say that no human being, be he ever so stupid, is without some power of classification, nay, that he is constantly exercising it with great care, and almost unavoidably, and acting upon the inferences to which it leads. He can tell a man from a horse. How? By attending to those things in which they differ. But he can also tell a stone from both, and he knows that the stone is different from both. How? By attending to those things in which the two animals agree, and to those things in which they differ from the stone. So every person having accurate eyes and the use of speech can call a sheet of paper and a patch of snow both white; a piece of hot iron and of hot brick both hot. He has therefore the idea in his mind of colour and of heat in these several cases, independent of other qualities, that is abstracted from other qualities; he classifies the white bodies together independent of their differences; the hot bodies independent of theirs; and he contrasts the white metal with the white snow, because they differ in temperature, without regarding their agreeing together in colour. All this is Abstraction, and all this is quite level to the meanest capacity of men. But is it not also level to brute intellect? Unquestionably all animals know their mates and their own kind. A dog knows his master, knows that he is not a dog, and that he differs from other men. In these very ordinary operations we see the animal mind at one time passing over certain resemblances and fixing on differences; at another time disregarding differences and fixing only on resemblances. Nay, go lower in the scale. A bull is enraged by a red colour, be the form of the body what you please. A fish is caught by means of a light, be it of any size or any form.

A. These things which you last mention are mere sensations. The red light or the flame impresses the retina and affects the animal's sensorium, his brain—irritating the quadruped, and attracting the fish.

B. What then? Other sensations pass to his mind through his senses at the same time. He has the sensation of form as well as colour; yet he passes this entirely over, and only considers the colour. However, take those cases in which animals are attracted to certain places. They are hungry and go to a certain field to eat, without the least regard to its position or its shape; because it agrees with other fields in bearing the food which the beast is in quest of. Flies approach the light because they believe it to be the open air where they wish to go. So the bird never throws stones into a river or puddle to raise the water; but it does throw them into the ewer. It abstracts water from the thing containing it; and could not reason upon the effects of the operation without a process of Abstraction. Indeed, upon the footing on which you would put it, I know not that all our own abstract ideas may not in the end be resolved into sensations and their immediate consequences. I know of no evidence that you have of our abstract ideas being formed in any other way, except on our consciousness, and our continual communication of ideas and experience through speech. In the case of the brute we have all the same phenomena, and, excluding the operation of blind Instinct, we are forced to the like conclusions.

A. I think we may go a step further; have not animals some kind of language? At all events they understand ours. A horse knows the encouraging or chiding sound of voice and whip, and moves or stops accordingly. Whoever uses the sound, and in whatever key or loudness, the horse acts alike. But they seem also to have some knowledge of conventional signs. If I am to teach a dog or a pig to do certain things on a given signal, the process I take to be this. I connect his obedience with reward, his disobedience with punishment. But this only gives him the motive to obey, the fear of disobeying. It in no way can give him the means of connecting the act with the sign. Now connecting the two together, whatever be the manner in which the sign is made, is Abstraction; but it is more, it is the very kind of Abstraction in which all language has its origin—the connecting the sign with the thing signified; for the sign is purely arbitrary in this case as much as in human language.

B. May we not add that they have some conventional signs among themselves? How else are we to explain their calls? The cock grouse calls the hen; the male the female of many animals. The pigeon and the fieldfare and the crow make signals; and the wild horse is a clear case of signals. All this implies not only Abstraction, but that very kind of Abstraction which gives us our language.

It is in fact a language which they possess, though simple and limited in its range.

A. As to the power of comparing, what is commonly called Reason, *par excellence,* comprising Judgment and Reasoning, this needs not detain us very long. The facts here are not well liable to dispute. There is no possibility of explaining the many cases which we began by going over without allowing this power. They all prove it in some degree. Several of them show it to exist in a very considerable degree. The acts of some birds and monkeys cannot be accounted for by Instinct; for they are the result of experience; and they are performed with a perfect knowledge of the end in view; they are directed peculiarly to that end; they vary according as the circumstances in which they are performed alter, and the alteration made is always so contrived as to suit the variation in the circumstances. Some of these acts show more sagacity, according to Mr. Locke's observation, than is possessed by many men. The existence of a comparing and contriving power is therefore plain enough. And on the whole I conceive that a rational mind cannot be denied to the animals, however inferior in degree their faculties may be to our own.

B. That inferiority is manifestly the cause why they have made so little progress, or rather have hardly made any at all. Some little is proved by such facts as Mr. Knight has collected, but they are only exceptions to the rule which has doomed them to a stationary existence. This difference, however, is merely the result of the inferior degree of their mental powers as well as the different construction of their bodily powers. The want of fingers endowed with a nice sense of touch is an obstruction to the progress of all, or almost all the lower animals. The elephant's trunk is no doubt a partial exception, and accordingly his sagacity is greater than that of almost any other beast. The monkey would have a better chance of learning the nature of external objects if his thumb were not on the same side of his hand with his fingers, whereby he cannot handle and measure objects as we do, whose chief knowledge of size and form is derived from the goniometer of the finger and thumb, the moveable angle which their motion and position give us. Insects work with infinite nicety by means of their antennæ; when these are removed they cease to work at all, as Huber clearly proved. Clearly this different external conformation, together with their inferior degree of reason, is sufficient to account for brutes having been stationary, and for their being subdued to our use, as the Deity intended they should, when He appointed this difference. To argue from the complex effect of all the faculties, bodily and mental, in giving different progress or power to our race and to theirs, and to infer from this difference that there is an essential and specific diversity in our mental structure, nay that they have not one single faculty the same with ours in kind, is highly unphilosophical. It is indeed contrary to one of the fundamental rules of philosophizing, that which forbids us needlessly to multiply causes. For we are thus driven to suppose two kinds of Intelligence, human and brutal, and two sets of faculties, a Memory and a Quasi Memory, as the lawyers would have it—an Abstraction and Reasoning, properly so called, and something in the nature of Abstraction and Reasoning, but, though like, yet not the same.

A. There is one matter to which we have not as yet adverted, but, after having considered the intelligent as well as instinctive powers, we may now as well do so. I mean the diversity in the operations of the latter, and the perfect sameness of the former—a sameness in all the operations of any given individual animal, and likewise of each of the species.

B. This is well worthy of consideration. When trying to explain instinctive operations upon the hypothesis of an intelligent principle acting under the impulse of sensations, I found in this perfect sameness and regularity of its operation a considerable difficulty, though not perhaps an insuperable one, not certainly so great a difficulty as those we have considered.

A. How did you endeavour to explain, on that hypothesis, the regularity of Reason or Intelligence?

B. The absolute sameness of moral and intellectual character, and the limited sphere of ideas and events, will account for much. We see far less diversity of action and speech among peasants of a very confined knowledge and very limited range of pursuits, than among persons of a higher degree of education and superior station in life. But still there is a great diversity. Taking, however, two men of most perfect resemblance in all their faculties, and all their feelings, similarly constituted in both body and mind, they would probably act nearly if not entirely alike. Whatever made one do a thing would make the other, and we must suppose them to be placed in perfectly similar circumstances, so that the same things would happen to both. Chance is here to be put out of view; because it only means ignorance of motives and circumstances, and assumes a diversity in these unknown to us, which by the supposition is here excluded. Suppose these two individuals thus placed in like circumstances as to food and building materials, why should they eat differently or make different habitations? What is there to make the one choose a plant which the other does not choose? or form a hut in any particular different from the other? If one kind of food was nearer the one, and another nearer the other individual, they might choose differently; but this assumes that both kinds are agreeable to the constitution of their palates.

A. As long as providing for merely physical wants was their whole occupation, it is probable that both would act alike, except that, if any difficulty occurred to be vanquished, I am not at all sure of their adopting the very same means to overcome it. One might break a nut with his teeth, another with a stone, or by bruising two nuts together. But there is the same diversity in the conduct of animals where they act by intelligent principles.

The general resemblance of their proceedings is explained by the consideration you are stating in the case you put of the boys. Their instinctive operations would never vary in the least particular. When they came to reason, or speculate, or converse, the sameness would probably cease. It seems inconsistent with imagination and with free will: yet of this I speak doubtingly, considering the hypothesis you have made of faculties and feelings perfectly alike in all respects.

B. Certainly, you ought to speak doubtingly, when such is the hypothesis that is now binding us. I do not see how, even in reasoning, anything should ever come into the mind of the one that did not suggest itself to the other. But our hypothesis is not easy to remain under. Suppose, to make the case like instinct, two untaught children in different parts of the country, viz., one in China and the other here, to be placed in a situation where the same kinds of food and building materials were placed, and a variety of each, we may assume that similar tastes and constitution of mind and body would make them eat the same things, perhaps choose to shelter themselves by building rather than by going into caves, possibly to build with the same materials selected out of a number; but it is much to say that they would exactly preserve the same figure and size and proportions in the huts they made. Each would certainly make blunders, and work inartificially; and it is difficult to fancy them exactly making the same blunders, deviating from the straight line or the circle by the same quantity of aberration, and from the perpendicular by the same angle: yet the bee in China and in England makes the same angles, and forms cells with the same proportions, and raises the grub the same height from the liquor provided for its nutriment, so as to let it have access to the liquor without incommoding or drowning it.

A. When instinct is interfered with by obstacles interposed, the animal's intelligent powers are brought into action, and then the uniformity and perfect regularity ceases. This seems to present under this head, as well as the other head of knowledge and design or intention, a sufficiently marked distinction.

B. Certainly: and it is to be observed that the more sagacious any animal is, the greater variety is perceived in his actions and habits. Thus the elephant and the dog present general resemblances throughout each species; but the instances of sagacity or reason which the different individuals exhibit are sufficiently various: whereas there is no more diversity in the ordinary working of the bee, than in the operations of crystallization, or the secretion of the sanguiferous or the lacteal system. In truth, we may compare the two cases together. Instinct seems to hold the same place in the mental which secretion and absorption do in the physical sysem. Intelligence or reason will sometimes interfere with Instinct, as our voluntary actions will interfere with the involuntary operations of secretion. But the instinctive operation proceeds whether the animal wills or no—proceeds without his knowledge, and beyond his design—as secretion goes on in our sleep without our knowledge and without any intention on our part. So as secretion goes on without any help from us, or any direct co-operation, Instinct works without any aid from Intelligence. But there is this difference in the connexion of will or Intelligence with Instinct, and the connexion of voluntary action with secretion—that the Instinct seems subservient to the Intelligent will far more than the secreting power is to the voluntary action. The bee, when obstructed, applies his

Instinct, as it were, to overcome the obstacle, whereas we cannot alter at will the course of secretion; we have some direct power over it, but very little.

A. One thing seems quite clear, that upon any view of this great question, whatever theory we adopt, all leaves the inference of design untouched; nay, the more we inquire, the more we perceive that all investigation only places in a stronger light the conclusion from the facts to a superintending intelligence.

B. Beyond all doubt it is so. The whole question is one of relations and connexions. Adaptation—adjustment—mutual dependence of parts—conformity of arrangement—balance—and compensation—everywhere appear pervading the whole system, and conspicuous in all its parts. It signifies not in this view whether we regard Instinct as the result of the animal's faculties actuated by the impressions of his senses—or as the faint glimmerings of Intelligence working by the same rules which guide the operations of more developed reason—or as a peculiar faculty differing in kind from those with which man is endowed—or as the immediate and direct operation of the Great Mind which created and which upholds the universe. If the last be indeed the true theory, then we have additional reason for devoutly admiring the spectacle which this department of the creation hourly offers to the contemplative mind. But the same conclusion of a present and pervading intelligence flows from all the other doctrines, and equally flows from them all. If the Senses so move the animal's mind as to produce the perfect result which we witness, those senses have been framed and that mind has been constituted, in strict harmony with each other, and their combined and mutual action has been adjusted to the regular performance of the work spread out before our eyes, the subject of just wonder. If it is Reason like our own which moves the animal mechanism, its modification to suit that physical structure and to work those effects which we are unable to accomplish, commands again our humble admiration, while the excellence of the workmanship performed by so mean an agent impresses us with ideas yet more awful of the Being who formed and who taught it. If to the bodily structure of these creatures there has been given a Mind wholly Different from our own, yet it has been most nicely adapted to its material abode, and to the corporeal tools wherewith it works; so that while a new variety strikes us in the infinite resources of creative skill, our admiration is still raised as before by the manifestation of contrivance and of expertness which everywhere speaks the governing power, the directing skill, the plastic hand. Nor is there upon any of these hypotheses room for doubting the identity of the Great Artificer of nature. The same peculiarity everywhere is seen to mark the whole workmanship. All comes from a supreme intelligence; that intelligence, though variously diversified, preserves its characteristic features, and ever shines another and the same.

NOTE TO THE DIALOGUES.

In Dialogue I. the Instinct of the duckling hatched under the hen and of the chicken in the oven is mentioned. The two following facts have occurred since that discussion was ended.

When a sow farrows, the pigs are expelled with some force, and to a little distance, by the action of the uterus and abdominal muscles. Each pig instantly runs up to one of the teats, which he ever after regards as his own peculiar property, and when more pigs than teats are produced, the latter ones run to the tail of some of the others, and suck till they die of inanition.

Mr. Davy, in his account of Ceylon, mentions a remarkable Instinct of the alligator. He saw an egg in the sand just ready to crack, and broke it with his stick. The animal came out, and made at once for the river. He held his stick before it, and immediately the reptile put itself in a posture of defence, as an adult alligator would have done in like circumstances.

In Dialogue III. there is some doubt expressed as to the water-moth loading its case, if too light in the water, with a kind of ballast. The larvæ of the *Phryganea* are stated by Mr. Lyell to do this habitually, and to use fresh-water shells for their ballast. This gives rise to many masses of calcareous matter in the tertiary formations. As many as 100 small shells are found surrounding one tube. (*Principles of Geology*, vol. ii. p. 232.)

In Dialogue IV. some remarks are made upon Hereditary Instincts. Mr. Roullin has related a similar instance of such Instinct in the hunting dogs of Mexico. Were they to attack the deer in front, whose weight exceeds their own six fold, they would be destroyed and have their backs broke, as happens to other dogs ignorant of the manœuvre, which consists in attacking from behind or laterally, and seizing the very moment when the deer, in running, rests upon two legs. The dog then takes hold of him by the belly and throws him over. The dog of pure breed inherits this stratagem and never attacks otherwise. Should the deer come upon him unawares (from not seeing him), he steps aside and makes his attack at the proper time in the animal's flank; other dogs, however superior in sagacity and strength, make the attack in front, and have their necks broken by the deer. So too some of our English miners carried out greyhounds to hunt the hares in Mexico. The air on that elevated platform, 9000 feet above the level of the sea, is so rare that the mercury stands at 19 inches generally, and the dogs were soon exhausted with running in such an atmosphere; but their whelps are not at all incommoded by it, and hunt as easily as the dogs of the country.

Respecting the elephant extraordinary accounts are told by military men who were in the Burmese war. They relate that when any extra task is to be performed by them, some favourite dainty is held up beforehand, and the sagacious animal, comprehending the promise of reward thus implied, exerts himself to earn it. This comes to the principle of barter as near as may be.

NOTE ON THE GLOW-WORM.

The facts relating to the light of this and other similar insects are by no means accurately known; and upon some material points able observers differ widely. Thus it was deemed very natural to suspect that some inflammable matter in a state of slow combustion caused the luminous appearance, the rather as it bears a striking resemblance to the light emitted by phosphorescent bodies. Accordingly the obvious course was pursued by different experimenters, of exposing the insects to heat and to oxygen gas, to see if the light was increased; and exposing them to carbonic acid and hydrogen gases, to see if the light was then extinguished. Forster and Spallanzani affirm that they have tried this experiment, and found the result to accord with the theory; they assert distinctly that in oxygen gas, and on the application also of heat, the light is more brilliant, and that none is given out in hydrogen and carbonic acid gases. But Sir H. Davy found that the light continued in the latter gases not sensibly diminished, and that oxygen did not increase its brightness;* Mr. Macartney observed the light in vacuo and under water, † while Dr. Hulme found that it was extinguished in hydrogen, carbonic acid, and nitrous gases, although he could not perceive that oxygen gas increased it. ‡ There seems reason to suspect that these able men made their experiments on different species of the insect, and that the animal or vital powers which regulate the secretion, or the use of the luminous matter, were affected by the gases applied. For it is admitted on all hands that the living insect has a power of extinguishing the light independent of any mechanical operation by which it may cover over the shining part; and although the fire-fly has that part usually covered with its wings, and therefore only shines when flying, the glow-worm's light is constant, unless she restrains or extinguishes it by a voluntary act.

That some luminous matter is secreted by the insect there can be no doubt. The fact that boys in South America rub their faces with bruised fire-flies, to make them shine, is asserted by travellers;

* Phil. Trans. 1810, p. 287. † Ib. 1810.
‡ Ib. 1801.

and this seems to render it probable that the glow-worm likewise secretes such an oil. But the experiments of an able chemist, Mr. Murray, have set this question at rest. He examined a box in which glow-worms had been kept, and found several luminous specks which they had left behind them. Some of these yielded a steady light for five or six hours. Mr. Murray says that the luminous matter is inclosed in a capsule of a transparent substance, which, when ruptured, lets out the matter in a liquid form of the consistency of cream. A French naturalist, M. Macaire, made some experiments upon this matter, the result of which differed materially in one respect from that of either Spalanzani, Davy, or Hulme; for he is said to have found that the presence of oxygen in the air prevents it from shining, a position not reconcileable with the worm shining in the atmosphere. But some of this author's experiments seem to furnish a solution of many difficulties; for their results refer the appearance to the animal functions. He found that the luminous matter is chiefly composed of albumen, and that any body which coagulates albumen destroys the shining quality; which it probably does by altering the albuminous state of the fluid. He also observed, that though a certain degree of temperature is necessary for it, a higher degree destroys it altogether; and also that common electricity has no effect in exciting it, but that voltaic electricity or galvanism does excite it. These observations, if accurate, are the most important that have been made upon this subject. They seem to indicate an immediate connexion between the vital powers of the insect and its luminous quality; and they account satisfactorily for the diversity in the results of former observers, who operated upon the animal apparently without taking its vital functions into the account.

The glow-worm (Lampyris Noctiluca) is not the only luminous insect. There are several other kinds both winged and apterous. Of these the fire-fly, a species of the Elater and of the beetle tribe, has already been mentioned. Indeed all the species of the Lampyris genus are supposed to be more or less luminous. Several other species of the Elater, as well as the fire-fly, are also luminous. Some species of the Fulgora (an hemipterous insect) shine so bright that they are called lantern flies. Of these the Fulgora Candelaria is a native of China, and the F. Lanternaria, which is two or three inches long, is a native of South America. The shining matter in these, and all others of the genus that shine at all, is confined in a transparent bulb projecting from the head.* Two species of centipes, the Geophilus Electricus and G. Phosphoreus, also shine; the former is a native of this country, the latter of Asia.

Several theories have been formed to explain the use of this luminous quality. It is observable that some of the insects which have it are apterous in one sex while the other is winged—as the glow-worm, the male of which is a fly, the female being a caterpillar. In others, both male and female are winged. Again, some have the light always in front, and it seems not to vary in brightness, as the Fulgora. Naturalists have supposed that in these it is serviceable in discovering their prey. But it has also been suggested that defensive or protective purposes may be the final cause of the light. Insects which prey on caterpillars have been observed running round the Geophilus Electricus, as if afraid to approach it.† But there is one peculiarity in the glow-worm's light which seems to sanction the commonly received opinion of its use being

* Kirby and Spence, ii. 413. † Ib. ii. 225.

chiefly, if not entirely, to attract and direct the approach of the male. Not only has the latter wings, and thus is by his habits little likely to be found near the unwinged female—there is also found to be much less light emitted by the male; insomuch that at one time the female alone was believed to shine at all, until Ray corrected this error. It is also remarked that the light is the strongest when the two are together, and that in some, if not all the species, the luminous quality is confined to the time when they are destined to meet. Nor is De Geer's objection, founded on the observation that the chrysalis and larva of the species have somewhat of the same luminous quality, of much force. For as the very learned entomologists just cited, Messrs. Kirby and Spence, have well observed, this instance may easily be set down with the analogous case of males having a kind of lacteal system in some animals, including our own species. It deserves further to be remarked, that in Brazil there is a glow-worm which is winged, both male and female, and the light given by this insect is not steady like that of our glow-worm, but sparkles or intermits. On the other hand, the fire-fly of Brazil is said to give a constant light.* But this may be owing to the greater luminousness of the tubercles in the thorax, which in the European fire-fly give so little light compared with the patches concealed by the cases (elytra) of the wings, that they seem only to shine when flying.

* Kirby, Bridgewater Treatise, ii. 366.

OBSERVATIONS, DEMONSTRATIONS, AND EXPERIMENTS UPON THE STRUCTURE OF THE CELLS OF BEES.

THE principal use of the cells in the comb is to provide places where the eggs may be deposited, the worms hatched, the pupæ or chrysalides formed, and the bees afterwards produced. The cells are also used for storing the bee's bread and the honey. But, to whatever use they are applied, it is of importance that in their construction the greatest possible saving should be made, both of space, of wax, and of labour.

The importance of saving room is obvious, quite independently of that saving conducing to an economy of wax and of labour, in the construction of the cells; for the whole hive may thus be made in places which would otherwise be too confined; the heat necessary to the health of the bees, to the process of hatching, and to the preservation of the honey, is thus economized; the labour of the bees in moving about the combs to superintend the various operations after the buildings are finished is materially abridged; the more compact each comb is, the stronger it will prove; and the fewer the interstices are, whether between the different combs, or between the cells of each, the greater will be the security against intruders.

The saving of wax is equally, perhaps more, important. That material is not abundant. It nowhere exists in nature; but is elaborated by the bees themselves. This capital discovery we owe to John Hunter and to Huber; but the step was principally made by the former. He found* that small rings or films of wax are protruded through the scales of the bee's belly; and Huber afterwards showed that this is secreted in the stomach, when the bee feeds either upon honey or other saccharine matter. But, beside the limited amount of such matter, the process of secretion appears to be one of time and difficulty, requiring the animal to be at rest, and in certain attitudes. Moreover it is

* Phil. Trans. 1792.

only one class of the hive that can produce wax at all. Nor is the wax when secreted in the stomach and given out through the scales in a state fit for use. It undergoes another process by the work of the bee, who moistens it with saliva, kneading it and working it, to give it the ductility, consistence, and opacity required. The building or sculpturing bee is observed to take into her mouth part of the scale furnished by the wax-worker (or bee producing wax), to work it about in her mouth, and to bring it out in the form of a long and slender thread or ribbon, which she coils and turns, and again and again passes through the mouth, until it is quite fit for building with. The limited supply, therefore, of this substance, as well as the labour required to prepare it for use, renders the economical employment of it a matter of great moment.

Lastly; it is evident that, although the saving of room and of wax were immaterial—supposing, for example, the saving of heat and increased strength or solidity not to be required, and a supply of wax ready for use without any labour in preparing it to be unlimited—still the construction would be the most advantageous which required least work, and enabled the bees to perform the operations allotted to them, of building, storing, and breeding, within the season to which their activity is confined. This saving is probably the most important of the whole; because the possibility of continuing the species may depend upon it. The two other savings, of room and of wax, conduce materially to this saving of work; but work may also be saved, independently of those two other savings, by the form and arrangements of the structure.

The mutual connexions of these three savings, as well as their possible independence of each other, may be illustrated by supposing a house with so many, say three, rooms required to be built, where land is dear, roofing materials scarce, and labourers few, or the time for finishing limited. Build the three rooms all on the ground floor, and you require more land, and more roof, and more labour than is necessary. Build them one above another, and you save in all the three particulars. But though there were no want of land, or of roofing materials, but only of workmen, or of time, build the three rooms on the ground, and at a distance from one another, and you lose unnecessarily work and time.

If we now conceive a given space which is to be used by the bee, and consider in what way the cells must be disposed in order to bring the greatest number possible within it, and not interrupt the operations of the animal, we shall be able to perceive the arrangement best suited to effect a saving of room.

The size of the cell, both as to length and breadth, must be determined by the dimensions of the young insect in its last stage before coming out a perfect bee, though no doubt the cells used for storing honey and bee's bread may be larger or smaller. The manner in which the cells must be disposed is in some measure also determined by the length; for one of the ends must be left open, and there is no necessity for the other being open; on the contrary, its being closed accommodates and protects the egg when deposited there, and the worm when first hatched. Therefore the cells should be arranged in double rows, with their ends in contact on one side and open on the other. This is the only disposition of them by which an interval between the two rows can be saved, and therefore the whole number of cells which are to be made in the given space must be disposed in double sets or rows, the cells on each side or face abutting on those of the other. Between each row or set—say comb—and the next, a space must be left sufficient for the bees to pass and repass; and to avoid the necessity of their going round in order to get from one vacant space or street to another, several openings must be made in each cómb, as it were cross streets, leading from one to the other main street. This is a sacrifice of room to save labour and time.

But in what way must the cells be made so as to place the greatest number in each set or comb? This leads us to consider the form of the cell, which must be such as both to accommodate the insect and to leave no interval between cell and cell. The form must correspond as nearly as possible with that of the insect, which both in the grub and perfect state approaches to cylindrical. But if the cells were cylindrical, there would necessarily be interstices. If it is required to fill any given space*

* The proposition must be limited in some such way as this. Nothing can be more incorrect than the usual statement of it, that the equilateral triangle, square, and hexagon, are the only figures which will fill space, unless it be meant of regular figures, that is, figures inscribed in a circle. But then the circumscribed circle is immaterial, and so is the equality of sides. The question is as to the circle inscribed. If the condition that the solid be one circumscribing a cylinder is not added, then equal and similar hexaedral pyramids, or frusta of these, would answer the condition of filling

with a number of equal and similar figures disposed in the same way, and each circumscribing a cylinder, there are only four which can be formed without leaving any interstices—the prism whose section is a parallelogram; the prism whose section is an equilateral triangle; the cube or parallelopiped, whose section is a square, and the hexagonal prism. If the inscribed cylinders are required to touch one another in four points, the first kind of prism must be rejected.* Let us take the sections only, for whatever is true as to the surface of the section must be true as to the solid contents generated by apposition or by motion of that surface. The square, the equilateral triangle, and the hexagon, are therefore the three figures which answer the condition of leaving no space unemployed.

But if the square or the triangle were chosen, though no space would be lost on the outside of

the space without leaving interstices, and we shall see that there are instances of a structure approaching to this in the wasp's nest.

* There would also be no limits to the loss of space internally so formed, nor to a certain loss of space externally, viz., at the edges or outward boundary of the given space; but if the figures are to be ranged round a given point, such parallelograms would certainly answer the conditions. Triangular prisms would also fill the space, but then they could not, to do so, be all disposed in the same manner.

the comb, and between the different cells, space would be lost on the inside of each cell, because the circular form of the insect would leave the angles empty. There would be more space thus lost in the triangle than in the square, and more in the square than in the hexagon. The radius of the circle representing the insect or grub being r, the space lost in the triangle would be $3\sqrt{3}\,r^2 - C$; in the square $4r^2 - C$; in the hexagon $\frac{6r^2}{\sqrt{3}} - C$; C being the area of the circle. Consequently the triangle would occasion more space to be lost than the square by the amount of $(3\sqrt{3} - 4)\,r^2$, and than the hexagon by the amount of $\sqrt{3} \cdot r^2$, while the square itself, though occasioning less loss than the triangle, would occasion more than the hexagon by the amount of $\frac{4\sqrt{3} - 6}{\sqrt{3}} \times r^2$. If r be taken equal to unity, the loss of the triangle compared with the square would be about 1·19, and compared with the hexagon about 1·73, and the loss by the square compared with that by the hexagon would be about ·53. So that suppose a comb a foot square, and containing, both sides included, 4608 cells, allowing a quarter of an inch for the breadth of each, and one-eighth of an inch for the radius of the circle (or insect), the loss of the triangle compared with the square would be above 85 square inches, or the room for 1360 bees; the loss of the triangle compared with the hexagon would be 124 square inches, or room for 1984 bees; and the loss even by the square compared with the hexagon would be above 38 square inches, or room for more than 608 bees.* The loss of space upon a whole hive of a cubic foot, supposing the combs an inch thick, and the interstices between them half an inch, would of course be eight times as much; so that such a hive would have more space by 992 cubic inches, and room for above 15,800 more bees, if the cells were hexagonal, than if they were triangular, and more space by 304 cubic inches, and room for above 4864 more bees, than if they were square. But as these calculations proceed upon the supposition of the same number of cells being crowded

* Upon the supposition of the cells being a quarter of an inch in breadth, and consequently the radius of the circle one-eighth, in any plane containing the comb, the loss of space in each cell, if triangular, would be about $\frac{1}{32}$ of an inch square; if square $\frac{1}{72}$; and if hexagonal, only $\frac{1}{188}$, or a ninth of the loss by the triangle, and two-fifths of the loss by the square.

into the space, whatever be their form, we may only take the supposition, which would certainly be more correct, of different numbers being crowded according to the form; viz., for the triangle 3546, the square 4608, and the hexagon 5320. This would make the loss in the three figures, respectively, for the whole comb, 113, 61, and 26 square inches, being accommodation for 2326, 1260, and 543 bees respectively; or the loss on the triangle, as compared with the hexagon, 1783 bees, and on the square, 717.

Nor are these computations confined to the case of the grub or the insect being regular cylindrical bodies. They only assume that the cell is to be of such a diameter as to contain the animal; and therefore that in some one part of the prism or parallelopiped the circular section of the animal touches the sides. The result of the computation is the same as to the space lost, whatever form the rest of the animal may have. Indeed, as C (the area) vanishes from the equation, the section of the animal needs not be circular,* so that there is room for its greater axis; only the whole

* This is only applicable to the calculation in the text; the computation in the last note, giving not the relative but absolute loss of space, proceeds upon the supposition of a circular area.

reasoning rests upon the supposition of the given space being divided into figures within which a circle may be inscribed touching each side. If the insect were of an oval form, and especially one of great eccentricity, the most economical division of the space would be into parallelopipeds, whose sides were tangents to the ellipse.

It is equally clear that if the cells were formed cylindrically, although there would be no loss of space in the inside, supposing the insect cylindrical, and that the loss would be the less the nearer its shape approached to the cylinder; yet the space between the cells would be a loss. Suppose they were arranged as close as possible, and so as to make the interstices the smallest possible, they must be placed around one in the centre, and touching it and each other. Therefore their tangents would form hexagons. Then the interstices or circular triangles would be space lost. This loss would (taking the surface as before) be between the cell in the centre, and the surrounding six, equal to $\left(\frac{6\,r^2}{\sqrt{3}} - C\right) \times 3$ or $\sqrt{3} \cdot 6\,r^2 - 3\,C$ equal to $1 \cdot 02 r^2$ nearly; between every four cells the interstices would be $\cdot\,34\,r^2$, and between every three $\cdot\,17\,r^2$; and the space in the first mentioned interstices would be $\frac{1}{62}$ square inch, on the former supposition of the radius being $\frac{1}{8}$ inch; in the second mentioned interstices $\frac{1}{188}$, and in the third $\frac{1}{376}$ square inch. The whole loss of space on a comb of 4608 cells would be 4607 times the space lost between any three cells—that is about 783 r^2—or (on the former supposition of $r = {}^{1}$) about $12\frac{1}{4}$ square inches, beside the loss on the outer edge, which would depend on the form of the boundary line of the comb. In any other disposition of the cylindrical cells the space lost would be much greater, and it could not be made less than this by any arrangement.*

It is evident that the saving of space by leaving the interstices between the cells is material, whatever use may be made of those cells. But the saving of space within each cell is only material where the cell is to be used for the lodging of the insect or its brood. Where it is to be used for storing honey or bee's bread, as these fill it, the form becomes immaterial. Therefore triangular or square cells, exhausting the whole space, and leaving no interstices, would have been as economical an arrangement as hexagonal ones for the cells used to

* See note at the end of this dissertation.

store provisions. They would however have had no advantage for this purpose over the hexagonal ones; indeed there would, especially in the triangular ones, have been some inconvenience in depositing the stores, because the bee could not so easily have reached every part of the cell. It thus appears that, supposing there were no consideration to enter the calculation except the saving of space, the hexagonal form is the best of any. But the saving of materials and of labour leads to the same conclusion still more strongly.

Suppose, now, therefore, that the space is of no moment, and that saving room is immaterial, and consider only the saving of wax. If the form of the cell were circular, or of any other curve, it is manifest that each cell must have its separate walls, as the neighbouring cells could only touch in one point each; and if the figure were rectilinear, but such as to leave interstices, the cells must have separate walls wherever these interstices occurred. The only figures which could enable each cell to afford walls for the contiguous ones, and thus to make each wall serve for two cells, are those which fill up the space without intervals, that is, the triangle, square, and hexagon. But it has been shown that in any given space more hexagonal cells than square or triangular cells can be placed. But let the proportions of the peripheries of these cells be considered, and we shall see more clearly the saving of wax and work effected by the hexagonal form. The triangular cell (it is immaterial whether we take the plane or the solid figures in stating the proportions) has a periphery of $\frac{18\,r}{\sqrt{3}}$, r being the radius of the insect or inscribed circle—the square $8\,r$ and the hexagon $\frac{12}{\sqrt{3}}\,r$ — consequently these figures require materials and labour to form them capable of containing the same kind of insect in the proportions of $10 \cdot 4$, 8, and $6 \cdot 9$; and the saving by the hexagonal form is therefore above one-eighth, compared with the square; and one-third as compared with the triangular form. It is true that the circular form upon a single cell would save even as compared with the hexagon—just as it would save room inside. The periphery would be about $6 \cdot 28$, or about a tenth less than the hexagons. But then the loss on several cells would be very great, beeause each cell must have separate walls; and we shall presently see in what proportion that would increase the expenditure of materials and labour. It may be sufficient here to observe, that suppose a circular (or cylindrical) cell surrounded by six others; the walls of the whole seven would be about $44 r$. Whereas if a hexagon cell is surrounded by six others, there are only 30 sides wanted instead of 42, and the whole amount is something less than $35 r$, or a saving of one-sixth.

It thus is demonstrated, that supposing the combs to be constructed of double sets of cells, each set open at one end, as they must be for the purposes of the bees, and that all the cells are of the same form, there is no form which could be chosen and no arrangement of the cells which could be made to save so much room, wax, and work as the hexagon form, and the disposition of the cells, so as to make each wall serve for two. It is also clear that, if they are to be entirely of one form, and that the hexagonal, the greatest saving will be effected by making their common junction, that is where the closed ends meet, one plane, so that the same hexagonal bottom shall serve for the opposite cells. But a much more refined contrivance is found in this part of the structure, and one better suited to the purposes of the animal, by which a considerable additional saving is made both of space, materials, and labour, and a considerable gain effected in solidity and strength. The hexagonal form, so well fitted for all the rest of the cell, is not the best adapted for its bottom; and the form of a prism, which the cell has in the greater part of its length, is changed when we approach the bottom. Let us now consider the use to which the bottom is applied, in what manner that purpose can best be answered by the form, and how that form can be best made to suit the purposes of the strict economy which is consulted throughout the whole structure.

For storing honey and bee's bread it is plainly immaterial how the space contained in the two opposite hexagonal prisms is divided; and a plane cutting them across, that is, giving to each cell a hexagonal bottom, and making that common to both, would afford the same room for the stores with any other construction. But it is not so with the other uses of the cells. The egg first deposited, and the worm in its earlier stage, require only a narrow space; and even when it has grown to its full size, from its tapering form it can easily be accommodated in a cell with extremities considerably narrower than the rest of the space. This is especially the case with the tail part, which is at the bottom of the cell; it is much more taper than the rest of the body, and considerably more so than the head, both in the grub and fly. If then the hexagonal form were preserved throughout, there would be a considerable waste of room towards the bottom. Suppose we had to pack two sets of parcels together each opposite to the other; if they were equally thick or broad throughout their whole length, as, for example, Stilton cheeses, we should place them one set upon or against the other, and could lose no space by this arrangement, nor gain any by another arrangement. But suppose each parcel tapered towards one end, like pears, or like wedges cut out of the cheeses; we should then lose room by placing the narrow ends opposite each other, and should save considerably by inserting the tapering ends of the one set of parcels in the vacant spaces left by the tapering of the other. The narrow end of each parcel would thus be inserted between the narrow ends of two others; and the whole space in which all the parcels could be packed would be shortened by the length of the centre line or axis of the parcels, reckoned from the part where the tapering begins to the narrow extremity. This is exactly what is done in the comb; each cell, from being a prism, becomes at the closed end a pyramid, terminating in a point; and the narrow end of the animal is thus placed between the narrow ends of these on the opposite side of the comb, so as to enable all of them to have the room required by their shape and size, with cells shorter than they must have been had each cell abutted on the single one opposite to it, and not been inserted, as it were, between several opposite ones.

But a further contrivance is necessary that no space may be lost between those opposite cells, and that the same bottoms may serve for both the opposite sets, else the hexagon common to both would have been more economical as to work and materials. The pyramids must be so formed as that each of its sides shall be one side of the opposite cell's pyramid. This is accomplished by the pyramid being trihedral, or composed of three planes, each of four sides. But if these three planes are inserted in the prism, they must cut off a portion of each of its walls; and it will depend upon the amount of this portion how far the surface of the whole pyramid shall be greater or less than the hexagonal bottom would have been. If a pyramid were raised upon the extremity of the walls of the cell to serve instead of the hexagonal bottom, its surface would manifestly be greater than that hexagonal bottom; but it is evident that, by inserting the pyramid partly in the angles of the hexagon, so much of the walls may be cut off as will make the whole surface that is left, walls and pyramid together, no greater than the whole walls and the hexagonal bottom would have been. But it is proved that so much of the walls may be thus cut off as to make the pyramid, together with what is left of the prism, have a smaller surface than the whole walls of the prism together with the hexagonal bottom.

Thus if this construction be adopted, each cell will be opposite to three others; its pyramidal bottom will consist of three plates or sides, each of which is the side of an opposite pyramidal bottom; the pyramid therefore furnishes on the one side the whole bottom of one cell, and on the opposite side it furnishes one-third of the pyramidal bottom of three other cells; wherefore it serves for as much bottom (for the bottoms of as many cells) as the hexagonal bottom could have done; and while room is saved by the cells being shortened as much as the height of the pyramid above the original prism, or half the whole height of that pyramid, wax and work are saved by the whole surface of the cell being less than it would have been had each cell been a prism terminating in an hexagonal plane. As for the solid space contained in the figure formed by the remaining part of the prisms and the pyramidal bottom, that will be exactly equal to the space contained in the prism terminated by the hexagonal plane, and before any part was cut off by the pyramidal sides; for the space contained in the whole pyramid is exactly equal to what is cut off from the prism together with the part of the prism which is left in the pyramid. All this is clear from the nature of the hexagonal prism and of the trihedral pyramid; and it also follows from the nature of those figures that each of the three sides or planes of the pyramid must be a rhomboid or figure of four equal sides; for the side of the pyramid must be inserted in the angle of the hexagon, whose sides being equal, so must the two sides of the pyramidal side inserted; and in order that the apex of the pyramid may be in the axis of the prism, which is necessary to make the opposite pyramids coincide, and the same sides serve for both the sets, the other two sides of the pyramidal side must be inserted in the opposite and equal hexagonal prisms, and must therefore be equal to the sides inserted in the first prism.

But we have not yet found what must be the altitude of this pyramid; or, which is the same thing, at what points the sides of the prism must be cut in order to form the bottom of the cell; or, which is the same thing, what must be the angle of the rhombus forming the pyramid's sides, regard being had to the proportion, which will make the whole surface the smallest. Thus if the top of the pyramid is a very little above the hexagonal bottom of the prism, the whole surface of the pyramid will be somewhat less than the whole surface of the hexagonal bottoms, together with the six triangles cut off from the walls; if the top is a little higher, the difference will be somewhat greater. But increase the height, and that difference will begin to lessen till it vanishes altogether, and at that point there will be no saving of surface, the pyramid being equal to the hexagonal bottom together with the six triangles cut off. Raise it higher still, and there will be a loss. Consequently, there is a point at which the saving will be the greatest possible. We may either inquire what that point is, in other words, what the altitude must be of the pyramid—or we may inquire what proportion the side of the rhombus must have to the side of the hexagon—or what must be the angles of the rhombus—or at what angle the rhombus must cut the sides of the prism—or at what angle the rhombuses must meet each other—or what must be the breadth of the rhombuses. Any one of these things being found, all the rest are determined.*

The investigation (which will be found at length in the Appendix†) gives the following result. The breadth of the rhombus—that is, a line drawn from any of the angles perpendicular to the opposite side—must be equal to the side of the hexagon; and it will follow from this, one of the diagonals of the rhombus being the diagonal joining the alternate angles of the hexagon, that the rhombuses are inclined to one another at angles of 120°, being the angles of the hexagon; in other words, the rhombuses must be a continuation of the hexagonal sides, and their angle a continuation of the angle of those sides. Hence, too, it will also follow that the side of the rhombus must be to that of the hexagon as 3 to $2\sqrt{2}$; the rhombus cuts the prism at a distance from the upper part of the prism equal to $\frac{1}{2\sqrt{2}}$ of the hexagon's side; the altitude of the pyramid is $\frac{1}{\sqrt{2}}$ of that side; or that altitude is to the side in the proportion of the side of a square to its diagonal; and, finally, it also follows, that the obtuse angle of the rhombus is

* This is demonstrated in Appendix, Prop. I.
† Appendix, Prop. II.

109° 28′, and the acute angle 70° 32′. These things are fully demonstrated in the Appendix of mathematical illustrations affixed to this Dissertation.

Let us now look to the fact, and observe whether or not the combs are constructed according to what the mathematical reasoning proves to be the best possible plan for saving surface in the cells.

In the *first* place, the cells are obviously fitted into each other as the theory requires; for the prismatic form is not continued to the end, but each has a pyramidal bottom or base, and that base is composed of three planes, each of which forms one side of the bases of three opposite cells, so that one set of those planes serves for the opposite pyramids; each cell is over against three cells on the opposite comb, and each cell has its base common to itself and those three opposite cells. The length of each cell is thus shortened without lessening the accommodation of the grub, or pupa, or bee; for in each of those states the pyramid is large enough, considering the tapering of the animal's form, and any wider space at the tail part, which is always inserted in the pyramid, would be so much room thrown away. *Secondly;* the form of the pyramid is that which the theory requires, in order that there may be no interstices, and that the pyramids may fit the hexagonal prism exactly :—the sides of each pyramid are three equal rhombuses. *Lastly;* each rhombus has the precise angles, and, consequently, as it is inserted in the hexagon, the precise length of sides also, which the theory requires in order to effect the greatest saving of surface in the work. For M. Maraldi, having measured the angles of the rhombus, found them to be 109° 28′ and 70° 32′, respectively. Therefore the other proportions must follow, and the precise point of the maximum is obtained by the bees; or they construct the bottoms of the cells in the form and of the proportions which enable them to gain the most space, and to save the most wax and work, of any forms and any proportions that could be imagined.

This eminent person, however, was not aware that those conditions had been fulfilled, and this result obtained by the bees. He saw that the pyramidal form of the base, and the fitting of the opposite rhombuses, saved both space and material in a considerable degree. He could not doubt that in order to fill the space, and make one set of pyramids serve for the opposite sets of cells, it would be necessary that the section of each pyramid should be an equilateral triangle, and consequently that the sides of each pyramid should be three in number, and equal to each other, Nor could he fail to perceive that the hexagonal figure of the cell, into which those sides were to fit, required that each should be a rhombus. But the three equal rhombuses might have an infinite variety in their angles; their sides might have proportions infinitely varying to the sides of the hexagon; and the pyramid formed by them might have infinitely various altitudes; and yet the same general structure might be preserved. The reason for the precise angles and proportions observed by M. Maraldi was not perceived by this distinguished mathematician. Though upon the verge of making the discovery, he contented himself with observing the angles, and did not ascertain that they were precisely such as made the saving the greatest possible.

This was reserved for a subsequent period; when M. Reaumur having considered the structure and the measurement of the angles, with the sagacity which peculiarly marked that great man, conjectured that this maximum point had been attained by the bees; but as no investigation of the question had ever been undertaken, it was only a conjecture. However, he soon took steps for changing it into a certainty. He proposed to M. Kœnig, an expert analyst, pupil of the celebrated Bernouilli's, the solution of the problem—To find the construction of a hexagonal prism terminated by a pyramid composed of three equal and similar rhombuses (and the whole of given capacity), such that the solid may be made with the least possible quantity of materials—which, in other words, was asking him to determine the angles of the rhombuses that should cut the hexagonal prism so as to form with it the figure of the least possible surface, since the hexagon being given this decided both their dimensions and their intersections with the sides of the cell. He did not inform M. Kœnig of Maraldi's measurement until after he had solved the problem, and had assigned 109° 28′ and 70° 34′ as the angles, when he sent him the Memoirs of the Academy of Sciences for 1712, containing M. Maraldi's paper, and M. Kœnig was equally surprised and pleased to find how nearly the actual measurement agreed with the result of his investigation. The difference was only two minutes; and it has generally been supposed since then either that M. Maraldi's measurement was erroneous, or that the bees failed by that small quantity to attain-the point of the minimum. There is, however, no foundation for either supposition; the measurement of Maraldi is correct, as we have every reason to believe, and the bees have with rigorous accuracy solved the problem; for the error turns out to be in M. Kœnig's solution. The steps of his process are not given by M. Reaumur, nor am I aware where they are to be found. Possibly it is in the logarithims that he has, by neglecting some decimal places, gone wrong. This much is certain, that the true solution is not 109° 26′ and 70° 34′, but 109° 28′ and 70° 32′, exactly as M. Maraldi found the angles to be by his measurement. That there may be no doubt respecting this matter, Mr. Maclaurin's subsequent solution* having been geometrical, while M. Kœnig's is stated to have been by the differential calculus, I investigated the problem by that

* It is singular that so learned a mathematician as Dr. Reid should have given so erroneous an account of the history of this discovery. He describes Mr. Maclaurin as having resolved the problem "by a fluxionary calculation," in the Philosophical Transactions, whereas his investigation there is purely geometrical, and intended to show the power of the ancient analysis. Dr. Reid also represents him as having ascertained the angles "by the most exact mensuration the subject would admit," whereas the measurement had been made thirty years before, and was never repeated by him at all.—Essays, vol. iii.

calculus in two several ways, and desired a learned and skilful mathematician* to investigate it in his own way, which turned out to be different from both mine. The result of the three methods was the same, and coincided not with M. Kœnig's result, but with M. Maraldi's measurement. It is also to be observed that, for the purpose of avoiding all doubt that might arise from the logarithms, one of my solutions is purposely addressed, not to the angles of the rhombus, but to the angle which is made by the planes of the two rhombuses, because, that being an angle of 120°, is found without any fraction or approximation. The whole investigation is given in the Appendix of Mathematical Illustrations. It may further be observed that the precise length of the perpendicular from the angle of the rhombus to the opposite side, that is, the breadth of the rhombus, being the side of the hexagon, as found in that solution, at once indicates the exact angles; for no other angle than 120°, formed by the two rhombuses inserted in a hexagonal prism, could give this exact breadth. This

* My worthy friend and neighbour, Mr. Slee of Tirrell (Westmoreland), well known to those who pursue their studies at Cambridge.

angle being a continuation of the hexagonal angle is a clear proof that the angles, as measured and actually made, are those given by the investigation; for no difference could on this part of the reasoning be introduced by the logarithmic approximations.

The construction of the cells, then, is demonstrated to be such that no other which could be conceived would take so little material and labour, to afford the same room. In order to ascertain how great a saving is effected by this construction, it is necessary to compare it with some other, and the one which most naturally suggests itself is that which of all others comes nearest to this, namely, the hexagonal prism terminating in a hexagonal bottom. For we have already seen that this is considerably more economical than the only other figures which fill up any given space, those whose sections are a square and an equilateral triangle. Compared then with such a prism, the cell which terminates in a pyramid whose angles are those formed by the bees, effects a saving of surface equal to $\left(\frac{3\sqrt{6}-6}{\sqrt{8}}\right)s^2$ or $\frac{3}{2}\left(\sqrt{3}-\sqrt{2}\right)s^2$, s being equal to the side of the hexagon. The saving then is $\frac{12}{25}$ of s^2 nearly, and taking $s=1{\cdot}387$ of a line is about $\frac{23}{25}$ of a line square, the whole surface of the bottom being about 8 square lines; taking the average of the working bees' cells $s=1{\cdot}38$, ($1{\cdot}387$,) and the height $=5$, the saving is ·91 square line upon a work of 8·1, or equal to between an eighth and a ninth of the work on the base; and on the whole work, (45·68,) nearly $\frac{1}{50}$. But this is in truth (though coinciding exactly with the amount of saving deduced from Maclaurin's solution) much under the real saving effected upon the whole—for this supposes the length of the cell to be given, proceeds upon that, and only compares the saving upon the bottom of the cells. But as a certain length of cell is required for the bee, if the cells were not fitted into each other by the pyramidal form, but were opposite to each other, and joined by the common hexagonal plane, each cell must be lengthened by a line equal to the height of the apex of the pyramid above the plane of the hexagon; consequently the two opposite cells, or the whole prism composed of these cells, must be lengthened by the whole altitude of the pyramid, the whole surface of the bottom being nearly 12; and this will make the difference between the surface of each two cells having a common hexagonal bottom, and the surface of the two cells with pyramidal bottoms but fitting into each other and to a third cell, equal to $\frac{(3\sqrt{6}-1)}{\sqrt{8}} s^2$; or 1·53 of s^2; and taking $s = 1\cdot38$ line as before, 2·94 square lines upon a work of about 85 square lines, supposing the length of the cell from the acute angle of the rhombus to be 5 lines, that is a saving of nearly one twenty-eighth on the whole of both sides of the comb. The saving of room in the hive by this shortening of the cells is also very considerable. It is equal to $\frac{s}{2\sqrt{2}}$ in each comb; if then there are ten combs it amounts to above $3\frac{1}{2}\ s$, or if s be taken at 1·38, to above $4\frac{1}{3}$ lines; and if the depth of the cells is 5, to near a twentieth of the whole space occupied by the combs.

This saving is effected, however, not merely by the angles of the rhombus being of the size pointed out, but also by the fitting of the opposite cells. Part of the saving therefore is owing to this, and part owing to the minimum proportion of the angles of the rhombs. If they only have one of their diagonals equal to the diagonal of the hexagon ($\sqrt{3}$.), they will fit each other and effect the saving in the length of the cell. But unless they also have the angles of a certain proportion, there may be a loss on the whole as compared with the hexagonal prism; and unless they be of the given proportion, there cannot be the greatest possible saving as compared with that prism.

The comparisons hitherto made have all proceeded upon the supposition that the cells must have not only a given capacity, but a given length. It is manifest that if they were only used for storing honey and bees' bread, the capacity alone would be material; the length is rendered material by the necessity of room being provided for the insect, and especially for the young bees. If the cells were not required to have more than a given capacity, a greater saving could be effected by a construction which should vary the proportions of their width and depth, leaving their capacity the same. By the same kind of investigation, which leads to ascertaining the form of the base most conducive to saving wax and labour, we find (as is shown in the Appendix) that the proportions between the hexagon's side and depth of the cells must be 2 to $\sqrt{2}+\sqrt{3}$, or about 2 to 3·14, the breadth and depth of the cells nearly equal, and the rhomboidal base cutting the plane of the walls at somewhat more than three-fourths from the open end. The saving effected by this construction as compared with the one actually employed by the bees, supposing still the sides of the hexagons actually made by them to be 1·387, and the depth 5 lines, would be 7·41 square lines upon a work of 38·28, or nearly a fifth of the work and wax, or if we include the outer base in both cases, the saving would be 3·8 upon 46·88, or above a twelfth upon a single cell.* If then the only object for which the cells are made, were the storing of bees' bread and honey, supposing

* M. L'Huillier (*Berlin Mem.* 1781, p. 280) states that P. Boscovich's solution agrees with Maclaurin's in the Philosophical Transactions, 1743; and yet he seems never to have seen Maclaurin's; for he says, "All these mathematicians have considered this matter as beyond the powers of elementary geometry, and as requiring of necessity the application of the general principles of maximum and minimum founded on the differential calculus or on the limits of ratios;" and he seems to think himself the first who has shown that the problem could be resolved by elementary geometry, whereas Maclaurin's solution is by purely elementary geometry.

that so shallow and wide a cell could equally serve these purposes with a deeper and narrower one, there would, *upon a single cell,* be a waste of materials in the construction employed by the bees as compared with that which we are considering. But the objection is manifest to an arrangement which would make the whole weight of the fluid in the case of the honey, press upon so wide a surface as between 8 and 9 square lines of the wax with which the outer orifice is closed, instead of somewhat less than 5 square lines, the average size of the present orifice in the common cells. The film of wax now sufficient to contain the honey would no longer be enough, and a surface of 8½ lines at least would be required, which could probably not be applied after the cell had been filled with honey, certainly not unless the honey was extremely viscid. But the other use of the cell, and the more material one, of breeding, is also to be considered. The worm would be deposited in the large pyramidal base instead of the one adapted to its size, and when it grew there would be no room for the length of it, or of the pupa and bee after its transformation, the whole depth of the cell from the apex being only about 3½ lines. But there would be no support for it unless it moved out of the base on the side, and then it could only be supported in one angle of the prism, for it would be too small to fill the whole; so that the line which it would have for its length would be little more than two lines, while all the width of the cell would be lost. In this position the worm could not be reared, and it could never spin its cocoon. But it will be afterwards clearly shown that if the whole structure of the comb is considered, whatever may be the relative saving of wax and work upon a single cell by taking the greatest width and the depth nearly equal, as in the above construction, there would be a considerable loss upon the whole structure, and that the actual proportions adopted by the bee are more economical.

The saving of materials effected by giving the cells such a form as enables each wall and each base to serve for two cells is obviously the greatest saving of all; and we have already adverted to it. But doubts have been of late years entertained how far the walls and bottoms are common to more cells than one; this part of the subject therefore requires further illustration, before we proceed to consider upon what those doubts rest.

Suppose we take any number of equal hexagonal cells, whether terminating in pyramids as in the combs, or formed as hexagonal prisms, and place them round one cell so that their sides touch; and suppose we place in the like manner an equal number of such cells in a second set or tier, so that their bases touch those of the first tier—it is manifest that the number of bases required will be double that which would be required if the bases of one tier served for the cells of the other, and that double the number of walls will also be required if we only reckon those walls which touch each other. But as there are the outer walls of each tier to be added, the whole proportion of difference occasioned by the cells being separate, and having each its own walls, will not be that of 2 to 1. If there are 14 cells in all, that is 6 placed round one on each tier, or each face of the comb, then if the cells are separate there will be required 14 bottoms instead of 7, and 84 walls instead of 60, or in the proportion of 2 to 1 as to the bottoms, and 7 to 5 as to the walls. If instead of two sets, that is one set of 6 round a cell, there are any number n of sets, including the first cell as one set, then the number of walls saved on each tier (or face of the comb) will be equal to $9n^2 - 15n + 6$, and the expense of labour and materials occasioned by each cell having separate walls will be in the proportion of 2 $(3n^2-3n+1)$ to $3n^2-n$. Suppose the sides of the cells as before 1·387 line, their depth 8 lines, and that there is a square foot of comb; this will make the breadth of the cells about 2·77 lines, and n will be between 27 and 28; but take it at 28, the waste will be in the proportion of about 110 to 56, or somewhat less than 29 to 15 on the walls, and exactly 2 to 1 on the bottoms, and on the whole work about 51 to 26. The number of square inches of wax required for the comb, if each cell were separate, would be 3115; if the walls and bottoms of one serve for those of the other, only 1588 would be wanted; so that nearly double the amount of labour and materials would be required if the cells were separate, and had each a base and walls of its own. If the walls only of each cell are separate, and the bases are common to the opposite cells, the waste would be somewhat less in proportion, but would still be very great—it would be 1366 square inches of wax upon a work of 1428.

Now we must admit that this renders it extremely improbable that such should be the structure of the comb, especially when we perceive the extraordinary refinement of the contrivance resorted to by the bees for the purpose of effecting a much less considerable saving in the construction of the bases, a saving of only one-tenth of the whole labour and materials employed. Nevertheless, if the fact is otherwise, the argument from probability must of course go for nothing. Let us therefore now examine the fact.

The statement rests upon a paper of the late Dr. Barclay, of Edinburgh, published in the transactions of the Wernerian Natural History Society (vol. ii). He sends to that body some pieces of honeycomb in which young bees had been reared, and observes, that the partitions between the cells at the sides and base are all double; that each cell is a distinct, separate, and "in some measure an independent structure agglutinated only to the neighbouring cells; and that when the agglutinating substance is destroyed, each cell may be entirely separated from the rest." He makes the same observation upon the cells of wasps, and adds, that the agglutinating substance is more easily destroyed in them. From a very allowable deference to the authority of this distinguished anatomist, and possibly from recollecting how much this branch of natural history owed to the discoveries of a great physiologist,* naturalists appear to have at once adopted his proposition, and they speak of it as "Dr. Barclay's discovery," without considering that it rests upon a single observation of one kind of cells, namely, those in which bees had been bred, and that it is wholly irreconcileable with the observations of Reaumur, Maraldi, and above all of Huber. That some had denied it, however, and upon this ground, appears from a note in Kirby and Spence (Introduction to Entomology, vol. i. p. 485), although those eminent naturalists, in the text both of that and other passages (as p. 502), lay down the position as admitted that the cells are double. Nothing, certainly, could be more unaccountable than that such a thing should have escaped the most laborious and accurate of observers, those illustrious foreigners whose names have just been mentioned. But that is not all; for if the position be true, the description of the process of the bees in making their cells, as given by Huber, must be wholly incorrect. The two accounts cannot possibly stand together. But there can be no doubt whatever that Dr. Barclay was misled by the cocoons of the chrysalis, the only cells which he

* J. Hunter, Phil. Trans. 1792.

examined having, by his own account, been those in which young bees had been hatched; and he having taken no step for ascertaining whether what he took for a second wall and base was made of wax or of silk. My reasons for stating this so confidently are as follows: but the experiments made and related hereafter complete the proof, and show how Dr. Barclay was deceived.

1. I have examined minutely a great number of combs with the help of powerful microscopes, as well as by the naked eye, and I never have been able to find the least appearance of a double wall, or double base. On the contrary, the sections of the wax, in what way soever they are made, plainly show that the plate is single in every instance. Combs have been thus examined of every kind, both those in which honey and bees' bread had been stored; those which were new made and had never received any stores; and those which having been filled with honey had been robbed by wasps. It was only cells where bees had been reared, and where the silk cocoon had been left, that presented anything like the appearance of a double plate. Nor can there be a doubt that this is always found in such cells. The exuviæ of

the larvæ, with the filth, are well known to be always removed; but the silken lining spun by the larva previous to taking the chrysalis, still are expressly said by all naturalists* to be suffered to remain as strengthening the cell. They are not waxen of course, but silken, and form a lining to the waxen plates, assuming their shape exactly; and in old cells, where many successions of bees have been bred, the space is visibly contracted by the cocoons remaining; and these may be taken out, leaving the wax entire, with its plates all single. The cocoons come out of the shape of the cells.

2. I have communicated with other observers upon this subject, and having set them upon examining the facts, I find that none of them can discover for Dr. Barclay's hypothesis any other foundation than the conformity of the cocoon in shape with the wax plates.

3. Not only are the accounts given by former observers, as Reaumur, and especially Huber, quite inconsistent with the hypothesis of double walls; it seems hardly conceivable that these should be made of wax with agglutinating matter between the plates. The wax contains none of this matter

* Reaumur, v. 600. Kirby and Spence, vol. ii. 197.

in itself; and it is inconceivable that the bees should be able to insert it between the plates, as indeed it is that the bee should make two plates in the manner of its working, which consists either in first raising a thick wall and then drawing it out, or in placing new wax upon it, but in either case in scraping it thinner and polishing it, and making it plainer as well as thinner after it has been first raised. How could it get between the two plates to scrape and plane them? and yet it is not pretended that each plate is not as plane on one side as on the other—as plane on the side, which by the supposition is the inside, or the side covered by the other plate, as on the side exposed to the air, and to the scraping and polishing operation of the bee. As for the agglutinating material, either it may be in the silk, or it may be only the adhesion of tha to the wax.

4. The examination of wasps' nests confirms the same opinion, and shows how Dr. Barclay has been misled. Indeed he has remarked, that in those nests the agglutinating material of which he speaks is less adhesive, and that the double walls are there more easily observed. If a wasp's comb, in which young have not been hatched, is examined, the

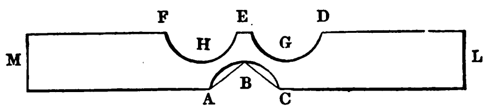
cells will be found single like the bees' cells. But where the larvæ have spun their cocoons it is found that each cell has a lining. While recent, the lining is moist and can be more easily extracted; but even when dry it can be taken out. It is greyish or white, like fine cambric paper, and semitransparent; the cell itself being brownish, like coarse paper, less tough than the white, but thicker and much more opaque. The white lining takes the hexagonal form of the cell exactly, and retains it if extracted when moist. The white cells thus formed by the cocoons which the larvæ spin are quite unconnected; and when removed leave the comb entire of brown paper hexagons. The walls of these cannot be split into two laminæ. But when they are lined with the white paper and you try to tear them asunder, you can easily do so; and the same wall appears so split in two; but one side only of the rent is brown, the other is white. So when the two papers adhere so closely that you cannot separate them entirely, some part of the white cell taken off will appear to be brown; but then there is a corresponding hole opposite in the brown cell from which it was taken, or if that cell is still lined with white paper, the white paper appears through it, at the vacancy where the brown was torn off. In short, nothing can be more clear than that the cells are originally made single, and that the apparently double wall is the lining of another material spun by the larvæ. It must, however, be observed that the economy of the material is not so great in the wasps' as in the bees' comb; the brown paper apparently being much more abundant than the wax, or we ought rather to say the material (filings of wood) from which it is made by the wasp being more easily procured than that from which wax is secreted by the bee (sugar). The hexagonal form is, therefore, chiefly important to save space and labour. The double wall would greatly increase the demand for the latter.

After all, it is possible that this white paper lining may be made by the wasp after the original brown cell has been formed. The necessity of economizing the material does not exist, and the labour of the wasp is much greater than that of the bee; for a single wasp makes the first portion of a comb without any assistance. It is impossible to compare the two kinds of paper together and not be satisfied that they are made by perfectly different processes. The brown may be made by kneading together the fragments of scraped wood and moistening them; but the perfectly uniform texture of the white plainly shows that it is the result of a secretion. No paper that we manufacture is more fine and perfect in its structure. It must have come from some pulp, the result of a chemical process and not of any mechanical operation, whether it be secreted in the body of the larva and spun by him, or secreted by the wasp and plastered upon the coarser, brown material.

Having then ascertained the facts with respect to the form and position of the bees' cell, the nature and amount of the saving in room, in work, and in materials, which their structure effects, and the precise manner in which that saving is made by the structure, we proceed to inquire how the insect works in order to form it; and here several explanations that have been given, founded for the most part upon an erroneous conception of the facts, must be first of all considered.

1. Buffon's is the most superficial, and, we may add, the most absurd of these; it has been universally given up, and yet the mistake upon which it rests has been at the bottom of some later

theories. This will oblige us to consider it more at large than its own merits would require. He supposes that the cells by pressing upon each other take the hexagonal form, in like manner as soap bubbles blown together in a heap are observed to do. That an *appearance* of hexagons would be exhibited by such bubbles from their apparent intersections is possible. No hexagonal prisms, however, are really so formed. But let us admit that he takes the bubbles only as a familiar illustration, and means to speak of a congeries of cylinders, if bubbles could be so blown; and let us also admit that he means to reason upon such cylinders disposed in sets, beginning with a set of six round one cylinder in their centre. This is the only way in which anything like the result could be obtained, and it is a perfectly gratuitous supposition; but let us grant it for argument's sake. Now, if the cylinders are so disposed, it is certain that the planes passing through their lines of contact will form hexagonal prisms; consequently it may be contended that if each cylinder is pressed upon those surrounding it, the curve surfaces will become planes, and hexagonal prisms be formed; and as of all the cylinders placed in juxtaposition each will be the centre of six others, it may be further said that the whole must become hexagonal prisms. But after making every such admission, there remain two requisites which cannot be admitted, because both are contrary to the facts. In order that the prisms may be thus formed there must be cylinders first formed touching each other, and then these must be pressed against one another so as to bring their sides into the form of planes instead of curve surfaces, and the pressure must be the same throughout the whole number, in order that all may be equally brought into the prismatic shape. But neither the separate existence of the cylinders, nor the pressure, exists at all in the structure of the comb. The cells are not first made cylindrical and then pressed together. They are seen by observers to be formed originally in planes, with the exception of the first excavation in the cake of wax, which is a cylindrical groove, and is immediately made plane. It is made plane, too, before any of the six supposed cells are formed, and when there are at the utmost only one on one side of the cake, and two on the other; but, in fact, the plane form is given to the curve surface when there are not three cylinders or parts of cylinders made contiguous to each other, but when there are three grooves in one cake of wax, and the bees on the opposite sides are working at the grooves respect-

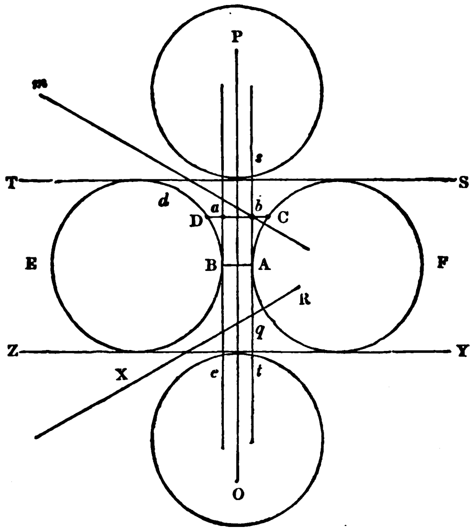


ively. It is not that A B C, D G E, and E H F, are first made and press against each other at their lines of contact, but that in the cake L M there is a groove A B C made, and on the other side two grooves, D G E, E H F, and by eating away and plastering on C B A the planes C B and B A are formed on one side the cake, while by the like process on the other side, and, if not at the same time, immediately after, the opposite grooves D G E and E H F are made planes, and planes coinciding with C B and B A. Then these planes are not the six walls of the prism at all, but only the three rhomboidal bottoms; and upon the edges of those bottoms the walls are afterwards raised, and are plane from the very first, and perpendicular to the plane in which the cylindrical grooves are formed. Nothing, therefore, can be more unconnected with any curve surface than the walls are in every stage of their progress. The first requisite then is entirely wanting, that of separate cylinders to press against each other.

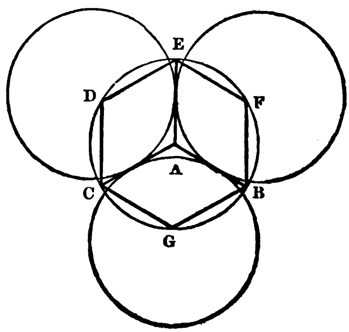
The pressure is equally wanting. That could only be given in one of two ways. Supposing, contrary to the fact, that there were a congeries of cylinders formed and touching each other, either these cylinders might by their gravity (which seems to be Buffon's hypothesis) press on each other, or the insect, by its growth or other efforts, might press from the inside of each cell. But the pressure of gravity, supposing there were no other objection to its operation, would not be equal; it would make the cells all of a different form according as they were lower or higher on the comb, while the upper ones of all sustaining no pressure would be cylindrical; besides that, a pressure sufficient to alter the form of the lower cylinders would be sufficient to tear the walls from the bases of the cells. The supposition of any expansion or motion of the insect, independent of other manifest objections, is precluded by the fact that the cells are as perfectly hexagonal in which no bees have been bred as those which have had young; and suppose it were admitted that the working bees could by internal exertion in the cells press them against each other, still this all rests upon a merely gratuitous assumption, verified by no observations, but contradicted by all observation; for no one ever has seen the cells in the supposed state of cylinders; on the contrary, all observers have found them in their progress of formation as prisms, and some have seen the very process of formation. Nothing therefore can be conceived more groundless than this hypothesis of pressure.

2. A much more ingenious and plausible theory has been in later times advanced, but founded in a great measure upon the same fallacy of supposing separate cylinders, although not upon the other fallacy respecting pressure. It is maintained with great ability in the article Bee, of the 'Penny Cyclopædia,' by a distinguished naturalist. It is supposed that the bee first makes cylindrical excavations, which are separated from each other at their contact by the thickness of the wall intended to be formed, and then cuts away so as to make the cylinders hexagonal prisms, the walls being of that thickness. Thus A F and B E being sections of two cells, which in their nearest parts are at the distance A B, the thickness of the intended wall; the

bees are supposed to excavate in all parts of A C F and B D E, so much as always to leave a part *a b*

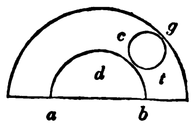


equal to A B; and as the planes O P thus formed are tangents to the cylinders they will form hexagonal prisms. But an additional hypothesis is requisite for this theory; and accordingly it is supposed that the bee has a peculiar instinct, which impels it to excavate in the direction of A B and C D, but prevents it from ever going so far as not to leave the requisite thickness A B, *a b*. It is then said that this instinct, together with the fact gratuitously assumed of the excavations being at first cylindrical with spherical bottoms, will of necessity lead to the formation of hexagonal prisms, with pyramidal bases composed of three rhombuses; in proof of which the intersections are given of three circles with a fourth from the centre of which the tangents drawn to the points of contact of the other three will no doubt form three rhombuses with the lines drawn parallel to these tangents, as A G, A F, and A D.



A little examination will show the entire fallacy of all this in every particular.

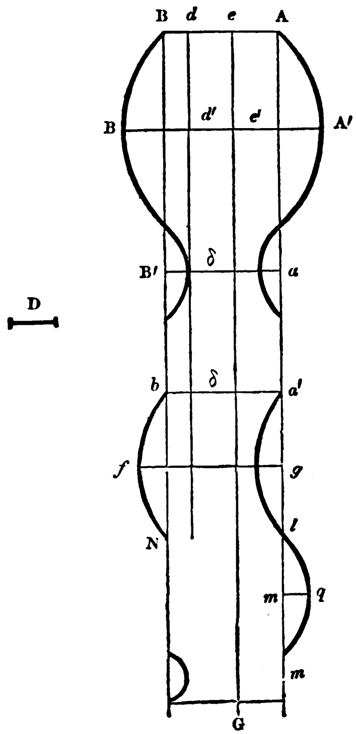
The assumption of the peculiar instinct respecting the thickness of the wax is only gratuitous, and it cannot of course be disproved. But the assumption of the cylindrical excavations is contrary to the known facts. The groove, or fluting, first made in the cake is no doubt, at first, not only cylindrical in the direction transverse to the axis of the cake (that is, the direction across the cake), but also in



the direction of the axis (that is, along the cake, as at *c*); and if the excavation were made deep like the cell, walls as well as base, and in the form *a b d*, and the next cells in the form of *g*, there might be some ground for the hypothesis. But we know that the groove *a b d* is only made deep enough to form the base, and that its outline is changed into straight lines before any of the wall is raised, the walls of the cell being made, not, as the hypothesis assumes, by a cylindrical excavation, but by raising planes on the outline of the base. But admitting this excavation, contrary to the fact, the supposed instinct would not account for the formation of the plane sides out of the curve surfaces. If the bees excavate in the direction A B (p. 268), and then in lines parallel to that, supposing them to have found that line by the instinct showing them the thinnest place, they would go on till they got to S T, the tangent of the two circles on the one side, and to Y Z, the tangent on the other side, making a plane of the thickness A B, and of the length *t s*. But that is not what is wanted to make the side of a hexagonal prism; they must stop at *q* and at *b*, the points where the tangents M R and *m c* intersect the tangent O P. So that there must be another instinct, wholly unconnected with the thickness of the plate, to prevent them from carrying the working beyond a certain length. Nor can this difficulty be supplied by supposing two bees to work, one from the thinnest part B, and the other from the thinnest part *d*, for the cell can only contain one bee. The supposition then must be an instinct to work a certain length and in a certain direction; but that is, in other words, an instinct to work a hexagon, which, therefore, is assuming the very matter in question.

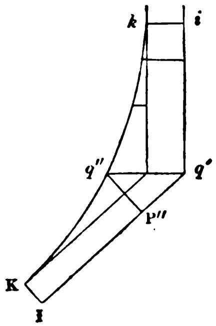
Nor is this all. The instinct respecting the thickness of the plate will not advance us even the first step, that is, the formation of the plane O P of parallel sides and equal thickness (the purpose of supposing it), unless at the same instant we suppose two bees at work one in each cell, one working from A and C towards B and D, and another from B and

D towards A and C; for if one bee only is at work from C towards D she will go through, beyond A, till she gets within less than the given distance A B



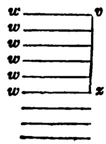
of the surface at D, and consequently will make a curve surface, and not a plane. Indeed all the cakes on which the bees actually work are unequal having on each side convexities, concavities, and planes; so that a section of the cake is as in p. 272, which represents all the possible combinations of those inequalities. If one bee works alone, D being the given distance or thickness at which the instinctive fear of perforating acts and stops the excavation, *d* B being taken equal to D, she would penetrate from A to *d*, and in like manner from A′ to *d*′, and from *a* to δ, B′ δ', *b* δ &c. being always equal to D; consequently the line *d d*′ δ would be parallel to B B′ *b f* N, and no plane could be formed. The length of the lines A *d*, A′ *d*′, and δ *a* being by the hypothesis immaterial, the surface formed would therefore be parallel to the opposite side of the cake, whatever might be the inequalities on either side, and a plane surface could only be formed in the one case of the side B B′ *b* being itself a plane surface, in which case the instinct is not wanted, there being a plane surface formed already. The hypothesis must therefore be, that while one bee is working from A towards *d*, another is working from B to *d*; and that the instinct operating as soon as the one arrives at *d* and the other at *e*, each retreats and excavates in the next line parallel to A B or *a b*; A *e* being equal to *e d* and each equal to $\frac{D}{2}$. But the two working together could only form a plane in the case of the axis of the cake *e* G being the axis of the opposite surfaces; that is, in the case of the opposite sides A A′ *a* and B B′ *b* being exactly similar; for if they are not (which is the case in nature), in order to form a plane by the supposed instinctive fear of perforating, the bee working on the side A A′ *a* must work with the same velocity as the other from A to *a*′; with a smaller velocity from *a* to *g*, with a greater velocity from *g* to *l*, the velocity of the other bee being accelerated from *b* to *f*, and retarded from *f* to N; and these accelerations and retardations must vary according to the form of the two surfaces.

If, however, the cylindrical excavations are supposed to be perfectly smooth, and of equal curvature, still the two bees must work with exactly the same velocity in order to form a plane, and must begin working at the same instant; and must each have the same instinct of stopping at the same point (p. 268) *q* and *b*; and then a third bee must begin to work in the cylinder P *s* towards the tangent C *m*, and the bee in F R must work towards X *q*, while a fourth bee works in O *t*. Suppose it could be arranged among them that each should be ready to begin working in one line at the very moment the other had finished working on another, yet no bee ever works long on any part of the comb, but is relieved by a succession of workers; and therefore this succession must also be so arranged that each shall be relieved exactly at the time when a line has been finished, and before another is begun upon. Then suppose a bee comes to the point q where she is to stop, and begins on the other line q M; when she is in the angle at q, he must work through, and indeed all along the line q X, because at that point q she has opposed to her not the thickness of the wax bounded by the circle *e t*, but the whole wax from q along towards X. The supposed instinct therefore would never stop her in that direction, there being no vacant space nearer than the circle X M. Indeed the theory wholly fails to provide for the excavation of the six angles of each hexagon, for the bee must work in lines parallel to the shortest line I K, *i k* (p. 276) which alone the supposed instinct shows her; that instinct giving her no other indication of any direction. When she comes to $q'' q'$ therefore, or to $q'' p''$ and stops, she must go on in the direction



q' i, in lines q'' q' or q'' p'' parallel to i K or I K respectively; and must therefore leave the wax in the space q'' p'' q' untouched. This wax can only be excavated by changing the direction of the working; as either by changing from the parallel lines when the bee comes to q'' or by all along working in the direction of the radius of the cylinder, and not in parallel lines. But she can neither make that change, nor find the direction of the radius by merely knowing the direction of the lines I K, the thinnest part, and the supposed instinct shows her nothing beyond this line I K.

But again, when we speak of the bees working towards one another in the lines A B, C D (p. 268), we are supposing them to excavate each with perfect equality, and to penetrate in that direction in precisely the same time. Now this implies that they must each not only work in the very direction to make them meet, but that each must remove



the same mass of wax in the same time; that each must every instant take exactly equal particles of the wax away; and the whole hypothesis rests upon their working in lines *w, w,* till by meeting at the given distance they leave the plane of the given thickness; and not upon their ever clearing away the wax in the direction *v z,* after once reaching from *w* to *v*; for if they are to do so, they must be able to draw that line *v z,* and this requires another instinct to be supposed. Now the moving from *w* to *v,* and throwing back the particles excavated, and then going back again to *w* and so on again towards *v z,* is one of the most difficult operations that can be conceived, especially when it must be performed in exactly equal times by the bees on the opposite sides; and after they have gone through this operation along the whole line *w v,* only one layer is excavated, and the same operation has to be performed on the next, and so on until the whole depth of the cylinder is thus excavated. And if in the whole of these hundreds of perforations or punctures one bee makes one single movement different as to the time or the direction she takes from the movement made by the opposite bee, there will be an inequality, or a hole in the wall.

But lastly, how was the original cylinder formed which this theory assumes? Is it at all easier for the bee to make an exact cylinder with a smooth curve surface, than to make a hexagonal prism? Apparently not. Then why assume the bee to have the power of doing that without any peculiar instinct, and suppose the instinct about the fear of perforation, in order to explain the making of the plane sides of the hexagon? The theory clearly is defective on this if on no other ground, and it either supposes unnecessarily a principle not wanted for explaining the phenomena, or it leaves the phenomena unexplained for want of a necessary principle.

But if all that has been stated were left out of consideration as regards the hexagonal walls of the cells, the theory would still fail completely as to the rhomboidal bases. It is an entire fallacy to suppose that the intersections exist as in p. 269, even upon the hypothesis of cylinders, and it is an equal fallacy to suppose that, if they existed, they would form the three rhombuses as they are found in the comb. The circle from which these intersections are supposed to arise, is merely ideal; admitting the three cylinders to be exactly as given in the figure, the fourth circle cannot possibly be part of any cylinder; for none of the cylinders by the hypothesis intersects three others. But if it did, it would not make the rhombuses required. Three rhombuses are no doubt formed by the intersections of four circles, as described in the figure; but their angles are 120° and 60° respectively. Now the angles of the rhombuses in the comb are according to the measurement 109·28′ and 70·32′; and these are the angles given by the solution of the problem of *maxima* and *minima*, as has been already shown. The mistake of supposing that because three rhombuses are formed by the circles intersecting, therefore this hypothesis tallies with the construction of the bottoms of the cells, appears to have been the chief reason for adopting the theory; and yet it is clear that the entire difference of those rhombuses, and this difference too in the most artificial and singular part of the whole structure, at once shows the necessity of rejecting the hypothesis, supposing there had been no other proof of its inapplicability.

3. The attempts that have been made to explain the construction of the cells by a reference to the form of the insect's body are equally unsuccessful, and proceed, indeed, upon an obvious fallacy. For unless it can be shown that there are some parts of her body of the very size as well as shape of the hexagon and the rhombus, nay, unless it can be shown that these parts are placed at the same inclination to each other as the rhombuses are to the walls and to one another, the argument would not be at all advanced. The consideration of this will render it unnecessary to show in detail that the mere possession of the parts which the bee has, and with which she works as with tools, cannot enable her, without more, to form the cells.

Suppose then it were found that there is in some part of the bee a completely formed hexagon, but much smaller than the cell; and also in some other part a rhombus of the angles 109·28 and 70·32 respectively; but also much smaller than the plates of the base—First, how would this lead her to form either the walls or the base? There is no reason for her making them of those shapes, merely because she has the model in one part of her body, any more than of the shape of her other members. But next; supposing all her members were hexagonal or rhomboidal, why should she make the cell hexagonal and the base rhomboidal? Again; suppose that difficulty got over, why should she take any part of her body for a model? All these objections apply to her intention, her choice of the design. But there remains an equally insuperable objection as to her power of working, supposing her selection of the design to be made; for the having one of her parts hexagonal does not enable her to make a hexagon of a larger size. If an artificer has a model, he can only work according to it either by having acquired great skill from experience, and thus possessing a practised eye and the requisite slight of hand—or by using instruments which enable him to follow the model without having so much practical skill of eye and hand; for some such skill he must have, even to follow a model by means of instruments. But the insect has no instruments; and even if we suppose that her limbs would turn out to be instruments did we thoroughly understand them, she still, without being taught, has the power of working by means of them to a model exactly as men learn by experience to do. But this is a violent supposition; for it is plain that she works without any instruments to guide her. Then granting her to have a hexagon and rhombus in her possession, nay, supposing them laid down before her, at the very least she is able to draw lines parallel to their sides. Now to do this with never-failing accuracy demands great skill, not much less than to make a hexagon and rhomb without any model at all.

It is, however, said that the bee may have not models merely in her own members but tools which can at once make the angles required. This, if it be not another form of the same hypothesis, is certainly open to the same answer. For suppose we should find some limb of the insect having a part with an angle of 120, and others with angles of 109·28 and 70·32, which are the three angles formed in the cells; and suppose we get over the first difficulty as before—why should she use those parts in making angles, and not only so, but use the proper parts in making the proper angles in the cell and base severally? The natural tools might enable her to make the angles, but they never could enable her to make them at the proper places;

that is, the angles of 120°, for example, at equal distrances from the centre which she has not found, of a circle which she has not described, and also at equal distances from each other, or to join the angles by straight lines; that is, to continue the lines forming each angle and join them with the lines forming the two adjoining angles. In truth, no hypothesis of this description will account for the phenomena, unless it should be assumed, contrary to the known and manifest fact, that the insect has some limb of the size and shape of the whole cell, and is endowed with the power of forcing it into a cake of wax, or that the insect's body, when coiled, is of the size and shape of the cell, rhombus as well as hexagon, and has the power of so forcing itself into the wax. This would account for the form of the hollow, and then there would still be left to explain how she is enabled to place her limb or her body so as exactly to form the contiguous cells, leaving everywhere the same thickness of wall, and not only making the wall of each cell of the same thickness in all its parts by forcing in the precise direction required, but making all the walls of all the cells equally thick (which implies the having found the centres of the circles), and making all the cells of exactly the same depth; operations not much less difficult than forming the hexagon and rhombuses themselves without any model or tool. No such hypothesis, then, would advance the question, even if the facts bore it out, and if the bee was found not only to possess the form required, but actually to make the cells in the way supposed, and had some method hardly to be conceived of disposing of the wax forced out of its place, instead of working, as she does, by digging, scraping, and moulding. These facts, did they exist, would deserve our attention no doubt; but they would not explain the whole phenomena, nor would they deserve our attention more than the facts which are found to exist.

That no such facts exist as we have last supposed is admitted; but not even any of those first supposed exist, as limbs or other parts having the angles required. Mr. Huber examined all the parts of the insect with the utmost care, and could detect nothing of the kind. The teeth, feet, and antennæ, present no appearance of angles, and the head has an acute angle, which, supposing it to be that of the rhombus, would leave unexplained its obtuse angle as well as the angle of 120°, at which all the plates of the wall are inclined to each other and those of the base to the walls. But it is with the teeth, feet, and antennæ, that the insect is, by actual observation of the same naturalist, known to work; and these present no appearance even resembling any portion of the structure, though they are most curiously contrived for enabling the operation of moulding to be performed with delicacy. The antennæ, in particular, composed of twelve pieces, cylindrical, globular, and conical, are plainly so contrived as to have every possible flexibility, in order that they may move easily in all directions, being the feelers by which the work is guided, the sight of the insect not being used.

4. It was at one time supposed that the thickness of the walls was determined for the bee by that of the scales of wax which are secreted; and J. Hunter, finding that the thickness of the rhomboidal plates composing the base does not materially differ from that of the scales, concluded that those scales were used at once in the construction, and formed parts of the cell, as it were, ready made. But not to mention that the walls are less thick, Huber found that the bee works at the cell differently, and not at all by juxtaposition of scales; nay, that the wax of which the cells are made is a material different

from that of the scales, having undergone a process which the bee is observed to perform, and offering results to chemical analysis, which the scales do not give, the latter being entirely and readily soluble, for example, in alcohol, whereas there are considerable parts of the wax altogether or all but insoluble. As for the shape of the laminæ which are secreted, it agrees with no part of the structure, being an irregular pentagon.

5. The discovery of Swammerdam, that the cornea of the bee's eyes is composed of hexagonal plates, or facettes, has been supposed by some to account for the form of the cells. But this is, if possible, a wilder hypothesis than any we have been considering. Indeed Swammerdam's answer to it is sufficient, that it might as well be supposed that men should build round houses because the pupil of the human eye is circular. In truth, the shape of these plates must be wholly unknown to the insect. They only transmit the light which is made to converge to a focus on the retina wholly independent of the form of the innumerable sides of the cornea; not to mention that the bee works in the dark by aid of the antennæ, and that if she did not, and if she saw the hexagonal form of the plates constantly

before her, this, though it might suggest, as a model would, the form of the cell, could give her no kind of aid in making the wax of that figure.

All the theories to which we have been adverting admit the construction of the cells to be effectual for securing the saving of room and wax; and I am not aware that any one has ever denied this generally and absolutely. But some opinions have been given questioning the amount of the saving in the most extraordinary part of the structure, the form of the base, in so far at least as that concerns the wax. The advantages of this construction have not been denied; but it has been supposed to effect so little saving of wax that this could not be the purpose of the arrangement. Of those who have held this doctrine it is sufficient to specify M. L'Huillier as the person who has brought the most mathematical learning to the discussion of the subject.

In his paper already referred to (Mém. Acad. Berlin, 1781), after giving a geometrical investigation of the question of maxima and minima, he adds some general reflexions; and though these are expressed with great doubt, and in language of becoming reverence, they certainly contain an indication that the author considered the saving of wax ascribed to the construction as a mistaken and fanciful view of the Final Cause, and as an abuse of that delicate speculation. For he thinks he has shown that only $\frac{1}{51}$ part of the wax is saved, and that one-ninth might have been saved if the dimensions of the cell had been those of what he terms the *minimum minimorum*, that is, if the proportions of depth and width had been such as to save most wax, among all the cells having the same form and containing the same space. He suggests, therefore, that this saving cannot be the object in view; but that there is either some other object, and he mentions none, or that this object of saving wax is modified by another, and he mentions as such the rearing of the young. As there can be no doubt that this latter view is the correct one, for the reason which I have assigned in treating of the amount of saving, there would have been no occasion to dwell further upon M. L'Huillier's doctrine, had it rested there. But he proceeds to say, that there is reason to suppose the saving of wax does not enter at all into the question, and that it may depend upon, or be a necessary consequence of the other arrangement, that, namely, for the care of the young. "On est même tenté de soupçonner que ce dernier (*i. e.* le bût d'économie) pourroit n'entrer pour rien dans la composition des alvéoles, lorsqu'on fait attention, qu'il peut être regardé comme une dépendance du premier (*i. e.* le bût de l'emplacement des germes le plus sûr, et la propagation de l'espèce)." And he adds, that the solidity of the structure requiring the contiguous cells to leave no space unoccupied, and the opposite cells to fit into one another, this condition is "très-heureusement remplie par des prismes hexagonaux terminés par des fonds, tels que ceux que la théorie et l'observation s'accordent à peu pres à assigner aux alvéoles." p. 292.

If, however, any doubt remained with regard to the meaning and drift of these observations, and of the whole paper, it would be removed by the introductory matter prefixed, of which M. Castillon is the author, as he is of some admeasurements of the cells subjoined to the paper and forming its conclusion. M. L'Huillier of course adopted this introduction, in which the purpose of his paper is set forth, and the accomplishment of that purpose described with some satisfaction. After a warm and just paneygyric upon the doctrine of Final Causes, upon the services which it has rendered to Natural Religion, and upon Natural Religion itself,

M. Castillon proceeds to lament that this doctrine has been abused, not only by writers who expressly treat of it, but by philosophers to whose physical inquiries the speculation is incidental. "Telle est notre foiblesse, nous abusons de tout. Nous tirons quelquefois de la riche mine des causes finales des décombres au lieu d'or. Notre esprit borné se laisse quelquefois éblouir par des fausses lueurs et croit voir des causes finales qui n'existent point." The example he gives is the Base of the Cells. "Par example, on a dit que le fond pyramidal qui termine les cellules des abeilles est destiné à procurer le maximum de l'épargne de la cire. Ceux qui ont avancé cette proposition ont-ils été guidés par la lumière ou par une fausse lueur?" His answer to this question is the Mémoire. "C'est-ce que M. L'Huillier examine dans un mémoire qu'il m'a transmis pour être présenté à cette savante compagnie."—"J'y ai trouvé de belles recherches sur le minimum de surface des solides qui ont même capacité, etc." (p. 277.)

M. L'Huillier has *not* proved what M. Castillon and he suppose; and a little attention to the preceding statement of the former will show this. The supposed proof rests upon three grounds.—*First*, that the saving is only about $\frac{1}{51}$ of the wax employed.—*Secondly,* that a much greater saving might have been effected by another construction, had economy of wax been the object.—*Thirdly,* that the object is the solidity of the structure by the opposite cells fitting into each other, and leaving no intervals.

1. It is extremely erroneous to represent the saving as only $\frac{1}{51}$ part; for suppose we lay entirely out of view the shortening of the cells, and merely consider the saving of the rhomboidal base as compared with the hexagonal one, the proportion is that of $\frac{1}{2}\left(\sqrt{3}+\sqrt{2}\right)s^2$ to $\frac{2s^2}{\sqrt{2}}$ (the whole rhomboidal base composed of the three rhombuses and the six triangles, s being the hexagonal side), or as 1·12 to 1. There is about one-ninth part therefore saved of the wax required for making the base. The proportion of $\frac{1}{51}$ is obtained by comparing the saving upon the base with the whole wax of the cell, including the walls; and supposing the height of the wall to be to the sides of the hexagon as 5 to 1·387. But why is the wax of the wall to be imported into the calculation, with which it has nothing to do? The question is

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between two forms of the bottom, not of the whole cell. Suppose two kinds of roof for a house were to be compared in order to choose the one that required least timber; though the house might be all made of wood we should only compare the expense of the roofs, and leave out the walls, which would be common to both plans; otherwise the relative amount of the saving would depend on the height of the house as well as the shape of the roof. This becomes the more evident in the case of the cells, from the circumstance of their depth varying in the same comb, and for the same kind of bee, according to many accidental circumstances, as the abundance of wax, the use to be made of the cell, the part of the comb where it is placed, and the obstacles in the way of the building; insomuch that I have seen in one comb cells ten and eleven lines in depth; others of the ordinary depth of working bee cells, five lines; and some hardly, if at all, deeper than the bottom, that is terminating nearly at where the rhomboidal plate begins. But in none of these various cells is any difference to be found in the proportions of the rhomboidal sides to the hexagonal side *s*, or in the depth of the bottom. The side of the hexagons is always the same for the same kind of bee; the depth of the pyramid is always $\frac{s}{\sqrt{2}}$, and the side of the rhombus $\frac{3s}{2\sqrt{2}}$. The saving therefore is somewhere about a ninth, and not somewhat less than a fifty-first part.

But there is another consideration which shows still more strikingly the fallacy of the argument derived from taking the whole walls of the cell into calculation. The thickness of the wax is very different in different parts of the cell, being much greater in the base, that is in the rhomboidal plates, and the part of the walls adjoining, the six small triangles, which are formed by a line drawn parallel to the base through the points where the rhomboidal plates cut the walls. This is manifest upon inspection; and I have tried it by weighing equal parts, in superficial extent, as far as it was possible, of the base and of the sides, and uniformly found the latter sensibly lighter. It did not seem that the proportion was always the same, but I never found the difference less than in the proportion of 3 to 2. The thickness of the walls varies much more than that of the base in different combs. But any considerable difference between the two portions at once destroys the argument of M. L'Huillier. If it is as 3 to 2, then the saving is nearly an eighth upon the thicker part, and consequently about $\frac{1}{35}$ instead of $\frac{1}{51}$ of the whole.

2. It is very inaccurate to say that because another form would have saved more wax, if that had been the object, therefore it is not the object at all, as M. L'Huillier ultimately contends, after at first stating much less inaccurately, that the saving is one object subordinate to another. But even this is not altogether correct. It is an object, but taken in conjunction with another object; that is to say, the purpose is not to make the cell of a given capacity with the smallest quantity of wax, but of a given capacity and capable of holding insects of a given length and fluids of a given consistency and weight. It no more follows that saving of wax is not a part of the design because another object is accomplished at the same time, and which prevents the saving of wax being greater, than it follows that each of the two conditions in any question of maxima and minima is not attended to, because both are attended to. Thus, to take a very simple instance, if it is required to dispose a given surface in a rectangle so that both the sides taken together shall be the shortest pos-

sible; we know these must be equal, and the figure be a square. By making the figure twice as long as it is broad, the breadth would be saved, but the whole periphery would be much increased. Would anybody contend that no regard is paid to a saving of the breadth, merely because the saving of the length is also taken into the account?

But is it true that, supposing the object had been saving of wax alone, and the problem solved had no other condition to qualify that one, any other form would have more effectually accomplished the single purpose? We are of course always to assume that no interstices shall be left between the cells. If but a single cell is in question, there exists no dispute that another form would have given the same capacity with less surface than the hexagonal prism with a three-sided pyramidal base. But to state this is is extremely superfluous, not to say puerile, and proves less than nothing; for if there is to be but one oblong cell, a cylinder would save most surface of all the regular oblong figures, and if it is not to be oblong, a sphere of all figures whatever would save most surface. Nor does M. L'Huillier's prism at all advance the argument; because, if he takes into the account the juxtaposition of the cells, he must also consider the opposite sides of the comb; and then he admits that his figure will not answer, for space would be lost and wax also.

But, suppose a cell must be chosen of the given shape, and which leaves no interstices, his argument is, that another proportion of the depth and width would have saved more wax. Now this, upon examination, turns out not to be true. We shall first suppose all the parts to be of equal thickness, and the walls no thinner than the base.

It is observable that he leaves entirely out of his computation the mouth of the cell and its hexagonal covering. He supposes the case of a cell open at that end and shaped according to his proportion, the length of the wall being to the width as 1 to $\sqrt{6}$ one way, and 1 to $\sqrt{8}$ the other; and he compares it with the cell actually made by the bees, also supposed to be open at the end. By thus leaving out the hexagon formed at the end or mouth of the cell, he makes it appear that there is a waste of wax in the cell made by the bees. But why is that hexagon to be left out? It is made of wax, like the rest of the cell; indeed, of thicker wax than the walls are made of. It is absolutely necessary for preserving the honey; and, if it is not required for the breeding-cells (which is by no means clear, for the grubs are covered over in general), still those could not, without deranging the whole structure of the comb, be of different dimensions from the cells used for storing honey, and without making it indeed necessary to have one comb for the one purpose, and another for the other, thus losing the great convenience of the cells being used indifferently for all purposes.

Now, taking the case of a single cell, it will be found that the solution of M. L'Huillier's problem gives a proportion by which, instead of any saving there is a loss, though to a trifling amount. The wax required for this construction exceeds that required in the cells actually formed by about $\frac{1}{6}$ of a square line, taking into the account the hexagonal plate required to close up the end of the cell. But, if this saving is trifling on one cell, it is very considerable indeed on the comb. In a hive of a cubic foot, the total loss would be nearly eighteen square feet of wax; because, instead of nine combs, with an interval of five lines between each, there must be $26\frac{4}{7}$ with the same interval. A waste, therefore, of between one-fifth and one-sixth of the whole wax required would be occasioned, instead of any saving. This, of course, supposes all the cells to be used for storing; but the argument applies, though in a diminished proportion, if we deduct the breeding cells.*

The only reason that I can assign for M. L'Huillier having made this extraordinary omission of the hexagonal plate at the end or mouth of the cell is that, in the investigation of the problem originally proposed by Reaumur to Kœnig, that hexagon does not enter. But in that problem it could have no place. The side being a constant quantity, so is of course the hexagon. It would have dropt out of the differential equation, and could not affect the result required; namely, the value obtained for the side of the rhombus, or for its angles. But then, M. L'Huillier ought to have considered that it *did* enter into the investigation of *his* problem very materially; and, had he solved that problem by the calculus instead of geometrically, he would have found that the hexagon is not a constant quantity, and must have affected the result.

* All these calculations are fully proved in the Appendix.

In truth, if the problem had been stated as it ought, it would have been this:—To find the proportions which would give the whole surface, of the cell (including the hexagon plate), a minimum. This is done in the Appendix, where it is shown that the result is not that of the wall, being $\frac{1}{\sqrt{2}}$ of the side or $\frac{1}{\sqrt{6}}$ and $\frac{1}{\sqrt{8}}$* of the width, but that of the wall being to the side as $\sqrt{2} + \sqrt{3}$ to 2, or the depth to the greatest width as 27 to 28 nearly; or (taking the solid content of the common bee's cell), instead of M. L'Huillier's proportion, of the depth to the greatest width as 2·53 to 4·75, it is that of 3·5 lines to 3·64. This is in reality the proportion in which, if the cell be constructed, there will be the greatest saving possible of wax and work—a saving on one cell of about 3·805 square lines, or nearly one-twelfth part—instead of a waste, as we have seen M. L'Huillier's proportion would occasion.

It may then be asked whether the argument of

* According as the greater or lesser breadth of the hexagon is taken. The whole depth of cell is always $\frac{s}{2\sqrt{2}}$ more than the length of the wall; which seems to be overlooked by M. L'Huillier.

M. L'Huillier is not thus revived, though placed upon a new ground, and referred to the cell of these proportions now determined, and why those proportions do not justify the inference which he drew from his erroneous solution, and which that solution could not support? But the solution which I have given, though it proves a saving in a single cell, and though it shows a loss of much less than M. L'Huillier's, still leaves a loss upon the whole comb. A comb of a foot square made of cells, whose width was to their depth as in the above minimum ratio, would take about $\frac{1}{375}$ more wax than one whose cells were of the construction actually used by the bees; and there would be a waste of $\frac{1}{125}$ upon a hive of $15\frac{5}{6}$ combs, which would be the number required to give as many cells as nine combs of the ordinary structure. But it must further be considered that the wax of which the bottom is made being thicker than that of the walls, and the bottom bearing a smaller proportion to the walls in the cells of the form actually employed by the bees, than in the form which saves the greatest extent of surface, an additional saving is made by the proportions actually used.

This leads us to consider what form of cell will give the largest proportions of the walls, and the smallest of the rhomboidal base. This problem, like that of the minimum of surface, may be considered in two ways; first, as regards the angles of the rhombus; and next, as regards the proportion of the depth to the width of the cell, the angles of the rhombus being given. The second of these problems admits of no solution, there being no limit to the disproportion between the base and the walls, if no limits are assigned to the depth of the cell. But the first problem may be solved; and it gives clearly the hexagonal prism as the form in which the base bears the smallest proportion to the walls. But there would be an obvious disadvantage in this form; because a loss of surface would be occasioned by deviating from the angles which give the minimum of surface, and this would not be counterbalanced by the small saving in the proportion of the thicker parts of the work to the thinner.

There is, however, an important circumstance to be regarded, beside the extent of the plates and their thickness. The angles formed by the plates are apparently the most difficult part of the work; they appear to be laboured with the greatest care; and they are the parts where the wax is thickest—the solidity of the structure depending mainly on them. Now the saving of these solid angles becomes on this account very material, and we may inquire as before, first, what must be the angles of the rhombus in order to make the length of the solid, or dihedral, angles the smallest possible; and, secondly, supposing those angles to be given, what must be the proportion of the depth to the width of the cell, which makes the length of the solid angles the smallest. Both problems admit of a determinate solution. In the first it is found that the angles of the rhombus must be 109° 28′ and 70° 32′, being the same form which saves the most surface. In the second it is found that of all cells with such pyramidal base, that has the smallest length of solid angle in which the length of the wall is to the hexagon side as $\sqrt{2} + 1$ to 1, or the whole depth to the greatest width, as $5 + 2\sqrt{2}$ to $4\sqrt{2}$, that is as 39 to 28 nearly. But if we only regard the minimum of the solid angles of the base and walls without considering the angles at the hexagonal opening, then the form is that of the wall being to the hexagon side as 1 to $\sqrt{2}$, being M. L'Huillier's *minimum minimorum*.

From hence it is evident that this kind of fine and difficult workmanship is saved by the angles of the cell being such as they are rather than such as would effect the greatest saving in the proportion of the bottom, or thick plates, to the walls or thin plates. This, therefore, is an additional economy and an additional reason, beside those already given, against the form which gives that proportion as a minimum. But it also appears that, retaining this form, the proportion of depth to width, which gives the minimum of solid angle, could only be adopted at an expense of surface. For if the angles round the hexagonal plate are left out of the consideration, then the form is such as has been already shown to lose somewhat upon a single cell, and upon the comb a great proportion; and if the angles round that plate are taken into consideration, though on a single cell there is a saving, there is a loss upon the whole comb, as compared with the common cell. All this is merely with regard to the saving of wax and work, supposing the breeding out of the question, and independent of the reasons against the shallower and wider cell derived from the form of the insect.*

We may therefore conclude, from the fullest examination of the question, that it is an error to suppose any saving could be effected by varying the form of the cell in any of the ways proposed; and therefore that, supposing there were nothing taken into consideration except the economy of labour and materials, the form adopted by the insect is the most conducive of all possible forms to that object. It follows consequently that the position is wholly unsupported which represents this saving as not being one of the objects of the particular structure adopted.

3. The third ground of the doubt raised by the Berlin Academicians is founded upon the proposition that the object in view is the solidity of the structure, and that the saving of the wax is only incidental to this main object. The language used is not marked by the precision which might be

* The Appendix contains the whole processes and calculations by which the mathematical positions in the text are proved.

expected in a mathematical discussion. After stating that the safety of the eggs and the process of breeding generally seems to be the object in view and not the saving of wax; it is added that the solidity of the structure so necessary for that object, appears to require that there should be no interstices between the cells, and that the opposite cells should, "*if possible,* fit into one another— conditions fulfilled by hexagonal prisms with bases, such as the theory and observation *nearly* agree in giving to the cells." The "nearly" is quite incorrect; there is an absolute and perfect agreement between the theory and the observation. But it is still more inaccurate to represent the actual structure of the prisms and base as necessary for the fulfilment of the conditions stated. Any hexagonal prism terminating in pyramids of three rhombuses would fulfil the conditions of leaving no interstices, and of having the opposite bases fitting into one another, whatever the angles of the rhombuses were. There is something, indeed, vague in the expression "conditions which are fulfilled;" and it may be said not to mean that the actual structure is the only one which fulfils those conditions. But then if that is not the meaning, the observation has no bearing upon the question; for the purpose in hand is to show that the structure such as we find it to be, is intended to fulfil the condition of the cells fitting, and this can only be answered by proving that structure necessary to the cells fitting, which it plainly is not. This third ground, therefore, fails as signally as the others.

M. Castillon has recourse to an argument of a perfectly different kind with a view to displacing the doctrine upon this subject. He calls in question the facts. Father Boscovich had supposed that the admeasurement of the angles was too nice to be accurately performed, and that the coincidence of M. Maraldi's measures with the theory could only arise from his assuming that the angle of inclination of the rhomboidal plane was the same with that of the hexagon, viz., 120°, from which, no doubt, it would follow that the angles of the rhombuses should be 109·28 and 70·32 respectively. M. Castillon and M. L'Huillier seem to adopt this supposition with some alacrity, and the former adds some measurements of his own in confirmation of it.

Admitting the profound respect which is due to any opinion or even conjecture of so great a man as Father Boscovich, it must at the same time be remarked, that we cannot adopt his opinion without imputing a very grave fault to another great man—Maraldi. If, instead of measuring the angles of the rhombuses, he supposed the other angles were the same with those of the hexagon, and then calculated the angles of the rhombus, and stated that he had found them to be so in fact, he unquestionably stated what was not true, and pretended to have made an experiment when he only made a supposition and deduced an estimate from it. If, indeed, he actually measured the angles which the rhombuses make with the sides or with one another, and found that angle to be the same with the angles of the hexagon (120°), he had a perfect right to state the angles of the rhombus to be 109·28 and 70·32, because that followed from his actual measurement of the other angle. But then that is just as good a measurement of the angles of the rhombus as if these angles had themselves been the subject of the observation; and no doubt this is an easier way of measuring the angles of the rhombus than a direct measurement of those angles. For take the two rhombuses which are a continuation of the dihedral angle of the prism and apply them to that angle, if the walls and the rhombuses accurately coincide, it shows the angles of inclination to be the same in the walls and in the rhombuses, and all the rest follows. Another measurement is also practicable without the operation, confessed to be of extreme nicety, of measuring the angles themselves of the rhombus. The breadth of these rhombuses—the line drawn from any part in one of the sides perpendicular to the opposite side —may be compared with the side of the hexagon; and if it is found equal to that side, all the rest follows; each rhombus makes with the other two and with the walls angles of 120°, and each rhombus has its two angles 109·28 and 70·32 respectively. This is the necessary consequence of the rhomboidal breadth being equal to the side of the hexagon. Now such a comparison is not very difficult to make, either by instruments or by placing the rhombuses over the walls, laying each, when separated, flat on a plane.

But M. Castillon's measurements, which are intended to confirm Father Boscovich's conjecture, and cast a doubt on Maraldi's statement, really deserve little attention, and yet they afford an unexpected confirmation of the latter, and not of the former.

They deserve little attention, because they are so few in number. There are five measurements of the whole depth of the cells; but that is immaterial to the question; and there are only two of the length of the longer solid angle of the prism as compared with that of its shorter solid angle. It does not appear that M. Castillon was aware of this proportion determining the angles; but he apparently gives his measures in order to show that they vary considerably, and that such observations cannot be relied upon. Now two such observations, differing from one another, would prove little or nothing; but it does so happen that one of the two agrees sufficiently well with Maraldi's. The first measures which he gives make the one length 4·622, and the other 4·144. Now the theory is not very different from this; for if the angles are as measured by Maraldi, and found by the calculations, supposing also the ordinary measure of the proportions between the width of the cells and length of the walls to be accurate, the proportion of the longer and shorter solid angle is that of 4·622 to 4·168, or within $\frac{1}{72}$ of a line, the same as M. Castillon found it to be.

The examination of the question into which we have been drawn has extended to a great length, and has been very minute; but it has not been superfluous, because the doubts raised by the Berlin Academicians have had considerable influence in shaking men's opinions upon the subject; and a disposition to suppose the whole doctrine respecting the structure of the cells erroneous, and the inferences connected with it fanciful, may be traced to the Memoir which we have been considering, although many who have treated the opinions of Maraldi and Reaumur as disproved by subsequent inquiry, have probably not looked at the work upon which this notion rests. The subject, too, is of the greatest importance; for it is by far the most remarkable, as it certainly is the most celebrated, of the operations of animal instinct; and if it had proved to be a mere groundless imagination, the whole of our opinions upon other less striking illustrations of the same views would have been very naturally unsettled. A full investigation, however, has proved to what the error must be ascribed, and has shown that the evidences of instinctive skill are in several material particulars even more remarkable than they had been before supposed to be.

We have hitherto been confining our attention to the structure of the cells as composed of wax, or wax and propolis, the only materials to which the attention of naturalists and mathematicians has been directed. As regards the cells only used for storing, there is no material except these employed. But the following observations and experiments seem to show that it is otherwise in breeding cells. It was the examination of these, with a view of satisfying myself as to the origin of Dr. Barclay's mistake, that led me to the more minute consideration of this subject, to which it is a matter of much regret that neither of those consummate observers, Reaumur and Huber, devoted sufficient attention. But it is to be hoped that others better qualified to continue their researches than I can pretend to be, will supply this defect; and it is with a view to excite their attention, rather than to aid their inquiries, that I venture to add the result of my very imperfect trials.

A portion of comb was selected, one part of which had never either been used for storing or for breeding, and the other had had a single brood. The former part was perfectly white; the latter slightly tinged with yellow or light brown, and in several places with the red streaks observed by

Huber, and shown by him to be a vegetable matter collected from trees, particularly the poplar. The whole belonged to a comb made in a glass hive by a very late swarm about the middle of August, and taken soon after the middle of September. Indeed, that any young had been bred in it I should not have supposed, if the cocoons had not shown it—always supposing these to be the webs spun by the pupæ, according to the prevailing opinion, which is assumed in the remarks that follow to be correct, although some may possibly think that the full grown working bee has something more to do with the cocoons than is generally supposed.

The piece of comb was placed in alcohol, and no part of it much affected until heat was applied, when the white part speedily melted, and in part dissolved, no vestige of the form of a cell, or even a plate, remaining. A good deal of wax also ran melted from the other part, that in which bees had been bred; but it retained its form, and nearly its dimensions, notwithstanding the heat was continued for some time. When the spirit was boiled, the latter, or part of the comb in which bees had been bred, separated into parts; but even then it required being stirred to assist the separation and let the wax be completely melted. When another comb of an older hive was used, the separation was very much more difficult; but continued boiling in the alcohol, with stirring, effected it: and then each cell was found entire and apart from the others, and, when the liquor cooled, all were covered and filled with small wax globules, being that considerable portion of the wax which the alcohol does not take up. The same experiment may be made with boiling water, and the result is the same, only that the water takes up none of the wax at all. If spirit of turpentine is used, the experiment is more effectually and easily made, the wax being easily combined with the spirit; but this form of the experiment is not of course applicable where it is wished to ascertain in what part of the cell the wax is formed. Sulphuric ether crumbles down the wax, without dissolving any considerable part of it, and separates the cells after steeping some time. The experiment was then made with pieces of old comb, in which several broods of young had been reared. The cells were somewhat smaller in width, the walls considerably thicker, and the colour much more dark, being a deep brown, in some places almost black.

The cells separated by these processes were now examined. Each was found to consist of a hexagonal prism, terminating in a pyramid of three equal rhombuses; in short, each cell had exactly the shape of the wax cells, but was formed of wholly different materials. The walls and base were made entirely of an extremely thin transparent or semi-transparent film, resembling gold-beater's leaf, but without a wrinkle. The old cells with thick walls kept the shape most distinctly. Indeed they had angles and planes as well defined as those of wax in the new comb. But they did not consist of a single film, like the cells where apparently only one brood had been raised; they had one film within another, and could be separated, so that as many as five or six could be extricated from the same cell; each of these had the hexagon form, and the first two, and sometimes three, had the rhomboidal form of the base also; but the innermost ones had the rhombuses less and less distinctly marked, till the last one or two of all had spherical, instead of pyramidal, bases. The hexagon's walls or the sides of the prism were in all distinctly marked. The bases were so much the less distinct after the first and second, in consequence of a much greater quantity of the red matter being placed in the base than in any part of the prism. In the prism it was generally traceable in the angles, as a kind of lining or coating, and not always continuous, for sometimes it was interrupted; and it seldom was of equal amount the whole length of the solid angle. Sometimes there was hardly any in these angles. In the base there was always a considerable quantity. The end or mouth of the cell was always edged round with a rim composed chiefly of this red matter, which I could not dissolve either in alcohol, spirit of turpentine, sulphuric ether, or caustic alkaline ley—whether these reagents were exhibited cold, and the cells with red matter macerated in them, or were heated even to boiling, and the cells with red matter stirred in them.

The *first* thing that was striking in these experiments was the closeness with which the film adhered to the wax. It defended it from the action of the solvents, and even for a time from that of the heat, at least it prevented the wax from melting for a considerable time; and it thus happened that long after the liquid had attained a temperature higher than that of melting wax the comb retained its form, and the cells continued to adhere.

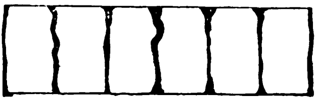
The *second* remarkable circumstance was the perfect stretching of the film all round the wax cell of which it had assumed the figure. There never was found the least wrinkle or laxity. Each film was tensely stretched in all its parts.

Thirdly. There was no interval whatever in any part. The whole of each cell was one entire piece of film, going all round the prism, and all through the pyramid, without any breach, and without any suture or joining. At first it seemed possible that the red matter might be a cement, or might cover the joinings, or conceal an interval; but on scraping it off, as well as examining parts where it never had been, this suspicion was ascertained to be groundless.

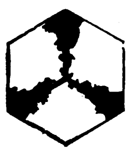
Fourthly. The red matter was not merely spread on the first or innermost cell, the one next the wax, but was by innumerable trials found to be indifferently applied to all the films, as well to the fifth and sixth as to the first and second.

Fifthly. The red matter always when examined appeared to be on the outside of the film; for there was constantly seen a film on the concave side between the eye and the red matter. It must, therefore, have adhered to the film spun over it, and come off from the one it was plastered on.

Sixthly. The red matter, though very irregularly spread on the solid angles of the prism, and on the plates of the base, and on the upper part especially of the solid angles, that is, at the mouth, and near to and adjoining the dark coloured rim of the cell, seemed in any given cell to be at the same parts of each of the films which lined it. For when the side of a hexagon of many films was cut through, so as to stretch out the sides of the prism into one plane, the red matter was always observed in defined parts; showing that where it was wanting in one film, it was also wanting in the other six or seven. The appearance was of this kind when the rim was cut off at the one end and the rhomboidal base at the other.



The base seems on a superficial inspection an exception to this observation, inasmuch as in cells which have had many broods it is of a uniformly dark purple and almost black colour on the outside and perfectly opaque, while each of the films of which it is formed is transparent, except in certain parts, so that it might be supposed that the dark parts of one were opposite to the transparent parts of the others. But a closer examination shows that the red matter in these bases, as well as in the walls, is distributed in the same manner in each of those of which the whole mass is composed, and that it is the diminution of their size which causes the appearance just adverted to. Thus the first, or innermost, the one next the wax, has a considerable space wholly free from red matter, but the dihedral angles are more or less lined with it, and the breadth of the red matter is greatest at the solid angle which the rhombuses make with the walls of the cell, and is very scanty indeed at the central trihedral angle made by the three rhombuses where it is probably not spread at all on the same side, but has the appearance of colour from the depth of that which is laid on the opposite sides. There is, however, a sensible proportional increase in the quantity of red in the smaller and innermost films. It probably increases gradually in each after the first or waxen cell.



It tapers in this way. The other films are covered in the same places; but as the quantity of red matter does not diminish, but rather increases, the whole base is gradually contracted, till in the sixth or seventh there is hardly any transparent part at all. But it thus appears that the matter is applied nearly alike in each.

Seventhly. The films are quite unaffected by maceration, or even boiling in alcohol, oil of turpentine, ether, or caustic potash. But the red matter seems to be more or less dissolved in all these substances. By stirring in it, the dark coloured cells give to spirit of turpentine a light yellow or golden tinge. By longer maceration, and especially by boiling, alcohol and ether are likewise tinged though not so deeply. It is probable that longer maceration and boiling in any of these liquids would dissolve the whole colouring matter of the red substance. Boiling in caustic potash converts it into a brown pigment, and seems to act upon the substance itself, as well as the colouring matter; but nothing affects the films.

Lastly. A film of the same substance, transparent, but considerably thicker, was found to line the cell of the queen bee. The red matter here was more equally diffused over its surface in clouds and streaks, there being no angles at all to line with it. The film assumed the pear or flask-like shape of the wax; but a very remarkable fact was observed—the film was not always in the inside; it sometimes lay embedded in the wax, at least a layer of wax was laid over it of a sensible thickness, indeed considerably thicker than some plates of the common cell, and in one or two specimens it was much thicker. In case a thinner layer of wax might be in like manner spread on each film of the common cells as the red matter was, great pains were taken to ascertain this by examining the older cells, which had been separated by boiling in water so as not to dissolve the wax; and there seemed every reason for believing that no wax existed between the eye and the film, that is on the inside of the innermost film, in any but the cell of the queen bee. No queen bee's cell was observed to have more than a single film even in the oldest comb, where there were six and more successions of films in the other cells. But the examination of these large cells should be more fully gone into, and they should be compared as to their lining, with the cells, made out of three common ones when a queen is lost and her place supplied.

The formation of these films is plainly deserving of much greater attention than has ever been given to it. Neither the observations of Maraldi, nor those of Reaumur, nor even of Huber, are full and satisfactory upon this subject. They speak of the worm lining and carpeting the cell in spinning, or rather weaving, the cocoon, and yet they also speak of its inclosing itself in the cocoon, as if it spun and wove a web which covered its body, and in the inside of which it underwent its transformation. But in the meantime there are certain things established by the foregoing observations which seem to deserve attention.

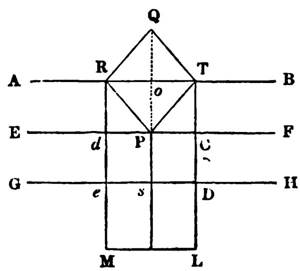
The process must be conducted in one of two ways—either by the worm forming a cocoon round itself, and of an oblong figure inside sufficient to contain it when it changes its position from a coil perpendicular to the axis of the cell, into an oblong worm placed in the axis—or by the worm lining the walls of the cell, as Huber has in one or two places described it, though his description is imperfect, and he does not seem to have watched the whole operation. In the former case the cocoon, originally made somewhat of the shape of the worm, must afterwards be applied by it or by the chrysalis so as to line and adhere to the walls;—in the latter case the walls are lined at first by the act of the

P 3

weaving or spinning. Let us observe the difficulties attending both these hypotheses, and the inferences to which they lead—inferences, in either case, as extraordinary, to say the least, as anything observed in the economy of this insect.

1. If the cocoon is formed loose and round, then when the transformation takes place the pupa must press against every part of the cell, so as to apply the film all round, and equally in every part. The wax may seize and retain the first film, which may be originally moist; or some propolis, being spread by the bees over the walls, may, with the agglutinating substance of the film itself, retain the film applied. That the immature animal itself should be able to do this is not more extraordinary than that it should be able to spin the film.

But the extraordinary part is the perfect adaptation of the cocoon to the cell. There is no wrinkle whatever; it fits exactly, in every part, both the planes and the dihedral angles and the trihedral angles. The extreme fineness of the texture may facilitate its fitting so many different shapes; but how is the size sufficient, and not at all more than sufficient, in any one place? Let us consider what the size must be in order to fit the different parts of the cell exactly. Take the base, and cut it by a plane at right angles to the axis of the cell, and passing through the acute angles of the rhombuses; this will cut off the pyramidal part of the base, and leave the rest of it composed of half the rhombuses and the six triangles. Then cut the prism by another plane parallel to the former, and passing through the obtuse angles of the rhombuses; and cut the prism by a third parallel plane, at the distance from the second of the altitude of the pyramid above the extremity of the prism; the three planes are equidistant, and cut the cell so as



to leave three equal lengths. A B, E F, G H, being the three planes, and *o* P equal to the altitude of the apex Q above B A, Q *s*, the axis, is divided into three equal parts by the planes A B and E F. Observe, then, the breadth which the cocoon must have in the length, Q *o*, from its termination in the bottom of the cell Q. The three intervals, or lengths, are in the common cell about one-tenth of the whole depth each. But the surfaces of the cell comprehended between the planes are of very different extents. The pyramidal part, Q R T, is 3·03 square lines; the next part, T C P *d* R, is 5·05; and the hexagonal part, C D *e d*, is 4·04. Yet the cocoon must have been so spun as to have the size of the web vary in these proportions. For the first half line wound along its axis the web must have been made so as to have six breadths to one length, for the next half line ten breadths, and for the next half line eight breadths. Let any one consider what difficulty there would be in making a bag of cloth which should thus vary in its dimensions at different parts.

But that is the least part of the difficulty overcome by the bee; for the extent of the web which they make (that is, which their grubs make) in proportion to its length, does not vary at definite points; there are not, for example, two precise proportions, one for the part of the cocoon answering to the pyramid Q R T, and one for the part which is to line the other part of the base between the pyramid and the prism, T C P *d* R. The proportions vary at every one of the innumerable points between the apex of the pyramid and the obtuse angle P of the rhombs where the hexagonal prism begins. At each point beginning from the apex, there is an increase in the extent of web required until we reach the acute angles R and T of the rhombus. There is then an increase from the acute angle till we reach the obtuse angle P, when the extent of the web is the greatest; and during all the rest of the web, which is to line D *e d* C, its length round the hexagon remains the same, that is, all throughout the hexagonal prism. Moreover the increase does not take place equally; the periphery of the cell to be lined does not increase in the proportion of the distance of each ring or infinitely small section of the surface from the apex Q along the axis of the cell. From the apex Q to the acute angles T, R, the periphery increases as the distance along the axis from the apex (being equal to $6\sqrt{6}$ of that distance). But from the acute to the obtuse angle it increases much less rapidly, being equal to $3(4\sqrt{2}-2\sqrt{6})$ of that distance along the axis, together with three times the longer diagonal of the rhombs; so that while the periphery is increasing fourfold from *t*, half the distance between the apex and acute angles to the acute angles, it is only increasing in the proportion of five to two in an equal distance from the acute to the obtuse angle, that is, from the point *u* in the axis corresponding with *o*, to a point on that axis corresponding with P.*

If we only consider what extreme complicacy and difficulty there would be in forming a cocoon which should thus increase at every hair's breadth, and increase in a ratio varying at different points, and should, on reaching its maximum size, continue afterwards stationary in dimensions, we shall be convinced how insuperable the difficulties of the workmanship would be to any artist ever so expert or careful. But even this is not all—for as the web is to be afterwards, by the supposition, applied to the circumscribed walls, the extent of the curved surface of the cocoon inscribed, must be less than that of the surface which it is afterwards to line, if that curve is wholly concave to the axis, in other words, if it have no points of contrary flexure. In order, therefore, that it may be exactly equal to the walls which it is to fit exactly, the cocoon must be of a form wholly different from that of the worm that made it. It must be concave at some points

* Neither *t* nor *u* can be seen in the figure, being in the axis.

and convex at others to the worm; it must be loose and bag, as it were, and the progress of its bagging or being loose must vary at every point in order that, when applied to the walls, it may exactly fit them at every part, from the apex to the obtuse angles of the rhombus, and afterwards be uniform to the end of the prism. Instead of being as in figure 7, where the worm is represented under the bag by a

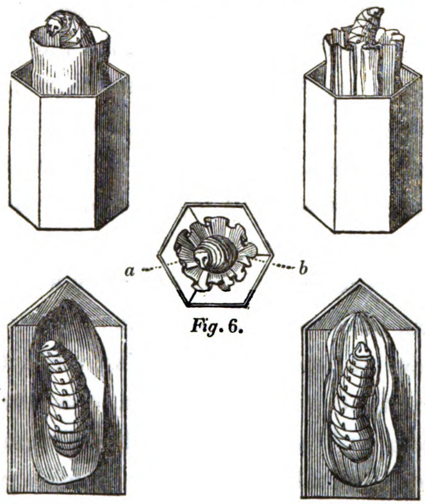


Fig. 6.

Fig. 7.

Fig. 8.

vertical section, it must be as in figure 8, where the shaded parts represent the doubled parts answering to *a* and *b* of the transverse or horizontal section (Fig. 6.), the circle being thus the insect, and the line the web. The performance of such a work by the worm appears scarcely conceivable. Astonishing as the known and ascertained works of the perfect insect are, this would surpass them in a proportion that might almost be called infinite.

2. These considerations, and the observations of naturalists as far as they go, lead us to adopt, almost of necessity, the second inference, that the worm applies the cocoon as it is made directly to the walls. In this case we get rid entirely of the former difficulty, for the operation is certainly much more easy of forming the film upon the walls. That it is executed with perfect nicety and precision, is, however, no less true. There is never a break to be found, and there is no part thicker than the rest; so that but one layer is applied every where; and the worm knows so accurately where it has begun as always to leave off on coming round to that point, without ever going again over the same ground for half a hair's breadth. The material is also very remarkable. A very high magnifying power shows no threads, or separate pieces of any kind; in the great bulk of the texture it is for the most part solid and perfectly transparent. There are interspersed irregularly a few fibres, but it

should seem as if the whole was a mucilage spread over the walls, rather than any web woven of threads. But though the difficulties attending the other theory are not found in this, it has difficulties of a different kind and exceedingly startling.

The first that strikes us immediately is the use of the cocoon formed on the waxen walls. The cell was already made, and of the required form and dimensions, in which the worm could be lodged, and grow, and undergo its transformation to the chrysalis, or from the chrysalis to the bee. How was the lining it with the film to assist the process? If the cocoon had been of another form, and wrapt round the worm, it might have served some such purpose of covering or support as cocoons generally do to the worm, and afterwards to the pupa; but here the cocoon exactly fits the cell, and in nowise alters its form; and by only an exceedingly small quantity its capacity. Still it is possible that the film may better suit the worm than the walls, or rather may better suit the worm when grown, and the chrysalis, for the worm was on the bare wax during the first ten or twelve days, and until it made the cocoon. But then, how are the second, and the subsequent cocoons, to be accounted for? The cell had been lined already completely with film, and the additional lining could add nothing to the advantage, whatever it was, which the first lining gave the worm and the chrysalis. If two linings were necessary for the second worm and pupa, how did the first do with one? and so of the third, and all the subsequent broods. Indeed, when many come to be accumulated, there is a positive detriment occasioned by the cells being contracted.

Now this difficulty cannot be got over by saying that the same kind of anomaly occurs in other cases; for it will be found that there has as yet been observed no second instance of it, and that the resemblances are wholly imaginary. The only appearance of anything like this operation is in those cases where an instinct manifestly given for the accomplishment of certain purposes, leads to acts which are fruitless in consequence of some apparent mistake on the part of the animal; as where the fly, mistaking the flower for carrion, lays its eggs in the folds of the calyx whose smell had attracted it. But the case in hand is very different; for we have here not an accidental, but a constant and regular action of the insect, and in the great majority of cases, with a total failure, nay rather with inconvenient results. For one film or cocoon that is spun to serve the purpose of shielding the grub from the wax of the cell, five or six are spun one within the other to no kind of purpose, but rather to the loss of space, and yet the instinct which leads to this operation, is that of saving wax and work, because it is that instinct which makes the bees always prefer breeding in combs already used, and therefore lined with film wherever a brood has been, to building new combs of virgin wax. Even if we suppose there were only two broods on an average in each cell, which is certainly much below the truth, the instinctive operation would be misplaced, and fail as often as it succeeded. This is assuredly a strange kind of instinct, considering that certainty, almost infallibility, is the characteristic of the operation in all other cases, and that wherever a failure is found, there seems an exception to an otherwise general rule. No other operation of any animal can be cited which fails as a general fact, either oftener than it succeeds, or even as often. To make the thing still more extraordinary, the fact is observed in the operations of an animal the model of perfection beyond all others in its instinctive faculties.

We are thus driven to the conclusion that some hidden use exists to which the cocoon is subservient. When the queen bee finds a worm or an egg in any cell, she never lays another egg there. When the nursing bees find liquor deposited in the bottom round the egg, they pour in no more. Why should the worm make a cocoon when it finds the cell already lined with film? Nor can any distinction be taken between the work and the faculties of the worm and those of the grown insect; first, because the worm, on any supposition, is endowed with perfect instinct; and, next, because the adult bee aids in the operation by lining the angles of the hexagon with the red matter, and does so each time a film is spun. This difficulty is at once got over if we suppose that, like other grubs, it spins the cocoon round itself as a covering, and separate from the walls of the cell. But then we get into all the extreme difficulties pointed out already as to the spinning a loose web which shall fit every part of the cavity without a blank or a wrinkle. There seems then no way of avoiding both difficulties, except by supposing that new made film has qualities different from old, and that these are in some way genial to the worm and the chrysalis. This is barely supposable. We cannot suppose that a contact with the red matter is necessary for the growth of the grub; for that matter being deposited on the inside of the earlier film, and adhering to the subsequent film, seems to coat its outside, but in fact never can be in contact with either the worm or the chrysalis, inasmuch as it never is laid on before the bee is fully formed, and has left the cell. The supposition now made of the peculiar qualities of new film is no doubt gratuitous, but there seems no other escape from the pressure of the difficulty with which the facts surround us.

The attention which has been paid at various times to the structure and habits of the bee is one of the most remarkable circumstances in the history of science. The ancients studied it with unusual minuteness, although being, generally speaking, indifferent observers of fact, they made but little progress in discovering the singular economy of this insect. Of the observations of Aristomachus, who spent sixty years, it is said, in studying the subject, we know nothing, nor of those which were made by Philissus, who passed his life in the woods for the purpose of examining this insect's habits;

but Pliny informs us that both of them wrote works upon it. Aristotle's three chapters on bees and wasps* contain little more than the ordinary observations, mixed up with an unusual portion of vulgar and even gross errors. How much he atended to the subject is, however, manifest from the extent of the first of these chapters, which is of great length. Some mathematical writers, particularly Pappus, studied the form of the cells, and established one or two of the fundamental propositions respecting the economy of labour and wax resulting from the plan of the structure.

The application of modern naturalists to the inquiry is to be dated from the beginning of the eighteenth century, when Maraldi examined it with his accustomed care, and Reaumur afterwards, as we have seen, carried his investigations much further. The interest of the subject seemed to increase with the progress made in these inquiries; and about the year 1765 a society was formed at Little Bautzen, in Upper Lusatia, whose sole object was the Study of Bees. It was formed under the patronage of the Elector of Saxony. The celebrated

* Hist. An., lib. ix. cap. 40, 41, 42.

Schirach was one of its original members; and soon after its establishment he made his famous discovery of the power which the bees have to supply the loss of their queen by forming a large cell out of three common ones, and feeding the grub of a worker upon royal jelly; a discovery so startling to naturalists, that Bonnet, in 1769, earnestly urged the Society not to lower its credit by countenancing such a wild error, which he regarded as repugnant to all we know of the habits of insects; admitting, however, that he should not be so incredulous of any observations tending to prove the propagation of the race by the queen bee without any co-operation of a male,* a notion since shown by Huber to be wholly chimerical. In 1771 a second institution, with the same limited object, was founded at Lauter, under the Elector Palatine's patronage, and of this Riem, scarcely less known in this branch of science than Schirach, was a member.

The greatest progress, however, was afterwards made by Huber, whose discoveries, especially of the queen bee's mode of impregnation, the slaughter of the drones or males, and the mode of working, have

* Œuvres, x. 100, 104.

justly gained him a very high place among naturalists. Nor are his discoveries of the secretion of wax from saccharine matter, the nature of propolis, and the preparation of wax for building, to be reckoned less important. To these truths the way had been led by John Hunter, whose vigorous and original genius never was directed to the cultivation of any subject without reaping a harvest of discovery. Since the time of Hunter and Huber no progress has been made in this branch of knowledge. For we have shown that the supposed discovery of Barclay is wholly without foundation; and the attempts made by some mathematical reasoners to cast doubt upon the result of former investigations have been also proved to be signal failures.

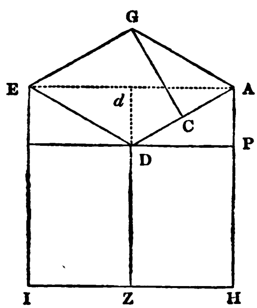
APPENDIX OF DEMONSTRATIONS.

Proposition I.

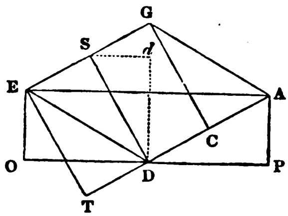
To solve the problem, of finding at what angles the rhomboidal plates forming the bottom of the cell must be inclined to one another, or, which is the same thing, at what angles they must cut the hexagonal prism, in such a manner as that the whole surface of the cell may be the smallest possible, the depth and diameter of the hexagonal prism being given; it is sufficient either to find the length of the dihedral angle of the prism cut off, or the proportion of the side of the rhombus to that of the hexagon, or, finally, the line drawn from one angle of the rhombus perpendicular to the opposite sides.

Let A Z, E Z be two contiguous sides of the prism, the problem being to cut it with a trihedral pyramid of which A G E D is one rhombus, so that the surface I Z H A G E I, composed of the rhombus D G, the two equal triangles E O D, D P A, and the two equal rectangles O Z, Z P, may be the smallest possible (this being the third part of the whole cell's surface), we may either seek, first, for the line *d* D or E O, cut off,—or, secondly, for the proportion between D E the side of the rhombus and D O the side of the hexagon,—or, thirdly, for the line G C perpendicular to D A. Any one of these three things being found must determine the other two, and also the angle at which the rhombus G D cuts the side Z E, and also the angle at which the rhombuses cut one another, and likewise the angles of the rhombus.

For O D being given, if E O is found, E O D being a right angle, E D is given. Also because



the rhombus cuts the hexagon in E and in A, the diagonal of the rhombus is given, being equal to the line subtending the angle of the hexagon, or $\sqrt{3} \times$ P D; therefore the triangle E D A is given and the angles of the rhombus.—Again, if the perpendicular G C be found and E T be drawn parallel to G C, E T is given; and in the triangle E T A, which is right angled, A E and E T being given, A T is also given. Therefore the angle E A D is given, and D E A, which is equal to it; therefore also the angle A D E, and the sides A D, D E of



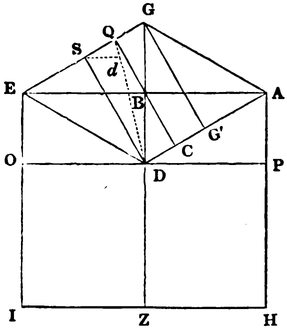
the triangle A D E or of the rhombus, and therefore both the angles and sides of the rhombus are given. —Again in the triangle D O E, D O the hexagon's side is given; and if the side D E be found, therefore E O is given and the angle O E D; and the perpendicular G C, the rhombus being found.— Lastly, D S being drawn parallel to G C is given in all the foregoing cases; and the other perpendicular S *d* from S upon the lower side of the opposite rhombus is equal to D S, the two rhombuses being equal. But D being one angle of the hexagon, that other rhombus's lower angle rests upon the opposite angle *d*, that is the angle next to O of the hexagon, and D*d* is the line subtending the hexagonal angle, and consequently is given, being equal to A E. Therefore the angle D S *d* of the triangle D S *d* is given, that is the angle which the two rhombuses make with each other.

Therefore G C the perpendicular being found, such that the surface is a minimum, the whole angles and lines are also found.—Q. E. D.

Proposition II.—*Problem.*

To find the perpendicular from the upper angle of the rhombus to the opposite side, which gives the minimum of surface.

Let G G′ be the perpendicular to be found and join D G; it bisects the other diagonal A E, and at



right angles; draw B C parallel to G G′. Therefore (by sim. tr.) A D : A B : A C and A D =

$$\frac{AB^2}{AC.} \quad \text{But } AC = \sqrt{AB^2 - BC^2}$$

Then $AD = \dfrac{AB^2}{\sqrt{AB^2 - BC^2}}$. Produce C B to Q; And because D B = B G, C B = ½ C Q = ½ G G′. Let G G′ = C Q = x, D P (side of the hexagon) = s, and E I (height or depth of the hexagonal prism) = b. Then by the property of the hexagon A E the line subtending the angle of the hexagon is equal to $\sqrt{3}$ P D = $\sqrt{3}\, s$, and

$$AD = \frac{3\,s^2}{4\sqrt{\frac{3\,s^2}{s^4} - \frac{x^2}{s^4}}} = \frac{3\,s^2}{2\sqrt{3\,s^2 - x^2}}$$

and the rhombus A D E G = G G′ × A D

$$= \frac{3\,s^2\,x}{2\sqrt{3\,s^2 - x^2}}.$$

Again, $AP = \sqrt{AD^2 - DP^2}$

$$= \sqrt{\frac{9\,s^4}{4\,(3\,s^2 - x^2)} - s^2} = \frac{s\,.\sqrt{4\,x^2 - 3\,s^2}}{2.\sqrt{3\,s^2 - x^2}}$$

and the triangle $APD = \dfrac{s^2\,.\sqrt{4\,x^2 - 3\,s^2}}{s^4\,.\sqrt{3\,s^2 - x^2}}$.

Also the rectangle P Z = (A H − A P) × P D = s

$$\times \left(b - \frac{s\,.\sqrt{4\,x^2 - 3\,s^2}}{2\,.\sqrt{3\,s^2 - x^2}}\right).$$

And the whole surface H I E G A H = (one-third of cell's surface)

$$= \frac{3s^2 x}{2\sqrt{3s^2 - x^2}} + \frac{s^2 \sqrt{4x^2 - 3s^2}}{2\sqrt{3s^2 - x^2}}$$

$$+ 2s\left(b - \frac{s \cdot \sqrt{4x^2 - 3s^2}}{2 \cdot \sqrt{3s^2 - x^2}}\right)$$

or

$$\frac{3s^2 x}{2\sqrt{3s^2 - x^2}} - \frac{s^2 \sqrt{4x^2 - 3s^2}}{2\sqrt{3s^2 - x^2}} + 2sb$$

or

$$\frac{s^2}{2} \times \left(\frac{3x - \sqrt{4x^2 - 3s^2}}{\sqrt{3s^2 - x^2}}\right) + 2sb.$$

The fluxion of this must therefore be put = 0, and that is the fluxion of the term containing fractions, and if its fluxion is equal to nothing, so will its fluxion divided by $\frac{s^2}{2}$ and multiplied by $3s^2 - x^2$, the denominator of that fluxion.

Therefore $\left(3dx - \frac{4x\,dx}{\sqrt{4x^2 - 3s^2}}\right) \times \sqrt{3s^2 - x^2}$

$$+ \frac{x\,dx}{\sqrt{3s^2 - x^2}} \times (3x - \sqrt{4x^2 - 3s^2}) = 0.$$

Dividing by dx and multiplying by

$$\sqrt{3s^2 - x^2} \times \sqrt{4x^2 - 3s^2},$$

we have

$$3\,(3\,s^2 - x^2)\sqrt{4\,x^2 - 3\,s^2} - 4\,x\,(3\,s^2 - x^2) +$$
$$+\,x\sqrt{4\,x^2 - 3\,s^2} \times (3\,x - \sqrt{4\,x^2 - 3\,s^2}) = 0$$
$$\text{or } 9\,s^2\sqrt{4\,x^2 - 3\,s^2} - 9\,s^2\,x = 0$$
$$\text{and } 4\,x^2 - 3\,s^2 = x^2; \text{ or } x = s.$$

Therefore, if the perpendicular G G′ is equal to P D, the side of the hexagon, the minimum of surface is obtained, which was the thing required.

Cor. 1. If the process had been performed upon the surface, D P A G E O, that is, a third of the three rhombuses and six triangles, without any regard to the prism, there would have been the same result, the difference being that the ultimate equation would have been $9\,s^2\sqrt{4\,x^2 - 3\,s^2} + 9\,s^2\,x = 0$, instead of the sign being negative, which makes none in the solution of $x = s$, so that the length of the cell is immaterial.

Cor. 2. If D S be drawn parallel to G G′ on the rhombus D G, and S d be drawn to the lower point d of the other rhombus, which makes with the rhombus D G the dihedral angle E G, then S d = D S, and D d = A E = $\sqrt{3}$. P D. Therefore the three lines D S, S d, and D d, are the two sides and diagonal of a hexagon, and the angle

D S d is the angle in a hexagon, or 120°. This, then, is the inclination of the two rhombuses to one another, and the other, or third rhombus, must be inclined to both of these at the same angle.

Cor. 3. Since $AD = \frac{AB^2}{\sqrt{AB^2 - BC^2}}$ and $BC = \frac{DP}{2} = \frac{s}{2}$, $AD = \frac{3s}{2\sqrt{2}}$. This, then, is the side of the rhombus.

Cor. 4. In like manner A P, half the altitude of the pyramidal bottom, $= \frac{s}{2\sqrt{2}}$, and the altitude $= \frac{s}{\sqrt{2}}$.

Cor. 5. The angles may be all determined, having the sides; and because of the equal triangles D E S and D O E, the angle D E O on the side E Z is equal to the angle D E S on the rhombus. If D E be considered as the radius, O E will be the cosine of O E D; and because

$$OE = \frac{s}{2\sqrt{2}} \text{ and } ED = \frac{3s}{2\sqrt{2}},$$

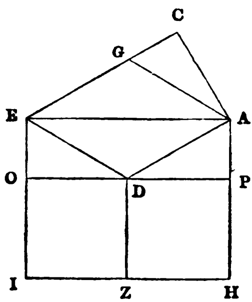
the angle is such that its sine is equal to one-third of its radius, or the latter being 1,000,000, the former is 333,333. The next lesser number to this in the table of natural cosines is 333,258, answering to an angle of 70° 32′, which is therefore the acute angle of the rhombus, and the obtuse is 109° 28′.

Scholium.

These are by measurement found to be the angles of the cells in the hive. The error into which Mr. Kœnig fell, in computing the angles of the rhombus 109° 26′ and 70° 34′, and which was always supposed to be a deviation from the minimum in the construction of the comb, originated, most probably, in the tables of sines, or in the logarithms which he used. That there might be no cause of mistake introduced into the process, it was desirable to pursue some such course as that taken above. Instead of making the perpendicular the unknown quantity x, the side of the rhombus might have been sought, and the calculus would then have given the value $\frac{3s}{2\sqrt{2}}$ (as we deduced it from the perpendicular), and from thence the perpendicular itself might have been found $= s$. But the method pursued here is very essential, because it enables us to determine the angles without having recourse to any tables of sines. It gives us the triangle, of which two sides are sides of the hexagon and the third the line joining its alternate angles. Consequently it gives us the angle subtended by that line, namely the angle of the hexagon, which is known to be 120°, without any logarithmic or other calculation. From this angle all the others follow. So we might have sought the angle made by the axis of the pyramid with the perpendicular to the dihedral angle of the rhombus, and we should have found it to be an angle whose cosine is half the radius, or an angle of 60°; and from this all the rest would follow. The advantage of these methods is, that they put any error from the tables out of the question; and indeed the solution given in the second proposition does not even result from trigonometry, for it gives the angle of the hexagon. Mr. Slee solved the problem by a different method; he sought the angles of the rhombus directly, and he obtained them correctly by the tables 109° 28′ and 70° 32′. In the next proposition it is not necessary to do more than seek the side of the rhombus. (Prop. I.)

Proposition III.—*Problem.*

To find the side of the rhombus cutting an hexagonal prism of a given diameter, so that the length of the whole dihedral angles of the solid formed by the prism and a trihedral pyramid whose sides are each equal to the rhombus, shall be the least possible.



The dihedral angle at the hexagonal base, or 6 Z H being constant, may be neglected, and then it is required to find A D such that 3 A H + 3 Z D + 6 A D + 3 A G, or 3 A H + 3 Z D + 9 A D may be a minimum. Then let A D $= x$, P D $= s$, A P $= \sqrt{x^2 - s^2}$; D Z $= b - \sqrt{x^2 - s^2}$.

And taking the fluxion $= 0$, we have

$$9\,dx - \frac{3\,x\,dx}{\sqrt{x^2 - s^2}} = 0,$$

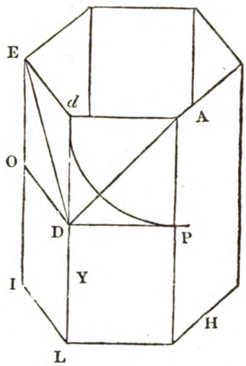
or $3\sqrt{x^2 - s^2} = x$; therefore $9x^2 - 9s^2 = x^2$,

and $$x^2 = \frac{9s^2}{8}, \text{ or } x = \frac{3s}{2\sqrt{2}}.$$

Wherefore A D is to P D as 3 to $2\sqrt{2}$; which was to be done.

Scholium.

It, therefore, appears that the same solution applies to this as to the former case, and that the form of the figure which gives the minimum of surface, gives also the minimum of solid angles. It may be further observed, that the rhombus which has these properties in relation to the hexagonal prism, is also connected with the right-angled triangle, whose sides are to each other as 1, $\sqrt{2}$, and $\sqrt{3}$, or the triangle the squares of whose sides are as 1, 2, 3—a figure well known to geometricians. If A C be drawn perpendicular to E G, then A C = A D and A E = $\sqrt{3}$. D P and E C = $\sqrt{2}$. A D. Also 2 A P, the height of the pyramid, is to P D, the side of the hexagon, as 1 to $\sqrt{2}$. P D, or in the well-known proportion of the diagonal to the side of the square, giving thus a very simple and easy geometrical construction of the figure. Take d Y = A d (the hexagonal side) upon d L, one of the dihedral angles, and from A as a centre describe a circle whose radius is $\frac{1}{4}$ of A Y



cutting A H in P; draw P D parallel to A D; join A D. Then A P is the depth at which the pyramid cuts the prism, and A D is one side of the rhombus, and D E the adjacent side.

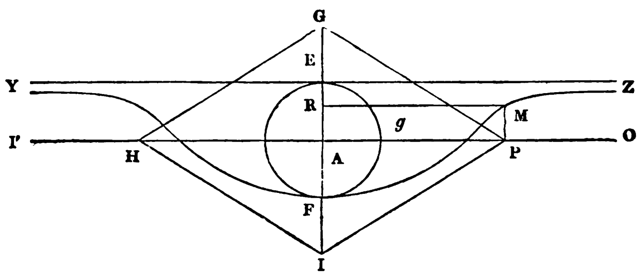
Proposition IV.

If upon the given diagonal of the rhombus (that which subtends the hexagonal angle) there be described a circle having the centre of the rhombus for its centre, and one half of the given diagonal for its diameter, and the Agnesian curve (the Versiera or Goblin*) be drawn to that circle upon the other

* Incorrectly translated in some books witch. The curve is described in Donna Agnesi's Instituzioni, vol. i. p. 381. A very elegant geometrical construction is there given of it.

diagonal of the rhombus as an axis, the ordinates are to the cosine of the adjacent angle of the rhombus (or the sine of the other angle) in a given ratio (that of $\frac{1}{2}$ the greater diagonal to 1.)

Let I G be the given diagonal of the rhombus ($= \sqrt{3.s}$), O P A a line bisecting I G at right angles, F g E a circle, whose diameter F E = A G, Y F M Z the Agnesian curvure, of which F g E is the generating circle, and Y Z the asymptote.



Then if P M be drawn, the ordinate at any point, and G P be joined, and the rhombus completed, P M is to the cosine of G P I or the sine of H G P in a given ratio.

Because, by the property of the curve, A P : R g :: E F : R E and R $g = \sqrt{AF^2 - AR^2}$ $= \sqrt{AF^2 - PM^2}$, and $AP^2 : Rg^2 :: EF^2 : RE^2$

calling

$$A\,P,\ x \text{ and } PM,\ y \text{ and } F\,E,\ \frac{\sqrt{3}\,.\,s}{2},$$

we have

$$x^2 : \overline{\frac{\sqrt{3}\,.\,s}{4}}\Big|^2 - y^2 :: \frac{3\,s^2}{4} : \left(\frac{\sqrt{3}\,.\,s}{4} - y\right)^2,$$

or

$$x^2 : \frac{\sqrt{3}\,.\,s}{4} + y :: \frac{3\,s^2}{4} : \frac{\sqrt{3}\,.\,s}{4} - y\,;$$

and therefore

$$\frac{\sqrt{3}\,.\,s}{4}\,x^2 - y\,x^2 = \frac{3\sqrt{3}\,.\,s^3}{16} + \frac{3\,s^2}{4}\,y,$$

and

$$y = \frac{4\sqrt{3}\,.\,s\,x^2 - 3\sqrt{3}\,.\,s^3}{4\,(4\,x^2 + 3\,s^2)}\,;$$

or separating $\frac{\sqrt{3}\,.\,s}{4}$,

$$y = \frac{\sqrt{3}\,.\,s}{4} \times \frac{4\,x^2 - 3\,s^2}{4\,x^2 + 3\,s^2},$$

the equation to the Agnesian curve.

Now in the isosceles triangle I G P, the cosine of G P I is equal to $\frac{2\,P\,G^2 - I\,G^2}{2\,P\,G^2}$, and

$$P\,G^2 = \frac{3\,s^2 + 4\,x^2}{4}.$$

Therefore

$$\text{Cos. } G\,P\,I = \frac{3\,s^2 + 4\,x^2 - 6\,s^2}{3\,s^2 + 4\,x^2} = \frac{4\,x^2 - 3\,s^2}{4\,x^2 + 3\,s^2}.$$

Wherefore

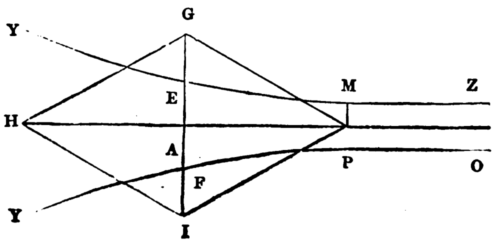
$$y : \cos. G P I :: \frac{\sqrt{3}.s}{4} : 1,$$

or P M : cos. G P I :: A F : 1. Q. E. D.

Cor. 1. If the radius (A F) is taken equal to unity, the ordinate P M is equal to the cosine.

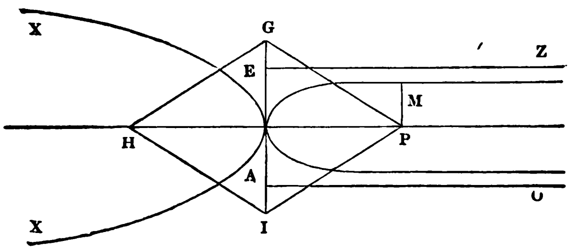
Cor. 2. If A P $= x = \frac{\sqrt{3}.s}{2\sqrt{2}}$, we have the side G P $= \frac{3s}{2\sqrt{2}}$, being the value for the minimum of surface, and $y = \frac{\sqrt{3}.s}{4} \times \frac{1}{3}$, or if A F = 1 $y = \frac{1}{3}$, which is the sine of the acute angle of the hexagon = 70′·32 or the cos. of the obtuse, which gives the minimum of surface.

Cor. 3. The same relation between the cosine of the angle I G P, or G P M and the ordinate



subsists in the curve of the fourth order Y Z O Y, whose equation is $y^2 = \frac{3 s^2}{4 x^2 + 3 s^2}$. P M is equal to the cosine of the adjacent angle G P M or the sine of G P A. Hence this curve is related to the Agnesian, by its ordinates being the sine of half the angle of the rhombus, of the whole of which angle the Agnesian ordinate is the cosine. The two curves have the ordinates to each other as the sin. of A : cos. 2 . A.

Cor. 4. The curve Y A O Z, whose equation is $y^2 = \frac{x^2}{4 x^2 + 3 s^2}$, has to the one last described,



Y A Z O, this relation, that its ordinate P M = cosine of the angle G P A or sine of G P M; and therefore its ordinate is to the ordinate of the other curve as the sine to the cosine of G P M, or as the cosine to the sine of G P A.

PROPOSITION V.—*Problem.*

The solid content of a prism terminating in a trihedral pyramid being given, and the angles at which the rhomboidal planes are inclined to one another, to find the proportion of the sides to the altitude of the prism which will make its surface the least possible.

Let $s =$ the side of the hexagon, $m\,s =$ the perpendicular from the angle of the rhombus to the opposite side, then the side of the rhombus $= \dfrac{3\,s}{2\sqrt{3 - m^2}}$

and the area of the rhombus $= \dfrac{3\,m\,s^2}{2\sqrt{s - m^2}}$

Also the distances from the acute angle of the rhombus to the part at which it cuts the side $= \dfrac{s\sqrt{4\,m^2 - 3}}{2\sqrt{3 - m^2}}$, and the two triangles $=$ $\dfrac{s^2\sqrt{4\,m^2 - 3}}{2\sqrt{3 - m^2}}$; and if y be the whole length of the hexagonal prism from the acute angle of the pyramidal base, we have for the third part of the whole surface

$$\frac{3\,m\,s^2}{2\sqrt{3 - m^2}} + 2\,s\,y - \frac{s^2\sqrt{4\,m^2 - 3}}{2\sqrt{3 - m^2}}.$$

But as there are here two variables, s and y, we must find y in terms of s. Now because the solid content of the figure is given $=\Delta$, and because it is equal to a hexagonal prism whose side is s and length y,

we have $\frac{3\sqrt{3}.s^2}{2} \times y = \Delta$, and $y = \frac{2\,\Delta}{3\sqrt{3}.s^2}$; and substituting this in the above value of the surface, we have

$$\frac{3\,m\,s^2}{2\sqrt{3-m^2}} + \frac{4\,\Delta}{3\sqrt{3}\,.\,s} - \frac{s^2\sqrt{4\,m^2-3}}{2\sqrt{3-m^2}},$$

of which the differential is

$$\frac{(3\,m - \sqrt{4\,m^2-3})}{\sqrt{3-m^2}}\,s\,d\,s - \frac{4\,\Delta\,d\,s}{3\sqrt{3}\,.\,s^2};$$

and this being $=0$, therefore

$$s = \sqrt[3]{\frac{4\sqrt{3-m^2}\times\Delta}{3\sqrt{3}\,(3\,m - \sqrt{4m^2-3})}}.$$

Substituting this for s in $y = \frac{2\,\Delta}{3\sqrt{3}.s^2}$, we obtain

$$s : y :: 2\sqrt{3-m^2} : 3\,m - \sqrt{4m^2-3},$$

being the proportion of the side of the hexagon to the length of the prism, which gives the minimum of surface, when the proportions of the sides of the rhombus to those of the hexagon are given, or the perpendicular of the rhombus from its angle to the opposite side is given (Prop. I.), and that perpendicular being to the side as $m : 1$ — which was to be done.

Cor. 1. This being the general solution for all cases, in the particular case of the minimum of surface among prisms whose length is in a given ratio to the hexagonal side, $m = 1$, and the proportion of the hexagonal side to the length is $\sqrt{2} : 1$, or the diagonal to the side of a square.

Cor. 2. If the proportion of the breadths of the solid to the whole altitude be sought, one breadth being $\sqrt{3} \cdot s$, the other $2\,s$, the proportion of the former to the altitude is that of $2\sqrt{6}$ to $\sqrt{2} + 1$, and of the latter that of $4\sqrt{2}$ to $\sqrt{2} + 1$, or 7 to 3 nearly.

Cor. 3. In this case the pyramidal bottom bisects the length of the prism, for $y = \frac{s}{\sqrt{2}}$, and the depth at which the rhombus meets the prismatic angle is $\frac{s}{2\sqrt{2}}$

Scholium.

This is the general solution of the problem of which M. L'Huillier solved one case, viz., that of $m = 1$, and the hexagonal covering of the base of the prism (the mouth of the bee's cell) was not taken into his process. Including that gives a different solution.

PROPORTION VI.—*Problem.*

The same things being given as in the last problem, to find the proportion of the length of the prism to the hexagonal side, which gives the smallest possible surface of the whole pyramid and prism, together with the hexagonal base of the prism.

Let s, y, m, be as in the last proposition. Then the steps are the same, excepting that the quantity to be differentiated has one-third of the hexagon base added, which base is $= \frac{3\sqrt{3}\,s^2}{2}$, and therefore the quantity to be differentiated is

$$\left(\frac{3m - \theta}{2\varphi} + \frac{\sqrt{3}}{2}\right) s^2 + \frac{4\Delta}{3\sqrt{3}\,s}$$

($\varphi = \sqrt{3 - m^2}$ and $\theta = \sqrt{4m^2 - 3}$),
of which the fluxion is

$$2\left(\frac{3m - \theta}{2\varphi} + \frac{\sqrt{3}}{2}\right) s\,ds - \frac{4\Delta\,ds}{3\sqrt{3}\,s^2};$$

and this being $= 0$, we have

$$s = \sqrt[3]{\frac{4\Delta\varphi}{3\sqrt{3}(3m - \theta + \sqrt{3}\varphi)}}$$

and

$$s : y :: \frac{4\Delta\varphi}{3\sqrt{3}(3m - \theta + \sqrt{3}\varphi)} : \frac{2\Delta}{3\sqrt{3}}$$

or,

$$s : y :: 2\varphi : 3m - \theta + \sqrt{3}\varphi$$

$$:: 2\sqrt{3 - m^2} : 3m - \sqrt{4m^2 - 3} + \sqrt{3} \cdot \sqrt{3 - m^2}.$$

Therefore the proportion between the side of the hexagon and the length of the prism, which gives the minimum of the whole surface, is found, which was the thing required.

Cor. 1. If $m = 1$, the case of the minimum proportion of the sides and angles of the rhombus, then the side of the hexagon is to the length of the prism as 2 to $\sqrt{2} + \sqrt{3}$.

Cor. 2. The proportion between the whole depth of the cell and side of the hexagonal prism which gives the minimum of surface being those of $3 + \sqrt{6}$ and $2\sqrt{2}$, and the diagonal of the hexagon being $\sqrt{3}\, s$, the whole depth is to the lesser width of the cell as $\sqrt{2} + \sqrt{3}$ to $2\sqrt{2}$, and to the greater width as $3 + \sqrt{6} : 4\sqrt{2}$, or as 181 to 188 nearly.

Proposition VII.—*Problem.*

The same things being supposed to find the proportion of the side of the hexagon to the length of prism which gives the smallest extent of dihedral angle bounding the planes of the side of the pyramid and prism.

Let s, y, m, Δ, be as before. We have

$$3y + 3\left(y - \sqrt{\frac{9s^2}{4(3 - m^2)} - s^2}\right) + \frac{27s}{2\sqrt{3 - m^2}};$$

and as $y = \frac{2\Delta}{3\sqrt{3}s^2}$, the quantity to be differentiated is

$$\frac{4\Delta}{\sqrt{3}.s^2} + \frac{3s}{2\sqrt{s - m^2}} \times (9 - \sqrt{4m^2 - 3}),$$ and

putting its fluxion $= 0$, we have

$$s = \sqrt[3]{\frac{16\sqrt{3 - m^2}\Delta}{3\sqrt{3}(9 - \sqrt{4m^2 - 3})}};$$

and substituting this in

$$y = \frac{2\Delta}{3\sqrt{3}s^2}, s : y :: 8\sqrt{3 - m^2} : 9 - \sqrt{4m^2 - 3};$$

and the ratio of the hexagonal side to the length is found, which was to be done.

Cor. 1. If $m = 1$ (the minimum proportion for the rhomboidal sides and angles), $s : y :: \sqrt{2} : 1$, or the side and altitude are as the diagonal to the side of the square, as was found respecting the surfaces, leaving out the hexagonal base.

Cor. 2. If the same proportions be required for the minimum of all the dihedral angles, including those of the hexagonal base, we have, by a similar process,

$$s : y :: 8\sqrt{3 - m^2} : 9 + 4\sqrt{3 - m^2} - \sqrt{4m^2 - 3};$$

where, if $m = 1$,

$$s : y :: 2 : \sqrt{2} + 1.$$

In the case of the hexagonal prism the proportion is that of the side being equal to the length or altitude.

Scholium.

It appears, thus, that if the cells of the bee had been made in the proportions suggested by M. L'Huillier, the breadth of the cell taken one way would exceed the depth in the proportion of $\sqrt{6}$ to 1; in the other way, as $2\sqrt{2}$ to 1, or not much less than 3 to 1, and the prismatic part of it would be between five and six times as broad as it is deep. It also appears that the proportions which save in reality most surface, that is, which give the minimum of surface, including the hexagonal base or covering of the mouth of the cell, come much nearer the common cell, being as 2 to $\sqrt{2} + \sqrt{3}$ for the sides, or not of considerably greater breadth than depth (being as 181 to 162 in one way, and as 181 to 188 the other). But then this way does not save most solid angles; the proportion which effects that object, including the angles of the base, is that of the breadth exceeding the height as 17 to 12 taken one way, and as 20 to 12 another. So that beside the total unfitness of such flat and broad cells for the breeding of the bees, there are insuperable obstacles to their being so constructed with a view to save labour in the manner suggested. But there is no saving even of surface upon a single cell compared with another, upon M. L'Huillier's plan; for if we take the hexagon side to the length as 1·387 to 5 in the common cell, and as 2·379 to 1·687 in the cell of $s : y :: \sqrt{2} : 1$; then because the whole surface, including the hexagonal bases, is equal to $6\,s\,y + \frac{3}{2}(\sqrt{2} + \sqrt{3})\,s^2$,* or to $6\,s\,y + 4{\cdot}718\,s^2$ nearly, in the cell actually made ($s = 1{\cdot}387$ $y = 5$) we have the

* The rhomboidal planes $= \frac{9\,s^2}{2\sqrt{2}}$, the six triangles $= \frac{3\,s^2}{2\sqrt{2}}$, the planes of the prism below the place where the pyramid cuts them $= 6\,s\left(y - \frac{s}{2\sqrt{2}}\right)$, and the hexagonal base $= \frac{3\sqrt{3}}{2}s^2$, which make the above quantity.

whole surface = 50·670, and in the cell proposed ($s = 2·379, y = 1·687$) = 50·737, being ·067 more surface, instead of any saving. In this it is to be observed that the amount of the hexagonal base in the former case is 4·992, and in the latter 14·683. This difference of 9·691, which is more than sufficient to counterbalance all the other saving, was entirely left out of view in M. L'Huillier's solution, because he investigated a wrong problem. And this is quite independent of the greater thickness of the wax at the rhomboidal and hexagon bases, which are much more extensive in proportion to the thin sides in the proposed cell than in the one actually made.

These comparisons, however, are all instituted as between single cells. When we come to calculate the difference between the quantity of wax and work saved in each, where there are many cells, the balance is in favour of the proportions used by the bees. For the wide and shallow cells can only be placed in comparatively small numbers on the comb, and consequently there must be so many more combs made to rear the same number of bees, and even to store the same quantity of honey. There is the same loss of work and materials that there is in building a house of one story high, and contriving as many rooms and of the same size with a house of three stories. There is a loss both in the roof and foundation, or pavement.

Compare the two plans upon only a work of seven cells on each side, or six placed round a central one. On the proportions of $s = 1\cdot387$, there would be for the walls, suppose each separate . . . . $= 276\cdot92$

Diminish this as 7:5 for common walls	197·8
Double this for the other side . .	395·6
Add rhomboidal part	42·97
Add hexagon ends both sides . .	69·888
Total	508·458
On the proposed plan of $s = 2\cdot379$.	$y = 1\cdot687$
Separate walls	126·1666
Diminish as 7:5.	90·3
Double	180·6
Rhomboidal part	126·497
Hexagon end doubled	205·604
Total	512·701
Leaving a balance against the proposed plan of	4·243

But this is a small part of the balance.

For compare the surface required for accommodating the same number of young bees—that is for giving the same number of cells in a given space in both places.

In a square foot, or 20,736 lines, there are in the cell $s = 1\cdot387$ $y = 5$, about 4,152 cells on each side of the comb, and if these were separate, there would be 24,927 walls exclusive of rhomboids and hexagonal bases. But these must be reduced in the proportion of 799 to 407 $(2 \times (3n^2 - 3n + 1) : 3n^2 - n$, n being the number of series or rows round the centre one), which gives for the whole number of walls 12,697, and the surface of these walls $\left(sy - \frac{s^2}{4\sqrt{2}}\right)$ is 83,724, which being doubled for the opposite comb gives 167,448; to this must be added the rhomboidal bottom $\left(\frac{9s^2}{2\sqrt{2}}\right)$ 25,550 taken once only, and the hexagonal bases 20,736, which must be doubled for the opposite side of the comb, and makes therefore 41,472; so the whole foot square of comb has a surface equal to 234,469.

Take now the proposed cell $s = 2\cdot379$ $y = 1{,}687$, and we have the number of cells = 1,414, the surface of the walls 8,484, to be reduced in the proportion of 673 to 347; this gives 4,374, of which the surface is 13,165, which being doubled is 26,330; add the rhomboidal bases, 25,536, and twice the hexagon bases, 41,472, making in all 93,338.

But to have an equal number of cells, there must in this latter case be more combs in the proportion of 4,162 : 1,414, and if 9 combs of $s = 1,387$, with intervals of 5 lines, go into $\sqrt{9}$ foot, there must be 26½ of $s = 2,379$; there will therefore be of the $s = 1,387$, a surface = 2,110,221, and of the $s = 2,379$, a surface of 2,473,457, being a loss of 363,236, or between a fifth and sixth of the whole work.

Thus much for the plan and proportions proposed by M. L'Huillier. But if the real minimum minimorum, including the hexagonal base, be taken from Prop. VI., we shall have the side $s = 1 \cdot 821$, and the length of the wall $y = 2,858$, which will give for the whole surface, including the hexagonal base, of a single cell = 47,844, being considerably less than either the cell actually made ($s = 1,387$) or the one proposed ($s = 2,379$); but upon a number of cells this balance is thrown the other way, by the considerations above referred to, of the number in a comb. This more than counterbalances the saving.

Thus the number of cells of this form ($s = 1 \cdot 821$, $y = 2{,}858$) in a square foot is 2,298; the number of walls if separate = 13,788, which reduced in the proportion of 1,141 : 590 gives 7,129, and the surface of these = 35,252, which doubled is 70,504; the hexagon bases doubled are 41,472, and the rhomboidal bottoms 25,539—in the whole a surface of 137,515. But to have an equal number of cells we must have more combs than in the actual cell ($s = 1{,}387$) in the proportion of $16\frac{1}{3}$ to 9, which gives the real amount of surface = 2,246,062, or 135,842 more than the actual cells, and a loss of nearly $\frac{1}{18}$ on the whole work. So that by the real minimum minimorum, although considerably less would be lost than by the plan which M. L'Huillier recommends, there would still be a loss as compared with the actual cell of this amount, independent of all the other disadvantages, and among others, that the actual cell has the minimum of dihedral angles.

But the consideration of those angles completes the proof in favour of the common cell. The following table shows the length of the dihedral angle required in the actual cell, in the one proposed by M. L'Huillier, and in the one which gives the real minimum minimorum of surface; both including the angle of the hexagonal base, and excluding that angle, in case it should be contended that the work is less fine and difficult there, although it must be observed that when no hexagonal plate is added to cover any cells, they are provided with a rim of the red matter used by the bees.

COMPARATIVE LENGTHS OF DIHEDRAL ANGLE.

	Actual Cell.	L'Huillier's.	Real Minimum.
Exclusive of angle at hexagon base . .	31,734	30,248	52,553
Including that angle .	40,056	34,522	63,479

It is thus demonstrated, *first*, that if we only take the extent of surface into the account, the actual cell saves considerably more than the one proposed by the Academician, though it loses somewhat in comparison with that cell in respect of the dihedral angle, and that it saves more surface, whether we compare cell with cell or the whole works of the hive with each other.—*Secondly*, That though there is a form which would save more surface, comparing single cells together, that form wastes surface upon the whole work, and creates a very great waste of the finer workmanship (and which also costs more wax) about the angles. Therefore it is proved that in every respect the actual cell is much more economical of wax and of work than any other which can be conceived.*

* The above calculations (which, though abridged, have been rendered necessary by the importance of the Berlin argument) require only elementary geometry, and are easily made; but care must be taken always to work to the same number of decimals, otherwise, as the whole relates to proportions, error will be committed.

The Propositions require an easy application of the differential calculus; which any one acquainted with the common rules of algebra will easily become able to make, by attending to the account of that or the method of fluxions in the Analysis of the Principia in Vol. II.

GENERAL NOTE RESPECTING EVIDENCES OF DESIGN.

All the inquiries in which we have been engaged lead to one conclusion of great importance. Notwithstanding the progress which has been made in various sciences, the things which have been discovered and ascertained bear an infinitely small proportion to those of which we are still either wholly ignorant, or imperfectly and dubiously informed. In a vast variety of instances, design and intelligence have been traced—instances so well deserving to be called innumerable, that we are entitled to believe in contrivance as the universally prevailing rule, and we never hesitate so to conclude. But the mode and manner of the working is still, in a prodigious number of cases, concealed from us; and we are entitled to infer that numberless things which now seem irregular, that is arranged according to no fixed rule, are, nevertheless, really disposed in an order which we have not discovered, but which would, if we knew all, be as complete as that observed and traced in the cases known to us.

Thus the regular working of bees, which we have been examining, is reducible to certain known rules; the figures formed by them are, in all their relations, familiar to mathematicians. The problems of maxima and minima, on the solution of which those operations proceed, may have parallels in the case of other animals; it is not at all improbable that the beaver forms his dike for protection against the water upon some such principle, namely, of the form which is better than any other conceivable form calculated to oppose a solid resistance to the pressure of water.* It appears probable that the works of spiders in concentric circles, and along their radii, are also regularly arranged in known figures, and upon similar principles. Many of the parts of plants wear the semblance of regular and symmetrical curve lines, insomuch that a mathematician once presented a paper to the Royal Society (on some propositions in the higher geo-

* The base of the dike being 12, the top 3 feet thick, and the height 6 feet, the face is the side of a right angled triangle, whose height is 8 feet; and if the materials were lighter than water in the proportion of 44 : 100, this construction would be the best one conceivable, to prevent the dam from turning round. But the form flatter than that which would best serve this purpose when the materials are heavier than water, is probably taken to prevent the dam from being shoved forward.

metry), which he entitled, from the form of the lines investigated, "*Fasciculus Florum Geometricorum.*" The orbits in which the heavenly bodies move, and which form the subject of consideration in the latter part of the second volume, come manifestly within the same remark still more certainly; for the forms of those paths, the relation of all their points to given straight lines, is in a great degree ascertained. But it seems very reasonable to conclude, that the small number of such regular figures which the state of science in its various branches has as yet enabled us to trace, is as nothing compared with those figures still so unknown to us, that in common speech we talk of them as irregular, while this is only a word, like chance, implying our own ignorance.

For the mathematical sciences, extraordinary as the progress already made may be reckoned, with regard to the difficulty of the subject, and the imperfect faculties of man, are most probably still in their infancy. Of the infinite variety of curve lines, we know but a very few with any particularity, to say nothing of our equal ignorance (connected with the former) of most of the laws of complex motion. In the parts of animal and vegetable bodies, especially of the larger kind, there are few symmetrical forms observed; greater convenience, in the former instance at least, is evidently attained by other shapes. Yet there seems no reason to doubt that all the forms which we see may be in reality perfectly regular, that is, that each outline is a curve, or portion of a curve, related to some axis, so that each of its parts shall bear the same relation to lines similarly drawn from it to this axis, which all its other points do. If we know little of algebraical curves, we know still less of those whose structure is not expressible by the relations of straight lines and numbers, the class called mechanical or transcendental, the forms of some of which are very extraordinary, but all whose points are related together by the same law. There is every reason to expect that the further progress of science will unfold to us much more of the principles upon which the forms of matter, both organic and inorganic, are disposed, so that the order pervading the system may be far more clearly perceived.

So of motion—In one most important branch, dynamics is still in its infancy; we know little or nothing of the minute motions by which the particles of matter are arranged, when bodies act chemically on each other. Even respecting the motions of fluids so much studied as electricity, and heat (if it be a fluid), and the operation of the magnetic influence, science is so imperfect, and our data from observation so scanty, that mathematical reasoning has as yet hardly ever been applied to the subject. It is the hope of men who reflect on these things, and it is probably the expectation of those who most deeply meditate upon them, that, in future times, a retrospect upon the fabric of our present knowledge, shall be the source of wonder and compassion—wonder at the advances made from such small beginnings—compassion for the narrow sphere within which our knowledge is confined;—and when the greater part of what we are now only able to believe regular and systematic from analogy and conjecture, will have fallen into an order and an arrangement certainly known and distinctly perceived.

END OF VOL. I.

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ERRATA.

Page	Line	
55,	4,	*for* " assederemus" *read* " assedimus."
168,	6,	*for* " conveneremus" *read* " conveniremus."
169,	—	*for* " INTELLIGENCE" *read* " ANIMAL INTELLIGENCE," and so in the running title to the next twenty-four pages.
107,	7,	*after* " being" *insert* " without."
229,	15,	*for* " the interstices" *read* " no interstices."
297,	9,	*for* " 27 to 28" *read* " 28 to 29."
ib.	14,	*for* " $\frac{1}{9}$" *read* " $\frac{1}{15}$."