# THE LIFE

OF

# SIR ISAAC NEWTON,

CONTAINING AN

ACCOUNT OF HIS NUMEROUS

INVENTIONS AND DISCOVERIES;

AND A BRIEF

SKETCH OF THE HISTORY OF ASTRONOMY

PREVIOUS TO HIS TIME.

COMPILED FROM AUTHENTIC DOCUMENTS.

BY GEORGE GRANT.

AUTHOR OF "SIR WILLIAM WALLACE" "ROBERT BRUCE," "SIR WALTER SCOTT," ETC.

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# PREFACE.

In compiling this Life of Sir Isaac Newton, there has been considerable difficulty experienced from the scantiness of materials collected by preceding biographers, both as regards the particulars of the early life and the historical details of the discoveries of one of the greatest men whom England has had the honour of having produced—a person alike distinguished for the great originality and depth of his philosophic views, and the simplicity and amiableness of his character—one whom we are perhaps called on to admire more than to imitate, yet one whose virtues and piety may well serve as an example to individuals in every sphere of life.

How frequently are the lives of men of ordinary talent recorded for our instruction. It certainly must be much more interesting to follow the most exalted genius through the incidents of common life;—to remark the steps by which he attained his lofty pre-eminence;—to observe how he performs the functions of the social and the domestic compact;—how he exercises his lofty powers of invention and discovery;—how he comports himself in the arena of intellectual strife;—and in what sentiments, and with what aspirations he quits the world which he has adorned. In almost all these bearings, the life and writings of Sir Isaac Newton abound with the richest counsel; and in them the philosopher, the moralist, and the divine, will find instruction.

Aware that several eminent and talented authors have preceded him, the editor acknowledges himself under many obligations for much valuable information; their works are more extended, and although he unhesitatingly admits, they possess much greater merit than the present unpretending volume, yet they are also more expensive, and consequently beyond the reach of the working-classes, who are now thirsting after scientific knowledge.

The object of the present publication is to present to the world a cheap edition of the Life of Sir Isaac Newton, and it is confidently hoped that it will meet with the approbation of a liberal and enlightened public.

*London, November,* 1849.

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# LIFE OF

# SIR ISAAC NEWTON.

## CHAPTER I.

**His Birth and Parentage. His Early Education. Is sent to Grantham School. His Attachment to Mechanical Pursuits in his youth. His Windmill; his Water Clock; his self-moving Carriage. His Sun-Dials. Preparations for the University. Enters Trinity College, Cambridge. His propensity for Mathematics. His Geometrical Studies. Purchases a Prism. Employed to revise Dr. Barrow's Optica Lectures. Takes his Degrees. Is appointed a Fellow of Trinity College. Succeeds Dr. Barrow in the Lucasian Chair of Mathematics.**

THE name of this celebrated philosopher has by general consent been placed at the head of those great men who have been acknowledged as the ornaments of their species. The character of Newton cannot be delineated and discussed like that of ordinary men; its unity is so beautiful, that the biographer must dwell upon it with delight, and the inquiry by what means he attained an undisputed superiority over his fellow-creatures, must be both interesting and useful. It has been asserted that all men are born equal in talents, and that the difference which exists amongst them is the effect of education; but this is disproved by the observation of every parent and teacher, a decided inequality in capacity, for receiving instruction being distinctly exhibited by children even in infancy. Newton was endowed with talents of the highest order, but those who are less eminently gifted may study his life with advantage, and derive instruction from every part of his career. With a power of intellect almost divine he demonstrated the motion of the planets, the orbits of the comets, and the causes of the tides of the ocean: he investigated with complete success the proportions of light and colours, which no man before had even suspected; he was the diligent, sagacious, faithful interpreter of Nature, antiquity, and Scripture; his philosophy tended to exalt the glory of the Creator, and he exhibited in his manners the purity and simplicity of the doctrines of the Gospel. He was a firm believer in Christianity, not as men in general believe, by coldly assenting to the truth of doctrines, merely because they have been early inculcated by parents and preceptors. He was deeply learned in history and chronology, and he applied the unrivalled powers of his mighty intellect to the complete examination of a subject compared with which all others sink into insignificance; the result was a clear conviction of the truth of revealed religion, which is demonstrated in all his works, and which was still more effectually shown in his life and conduct. Those who consider the character of an individual so highly renowned will duly appreciate the value of his testimony. His biography, therefore, cannot fail to excite a general interest.

Sir Isaac Newton was born at Woolsthorpe, a hamlet in the parish of Colsterworth, in Lincolnshire, about six miles south of Grantham, on the 25th day of December (old style) 1642, and was baptized at Colsterworth on the 1st day of January 1642-3. His father, Mr. Isaac Newton, died at the early age of thirty-six, only a few months after his marriage to Harriet Ayscough, daughter of James Ayscough of Market Overton in Rutlandshire. The consequence was, that the mother of the interesting subject of these pages was left in a state of pregnancy, and appears, from the grief occasioned by the death of her husband, to have given birth to her first and posthumous child. The helpless infant thus ushered into the world was of such an extremely diminutive, and seemed of so delicate a frame, that two women who were hurried off to Lady Pakenham's at North Witham, to procure some medicine for the purpose of strengthening the puling infant, declared that they did not expect to find him in life on their return. Sir Isaac in after life has repeatedly asserted that he had often heard his mother say, that "when he was born he was so little that they might have put him into a quart mug, and did not expect that he could live." But an all-wise Providence had otherwise decreed, and that feeble body which appeared scarcely qualified to contain its immortal mind, was destined to shoot forth and enjoy a vigorous maturity, and to survive the threescore years and ten allotted as the term of human existence. The property of Woolsthorpe, in the manor-house of which this remarkable birth took place, has been in the possession of the family of Newton for more than a hundred years. This family came originally from Newton in Lancashire, but, before purchasing the estate of Woolsthorpe, had settled at Westby in Lincolnshire. The manor-house is situated in a beautiful little valley, celebrated for its copious wells of excellent spring water, on the west side of the river Witham, which takes its rise in the vicinity, and commands a remarkably pleasing prospect to the east towards Colsterworth. The paternal property of Woolsthorpe was worth no more than £30 a-year; but Mrs. Newton was proprietor of another small estate at Sewstern, in Leicestershire, and only three miles distant from Woolsthorpe which raised the yearly value of their property to nearly £80; and being a woman of an active disposition, it is probable that the cultivation of the small farm on which she resided somewhat increased the limited income upon which she had to support herself and her favourite child, as well as to provide for his education.

Mrs. Newton continned to nurture and watch over her tender charge for three years, with the most exemplary parental solicitude; but at the end of that period, in consequence of her marriage to the Reverend Barnabas Smith, rector of North Witham, she removed to the parsonage, and Isaac was left under the care of his maternal grandmother. At an early age he was sent to two day schools at Skillington and Stoke, where he acquired the little learning usually afforded at these seminaries; but upon reaching his twelfth year he went to the public school at Grantham, conducted by Mr. Stokes, and was at the house of Mr. Clark, an apothecary in that town. At this time he was a thoughtless boy, very inattentive to his lessons, and much attached to boyish amusements, and consequently very low in the school, and presented little hopes of his future success. The boy, however, who happened to be above him in the class, having one day given him a severe kick upon his stomach, from which he suffered great pain, Isaac's mode of revenge was peculiar; from his little size, and delicate constitution, he was unable to punish his antagonist in the usual method, by giving him a good beating; but he laboured incessantly at his studies, and gave up his amusements, till he got above him in the school; and from that period he rose by degrees, till he was the head boy. From the industrious habits of application which this trifling incident had led him to form, the peculiar character of mind of the future philosopher was speedily displayed.—

During the hours of play, when his companions were occupied with these amusements, his attention was taken up with mechanical contrivances, either in imitation of something which he had previously observed, or in the execution of some original conception of his own. To enable him to do so, he embraced every opportunity of providing himself with little saws, hatchets, hammers, and all sorts of tools, which, by constant practice, he soon acquired the art of using with a workmanlike dexterity. The principal pieces of mechanism which he thus constructed at this early age were a windmill, a water-clock, and a carriage put in motion by the person who sat in it. When a windmill was in the course of erection in the neighbourhood of Grantham, on the road to Gunnerly, young Newton frequently attended and anxiously watched the operations of the workmen, and attained such a complete knowledge of the machinery, that in a short time he completed a working model of it, to the great surprise and admiration of every person who beheld it. The young mechanist frequently placed this model on the top of Mr. Clarke's house, in which he lodged, where it was put in motion by the action of the wind upon its sails; but not content with this exact representation of the original machine, he conceived the idea of driving it by animal power; and for this purpose he entrapped a mouse, which he called the miller; and this little animal, by acting on a sort of tread-wheel, gave motion to the machine.—According to some accounts, the mouse was made to advance by having a string tied to its tail; while others allege that the power of the diminutive agent was instigated by its fruitless attempts to reach a small piece of toasted cheese placed above the wheel.

His water-clock was formed out of a box, which, with great earnestness, he had solicited from the brother of his landlady, without giving any notice of his intentions. It was about four feet high, and of proportionate breadth, not unlike a common house-clock. The index of the dial-plate was turned by a piece of wood, which either rose or fell by the action of dropping water. As it stood in Isaac's own bed-room, he supplied it every morning with the requisite quantity of water; and this same machine was used as a time-keeper by Mr. Clarke's family, and remained in the house many years after its inventor had left Grantham. Dr. Stukely, in a letter to Dr. Mead says, "I remember once, when I was deputy to Dr. Halley, Secretary to the Royal Society, Sir Isaac talked of these kind of instruments. That he observed the chief inconvenience in them was, that the hole through which the water was transmitted was necessarily very small, and subject to be furred up by the impurities in the water, as those made with sand will wear bigger, which at length causes an inequality in time." This satisfactorily proves that Newton's mind was occupied by the consideration of his youthful inventions, when more important avocations attracted his attention in after years.

His mechanical carriage was a vehicle with four wheels, which was put in motion with a handle wrought by the person who sat in it, but it seems only to have been calculated for a smooth surface, and not fitted to overcome the inequalities of a road. It was, however, a most useful invention for invalids, which enabled them to move from one place to another in a room, when unable to walk.

Isaac Newton was at this period "a sober, silent, thinking lad" who seldom ever joined in the ordinary games and more boisterous amusements of his companions, yet he took great delight in providing them with pastimes of a scientific character. He introduced among his schoolfellows the flying of paper kites; and he is said to have been at great pains in determining their best forms and proportions, and in ascertaining the position and number of the points by which the string ought to be attached. He also made paper lantherns, by the light of which he went to school in the winter mornings; and in dark nights he frequently attached these lantherns to the tails of his kites, which inspired the country people with the belief that they were comets.

In the house of Mr. Clark at Grantham, where he lodged, there were also some female boarders in whose company he appears to have taken much pleasure. One of these young ladies, a Miss Storey, sister to Dr. Storey, a physician at Buckminster, near Colsterworth, was Newton's junior by two or three years, and to great personal attractions she seems to have added more than the customary share of female talent. The society of Miss Storey and her companions was always preferred to that of his own schoolfellows, and it was one of his most agreeable occupations to exercise his mechanical abilities in constructing for them little tables, chairs, cupboards, and other utensils for holding their dolls and their trinkets. He had lived nearly six years in the same house with Miss Storey, and there is every reason to believe, that during this time their youthful friendship gradually rose to a stronger passion; and his attachment is said to have continued even after he was sent to college, but as he could not marry without forfeiting his chance of a fellowship, and as he had no means of supporting a wife and family, he subdued his predilection in silence, and found consolation in the severest labour of study. This young lady was afterwards twice married, and under the name of Mrs. Vincent, Dr. Stukely visited her at Grantham in the year 1727, when she had attained the mature age of eighty-two, and from her he obtained many interesting particulars respecting the early history of Sir Isaac. Newton's esteem for this his first love, continued unabated during his life. He never visited Lincolnshire without calling upon her;

and, when she sunk into poverty, liberally supplied her wants, by relieving her from little pecuniary difficulties which appear to have frequently perplexed her.

But Newton's genius was not engrossed by mechanical pursuits. Among his early passions we have to recount his love of drawing, and even of writing verses. His own room was furnished with pictures, drawn, coloured, and neatly framed by himself, sometimes from copies, but often from life. Mr. Clark informed Dr. Stukely that the walls of the room in which Sir Isaac lodged were covered with charcoal drawings of birds, beasts, men, ships, and mathematical figures, all of which were very well designed.

Among the portraits in Newton's room were those of Dr. Donne, Mr. Stokes, the master of Grantham school, and King Charles I., under whose picture were the following lines :—

> A secret art my soul requires to try,
> If prayers can give me what the wars deny.
> Three crowns distinguished here, in order do
> Present their objects to my knowing view.
> Earth's crown, thus at my feet I can disdain,
> Which heavy is, and at the best but vain.
> But now a crown of thorns I gladly greet,
> Sharp is this crown, but not so sharp as sweet;
> The crown of glory that I yonder see
> Is full of bliss and of eternity.

These verses were repeated to Dr. Stukely by Mrs. Vincent, who believed them to be the composition of Newton, a circumstance which seems to be the more probable, as he himself many years after assured Mr. Conduit, with some expression of pleasure, that he "at one time excelled in making verses," though he had been frequently heard to express his great contempt for poetical composition; but like all young men who are anxious in gaining a respectable livelihood, he gave up the idle trade for a more serious calling.

But during these early years, while the mind of Newton was occupied principally with the pursuits which we have already detailed, it was by no means inattentive to the motions of the celestial bodies, on which he was afterwards enabled to throw such a flood of brilliant light. His thoughts had probably been directed to the more accurate measure of time which the motion of the sun afforded, by the imperfections of his water-clock. In the back-yard of Mr. Clark's house where he lived, he marked the varying movements of that luminary upon the walls and roofs of the outbuildings, and by means of fixed pins he noted the hourly and half-hourly subdivisions. One of these dials, which went by the name of "Isaac's dial," and was long after frequently referred to by the country people for the hour of the day, appears to have been drawn solely from the observations of several years; but we have now no means of obtaining information whether the numerous dials which he drew on the walls of his house at Woolsthorpe, and which existed many years after his death, were of the same description, or were projected from his knowledge of the doctrine of the sphere.

In the year 1656, upon the death of the Reverend Mr. Smith, his widow, the mother of Newton, left the rectory of North Witham, and took up her residence at Woolsthorpe along with her three children, Mary, Benjamin, and Hannah Smith. Isaac had now attained the fifteenth year of his age, and had made great progress in his studies under Mr. Stokes; and as his mother considered that he was capable of being useful in the management of the farm and country business at Woolsthorpe, she, chiefly from a motive of economy, recalled him from the school at Grantham, and brought him home. In order to accustom the young farmer to two of the most important branches of agricultural labour—the art of selling and buying—he was generally sent on the Saturdays to Grantham market, to dispose of grain and other farm produce, and to purchase such articles as was requisite for the use of the family. But as Isaac was yet void of experience in such matters, an old trust-worthy servant generally accompanied him for the purpose of initiating him into the mysteries of the business of market-making. The inn where they put up was the Saracen's Head at West Gate; but no sooner had they put up their horses than our youthful philosopher deserted his commercial concerns, and betook himself to his former lodgings in Mr. Clark's garret, where a number of the apothecary's old books afforded him abundance of entertainment till his aged guardian had finished the more intricate business of market-making, and called for him with the announcement that it was time for them to return home. At other times he did not trouble himself by going so far, but deserted his duties on the road, and ensconced himself under a hedge, where he remained with some favourite volume till the servant returned from market at Grantham. "One of his uncles," says Mr. Biot, "having one day found him under a hedge with a book in his hand and entirely absorbed in meditation, took it from him, and found that he was occupied in the solution of a mathematical problem. Struck with finding so serious and so active a disposition at so early an age, he urged his mother no longer to thwart him, and to send him back to Grantham to continue his studies." This advice his mother shortly after found it necessary to comply with, as the more immediate affairs of the farm were not more prosperous under his management, than would have been his marketings at Grantham if they had been left to his own discretion. The perusal of a book, the execution of a model, or the superintendence of a water-wheel of his own construction, whirling the glittering spray from some neighbouring stream, absorbed all Isaac's thoughts, while the sheep were left to wander at will, and the cattle making sad havock in the cornfields.

Mrs. Smith was soon convinced from experience that her son's genius took a higher flight than the cultivation of the soil, and as his passion for study, and his dislike for rural occupations increased with his years, she wisely resolved to give him all the advantage which education could confer. He was accordingly sent back to Grantham school, and his lodgings in Mr. Clark's house, where he continued for some months busily engaged in preparation for his academical studies. His uncle, the Reverend William Ayscough, who was rector of Burton Coggles, about three miles east of Woolsthorpe, and who had himself studied at Trinity College, recommended it to his nephew to enter that society, and it was acccordingly determined that he should proceed to Cambridge at the commencement of the approaching term.

It has been justly remarked by a celebrated scientific author that, "to a young mind thirsting for knowledge, and ambitious of the distinction which it brings, the transition from a village school to an university like that of Cambridge,—from the absolute solitude of thought to the society of men imbued with all the literature and science of the age, must be one of eventful interest. To Newton, it was a source of peculiar excitement. The history of science affords many examples where the young aspirant had been early initiated into the mysteries, and had even exercised his powers of invention and discovery before he was admitted within the walls of a college; but he who was to give philosophy her laws did not exhibit such early talent;—no friendly counsel regulated his youthful studies, and no work of scientific eminence seems to have guided him in his course. In yielding to the impulse of his mechanical genius, his mind obeyed the laws of its own natural expansion, and, following the line of least resistance, it was thus drawn aside from the strongholds with which it was destined to grapple. When Newton, therefore, arrived at Trinity College, he brought with him a more slender portion of science than falls to the lot of ordinary scholars; but this state of his acquirements was, perhaps, not unfavourable to the development of his powers. Unexhausted by premature growth, and invigorated by healthful repose, his mind was the better fitted to make those vigorous and rapid shoots which soon covered with foliage and with fruit, the genial soil to which it had been transferred. Cambridge was consequently the real birth-place of his (Newton's) genius. Her teachers fostered his earliest studies—her institutions sustained his mightiest efforts—and within her precincts were all his discoveries made and perfected. When he was called to higher official functions his disciples kept up the pre-eminence of their master's philosophy, and their successors have maintained this seat of learning in the fullness of its glory, and rendered it the most distinguished among the universities of Europe."

Isaac Newton was admitted into Trinity College, Cambridge, on the 5th day of June, 1660, in the 18th year of his age. Dr. Barrow was elected professor of Greek in that university during the same year. Newton's attention was first directed to the study of mathematics by an ardent desire to inquire into the truth of judicial astrology; and he is said to have discovered the folly of that study by erecting a figure with the aid of one or two of the problems of Euclid. The propositions contained in this ancient system of geometry he regarded as self-evident truths; and, by his genius and patient application, without any preliminary study, he made himself master of Descartes's Geometry. He afterwards discovers, as a mistake in his mathematical studies, this neglect of the elementary truths of geometry; and he expressed to Dr. Pemberton his regret that "he had applied himself to the works of Descartes and other algebraic writers before he had considered the elements of Euclid with that attention which so excellent a writer deserved." Dr. Wallis's Arithmetic of Infinities, Saunderson's Logic, and the Optics of Kepler, were among the works which he appears to have studied with much care. On these books he wrote numerous comments during their perusal; and so great was his progress in his scientific studies, that he is reported to have found himself more deeply versed in several branches of knowledge than the tutor who was employed to direct his studies.

During the first three years that he spent at Trinity College, neither history nor tradition has handed down to us any particular account of his progress. It appears from an account (which he regularly kept) of his expenses, that in the year 1664 he purchased a prism, for the purpose, as has been reported, of examining Descartes's Theory of Colours; and it is stated by Mr. Conduit, that he not only soon established his own views on the subject, but detected the errors in those of the French philosopher. This, however, does not appear to have been the case. Had he really made such an important discovery as the composition of light so early as the year 1664 or 1665, it is not at all probable that he would have withheld it not only from the Royal Society, but from his personal friends at Cambridge, till 1671. Dr. Barrow, his friend and tutor, was made Lucasian Professor of Mathematics in 1663, and he delivered a course of optical lectures, which he afterwards published in 1669. In the preface of this work he acknowledges his obligations to his esteemed colleague, Mr. Isaac Newton, for having revised the MSS., and corrected several oversights, and made some important suggestions. In the twelfth lecture there are some observations on the origin and nature of colours, which Newton, had he been then in possession of their true theory, could not have permitted his friend to publish. According to Dr. Barrow, WHITE is that which discharges a copious light, equally clear in every direction. BLACK is that which does not emit light at all, or which does it very sparingly. RED is that which emits a light more clear than usual, but interrupted by shady interstices. BLUE is that which discharges a rarified light, as in bodies which consist of white and black particles arranged alternately. GREEN is nearly allied to blue. YELLOW is a mixture of much white and a little red; and PURPLE consists of a great deal of blue, mixed with a small portion of red. The blue colour of the sea arises from the whiteness of the salt which it contains, mixed with the blackness of the pure water in which the salt is dissolved; and the blueness of the shadows of bodies, seen at the same time by candle and daylight, arises from the whiteness of the paper mixed with the faint light or blackness of the twilight. These opinions savour so little of genuine philosophy that they must have attracted the observation of Newton; and had he discovered at that time, when he revised these MSS., that white was a mixture of all the colours, and black a privation of them all, he could not have permitted the absurd speculations of his master to pass uncorrected.

That Newton had not distinguished himself by any positive discovery, so early as 1664 or 1665, we think may be satisfactorily proved; but it may also be inferred from the circumstances which attended the competition for the law-fellowship of Trinity College. The only candidates for this appointment were Newton and Mr. Robert Uvedale; and, on examination, Dr. Barrow, then Master of Trinity, having found them perfectly equal in their attainments, conferred the fellowship on Uvedale as the senior candidate. Had Isaac Newton made the important discovery alluded to, at this time, there can be little doubt that he would have received the appointment from his friend.

In the books of the University, we find Isaac Newton recorded as having been admitted sub-sizor in 1661. He became a scholar in 1664. In 1665, he took his degree of Bachelor of Arts; and in 1666, in consequence of the breaking out of the plague, he returned home to Woolsthorpe. In 1667, he was made Junior Fellow. In 1668, he took his degree of Master of Arts; and in the same year he was appointed to a Senior Fellowship. In 1669, when Dr. Barrow had resolved to devote his attention to theology, he resigned the Lucasian Professorship of Mathematics in favour of Newton, who may now be considered as having entered upon that brilliant career of discovery, which has, by general consent, placed his name at the head of those great men who have been the ornaments of their species. However imposing be the attributes with which time has invested the sages and the heroes of antiquity, the brightness of their fame has been eclipsed by the splendour of Sir Isaac Newton's reputation; and neither the partiality of rival nations, nor the vanity of a presumptuous age, has ventured to dispute the ascendancy of his genius.

"The life and writings of this great philosopher abound with the richest counsel. Here the philosopher will learn the art by which alone he can acquire an immortal fame. The moralist will trace the lineaments of a character adjusted to all the symmetry of which an imperfect nature is susceptible; and the Christian will contemplate with delight the high priest of science quitting the study of the material universe —the scene of his intellectual triumphs—to investigate, with humility and patience, the mysteries of his faith."

The history of the scientific discoveries of this eminent philosopher will form the subject of some of the following chapters.

## CHAPTER II.

**Newton engaged in forming lenses. His first experiments with the prism. Discovers the composition of White Light, and the different refrangibility of the rays which compose it. Abandons his attempts to improve Refracting Telescopes, and resolves upon the construction of Reflecting ones. Quits Cambridge on account of the Plague. Constructs the Reflecting Telescope, the first ever executed. One of them examined by the Royal Society and shown to the king. Constructs a Telescope with Glass Specula. Delivers a course of Optical Lectures. Elected a Fellow of the Royal Society. Communicates to them his discoveries. Popular account of them. They involve him in various controversies.**

The appointment of Isaac Newton to the Lucasian Chair, at Trinity College, Cambridge, appears to have been contemporary with some of his most important discoveries. The first of these, of which the date is properly authenticated, is that of the different refrangibility of the rays of light, which he satisfactorily established in 1666. The germ of the doctrine of universal gravitation seems to have presented itself to him in the same year, or at latest in the year following; and in the year 1666 or before, as he expresses himself in a letter to the Abbe Conti, he was in possession of his method of fluxions, and he had brought it to such a state in the beginning of the year 1669, that he permitted his friend, Dr. Barrow, to communicate it to Mr. Collins on the 20th of June in that year.

Although we have already mentioned, on the authority of accounts kept by Newton himself, that he had purchased a prism, in 1664, at Cambridge, yet he does not appear to have made any use of it, as he informs us that it was in 1666 that he "purchased a triangular glass prism to try therewith the celebrated phenomena of colours." During the same year he had applied himself to the grinding of "optic glasses, of other figures than spherical," and having, no doubt, experienced the impractibility of executing such lenses, the idea of examining the phenomena of colour was one of those sagacious and fortunate impulses which more than once led him to discovery. Descartes in his *Dioptrice*, published in 1629, and more recently James Gregory in his *Optica Promota* published in 1663, had shown that parallel and diverging rays could be reflected, with mathematical accuracy, to a point or focus, by giving the surface a parabolic, an elliptical, or a hyperbolic form, or some other form not spherical. Descartes had even invented and described machines by which lenses of these shapes could be ground and polished, and the perfection of the refracting telescope was supposed to depend on the degree of accuracy with which they could be executed.

In endeavouring to grind glasses that were not spherical, Newton seems to conjecture that the defects of lenses, and consequently of refracting telescopes, might arise from some other cause than the imperfect conveyancy of rays to a simple point, and this conjecture was happily realised in those fine discoveries of which we shall endeavour to give some account.

When Newton commenced making this inquiry, philosophers of the highest genius were directing all the energies of their mind to the subject of light, and to the improvement of the refracting telescope. Mr. Gregory, of Aberdeen, had invented his reflecting telescope. Descartes had explained his theory, and exerted himself in perfecting the construction of the common refracting telescope, and Huygens had not only executed the magnificent instruments, by which he discovered the ring and the satellites of Saturn, but had begun those splendid researches respecting the nature of light, and the phenomena of double refraction which have led his successors to such brilliant discoveries.

Newton, therefore, came forward when the science of light was prepared for some great accession, and at the very time when he was required to propagate the impulse which it received from his illustrious predecessors.

The ignorance which at that time prevailed respecting the nature and origin of colours, is sufficiently apparent from the account we have previously given of Dr. Barrow's speculations on this subject, as published in his lectures, which were revised by Newton. It was then the general supposition that light of every colour was equally refracted or bent out of its direction, when it passed through any lens or prism, or other refracting medium; and though the exhibition of colours by the prism had frequently been made before the time of Newton, yet no philosopher appears to have attempted to analyse the phenomena.

Newton was well aware that when a ray of light proceeds through the same medium—say the air, it moves in a straight line, and if admitted through a small hole into a dark room, appears white; but if it passes obliquely from a rarer into a denser medium, as from air into water, it is then urged out of the straight line, and appears as if it had been suddenly bent. For this reason a straight rod or stick, when immersed in water, appears to be broken at the surface of the water; and the portion immersed seems to be bent upwards. The light thus proceeding out of its straight course is said to be reflected, and our observations on objects placed in water, are liable from this circumstance, to considerable deception. For example, a deep-bodied fish seen near the surface of the water, appears a flat-fish; a round body there seems oval, and objects seen at the bottom do not appear to be so deep as they really are. Now we are to bear in mind that light in passing into our atmosphere, has moved from a rarer into a denser medium—that is from the thin ether above the highest stratum of air into the denser mass of air which more immediately surrounds the earth. This air is generally loaded with watery vapour, so that the medium through which light has to pass in our atmosphere, undergoes many remarkable changes. Hence the distance, height, and relative position of the mountains, hills, valleys, towns, often appear altered. A certain range of mountains in one condition of the atmosphere, will appear nearer; in another, more remote from the spectator than usual. But in being thus bent out of its course, a ray of light exhibits a variety of beautiful colours, the cause of which it fell to the lot of our illustrious professor to discover.

We have already observed that if a ray of light be admitted through a small hole into a dark room, it appears of uniform whiteness; but Newton discovered that this white ray of light is a combination of seven differently coloured rays, which may be separated easily from each other. He proceeded in his experiment thus;—he made a hole in one of his window shutters, and having darkened his chamber, let in a convenient quantity of the sun's light. He then intercepted this light with his triangular glass prism, and found that in passing through the glass, the light was so refracted, as to exhibit on the wall an image of seven different colours, viz:—violet, indigo, blue, green, yellow, orange, and red; the image was about five times as long as it was broad. It was at first, says our author, "a very pleasing divertizement to view the vivid and intense colours presented thereby;" but this pleasure was immediately succeeded by surprise at various circumstances which he had not expected. According to the received laws of refraction, he expected the image from the prism to be circular, like the white image which the sunbeam had formed on the wall, previous to the interposition of the prism; but when he found it to be no less than five times longer than it was broad, it "excited in him more than ordinary curiosity to examine from whence it might proceed. He could scarcely think that the various thickness of the glass, or the termination with shadow or darkness, could have any influence on light to produce such an effect; yet he thought it not amiss first to examine those circumstances, and so find what would happen by transmitting through parts of the glass of divers thicknesses, or through holes in the window shutter of divers bignesses, or by setting the prism without, (the window), so that the light might pass through it, and be refracted before it was terminated by the hole; but he found none of these circumstances natural. The fashion of the colours was, in all these cases the same."

Newton next suspected that some unevenness in the glass or other accidental irregularity, might cause the extension of the colours. In order to try this, he took another prism, and placed it in such a manner that the light passing through them both might be refracted contrary ways, and thus returned by the second prism into that course from which the first had diverted it, for by this means he thought the regular effects of the first prism would be destroyed by the second, and the irregular effects more augmented by the multiplicity of refractions. The result was, that the light which was diffused by the first prism into an oblong form, was reduced by the second prism into a circular one, with as much regularity as when it did not pass through them at all; so that whatever was the cause of the length of the image, it did not arise from any irregularity in the prism.

Our philosopher next proceeded to examine more critically what might be effected by the difference of the incidence of the rays proceeding from different parts of the sun's disc; but by taking accurate measures of the lines and angles, he found that the angle of the emergent rays should be 31 min. equal to the sun's diameter, whereas the real angle subtended at the hole in the shutter was 2 deg. 49 min. But as this computation was founded on the hypothesis, that the sine of the angle of incidence was proportional to the sine of the angle of refraction, which, from his own experience, he could not imagine to be so erroneous as to make that angle but 31 min., which was in reality 2 deg. 49 min.; yet "his curiosity caused him again to take up his prism," and having turned it round in both directions, so as to make the rays fall both with greater and with less obliquity upon the face, he found that the colours on the wall did not sensibly change their place; and hence he obtained a decided proof that they could not be occasioned by a difference in the incidence of the light radiating from different parts of the sun's disc.

Sir Isaac then began to suspect that the rays after passing through the prism might move in curved lines, and, in proportion to the different degrees of curvature, might tend to different parts of the wall; and this suspicion was strengthened by the recollection that he had often seen a tennis-ball struck with an oblique racket describe such a curved line. In this case a circular and a progressive motion is communicated to the ball by the stroke, and in consequence of this, the direction of its motion was curvilineal, so that if the rays of light were globular bodies, they might acquire a circular motion by their oblique passage out of one medium into another, and thus move like the tennis-ball in a curve line. Notwithstanding "this plausible ground of suspicion," as he terms it, he could discover no such curvature in their direction, and, what was enough for his purpose, he observed that the difference between the length of the image and the diameter of the hole was proportional to their distance, which could not have happened had the rays moved in curvilineal paths.

These different hypotheses, or suspicions, being thus gradually removed, he was at length led to an experiment which determined beyond a doubt the true cause of the elongation of the coloured figure. Having taken a board with a small hole in it, he placed it behind the face of the prism, and close to it, so that he could transmit through the hole any one of the colours in the figure, and keep back all the rest. When the hole, for example, was near the corner of the prism, no other light but the red fell upon the wall. He then placed behind this another board with a hole in it, and behind this board he placed another prism, so as to receive the red light which passed through this hole in the second board. He then turned round the first prism, so as to make all the colours in succession pass through these two holes, and he marked their places on the wall. From the variation of these places, he saw that the red rays were less refracted by the second prism than the orange rays, the orange less than the yellow, and so on, the violet being more refracted than all the rest.

Hence he drew the general conclusion, "that light was not homogeneous, but consisted of rays, some of which were more refrangible than others."

This discovery revealed at once the explanation of many of the most interesting appearances in nature. Nothing can be more beautiful than the colours with which the tops of mountains, the surface of the ocean, and the different-shaped clouds are tinged at sunrise and at sunset, the cause of which now became obvious. The white rays of the sun, entering into an atmosphere of varying density, pass through it like the sunbeams through Sir Isaac's glass prism, and are in a similar way decomposed. The rays that are the least refracted, or diverted, out of their course, reach the earth in all the purity and beauty of their own individual colours, and enter into a thousand varied combinations. When the sun is shining in all its noon of splendour high above the horizon, its rays, falling on the wide expanse of the ocean, are reflected back unchanged in all their original silvery lustre; but when the sun is setting, and its rays fall obliquely on the waters, the red being the least refrangible of the primary rays, floods with a fiery glow the heaving billows and the line of the visible horizon. Around the heavenly bodies—the sun and the moon—circles of light of the most varying colours often appear, which may be explained on the same principle, these halos being nothing more than the rays of light reflected and refracted by the globules of vapour through which they pass before they reach the sphere of our vision. But the most familiar instance of the decomposition of light into its prismatic colours is exhibited to us by the rainbow, which is occasioned by the light of the sun shining on the spherical drops of water falling in an opposite shower. In this instance the rays of white light are refracted or resolved into their primary rays, which are reflected in the form of an arch across the heavens.

As soon as Sir Isaac had established this important truth, he saw that a lens which refracts light exactly like a prism, must also refract the different-coloured rays with different degrees of force, bringing the violet rays to a focus nearer the glass than the red rays, and so on. As soon as he perceived this result of his discovery, he abandoned all attempts to improve the refracting telescope, and took into his consideration the principle of reflexion; and, as he found that rays of colours were reflected regularly, so that the angle of reflexion was equal to the angle of incidence, he concluded that, upon this principle, "optical instruments might be brought to any degree of perfection imaginable," provided that a reflecting substance could be found which could polish as finely as glass, and reflect as much light as glass transmits, and provided a method of communicating to it a parabolic figure could be obtained. These difficulties appeared to him very great, and he even thought them insuperable, when he considered that, as any irregularity in a reflecting surface makes the rays deviate five or six times more from their true path than similar irregularities in a refracting surface, a much greater degree of nicety would be required in figuring reflecting specula than refracting lenses.

Such was the progress of Newton's optical discoveries, when the students of the University of Cambridge were suddenly dispersed by the breaking out of the plague, in 1666, which then desolated England. Newton retired for safety to his paternal estate; and though he lost for a time the advantages of public libraries and literary conversation, he rendered the two years of his retreat a memorable era in his own existence and in the history of science, by another of his great discoveries, that of the theory of gravitation, or the tendency of bodies towards the centre of the globe. His time was never idle; experiments, conclusions, and reflections, occupied it continually. He saw an apple fall from a tree, and immediately began to consider the general laws which must regulate all falling bodies. At that time a degree had never been actually measured upon the face of the earth; his first attempts to account for the wonders of the whole solar system, by the principle of gravitation alone, were, therefore, imperfect, from the want of sufficient data; but these subjects he afterwards resumed.

On his return to Cambridge, in 1668, he resumed the subject of a reflecting telescope, and having thought of a delicate method of polishing proper for metals, by which, as he conceived, "the figure would be corrected to the last," he began to put this method to the test of experiment. At this time he was acquaint with the proposal of Mr. James Gregory, contained in his "Optima Promata," to construct a reflecting telescope with two concave specula, the largest of which had a hole in the middle of the larger speculum to transmit the light to an eye-glass; but he conceived it would be an improvement on this instrument to place the eye-glass at the side of the tube, and to reflect the rays to it by an oval plane speculum. M. Biot, in his Life of Newton, has erroneously stated that Newton was preceded in the invention of the reflective telescope by Gregory, *but probably without knowing it.* It is quite certain, that Newton was perfectly well acquainted with Mr. Gregory's invention, as appears from the following avowal of it, in a letter to Oldenburg. "When I first applied myself to try the effects of reflection, Mr. Gregory's 'Optima Promata,' (printed in the year 1663,) having fallen into my hands, where there is an instrument described with a hole in the midst of the object-glass to transmit the light to an eye-glass placed behind it, I had thence an occasion of considering that sort of construction, and found their disadvantages so great, that I saw it necessary, before I attempted anything in the practice, to alter the design of them, and place the eye-glass at the side of the tube, rather than at the middle."

One of these instruments he actually constructed with his own hands; and he gave an account of it in a letter to a friend, dated February 23rd, 1668-9, a letter which is also remarkable for containing the first allusion to his discoveries respecting colours. Previous to this, he was in correspondence with Mr. Ent, (afterwards Sir George Ent,) one of the original council of the Royal Society, an eminent medical writer of his day, and President of the College of Physicians. In a letter to Mr. Ent, he had promised an account of his telescope to their mutual friend, and the letter to which we now allude contained the fulfilment of that promise. The telescope was six inches long. It bore an aperture in the large speculum something more than an inch, and as the eye-glass was a plano-convex lens, whose focal length was one-sixth or one-seventh of an inch, it magnified about forty times, which, as

Newton remarks, was more than any six foot tube (meaning refracting telescopes) could do with distinctness. On account of the badness of the materials, however, and the want of a good polish, it represented objects less distinct than a six feet tube, though he still thought it would be equal to a three or four feet tube directed to common objects. He had seen through it Jupiter distinctly, with his four satellites, and also the horns or moon-like phases of Venus, though, he confessed, this last phenomenon required particular niceness in adjusting the instrument.

Although Newton considered this little instrument as in itself contemptible, yet he regarded it as an "epitome of what might be done" and he expressed his thorough conviction that a six feet telescope might be made after this method, which would perform as well as a sixty or a hundred feet telescope made in the common way; and that if a common refracting telescope could be made of the "purest glass exquisitely polished, with the best figure that any geometrician (Descartes, &c.) hath or can design, it would scarcely perform better than a common telescope. This," he adds, "may seem a paradoxical assertion, yet" he continues, "it is the necessary consequence of some experiments which I have made concerning the nature of light."

This telescope, which we have been describing possesses a very peculiar interest, as being the first reflecting telescope which was ever executed and directed to the heavens. Mr. James Gregory had, indeed, attempted to construct his instrument some years previous. Messrs. Rivers and Cox, who were celebrated glass-grinders of that time, were employed by him, to execute a concave speculum of six feet radius, and likewise a small one; but as they had failed in polishing the large one, and as Mr. Gregory was on the eve of going abroad, he gave himself no further trouble about the experiment, and the tube of the telescope was never made. Some time afterwards, it is true, he "made some trials both with a little concave and convex speculum, but possessed with the fancy of the defective figure, he would not be at the pains to fix every thing in its due distance."

Such were the earliest attempts to construct the reflecting telescope, that valuable instrument which has since been the means of effecting such splendid discoveries in astronomy. When we look back from the present advanced state of practical science, how great is the contrast between the loose specula of Gregory, and the fine Gregorian telescopes of Hadley, Short, and Veitch—between the diminutive six inch tube of Sir Isaac Newton and the forty-feet instrument of Herschel, and the huge telescope of Ramage.

The success of his first experiment inspired Newton with fresh zeal, and though his mind was now occupied with his optical discoveries, with the elements of his methods of fluxions, and with the expanding germ of his theory of universal gravitation, yet with all the ardour of youth he applied himself to the laborious operation of constructing another reflecting telescope with his own hands. This instrument which was considerably better than the first, though it lay by him several years, excited great interest at Cambridge; and Sir Isaac himself informs us, that one of the fellows of Trinity College, had completed a telescope of the same kind, which he considered as somewhat superior to his own. The existence of these telescopes having become known to the Royal Society, as well as the history of their invention, Newton received a letter from the secretary of the institution, earnestly requesting him to send his instrument for examination to that learned body. In accordance with this request he transmitted it to Mr. Oldenburg in December 1671, and from this epoch the name of Newton began to acquire that celebrity by which it has continued ever since to be so peculiarly distinguished.

On the 11th of January, 1671-2, it was announced to the Royal Society, that Newton's reflecting telescope had been shown to the king, and had been examined by the president Sir Robert Moray, Sir Paul Neale, Sir Christopher Wren, and Mr. Hook. These scientific gentlemen entertained so high an opinion of the telescope, that, in order to secure the honour of the invention to its author, they advised him to forward a drawing and particular description of it to Mr. Huygens at Paris. Mr. Oldenburgh according drew up a description of the instrument in Latin, which, after being revised and corrected by Mr. Newton, was transmitted to that celebrated philosopher. This telescope is still carefully preserved in the library of the Royal Society of London, with the following inscription:—

"INVENTED BY SIR ISAAC NEWTON, AND MADE WITH HIS OWN HANDS, 1671."

We have no evidence that Newton executed any other reflecting telescopes than the two we have mentioned. He informs us, however, that he repolished and greatly improved a fourteen feet object-glass, executed by a London artist of celebrity, and having, in 1678, proposed to substitute glass reflectors in place of metallic specula, he tried to make a reflecting telescope on this principle four feet long, and with a magnifying power of 150. The glass was wrought by a London artist, and though it appeared to be well finished, yet, when it was quicksilvered on its convex side, it exhibited all over the glass innumerable inequalities, which gave an indistinctness to every object. He expresses his firm conviction, however, that nothing but good workmanship is wanting to perfect these telescopes, and he recommends their careful consideration "to the curious in figuring glasses."

Strange as it may appear, a period of fifty years was allowed to elapse before this recommendation excited any notice. About that time, however, Mr. James Short, of Edinburgh, an artist of consummate skill, constructed, in the year 1730, no fewer than six reflecting telescopes with glass specula; three of these were fifteen inches, and three were nine inches in focal length. He found it extremely troublesome to give them a true figure with parallel surfaces; and several of them when finished turned out useless, in consequence of the veins which appeared in the glass. Although these instruments performed perfectly well, yet the light was fainter than he expected, and from this cause, combined with the difficulty of finishing them, he afterwards devoted his labours solely to those with metallic specula.

At a later period, in 1822, Mr. G. B. Airy of Trinity College, Cambridge, and one of the distinguished successors in the Lucasian chair of Mathematics in that university, resumed the consideration of glass specula, and clearly demonstrated that the observation both of figure and colour might be corrected in these instruments. Upon this ingenious principle, Professor Airy executed more than one telescope, but though the result of the experiment was such as to excite hopes of ultimate success, yet the construction of such instruments is still a desideratum in practical science.

Such were the attempts which Sir Isaac Newton made to construct reflecting telescopes; but notwithstanding the success of his labours, neither the philosopher nor the practical optician seems to have been possessed of that patient and accurate investigation necessary to pursue them. A London artist, of considerable skill, undertook to imitate these instruments; but Sir Isaac informs us that "he fell much short of what he had attained, as he afterwards ascertained by discoursing with the under-workmen he had employed." After a long period of fifty years, John Hadley, Esq., of Essex, a Fellow of the Royal Society, began, in 1719 or 1720, to execute a reflecting telescope. The scientific knowledge of this gentleman, and his manual dexterity, fitted him admirably for the task; and—probably after many failures—he constructed two large telescopes, about five feet three inches long—one of which, with a speculum six inches in diameter, was presented to the Royal Society, in 1723. The celebrated Dr. Bradley and the Rev. Mr. Pound compared this telescope with the great Huygenian refractor, 123 feet long. It bore as high a magnifying power as the Huygenian telescope: it showed objects equally distinct, though not altogether so clear and bright: and it exhibited every celestial object that had been discovered by Huygens—viz., the five satellites of Saturn; the shadow of Jupiter's satellites on his disc, the black list in Saturn's ring, and the edge of his shadow cast on the ring. Encouraged and instructed by Mr. Hadley, Dr. Bradley began the construction of reflecting telescopes, and succeeded so well, that he would have completed one of them, had he not been under the necessity of changing his residence. Some time afterwards, he and the Honourable Samuel] Molyneux undertook the task together, at Kew, and attempted to execute specula about twenty-six inches in focal length; but notwithstanding the Doctor's former experience, and Mr. Hadley's frequent instructions, it was a long time before they succeeded in their undertaking. The first good instrument which they completed was in May, 1724. It was twenty-six inches in focal length; but they afterwards constructed a very large one of eight feet, the largest that had ever been made. The first of these instruments was afterwards elegantly fitted up, and presented by Mr. Molyneux, to his Majesty, John V., King of Portugal, a zealous patron of all the sciences.

The great object of these two able astronomers appears to have been to reduce the method of making specula to such a degree of certainty, that they could be manufactured for public sale. Mr. Hauksbee had indeed made a good one, about three and a half feet long, and proceeded to the execution of two others—one of six feet, and another of twelve feet in focal length; but Mr. Scarlet and Mr. Hearne, having received all the information which Mr. Molyneux had acquired, constructed them for public sale; and the reflecting telescope has ever since continued to be an article of trade with every regular optician.

"As Sir Isaac Newton," observes his modern biographer, "was at this time President of the Royal Society, he had the high satisfaction of seeing his own invention become an instrument of public use, and of great advantage to science; and he no doubt felt the full influence of this trial of his skill. Still, however, the reflecting telescope had not achieved any new discovery in the heavens. The latest accession to astronomy had been made by the ordinary refractors of Huygens, labouring under all the imperfections of coloured light; and this long pause in astronomical discovery seemed to indicate that man had carried to its furthest limits his power of penetrating into the depths of the universe. This, however, was only one of those stationary positions from which human genius takes a new and loftier elevation. While the English opticians were thus practising the recent art of grinding specula, Mr. James Short, of Edinburgh, was devoting to the subject all the energies of his youthful mind. In 1732, and in the 22nd year of his age, he began his labours; and he carried to such high perfection the art of grinding and polishing specula, and of giving them the true parabolic figure, that, with a telescope fifteen inches in focal length, he read in the Philosophical Transactions, at the distance of 500 feet, and frequently saw the five satellites of Saturn together—a power which was even beyond the reach of Hadley's six-foot instrument. The celebrated Maclaurin compared the telescopes of Short with those made by the best London artists; and so great was their superiority, that his small telescopes were invariably superior to larger ones from London. In 1742, after he had settled as an optician in the metropolis, he executed for Lord Thomas Spencer a reflecting telescope, twelve feet in focal length, for £630; in 1752, he completed one for the King of Spain, at the expense of £1200; and a short time before his death, which took place in 1768, he finished the specula of the large telescope which was mounted equatorially for the observatory of Edinburgh, by his brother, Thomas Short, who was offered twelve hundred guineas for it by the King of Denmark.

"Although the superiority of these instruments, which were all of the Gregorian form, demonstrated the value of the reflecting telescope, yet no skilful hand had yet directed it to the heavens; and it was reserved for Dr. Herschel to employ it as an instrument of discovery—to exhibit to the eyes of men new worlds, and new systems; and to bring within the grasp of his reason those remote regions of space to which his imagination even had scarcely ventured to extend its power. So early as 1774, he completed a *five*-foot Newtonian reflector; and he afterwards executed no fewer than two hundred seven-feet, one hundred and fifty ten-feet, and eighty twenty-feet specula. In 1781, he began a reflector thirty feet long, and having a speculum thirty-six inches in diameter; and under the magnificent patronage of George III. he completed, in 1789, his gigantic instrument, forty feet long, with a speculum forty-nine and a half inches in diameter. The genius and perseverance which created instruments of such transcendant magnitude were not likely to terminate with their construction. In the examination of the starry heavens, the ultimate object of his labours, Dr. Herschel exhibited the same exalted qualifications; and in a few years he rose from the level of humble life to the enjoyment of a name more glorious than that of the sages and warriors of ancient times, and as immortal as the objects with which it will be for ever associated. Nor was it in the ardour of the spring of life that these triumphs of reason were achieved. Dr. Herschel had reached the middle of his course before his career of discovery began; and it was in the autumn and winter of his days that he reaped the full laurels of his glory. The discovery of a new planet at the verge of the solar system was the first trophy of his skill; and new double and multiple stars, and new nebulæ and groups of celestial bodies, were added in thousands to the system of the universe. The spring-tide of knowledge which was thus let in upon the human mind, continued for a while to spread its waves over Europe; but when it sank to its ebb in England, there was no other bark left upon the strand than that of the Deucalion of Science, whose home had been so long upon the waters.

"During the life of Dr. Herschel, and during the reign, and within the dominions of his royal patron, four new planets were added to the solar system, but they were detected by telescopes of ordinary power; and, we venture to state, that since the reign of George III. no attempt has been made to keep up the continuity of Dr. Herschel's discoveries.

"Mr. Herschel, his distinguished son, has indeed completed more than one telescope of considerable size;—Mr. Ramage, of Aberdeen, has executed reflectors rivalling almost those of Slough;—and Lord Oxmanstown, an Irish nobleman of high promise, is now engaged on an instrument of great size. But what avails the enthusiasm and the efforts of individual minds in the intellectual rivalry of nations? When the proud science pines in obscurity, blighted by the absence of royal favour, and of the nation's sympathy;—when its chivalry fall unwept and unhonoured;—how can it sustain the conflict against the honoured and marshalled genius of foreign lands?"

Newton's great discoveries in the science of optics formed the principal subjects of his lectures in the University of Cambridge during the years 1669, 1670, and 1671, and his new theory of light and colours was explained, with a clearness arising from perfect knowledge, to the satisfaction of a crowded and admiring audience; yet it is a singular circumstance that these discoveries should not have become public through the conversation or correspondence of his pupils. The Royal Society had acquired no knowledge of them till the beginning of 1672, and his reputation in that body was founded chiefly on his reflecting telescope. On the 23rd of December, 1671, the celebrated Dr. Seth Ward, Lord Bishop of Sarum, who was the author of several able works on astronomy, and had filled the astronomical chair at Oxford, proposed Mr. Newton as a Fellow of the Royal Society. The satisfaction which he derived from this circumstance appears to have been considerable; and in a letter to Mr. Oldenburg, of the 6th of January, he says, "I am very sensible of the honour done me by the Bishop of Sarum in proposing me a candidate; and which, I hope, will be further conferred upon me by my election; and if so, I shall endeavour to testify my gratitude, by communicating what my poor and solitary endeavours can effect towards the promoting your philosophical designs." His election accordingly took place on the 11th of January, the same day on which the Society agreed to transmit a drawing and description of his reflecting telescope to Mr. Huygens at Paris. The notice of his election, and the thanks of the Society for the communication of his telescope, were conveyed in the same letter, with an assurance that the society "would take care that all right should be done him in the matter of this invention." In his next letter to Mr. Oldenburg, written on the 18th of January, he announces his optical discoveries in the following remarkable manner :—"I desire that in your next letter you would inform me for what time the society continue their weekly meetings; because if they continue them for any time, I am purposing them, to be considered of and examined, an account of a philosophical discovery which induced me to the making of the said telescope; and I doubt not but will prove much more grateful than the commnnication of that instrument; being in my judgment the oddest, if not the most considerable, detection which hath hitherto been made in the operations of nature."

This "considerable detection" was the discovery of the different refrangibility of the rays of light which we have already explained, and which led to the construction of his reflecting telescope. It was communicated to the Royal Society in a letter to Mr. Oldenburg, dated February 6th, and excited, as might have been expected, the greatest interest among the members of that distingished body. The "solemn thanks" of the meeting were ordered to be transmitted to its author for his "very ingenious discourse." A strong desire was expressed to have it immediately printed, both for the purpose of having it well examined and considered by philosophers, and for "securing the considerable notices thereof to the author against the arrogation of others," and Dr. Seth Ward, Bishop of Sarum, Mr. Boyle, and Dr. Hooke, were desired by the Society to peruse and consider it, and to bring in a report upon it.

It is reputed that Isaac Newton was at this period so poor that he was compelled to apply for a dispensation from the usual payment of one shilling weekly, which is contributed by each member towards the expenses of the Royal Society. He had no income but what he derived from his college and his professorship, the produce of his small estate being absorbed in supporting his mother and her family. His personal wishes, however, were so moderate, that he never could regret the want of money, except in as much as it limited his purchases of books and scientifical instruments, and restricted his power of relieving the distresses of others.

The kindness of the Royal Society in admitting him as a member, and the anxiety which they had already evinced for his reputation, excited on the part of Newton a corresponding feeling, and he gladly accepted of their proposal to publish his discourse in the monthly numbers in which the Tansactions of the Society were then given to the world. In a letter to Mr. Oldenburg, dated February 10th, he says, "It was an esteem of the Royal Society for most candid and able judges in philosophical matters, encouraged me to present them with that discourse of light and colours, which since they have so favourably accepted of, I do earnestly desire you to return them my cordial thanks. I before thought it a great honour to be made a member of that honourable body; but I am now more sensible of the advantages; for, believe me, sir, I do not only esteem it a duty to concur with you in the promotion of real knowledge; but a great privilege, that, instead of exposing discourses to a prejudicial and common multitude, (by which means many truths have been baffled and lost;) I may, with freedom, apply myself to so judicious and impartial an assembly. As to the printing of that letter, I am satisfied in their judgment, or else I should have thought it too straight and narrow for public view. I designed it only to to those that know how to improve upon hints of things; and, therefore, to spare tediousness, omitted many such remarks and experiments as might be collected by considering the assigned laws of refractions; some of which, I believe, with the generality of men, would yet be almost as taking as any I have described. But yet, since the Royal Society have thought it fit to appear publicly, I leave it to their pleasure: and, perhaps, to supply the aforesaid defects, I may send you some more of the experiments to second it (if it be so thought fit) in the ensuing Transactions."

In following the order which Newton himself adopted, we have endeavoured to give an account of the leading doctrine of the different refrangibility of light, and of the various attempts to improve the reflecting telescope which that discovery suggested. We shall now, therefore, further endeavour to make the reader acquainted with the discoveries respecting colours, which he at this time communicated to the Royal Society of London.

Having determined, by experiments which we have already described, that a beam of white light, as emitted from the sun, consisted of seven different colours, which possess different degrees of refrangibility; he measured the relative extent of the coloured spaces on the wall, and found them to have the proportions according to their different degrees of refrangibility; the prismatic spectrum is nothing more than an elongated image of the sun produced by the rays of the sun being separated in different degrees from their original direction, the red being refracted least, and the violet most powerfully.

If we consider light as consisting of minute particles of matter, we may form some notion of its decomposition by the prism from the following popular illustration. "If we take steel filings of seven different degrees of fineness and mix them together, there are two ways in which we may conceive the mass to be decomposed, or, what is the same thing, all the different kinds of filings separated from each other. By means of seven sieves of different degrees of fineness, and so made that the finest will just transmit the finest powder and detain all the rest, while the next in fineness transmits the two finest powders and detains all the rest, and so on, it is obvious that all the powders may be completely separated from each other. If we again mix all the steel filings, and laying them upon a table, hold high above them a flat bar magnet, so that more of the filings are attracted, then if we bring the magnet nearer and nearer, we shall come to a point where the finest filings are drawn up to it. These being removed, and the magnet brought nearer still, the next finest powder will be attracted, and so on till we have thus drawn out of the mass all the powders in a separate state. We may conceive the bar magnet to be inclined to the surface of the steel filings, and so moved over the mass, that at the end nearest to them the heaviest or coarsest will be attracted, and all the remotest and the finest or lighter filings, while the rest are attracted to intermediate points, so that the seven different filings are not only separated but are found adhering in separate patches to the surface of the flat magnet. The first of these methods with the sieves may represent the process of decomposing light by which certain rays of white light are absorbed, or stifled, or stopped in passing through bodies, while certain rays are transmitted. The second method may represent the process of decomposing light by refraction, or by the attraction of certain rays further from their original direction than other rays, and the different patches of filings upon the flat magnet may represent the spaces on the spectrum.

When a beam of light is decomposed into the seven different colours of the spectrum, every particular colour, when once separated from the rest, is not susceptible of any change, or farther decomposition, whether it is refracted through prisms or reflected from mirrors. It may become fainter or brighter, but Newton never could, by any process, alter its colour or its refrangibility.

Among the various bodies which act upon light, it is conceivable that there might have been some which acted least upon the violet rays and most upon the red rays. Newton found, however, that this never took place; but that the same degree of refrangibility always belonged to the same colour to the same degree of refrangibility.

Having thus determined that the seven different colours of the spectrum were original or simple, he was led to the conclusion that whiteness, or white light, is a compound of all the seven colours of the spectrum, in the proportions in which they are represented on the wall. In order to prove this, or what is called the recomposition of white light out of the seven colours, he employed three different methods.

When the beam was separated into its elementary colours by the prism, he received the colours upon another prism, held either close to the first or a little behind it, and by the opposite refraction of this prism they were all refracted back into a beam of white light, which formed a white circular image on the wall, similar to what took place before any of the prisms were placed in its way.

The other method of recomposing white light consisted in making the spectrum fall upon a lens at some distance from it. When a sheet of white paper was held behind the lens, and removed to a proper distance, the colours were all refracted into a circular spot, and so blended as to reproduce light so perfectly white as not to differ sensibly from the direct light of the sun.

The last method he adopted of recomposing white light was one more suited to common apprehensions. It consisted in attempting to compound a white by mixing the coloured powders used by painters. He was aware that such colours, from their very nature, could not compose a pure white; but even this imperfection in the experiment he removed by an ingenious device. He accordingly mixed one part of red lead, four parts of blue bise, and a proper proportion of orpiment and verdigris. This mixture was dun, like wood newly cut, or like the human skin. He now took one-third of the mixture and rubbed it thickly on the floor of his room, when the sun shone upon it through the opened casement, and beside it, in the shadow, he laid a piece of white paper of the same size. "Then going from them" he says, "to the distance of twelve or eighteen feet, so that I could not discern the unevenness of the surface of the powder, nor the little shadows let fall from the gritty particles thereof; the powder appeared intensely white, so as to transcend even the paper itself in whiteness." By adjusting the relative illumination of the powders and the paper, he was able to make them both appear of the very same degree of whiteness. "For," says he, "when I was trying this, a friend coming to visit me, I stopped him at the door, and before I told him what the colours were, or what I was doing, I asked him which of the two whites were the best, and wherein they differed? And after he had, at that distance, viewed them well, he answered that they were both good whites, and that he could not say which was best, nor wherein their colour differed." For this reason Newton inferred that perfect whiteness may be compounded of different colours.

As all various shades of colour which appear in the material world can be imitated by intercepting certain rays in the spectrum, and uniting all the rest, and as bodies always appear of the same colour as the light in which they are placed, he concluded, that the colours of natural bodies are not qualities inherent in the bodies themselves, but arise from the disposition of the particles of each body to stop or absorb certain rays, and thus to reflect more copiously the rays which are not thus absorbed.

No sooner were these discoveries given to the world than they were opposed with a degree of virulence and ignorance which have rarely been combined in scientific controversy. Unfortunately for Newton, the Royal Society at that time contained very few individuals of pre-eminent talents, capable of appreciating the truth of his valuable discoveries, and of protecting him against the malevolent shafts of his envious and ignorant assailants. This eminent body, while they held his labours in the highest esteem, were still of opinion that his discoveries were fair subjects of discussion, and their secretary accordingly communicated to him all the the papers which were written in opposition to his views. The first of these was by a Jesuit named Ignatius Pardies, Professor of Mathematics, at Clermont, who pretended that the elongation of the sun's image arose from the unequal incidence of the different rays on the first face of the prism, although Newton had demonstrated to a certainty in his own discourse that this was not the case. In April, 1672, Newton transmitted to Oldenburg a decisive reply to the animadversions of Pardies; but, unwilling to be vanquished, this disciple of Descartes took up a fresh position, and maintained that the elongation of the spectrum might be explained by the diffusion of light on the hypothesis of Grimaldi, or by the diffusion of undulations on the hypothesis of Hook. Newton again replied to these feeble reasonings; but contented himself with repeating his original experiments, and confirming them by more popular arguments, apparently better suited to the comprehension of Pardies, as the vanquished Jesuit wisely quitted the field.

Another combatant soon sprung up in the person of one Francis Linus, a physician in Liege, who, on the 6th of October, 1674, addressed a letter to a friend in London, containing animadversions on Newton's doctrine of colours. He boldly affirmed, that in a perfectly clear sky, the image of the sun made by a prism is never elongated, and that the spectrum observed by Newton was not formed by the true sunbeams, but by rays proceeding from some bright cloud. In support of these assertions, he appeals to frequently repeated experiments on the refractions and the reflexions of light which he had exhibited thirty years before to Sir Kenelm Digby, who took notes upon them; and he unblushingly states, that if Newton had used the same industry as he did, he would never have "taken so impossible a task in hand, as to explain the difference between the length and breadth of the spectrum by the received laws of refraction." When this letter was shown to Newton, he considered it below his notice, and refused to answer it; but a letter was sent to Linus referring him to the answer given by Mr. Newton to Pardies, and assuring him that the experiments on the spectrum were made at a time when there was no bright cloud in the heavens. This reply, however, did not satisfy the Dutch experimentalist, and he addressed another letter to his friend in London, on the 25th of February, 1675, in which he gravely attempted to prove that the experiment of Newton was not made in a clear day; that the prism was not close to the hole; and that the length of the speculum was not perpendicular or parallel to the length of the prism. Such assertions could not but irritate even the patient mind of Newton. He several times declined the earnest request of Mr. Oldenburg to answer these observations; he observes, that as the dispute referred to matters of fact, it could only be decided before competent witnesses, and he referred to the testimony of those who had seen his experiments. The entreaties of Oldenburg, however, prevailed over his own better judgment; and "lest Mr. Linus should make more stir," this illustrious philosopher was compelled to draw up a long and explanatory reply to reasonings which he considered utterly contemptible, and to assertions, altogether without foundation. This answer, dated November 13th, 1675, would scarcely have been perused by Linus, who died on the 15th of December, when his pupil Mr. Gascoigne took up the cause, and declared that Linus had shown to various persons in Liege his experiment, which proved the spectrum to be circular, and that Newton could not be more confident on his side than they were on the other. He admitted, however, that the different results might arise from different ways of placing the prism. Pleased with the "handsome genius of Mr. Gascoigne's letter," Newton replied to it, and suggested that the spectrum seen by Linus may have been the circular eye formed by one reflection, or, what he thought more probable, the circular one formed by two refractions, and one intervening reflexion from the base of the prism, which would be coloured if the prism was not an isosceles one. This suggestion appears to have enlightened the Dutch philosophers. Mr. Gascoigne having no conveniences for making the experiments pointed out by Newton, requested Mr. Lucas of Liege to perform them in his own house. This ingenious gentleman, whose letter gave evident satisfaction to Newton; and deserves the highest praise, confirmed the leading results of the English philosopher; but though the refractiug angle of his prism was 60 degrees, and the refractions equal, he never could obtain a spectrum whose length was more than from three to three and a half times its breadth, while Newton found the length to be five times its breadth. In Newton's reply he direets the attention of his correspondent principally to this point of difference. He repeated his measures with each of the three angles of three different prisms, and he affirmed that Mr. Lucas might "make sure to find the image as long or longer than he had yet done," by taking a prism with plain surfaces, and with an angle of 66 or 67 degress. He admitted that the smallness of the angle in Mr. Lucas's prism, viz, 60 degrees, did not account for the shortness of the spectrum which he obtained with it; and he observed in one of his own prisms that the length of the image was greater in proportion to the refracting angle than it should have been; an effect which he ascribes to its having a refractive power. Newton speaks with singular positiveness on this subject. "For I know," says he, "that Mr. Lucas's observations *cannot hold* where the refracting angle of the prism is full 60 degrees, and the day is clear, and the full length of the colours is measured, and the breadth of the image answers to the sun's diameter: and seeing I am well assured of the truth and exactness of my own observations, I shall be unwilling to be diverted by any other experiments from having a fair end made of this in the first place." There can be no doubt that the prism made use of by Lucas had actually a less dispersive power than that of Newton: and had the Dutch philosopher measured its refractive powers instead of guessing it, or had Newton been less confident than he really was, that all other prisms must give a spectrum of the same length as his in relation to its refracting angle and its index of refraction, the invention of the achromatic telescope would have been the necessary result. The objections of Lucas, however, forced our philosopher to make experiments which he had never before thought of—to measure accurately the length of the spectrum with different prisms of different angles and different refractive powers; and had Lucas maintained his position with greater obstinacy, he would have conferred a distinguished favour upon science, and would have rewarded Newton for all the vexation which had sprung from the disagreeable discussion of his optical experiments.

Such was the termination of Newton's disputes with the Dutch philosophers, and there is every reason to believe that it cost him more trouble to detect the origin of his adversaries' mistakes, than to establish the important truths which they had attempted to overturn.

Harrassing as such a controversy must have been to a philosopher like Newton, yet it did not touch those deep-seated feelings which characterize the noble and generous soul. No rival jealousy yet embittered the arguments of his opponents ;—no charges of plagiarism were yet directed against his personal character. These aggravations of scientific controversy, however, were yet in store for him; and the dispute which he was called to maintain with Hooke and Huygens was painfully increased by the personality and jealousy with which it was conducted.

Dr. Robert Hooke was about seven years older than Newton, observes the biographer of Newton, and was one of the ninety-eight original or unelected members of the Royal Society of London. He possessed great versatility of talent, yet, though his genius was of the most original cast, and his acquirements extensive, he had not devoted himself with fixed purpose to any particular branch of knowledge. His numerous and ingenious inventions, of which it is impossible to speak in terms of too much praise, gave to his studies a practical turn which unfitted him for that patient labour and investigation which physical researches so imperiously demand. The subjects of light, however, and of gravitation, appear to have deeply occupied his thoughts before Newton appeared in the field, and there can be but little doubt that he had made considerable progress in both of these inquiries. With a mind less divergent in its pursuits, and more endowed with patience of thought, he might successfully have unveiled the mysteries in which both these subjects were enveloped, and pre-occupied the intellectual throne which was destined for his rival; but the in-firm state of his health; the peevishness of temper which this occasioned; the number of unfinished inventions from which he looked both for fortune and fame; and, above all, his extravagant love of reputation, distracted and broke down the energies of his powerful intellect. In the more extensive inquiries of Newton, he often recognised his own incompleted speculations; and when he saw others reaping that glorious harvest for which he considered he had prepared the ground, and of which he had sown the seed, it was not easy for him to suppress the deep mortification which Newton's success inspired. In the history of science, as in all other history, it is a difficult matter to adjust the rival claims of competitors, when the one was allowed to have completed what the other had begun. He who commences an inquiry and proclaims the results to the world, often goes much further than he has announced, and, pushing his speculations into the very heart of the subject, frequently submits them to the ear of friendship. From the pedestal of his published labours his rival begins his researches, and brings them to a successful issue; while he has, in fact, done nothing more than completed and demonstrated the imperfect speculations of his predecessor. To the world, and to himself, he is in the position of the principal discoverer; but there is still some apology for his rival when he brings forward his unpublished labours; and some excuse for the exercise of personal feeling, when he measures the speed of his rival by his own proximity to the goal.

The conduct of Dr. Hooke would have been viewed with some such feeling, had not his arrogance on other occasions checked the natural current of our sympathy. When Newton presented his reflecting telescope to the Royal Society, Dr. Hooke not only criticised the instrument with undue severity, but announced that he possessed an infallible method of perfecting all kinds of optical instruments, so that "whatever almost hath been in notion or imagination, or desired in optics, may be performed with great facility and truth."

Hooke had been strongly impressed with the belief, that light consisted in the undulations of a highly elastic medium pervading all bodies; and, guided by his experimental investigation of the phenomena of diffraction, he had even announced the great *principle of interference*, which has performed such an important part in modern science. Regarding himself, therefore, as in possession of the true theory of light, he examined the discoveries of Newton in their relation to his own speculative views, and finding that their author was disposed to consider that element as consisting of material particles, he did not hesitate to reject doctrines which he believed to be incompatible with truth. Dr. Hooke was too accurate an observer, not to admit the general correctness of Newton's observations. He allowed the existence of different refractions, the unchangeableness of the simple colours, and the production of white light by the union of all the colours of the spectrum; but he confidently maintained that the different refractions arose from the splitting and rarefying of ethereal pulses, and that there are only two colours in nature, viz: *red* and *violet*, which produce by their mixture all the rest, and which are themselves formed by two sides of a split pulse or undulation.

In reply to these observations, Newton wrote an able letter to Mr. Oldenburg, dated June 11th, 1762, in which he examined with great boldness and force of argument, the various objections of Dr. Hooke, and maintained the truth of his doctrine of colours, as independent of the two hypotheses respecting the origin and production of light. He acknowledged his own partiality to the doctrine of the materiality of light; he pointed out the defects of the undulatory theory; he brought forward new experiments in confirmation of his former results; and he refuted the opinions of his opponent, respecting the existence of only two simple colours. No reply was made to the powerful arguments of Newton, which our limits prohibit us from giving in full, and Dr. Hooke contented himself with laying before the society, his curious and interesting observations on soap bubbles, and of plates of air, and in pursuing his experiments on the diffraction of light.

Mr. Newton had no sooner silenced this most powerful of his opponents, than he was again called into the field to defend his discoveries against a new enemy. Christian Huygens, an eminent mathematician and natural philosopher, who, like Dr. Hooke, had maintained the undulatory theory of light, transmitted to Mr. Oldenburg various animadversions on the Newtonian doctrine, but though his knowledge of optics was most extensive, yet his objections were nearly as groundless as his less-enlightened countryman. Attached to his own hypothesis respecting the nature of light, namely to the system of undulation, he seems like Hooke, to have regarded the discoveries of Newton, as calculated to overturn it, but his principal objections related to the composition of colours, and particularly of white light, which he alleged could be obtained from the union of two colours, YELLOW and BLUE. To this and similar objections Newton replied, that the colours in question were not simple yellows and blues, but were compound colours, in which, together, all the colours of the spectrum were themselves blended; and though he evinced some strong traces of feeling at being again called upon to answer such frivolous objections, yet his respect for Huygens induced him to enter with patience on a fresh development of his doctrine. Huygens felt the reproof which the tone of this answer so gently conveyed, and in writing to Oldenburg, he made use of the words, that Mr. Newton "maintained his doctrine with some concern." To this Newton replied, "As for Mr. Huygens' expression, I confess it was a little ungrateful to me, to meet with objections which had been answered before, without having the least reason given me why those were insufficient." But though Huygens appears in this controversy as a rash objector to the Newtonian doctrine, it was afterwards the fate of Newton to play a similar part against the Dutch philosopher, when he published his beautiful law of double refraction in Iceland spar, founded on the finest experimental analysis of the phenomena, though presented as a result of the undulatory system. Newton unhesitatingly rejected it, and substituted for it another law entirely inconsistent with the experiments of Huygens, and with those of all succeeding philosophers.

Even the calm temper of Newton was ruffled on account of these controversies; the satisfaction of humbling all his antagonists was not a sufficient compensation for the disturbance of his tranquillity. In his letter to Oldenburg, containing his first reply to Huygens, he says—"I intend to be no farther solicitous about matters of philosophy. And, therefore, I hope you will not take it ill if you find me never doing anything more in that kind; or rather, that you will favour me in my determination, by preventing, so far as you can conveniently, any objections or other philosophical letters that may concern me." In a subsequent letter to the same gentleman, he says—"I had some thoughts of writing a further discourse about colours, to be read at one of your assemblies; but find it yet against the grain to put pen to paper any more on that subject:" and in a letter to Leibnitz, dated December 9th, 1675, he observes—"I was so persecuted with discussions arising from the publication of my theory of light, that I blamed my own imprudence for parting with so substantial a blessing as my quiet to run after a shadow."

# CHAPTER III.

**Hall invents the Achromatic Telescope. Principles of it explained. Re-invented by Dolland, and Improved by future Artists. Blair's Achromatic Telescope. Mistakes in Newton's Analysis of the Spectrum. Structure of the Spectrum. Colours of thin Plates. Newton determines the law of their production. His Theory of fits of easy reflexion and transmission. Colours of thick Plates. Newton's Theory of the Colours of Natural Bodies explained.**

THERE is no fact in the history of science more singular than that Newton should have concluded that all bodies produced spectra of equal length, or separated the red and violet rays to equal distances when the refraction of the mean rays was the same. This opinion, unsupported by experiments, and not even sanctioned by any theoretical views, seems to have been impressed firmly upon his mind with all the force of a self-evident proposition. Even the shortness of the spectrum, observed by his opponent at Liege, did not rouse him to further inquiry; and when, under the influence of this blind conviction, he pronounced the improvement of the refracting telescope to be desperate, he checked for a long time the progress of this branch of science, and furnished to future philosophers a lesson which cannot be too deeply studied.

In 1729, about two years after the death of Sir Isaac Newton, an individual unknown to the scientific world broke the spell in which the subject of the spectrum had been so long and so singularly bound. Mr. Chester More Hall, of More Hall, in Essex, while studying the mechanism of the human eye, was led to suppose that telescopes might be improved by a combination of lenses of different refractive powers, and he actually completed several object glasses upon this principle. We are not informed of the steps by which he arrived at such a construction: but it is certain that he must have made a discovery which had escaped the sagacity of Newton—that prisms made of different kinds of glass produced different degrees of separation of the red and violet rays, or gave spectra of different lengths when the refraction of the middle ray of the spectrum was the same.

In order to explain how such a property led Mr. Hall to the construction of a *telescope without colour*, or an *achromatic* telescope, let us take a lens of crown or plate glass, whose focal length is about twelve inches. When the sun's rays fall upon it the red will be refracted more than the yellow, and the yellow than the violet. If we now place behind it a concave lens of the same glass and of the same focus or curvature, it will be found both by experiment, and by drawing the refracted rays, according to the rules given in elementary works, that the concave glass will refract the rays free of all colour; but as these rays will be parallel, the two lenses will not have a focus, and consequently cannot form an image so as to be used as the object glass of a telescope. This is obvious from another consideration; for since the curvatures of the convex and concave lenses are the same, the two put together will be exactly the same as if they were formed out of a single piece of glass, having parallel surfaces like a watch glass, so that the parallel rays of light will pass on in the same direction affected by equal and opposite refractions as in a piece of plane glass.

Now since the convex lens separated the white light into its component coloured rays, the extreme violet and the extreme red, it follows that a similar concave lens of the same glass is capable of uniting the violet and the red into white light. Consequently, if we take a concave lens of the same or of a greater refractive power than the convex one, and having the power of uniting rays farther separated than the violet and the red are, a less concavity in the other lens will be sufficient to unite the violet and red rays into a white ray; but as the one lens is now less concave than the other is convex, the concavity will predominate, and the uncoloured rays will no longer be parallel, but will converge to some point where they will form a colourless or achromatic image of the sun.

The effect now described may be obtained by making the convex lens of crown or of plate glass, and the concave lens of flint glass. If the concave lens has a greater refractive power than the convex, which is always the case, the only effect of it will be to make the rays converge to a focus more remote, or to render a less curvature necessary in the concave lens.

Such is the principle of the achromatic telescope invented by Mr. Hall. This ingenious gentleman employed working opticians to grind his lenses, and he furnished them with the radii of the surfaces, which were adjusted to correct the aberrations of figure as well as of colour. His invention, therefore, was not an accidental combination of a convex and a concave lens of different glass, which might have been made merely for experiment; it was a matter of deep study; and was a complete achromatic telescope, founded on a thorough knowledge of the different dispersive powers of crown and flint glass. It is a curious circumstance, however, in the history of the telescope, that this invention was actually lost. This gentleman never published any account of his labours, keeping them secret, no doubt, till he should be able to present his instrument in a more perfect form to the public; and it was till John Dolland had discovered the property of light upon which the instrument depends, and had actually constructed some very fine telescopes, that the previous labours of Mr.

Hall were made public. From this period the achromatic telescope underwent gradual improvement, and by the successive labours of several philosophers, it has become one of the most valuable instruments in physical science.

Although the achromatic telescope, as constructed by Dolland, was founded on the principle that the spectra formed by crown and flint glass differed only in their relative lengths, when the refraction of the mean ray was the same, yet by a more minute examination of the best instruments it was found that they exhibited white or luminous objects tinged on one side with a green fringe, and on the other with one of a claret colour. These colours, which did not arise from any defect of skill in the artist, were discovered to arise from a difference in the extent of the coloured spaces in two equal spectra formed by crown and flint glass. This property was called the *irrationality* of the coloured spaces, and the uncorrected colours which remain when the primary spectrum of the crown glass was corrected by the primary spectrum of the flint glass, were called the *secondary* or *residual spectrum*. By a happy contrivance, which our limits prohibit us from describing, the celebrated Dr. Blair succeeded in correcting this secondary spectrum, or in removing the green and claret coloured fringes which appeared in the best telescopes previously, and to this contrivance he gave the name of the Aplanatic Telescope.

But, while these remarkable properties of the prismatic spectrum, as formed by different bodies, were overlooked by Newton, he committed some considerable mistakes in his examination of the spectrum, which was under his own immediate examination. It does not appear to have occurred to him, that the relations of the coloured spaces must be greatly modified by the angular magnitude of the sun or the luminous body, or aperture from which the spectrum is obtained; and, misled by an apparent analogy between the length of the coloured spaces and the divisions of a musical chord, he adopted the latter, as representing the proportion of the coloured spaces in every beam of white light. "This result," says Newton in his Optics, "was obtained by an assistant whose eyes were more critical than mine, and who, by right lines drawn across the spectrum, noted the confines of the colours. And this operation being divers times repeated, both on the same and on several papers, I found that the observations agreed well enough with one another." Had two other observers, one situated in Mercury, and the other in Jupiter, studied the prismatic spectrum of the sun by the same instruments, and with the same sagacity as Newton, it is evident that they would have obtained very different results. On account of the apparent magnitude of the sun in Mercury, the observer there would obtain a spectrum entirely without green, having red, orange, and yellow at one end, the white in the middle, and terminated at the other end with blue and violet. The observer in Jupiter would, on the contrary, have obtained a spectrum in which the colours were much more condensed. On the planet Saturn, a spectrum exactly similar would have been obtained, notwithstanding the greater diminution of the sun's apparent diameter. It may now be asked, which of all these spectra are we to consider as exhibiting the number, and arrangement, and extent of the coloured spaces proper to be adopted as the true analysis of a solar ray.

The spectrum of Newton has surely no claim to our notice merely because it was observed on the surface of the earth. The spectrum obtained in Mercury affords no analysis at all of the incident beam, the colours being almost all compound and not homogeneous, and that of Newton is liable to the same objection. Had Newton examined his spectrum under the very same circumstances in winter and in summer, he would have found the analysis of the beam more complete in summer, on account of the diminution of the sun's diameter; and, therefore, we are entitled to say, that neither the number nor the extent of the coloured spaces, as given by Newton, are those which belong to homogeneous and uncompounded light.

The spectrum obtained in Jupiter and Saturn is the only one where the analysis is complete, as it is incapable of having its character altered by any further diminution of the sun's diameter. Hence we are forced to conclude, not only that the number and extent of the primitive homogeneous colours, given by Newton, are incorrect; but that if he had attempted to analyse some of the primitive tints in the spectrum, he would have found them decidedly composed of heterogeneous rays. There is one consequence of these observations which is somewhat interesting. A rainbow formed in summer when the sun's diameter is least, must have its colours more condensed and homogeneous than in winter, when the size of its disc is a maximum, and when the upper or under limb of the sun is eclipsed, a rainbow formed at that time will lose entirely the yellow rays, and have the green and the red in perfect contact. For the same reason, a rainbow formed in Venus and Mercury will be destitute of green rays, and have a brilliant bow of white light separating two coloured arches, while in Mars, Jupiter, Saturn, and the Georgian planet, the bow will exhibit only four homogeneous colours.

From his analysis of the solar spectrum, Newton concluded, "that to the same degree of refrangibility ever belonged the same colour, and to the same colour ever belonged the same degree of refrangibility;" and from this he inferred, that red, orange, yellow, green, blue, indigo, and violet, were primary and simple colours. He admitted, indeed, that "the same colours in specie, with those primary ones, may be also produced by composition. For a mixture of yellow and blue makes green, and of red and yellow makes orange;" but such compound colours were easily distinguished from the simple colours of the spectrum by the circumstance, that they are always capable of being resolved by the action of the prism into the two colours which compose them.

This view of the composition of the spectrum might have long remained unchallenged, had we not been able to apply to it a new mode of analysis. Though we cannot separate the green rays of the spectrum into yellow and blue by the refraction of prisms, yet if we possessed any substance which had a specific attraction for blue rays, and which stopped them in their course, and allowed the yellow rays to pass, we should thus analyse the green as effectually as if they were separated by refraction. The substance which possesses this property is a purplish blue glass, similar to that of which finger-glasses are made. When we view through a piece of this glass, about the twentieth part of an inch thick, a brilliant prismatic spectrum, we find that it has exercised a most extraordinary absorptive action on the different colours which compose it. The red part of the spectrum is divided into two red spaces, separated by an interval entirely devoid of light. Next to the inner red space comes a space of bright yellow, separated from the red by a visible interval. After the yellow comes the green, with an obscure space between them, then follows the blue and the violet, the last of which has suffered little or no diminution. Now it is very obvious, that in this experiment the blue glass has actually absorbed the red rays, which, when mixed with the yellow on one side, constituted green, so that the insulation of the yellow rays thus effected, and the disappearance of the orange, and of the greater part of the green light, proves beyond a doubt that the orange and green colours in the spectrum are compound colours, the former consisting of red and yellow rays, and the latter of yellow and blue rays of the very same refrangibility. If we compare the two red spaces of the spectrum seen through the blue glass with the red space seen without the blue glass, it will be obvious that the red has experienced such an alteration in its tint by the action of the blue glass, as would be effected by the absorption of a small portion of yellow rays; and from this we conclude, that the red of the spectrum contains a slight tinge of yellow, and that the yellow space extends over more than one-half of the spectrum, including the red, orange, yellow, green, and blue spaces.

"It has been found also," says a modern scientific writer, "that red light exists in the yellow space, and it is certain, that in the violet space, red light exists in a state of combination with the blue rays. From these and other facts, which it would be out of place here to explain, I conclude, that the prismatic spectrum consists of three different spectra, viz., red, yellow, and blue, all having the same length, and all overlapping each other. Hence, red, yellow, and blue rays of the very same refrangibility co-exist at every point of the spectrum; but the colour at any one point will be that of the predominant ray, and will depend upon the relative distance of the point from the maximum ordinate curve which represents the intensity of the light of each of the three spectra."

After explaining the structure of the spectrum by means of a diagram, he proceeds, "At the red extremity of the spectrum, the pure red is scarcely altered by the very slight intermixture of yellow and blue. Farther on in the red space, the yellow makes the red incline to scarlet. It then exists in sufficient quantity to form orange, and, as the red declines, the yellow predominates over the feeble portion of red and blue which are mixed with it. As the yellow decreases in intensity, the increasing blue forms with it a good green, and the blue rising to its maximum speedily overpowers the small portion of yellow and red. When the blue becomes very faint, the red exhibits its influence in converting it into a violet, and the yellow ceases to exercise a marked influence on the tint. The influence of the red over the blue space is scarcely perceptible, on account of the great intensity of the blue light; but we may easily conceive it to reappear and form the violet light, not only from the rapid decline of the blue light, but from the greater influence of the red rays upon the retina.

"These views may perhaps be more clearly understood by supposing that a certain portion of white light is actually formed at every point of the spectrum by the union of the requisite number of the three coloured rays that exist at any point. The white light thus formed will add to the brilliancy without affecting the tint of the predominant colour. In the violet space we may conceive the small portion of yellow which exists there to form white light with a part of the blue and a part of the red, so that the remaining tint will be violet, composed of the blue and the small remaining portion of red, mixed with the white light. This white light will possess the remarkable property of not being susceptible of decomposition by the analysis of the prism, as it is composed of red, yellow, and blue rays of the very same refrangibility. The insulation of this white light by the absorption of the predominant colours I have effected in the green, yellow, and red spaces, and by the use of new absorbing media we may yet hope to exhibit it in some of the other colours, particularly in the brightest part of the blue space, where an obvious approximation to it takes place.

Among the most important modern discoveries respecting the spectrum we must enumerate that of fixed dark and coloured lines, which we owe to the sagacity of Dr. Wollaston and Mr. Fraunhofer. Two or three of these lines were discovered by Dr. Wollaston, but nearly 600 have been detected by means of the fine prisms and the magnificent apparatus of the Bavarian optician. These lines are parallel to one another, and perpendicular to the length of the spectrum. The largest occupy a space from five to ten seconds in breadth. Sometimes they occur in well-defined lines, and at other times in groups; and in all spectra formed from solar light, they preserve the same order and intensity, and the same relative position to the coloured spaces, whatever be the nature of the prism by which they are produced. Hence these lines are fixed points, by which the relative dispersive powers of different media may be ascertained with a degree of accuracy hitherto unknown in this branch of science. In the light of the fixed stars, and in that of artificial flames, a different system of lines is produced, and this system remains unaltered, whatever be the nature of the prism by which the spectrum is formed.

The most important fixed lines in the spectrum formed by light emitted from the sun, whether it is reflected from the sky, the clouds or the moon, may be easily seen by looking at a narrow slit in the window shutter of a dark room, through a hollow prism formed of platés of parallel glass, and filled with any fluid of a considerable despersive power. The slit should not greatly exceed the twentieth of an inch, and the eye should look through the thinnest edge of the prism where there is the least thickness of fluid. These lines I have found to be the boundaries of spaces within which the rays have particular affinities for particular bodies.

While Sir Isaac was examining the nature and origin of colours as the component parts of white light, his attention was directed to the curious subject of the colour of thin plates, and to its application to explain the colours of natural bodies. He communicated his earliest researches on the subject in his Discourse on Light and Colours, to the Royal Society, on the 9th December, 1675, and they were read at subsequent meetings of that body. This discourse contained more particular details respecting the composition and decomposition of light, than he had previously given in his letter to Mr. Oldenburg, and was concluded with nine propositions, showing how the colours of thin transparent plates stand related to those of all natural bodies.

The colours of thin plates appear to have been first observed by the celebrated philosopher Mr. Boyle. Dr. Hook who had lived with Mr. Boyle, as his assistant, afterwards studied them with some care, and gave a correct account of the leading phenomena, as exhibited in the coloured rings of soap bubbles, and between plates of glass pressed together. He recognised that the colour depended upon some certain thickness of the transparent plate, but he acknowledges that he had attempted in vain to discover the relation between the thickness of the plate and the colour which it produced.

Dr. Hooke succeeded in splitting a mineral substance called mica, into films of such extreme thinness as to give brilliant colours. One plate, for example, gave a yellow colour, another a blue colour, and the two together a deep purple; but as plates which produced those colours were always so extremely thin— less than the twelve thousandth part of an inch thick, it was quite impracticable, by any contrivance yet discovered, to measure their thickness, and determine the law according to what the colour varied with the thickness of the film. Newton surmounted this difficulty by laying a double convex lens, the radius of curvature of each side of which was fifty feet, upon the flat surface of a plano-convex object glass where it touched the plane surface, to a considerable thickness at the circumference of the lens. When light was allowed to fall upon the object glass, every different thickness of the plate of air between the object-glass gave different colours, so that the point where the two object-glasses touched one another was the centre of a number of concentric coloured rings. Now, as the curvature of the object-glass was known, it was easy to calculate the thickness of the plate of air at which any particular colour appeared, and thus to determine the law of the phenomena.

When the object-glasses are illuminated by white light, the seven systems of rings, formed by all the seven colours which compose white light, will be seen at once. Had the rings in each colour been all of the same diameter, they would all have formed brilliant white rings, separated by dark intervals; but as they have all different diameters, they will overlap one another, producing rings of various colours by their mixture. These colours, reckoning from the centre, are as follows:—

1st Order.—Black, blue, white, yellow, orange, red.

2nd Order.—Violet, blue, green, yellow, orange, red.

3rd Order.—Purple, blue, green, yellow, red, bluish-red.

4th Order.—Bluish-green, green, yellowish-green, red.

5th Order.—Greenish-blue, red.

6th Order.—Greenish-blue, red.

Sir Isaac Newton deduces the phenomena thus briefly described, that ingenious, though hypothetical property of light, called its fits of easy reflexion and transmission. This property consists in supposing that every particle of light from its first discharge from a luminous body possesses, at equally distant intervals, dispositions to be reflected from, and transmitted through, the surfaces of bodies upon which it is incident. Hence if a particle of light reach a reflecting surface of glass when it is in its fit of reflexion, or in its disposition to be reflected, it will yield more readily to the reflecting force. Sir Isaac has not ventured to inquire into the cause of this property, but we may form a very intelligible idea of it by supposing that the particles of light have two attractive and two repulsive poles at the extremities of two axis at right angles to each other, and that the particles revolve round their axis, and at equi-distant intervals, bring one or other of these axis into the line of the direction in which the particle is moving. If the attractive axis is in the line of the direction in which the particle moves when it reaches the refracting surface, the particles will yield to the attractive force of the medium, and be refracted and transmitted; but if the repulsive axis is in the direction of the particle's motion when it reaches the surface, it will yield to the repulsive force of the medium, and be reflected from it.

If the thickness of the plate does not vary according to a regular law, but if, like a film of blown glass, it has numerous inequalities, then the alternate fringes of light and darkness will vary with the thickness of the film, and throughout the whole length of each fringe the thickness of the film will be the same.

We have supposed that the light employed is homogeneous. If it is white, then the differently coloured fringes will form, by their superposition, a system of fringes analogous to those seen between two object-glasses.

The same periodical colours which we have now attempted to describe, as exhibited by thin plates, were discovered by Newton in thick plates, and he has explained them by means of the theory of fits; but it would lead us far beyond our limits to enter into any detail of his observations in this place, or to give an account of the numerous and important additions which this branch of optics has received from the discoveries of succeeding philosophers.

If the objects of the material world had been illuminated with white light, observes the learned author whom we have already noticed, all the particles of which possessed the same degree of refrangibility, and were equally acted upon by the bodies on which they fall, all nature would have shone with a leaden hue, and all the combinations of external objects, and all the features of the human countenance, would have exhibited no other variety but that which they possess in a pencil sketch or a china-ink drawing. The rainbow itself would have dwindled into a narrow arch of white light—the stars would have shone through a grey sky—and the mantle of a wintry twilight would have replaced the golden vesture of the rising and setting sun. But He who has exhibited such matchless skill in the organization of material bodies, and such exquisite taste in the forms upon which they are modelled, has superadded that ethereal beauty which enhances their more permanent qualities, and presents them to us in the ever-varying colours of the spectrum. Without this the foliage of vegetable life might have filled the eye and fostered the fruit which it veils; but the youthful green of its spring would have been blended with the dying yellow of its autumn. Without this the diamond might have displayed to science the beauty of its forms, and yielded to the arts its adamantine virtues; but it would have ceased to shine in the chaplet of beauty, and to sparkle in the diadem of princes. Without this the human countenance might have expressed all the sympathies of the heart; but "the purple light of love" would not have risen on the cheek, nor the hectic flush been the herald of its decay.

The gay colouring with which the Almighty has decked the pale marble of nature, is not the result of any quality inherent in the coloured body, or in the particles by which it may be tinged, but is merely a property of the light in which they happen to be placed. Newton was the first person who placed this great truth in the clearest evidence. He found that all bodies, whatever were their peculiar colours, exhibited these colours only in white light. When they were illuminated by homogeneous red light, they appeared red, by homogeneous yellow light, yellow, and so on; "their colours being most brisk and vivid under the influence of their own day-light colours." The leaf of a plant, for example, appeared green in the white light of day, because it had the property of reflecting that light in greater abundance than any other. When it was placed in homogeneous red light, it could no longer appear green, because there was no green light to reflect; but it reflected a portion of red light, because there was some red in the compound green which it had the property of reflecting. Had the leaf originally reflected a pure homogeneous green, unmixed with red, and reflected no white light from its outer surface, it would have appeared quite black in pure homogeneous red light, as this light does not contain a single ray which the leaf was capable of reflecting. Hence the colours of material bodies are owing to the property which they possess of stopping certain rays of white light, while they reflect or transmit to the eye the rest of the rays of which light is composed.

So far the doctrine of Newton regarding colours is capable of rigid demonstration; but its author was not content with carrying it thus far: he sought to determine the manner in which particular rays are stopped, while others are reflected or transmitted; and the result of this profound inquiry was his theory of the colours of natural bodies, which was communicated to the Royal Society on the 10th of February, 1675. This theory is, perhaps, the loftiest of all his speculations; and though, as a physical generalization, it stands on a perishable basis, and must soon be swept away in the progress of science, it yet bears the deepest impress of the grasp of his powerful intellect.

The principles upon which this theory is founded are the following:—

1. Bodies that have the greatest refractive powers reflect the greatest quantity of light; and at the confines of equally refracting media there is no reflection.

2. The least particles of almost all natural bodies are in some measure transparent.

3. Between the particles of bodies are many pores or spaces, either empty or filled with media of less density than the particles.

4. The particles of bodies and their pores, or the spaces between the particles, have some definite size.

Upon these principles Newton explains the origin of transparency, opacity, and colour.

Transparency he considers as arising from the particles and their intervals or pores being too small to cause reflexion at their common surfaces, so that all the light which enters transparent bodies passes through them without any portion of it being turned from its path by reflexion. If we could obtain, for example, a film of mica, whose thickness does not exceed two-thirds of the millionth part of an inch, all the light which fell upon it would pass through it, and none would be reflected. If this film was then cut into fragments, a number of such fragments would constitute a bundle, which would also transmit all the light which fell upon it, and be perfectly transparent.

Opacity in bodies, he is of opinion, form an opposite cause, viz., when the parts of bodies are of such a size as to be capable of reflecting the light which falls upon them, in which case the light is "stopped or stifled" by the multitude of reflexions.

The colours of natural bodies have, in the hypothesis of Newton, the same origin as the colours of thin plates, their transparent particles, according to their several sizes, reflecting rays of one colour, and transmitting those of another. "For if a thinned or plated body which, being of an immense thickness, appear all over of one uniform colour, should be slit into threads, or broken into fragments of the same thickness with the plate or film, every thread or fragment should keep its colour, and consequently a heap of such threads should constitute a mass or powder of the same colour which the plate exhibited before it was broken: and the parts of all natural bodies being like so many fragments of a plate, must, on the same grounds, exhibit the same colour."

Such is the theory of the colours of natural bodies, stated as clearly and briefly as we can. It has been very generally admitted by philosophers, both of our own and of other countries, and has been illustrated and defended by a French philosopher of distinguished eminence. That this theory affords the true explantion of certain colours, or, to speak more correctly, that certain colours in natural bodies are the colours of thin plates, cannot be doubted; but it will not be difficult to show that it is quite inapplicable to that great class of phenomena which may be considered as representing the colours of natural bodies.

The first objection to the theory of Newton is the entire absence of all reflected light from the particles of transparent coloured media, such as coloured gems, coloured glasses, and coloured fluids. This objection was urged long ago by Mr. Delaval, a philosopher of great repute, who placed coloured fluids on black grounds, and never could perceive the least trace of the reflected tints. Sir David Brewster repeated the experiment with every precaution, and with every variation that he could think of, and he considered it as an established fact, that in such coloured bodies the complementary reflected colour cannot be rendered visible. If the fluid, for example, be red, the green light from which the red has been separated, ought to appear either directly by looking into the coloured mass, or ought to be recognised by its influence in modifying the light really reflected; but as it cannot be seen, we must conclude that it has not been reflected, but has been destroyed by some other property of the coloured body.

A similar objection may be drawn from the disappearance of the transmitted complementary colours, in the leaves of plants and petals of flowers. It has been ascertained from numerous experiments, that the transmitted colour is invariably the same with the reflected colour, and the same holds true with the coloured juices expressed from them. The complementary tints are never seen, and when there has been anything like an approximation of two tints, it has been invariably found that it arose from there being two coloured juices existing in different sides of the leaf.

In the phenomena of the light transmitted by coloured glasses, there are some peculiarities which we think demonstrate that their colours are not those of thin plates. The light, for example, transmitted through a particular kind of blue glass, has a blue colour of such a peculiar composition, that there is no blue in any of the orders of colours in thin plates which has any resemblance to it. It is entirely destitute of the red rays which form the middle of the red space in the spectrum; so that the particles on which the colour depends must reflect the middle red rays, and transmit those on each side of it—a property which cannot be deduced from the doctrine of Newton.

The explanation of *opacity*, as arising from a multitude of reflexions, is liable to the same objection which we have urged against the explanation of colour. In order to appreciate its weight, we must distinguish opacity into two kinds, namely, the *opacity of whiteness*, and the *opacity of blackness*. Those bodies which possess the power of reflexion in the highest degree, such as white metals, chalk, and plaster of Paris, never reflect more than one half of the light which falls upon them. The other half of the incident light is, according to Newton, lost by a multitude of reflexions. But how is it lost? Reflexion merely changes the direction of the particles of light, so that they must again emerge from the body, unless they are reflected into fixed returning orbits, which detain them for ever in a state of motion within the body. In the case of black opacity, such as that of coal, which reflects from its first surface only one twenty-fifth of the white light, the difficulty is still greater, and we cannot conceive how any system of interior reflexions could so completely stifle 24-25ths of the whole incident light, without some of it returning to the eye in a visible form.

In determining the constitution of bodies that produces transparency and blackness, the theory of Newton encounters a difficulty which its author has by no means surmounted. Transparency, as we have already seen, arises from the "particles and their interstices being too small to cause reflexions in their common surfaces," that is, they must be "less than any of those which exhibit colours," or "less than is requisite to reflect the white and very faint blue of the first order." But this is the very same constitution which produces blackness by reflexion, and in order to explain the cause of blackness by transmission, or black opacity, Newton is under the necessity of introducing a new principle.

"For the production of *black*," says he, "the corpuscles must be less than any of those which exhibit colours. For at all greater sizes there is too much light reflected to constitute this colour. But if they be supposed a little less than is requisite to reflect the white and very faint blue of the first order, they will reflect so very little light as to appear intensely black, and yet may perhaps variously refract it to and fro within themselves so long, until it happens to be stifled and lost, by which means they will appear black in all positions of the eye, without any transparency." In the same paragraph, when speaking of black bodies becoming hot, and burning sooner than others, he says that their "effect may proceed partly from the multitude of refractions in a little room, and partly from the easy commotion of so very small corpuscles."

This very remarkable pasage in his "Optics" exhibits, in a striking manner, the perplexity in which our philosopher was involved by the difficulties of his subject. As the particles which produce blackness by reflexion are necessarily so small as to exclude the existence of any reflective forces, he cannot ascribe the loss of the intromitted light, as he does in the case of white opacity, to a "multitude of reflexions;" and, therefore, he is compelled to have recourse to refracting forces to perform the same office. The reluctance with which he avails himself of this expedient, is well marked in the mode of expression which he adopts; and it is almost certain that when he wrote the above passage, he felt the full force of the objections to this hypothesis, which cannot fail to present themselves to every discriminating reader. As the size of the particles which produce blackness are intermediate between those which produce transparency and those which produce colour, approaching closely to the latter, it is difficult to conceive why *they* should refract the intromitted light, while the greater and smaller particles, and even those almost of the same size, should be destitute of that property. It is besides not easy to understand how a refraction can take place within bodies which shall stifle all the light, and prevent it from emerging. Nay, we may admit the existence of such refractions and yet understand how, by a compensation in their direction, the refracted rays may all emerge from the opaque body.

The force of these objections is tacitly recognized in Pemberton's View of Sir Isaac Newton's Philosophy; and as Newton not only read and approved of that work, but even perused a great part of it along with its author, we may fairly consider the opinion there stated to be his own.

"For producing *black*, the particles ought to be smaller than for exhibiting any of the colours, viz., of a size answering to the thickness of the (soap) bubble, whereby reflecting little or no light it appears colourless; 'but yet they must not be too small,' for that will make them transparent 'through deficiency of reflexions' in the inward parts of the body, sufficient to stop the light from going through it; but they must be of a size 'bordering upon that' disposed to reflect the faint blue of the first order, which affords an evident reason why blacks usually partake a little of that colour." In this passage all idea of refraction has been abandoned, and that precise degree of size is assumed for the particles which leave a small power of reflexion, which is deemed sufficient to prevent the body from becoming transparent; that is, sufficient to render it opaque or black.

The last objection which we shall state to this theory is one to which we attach great weight, and, as it is founded on discoveries and views which have been published since the time of Newton, we venture to believe, that, had he been aware of them, he would never have proposed the theory which is now under consideration.

When light falls upon a thin film so as to produce the colours of thin plates, it follows, from Sir Isaac Newton's theory of fits, that a portion of the light is, as usual, reflected at the first surface, while the light that forms the coloured image is that which is reflected from the second surface, so that all the colours of thin plates are diluted with the white light reflected from the first surface. Now, in the modern theory, which ascribes the colours of thin plates to the interference of the light reflected from the second surface, with the light reflected from the first surface, the resulting tint arises from the combination of these two pencils, and, consequently, there is no light reflected from the first surface. In like manner, when the thickness of the film is such, that the two interfering pencils completely destroy one another, and produce black, there is not a ray of light reflected from the first surface. Here, then, we have a criterion for deciding between the theory of fits and the theory of interference; for if there is no white light reflected from the first surface, the theory of fits must be rejected. In a remarkable phenomenon of blackness arising from minute fibres, there was no perceptible reflexion from the surface of the fibres; and M. Fresnel describes an experiment made to determine the same point, and states the result of it to have been unequivocally in favour of the doctrine of interference.

In order to apply this important fact, let us take a piece of coal, one of the blackest and most opaque of all substances, and which does not reflect to the eye a single ray out of those which enter its substance. The size of its particles is so small, that they are incapable of reflecting light. When a number of these particles are placed together, so as to form a surface, and other particles behind them so as to form a solid, they will not acquire by this process the power of reflexion; and, consequently, a piece of coal so composed should be destitute of the property of reflecting light from its first surface. But the reverse is the case—light is abundantly reflected from the first surface of the coal, and, of course, its elementary particles must possess the same power. Hence the blackness of coal must be ascribed to some other cause than to the minuteness of its transparent atoms.

To transparent bodies this argument has a similar application. As their atoms are still less than those of black bodies, their inability to reflect light is still greater, and hence arises their transparency. But the particles forming the surface of such bodies do reflect light, and, therefore, their transparency must have another origin.

In the case of coloured bodies, too, the particles forming their surfaces reflect white light like those of all other bodies, so that these particles cannot produce colour on the same principles as those of thin plates. In many of those cases of colour which seem to depend upon the minuteness of the particles of the body, the reflexion of white light may nevertheless be observed, but this will be found to arise from a thin transparent film, behind which the colorific particles are placed.

## CHAPTER IV.

New Classification of Colours. Outline of a New Theory. Newton's Discoveries respecting the Inflexion or Diffraction of Light. Previous Discoveries. Labours of succeeding Philosophers. New Theory of Inflexion. Miscellaneous Optical Researches of Newton. Experiments on Refraction. Conjecture respecting the Inflammability of the Diamond. Law of Double Refraction. Observations on the Polarization of Light. Newton's Theory of Light. His "Optics." Astronomical Discoveries. Sketch of the History of Astronomy previous to the time of Newton.

WHATEVER answer may be made to the previous objections, we think it will be admitted by all those who have studied the subject most profoundly, that a satisfactory theory of the colours of natural bodies is still a desideratum in science. How far we may be able to approach it in the present state of optics, the reader will judge from the following views.

Colours may be arranged into seven classes, each of which depends upon different principles.

1. Transparent coloured fluids—transparent coloured gems—transparent coloured glasses—coloured powders—and the colours of the leaves and flowers of plants.

2. Oxidations of metals—colours of Labrador feld-spar—colours of precious and hydrophanous opal, and other opal essences—the colours of the feathers of birds, of the wings of insects, and of the scales of fishes.

3. Superficial colours, as those of mother of pearl and striated surfaces.

4. Opal essences and colours in composite crystals having double refractions.

5. Colours from the absorption of common and polarized light by double refracting crystals.

6. Colours at the surfaces of media of different dispersive powers.

7. Colours at the surface of media in which the reflecting forces extend to different distances, or follow different laws.

The two first of these classes are the most important. The Newtonian theory appears to be strictly applicable to the phenomena of the second class; but those of the first class cannot, we conceive, be referred to the same cause.

The rays of solar light possess several remarkable physical properties: They heat—they illuminate—they promote chemical combination—they effect chemical decompositions—they impart magnetism to steel—they alter the colours of bodies—they communicate to plants and flowers their peculiar colours, and are in many cases necessary to the development of their characteristic qualities. It is impossible to admit for a moment that these varied effects are produced by a mere mechanical action, or that they arise from the agitation of the particles of bodies by the vibration of ether which is considered to be the cause of light. Whatever be the difficulties which attach to the theory which supposes light to consist of material particles, we are compelled, by its properties, to admit that light acts as if it were material, and that enters into combinations with bodies, in order to produce the effects which we have enumerated.

When a beam of light falls upon a body, and the whole or a part of that which enters its substance totally disappears, we are entitled to say, that it is detained by some power exercised by the particles of the body over the particles of light. When this light is said to be lost by a multitude of reflexions or refractions, the statement is not only hypothetical, but it is a hypothesis incompatible with optical principles.

That the light detained within bodies has been stopped by the attractive force of the particles seems to be highly probable, and the mind will not feel any repugnance to admit that the particles of all bodies, whether solid, fluid, or aeriform, have a specified affinity for the particles of light. Considering light, therefore, as material, it is not difficult to comprehend how it should, like other elementary substances, enter into combination with bodies, and produce many chemical and physical effects, but particularly the phenomena of transparency, opacity, and colour.

In transparent colourless bodies, such as water and glass, the intromitted light experiences a considerable loss, because a certain number of its particles are attracted and detained by the atoms of the water and glass, and the light which emerges is colourless, because the particles exercise a proportional action over all the simple colours which compose white light.

When the transparent body has any decided colour, such as those ennumerated in Class I., then the particles of the body have exercised a specific attraction over those rays of white light which are complementary to those which compose the colour of the transmitted light. If the transparent body, for example, is red, then its particles have detained the green rays which entered into the incident light, or certain other rays, which, with the red, are necessary to compose white light. In compound bodies, like some of the artificial glasses, the particles will attract and detain rays of light of different colours, as may be seen by analysing the transmitted light with a prism, which will exhibit a spectrum deprived of all the rays which have been detained. In black bodies the particles exercise a powerful attraction over light, and detain all the intromitted rays.

When coloured bodies are opaque, so as to exhibit their colours principally by reflexion, the light which is reflected back to the observer has received its colour from transmission through part of the thickness of the body, or, what is the same thing, the colour reflected to the eye is complementary to that which has been detained by the particles of the body while the light is passing and re-passing through a thickness terminated by the reflecting surfaces; and as only part of this light is reflected, as in the case of leaves and flowers, the transmitted light must have the same colour as the reflected light.

When coloured bodies exhibit two different colours complementary to each other, the one seen by reflexion and the other by transmission, it is then highly probable that the colours are those of thin plates, though there are still other optical principles to which they may be referred. As the particles of bodies, and the medium which unites them, or, as the different atoms of a compound body may have different dispersive powers, while they exercise the same refractive force over a particular part of the spectrum, the rays for which this compensation takes place will be transmitted, while part of the complementary light is reflected. Or in cases where the refractive and dispersive powers are the same, the reflective forces of the particles may vary according to a different law, so that at the separating surfaces either white or coloured light may be reflected.

In those cases of colour where the reflected and the transmitted tints are not complementary, as in leaf gold, where the former is yellow and the latter green; in leaf-silver, where they are white and blue, and in certain pieces of fir-wood, where the reflected light is whitish yellow, and the transmitted light a brilliant homogeneous red, we may explain the separation of the colours either by the principles we have already laid down, or by the doctrine of their plates. On the first principle, the colour of the reflected light, which is supposed to be the same as that of the transmitted light, will be modified by the law according to which the particles of the body attract different rays out of the beam of white light. In pitch, for example, the blue rays are first absorbed, so that at small thicknesses the transmitted light is a fine yellow, while, by the action of greater thickness, the yellow itself is absorbed, and the transmitted light is a bright homogeneous red. Now, in leaf-gold the transmitted colour of thinner films than we can obtain may be yellow, and, consequently, the light reflected from the first strata of interrupting faces will be yellow, and will determine the predominate tint of the reflected light. On the Newtonian doctrine Mr. Herschel has explained it by saying "that the transmitted rays have traversed the whole thickness of the medium, and therefore undergo many more times the action of its atoms than those reflected, especially those near the first surface to which the brighter part of the reflected colour is due."

The phonomena of the absorption of common and polarized light, which have been described by Sir David Brewster in the Philosophical Transactions, throw considerable light on the subject of coloured bodies. The relation of the absorbent action to the axis of double refraction, and consequently to the poles of the molecules of the crystal, shows how the particles of light attracted by the molecules of the body will vary, both in their nature and number, according to the direction in which they approach the molecules; and explains how the colour of a body may be changed, either temporarily or permanently, by heat, according as it produces a temporary or a permanent change in the relative position of the molecules. This is not the proper place to enlarge on the subject; but we may be permitted to apply the idea to the curious experiment of M. Thenard, on phosphorus. When this substance is rendered pure by repeated distillation, it is transparent, and transmits yellow light; but when it is thrown in a melted state into cold water, it becomes jet-black. When again melted, it resumes its original colour and transparency. According to the theory of Newton, we must suppose that the atoms of the phosphorus have been diminished in size by sudden cooling—an effect which it is not easy to comprehend; but, according to the preceding views, we may suppose that the atoms of the phosphorus have been forced by sudden cooling into relative positions quite different from those which they take when they slowly assume the solid state; and their poles of maximum attraction, in place of being turned to one another, are turned in different directions, and then allowed to exercise their full action in attracting the intromitted light, and detaining it wholly without the body. If this view of the matter be just, we should expect that the specific gravity of the black would exceed that of the yellow.

Before concluding this portion of our subject, there is one topic peculiarly observing our notice, namely, the change of colour produced in bodies by continued exposure to light. The general effect of light is to diminish or dilute the colours of bodies, and in many cases to deprive them entirely of their colour. Now, it is not easy to understand how repeated undulations propagated through a body could diminish the size of its particles, or how the same effect could be produced by a multitude of reflexions from particle to particle. But if light is attracted by the particles of bodies, and combines with them, it is easy to conceive, that when the molecules of a body have combined with a great number of particles of a green colour, for example, their power of combination with others will be diminished, and, consequently, the number of particles of any colour absorbed or detained must diminish with the time that the body has been exposed to the light; that is, these particles must enter into the transmitted and reflected pencils, and diminish the intensity of their colour. If the body, for example, absorbs red light, and transmits and reflects green; then, if the quantity of absorbed red light is diminished, it will enter into the reflected and transmitted pencils; and, forming white light by its mixture with a portion of the green rays, will actually dilute them in the same manner as if a portion of white light had been added.

Although the discoveries of Sir Isaac Newton respecting the INFLEXION OF LIGHT were first published in his OPTICS in 1704, yet there are many reasons to conclude that they were made at a much earlier period. He, indeed, informs us, in his preface to that great work, that the third book, which contains these discoveries, "was put together out of scattered papers;" and he adds at the end of his observations, that "he designed to repeat most of them with more care and exactness, and to make some new ones for determining the manner how the rays of light are bent in their passage by bodies, for making the fringes of colour with the dark lines between them. But I was then interrupted, and cannot now think of taking these things into consideration." On the 18th of March, 1674, Dr. Robert Hooke had read a valuable paper on the phenomena of diffraction; and, as Sir Isaac makes not the slightest allusion to this work, it is the more probable that his "scattered papers" had been written previous to the communication of Dr. Hooke's experiments.

The phenomena of the inflexion of light were first discovered by Francis Maria Grimaldi, a learned Jesuit, who has fully described them in a posthumous work, published in 1665, two years after his death.

Having admitted a beam of the sun's light through a small pin-hole in a piece of lead or card into a dark chamber, he found that the light diverged from this aperture in the form of a cone, and that the shadows of all bodies placed in this light were not only larger than might have been expected, but were surrounded with three coloured fringes, the nearest being the widest, and the most remote the narrowest. In strong light he discovered analogous fringes within the shadows of bodies, which increased in number with the breadth of the body, and became more distinct when the shadow was received obliquely and at a greater distance. When two small apertures or pin-holes were placed so near each other that the cones of light formed by each of them intersected on another, Grimaldi observed, that a spot common to the circumference of each, or, which is the same thing, illuminated by rays from each cone, was darker than the same spot when illuminated by either of the cones separately; and he announced this remarkable fact in the following apparently absurd proposition:—"That a body actually illuminated may become more dark by adding a light to that which it already receives."

Without being aware of what had been done by the Italian philosopher, Dr. Hooke had been diligently occupied with the same subject. In 1672, he communicated his first observations to the Royal Society, and he then spoke of his paper as "containing the discovery of a new property of light not mentioned by any optical writers before him." In his paper of 1764, already mentioned, and which is, no doubt, the one to which he alludes, he has not only described the leading phenomena of the inflexion, or the deflexion of light, as he calls it, but he has distinctly announced the doctrine of interference, which has performed so conspicuous a part in the subsequent history of optics. He thus announces this doctrine. 1st. That the same rays of light falling upon the same point of an object will turn into all sorts of colours by the various inclinations of the object. 2nd. That colours begin to appear when two pulses of light are blended so well and so near together that the sense takes them for one.

Such was the state of the subject when Newton directed to it his powers of acute and accurate observation. His attention was turned only to the enlargement of the shadow, and to the three fringes which surrounded it; and he begins his observations by ascribing the discovery of these facts to Grimaldi. After taking exact measures of the diameter of a human hair, and of the breadth of the fringes at different distances behind it, he discovered the remarkable fact that these diameters and breadths were not proportional to the distances from the hair at which they were measured. In order to explain this phenomena, Newton supposed that the rays which passed the edge of the hairs are deflected or turned aside from it, as if by a repulsive force, the nearest rays suffering the greatest, and those more remote a less degree of deflexion.

The explanation given by Sir Isaac of the coloured fringes is less precise, and can be inferred only from the two following queries.

1. "Do not the rays which differ in refrangibility differ also in flexibility, and are they not by these different inflexions separated from one another, so as after separation to make the colours in the three fringes above described? And after what manner are they inflected to make those fringes.

2. "Are not the rays of light in passing by the edges and sides of bodies bent several times backwards and forwards with a motion like that of an eel? And do not the three fringes of light above mentioned arise from three such bendings?"

The idea thus indistinctly thrown out in the preceding queries has been ingeniously interpreted by Mr. Herschel, who supposes that the rays undergo several bendings, and the particles of light are thrown off at one or other of the points of contrary flexure, according to the state of their fits or other circumstances. Those that are thrown outwards will produce as many caustics by their intersections as there are deflected rays, and each caustic, when received on a screen at a distance, will depict on it the brightest part of a maximum of a fringe.

In this unsatisfied state was the subject of the inflexion of light left by Sir Isaac Newton. His inquiries were interrupted and never again renewed; and though he himself found that the phenomena were the same, "whether the hair was encompassed with air or with any other pellucid substance," yet this important result does not seem to have shaken his conviction, that the phenomena had their origin in the action of bodies upon light.

During two sets of experiments which Sir David Brewster made on the inflexion of light, the first in 1798, and the second in 1812 and 1813, he was desirous of examining the influence of density and refractive power over the fringes produced by inflexion. He compared the fringes formed by gold leaf with those formed by masses of gold—and those produced by films which gave the colours of thin plates with those formed by masses of the same substance. He examined the influence of platinum, diamond, and cork, in inflecting light, the effect of non-reflecting grooves and spaces in polished metals, and of cylinders of glass immersed in a mixture of oil of cassia and oil of olives of the same refractive power, and, as the fringes had the same magnitude and character under all these circumstances, he concluded that they were not produced by any force inherent in the bodies themselves, but arose from a property of the light itself, which always showed itself when light was stopped in its progress.

Dr. Thomas Young, who had supported with considerable ingenuity and great force of argument the undulatory theory of light, as maintained by Hooke and Huygens, was the first who gave a plausible explanation of the inflexion of light. By interposing a small screen, and intercepting the rays that passed near the hair, he was surprised to find that all the fringes within the shadow disappeared. The same effect took place when the screen intercepted the rays on the other side; and hence he concluded, that the rays on each side of the hair were necessary to the production of the inner fringes, and that the fringes were produced by the interference of the rays that passed on the one side of the hair with those that passed on the other side. In order to account for the coloured fringes without the shadow, Dr. Young conceived that the rays which pass near the edge of the hair interfere with others, which he supposes may be reflected after falling very obliquely upon its edge—a supposition which, if correct, would certainly produce fringes very similar to those actually observed.

In pursuing these researches so successfully begun by Dr. Young, M. Fresnel had the good fortune to explain all the phenomena of inflexion by means of undulatory doctrine combined with the principle of interference. In place of transmitting the light through a small aperture, he caused it to diverge from the focus of a deep convex lens, and, instead of receiving the shadow and its fringes upon a smooth white surface as was done by Newton, he viewed them directly with his eye through a lens placed behind the shadow; and by means of a microscope he was able to measure the dimensions of the fringes with the greatest exactness. By this mode of observation he made the remarkable discovery, that the inflexion of the light *depended on the distance of the inflecting body from the aperture or from the focus of divergence*, the fringes being observed to dilate as the body approached that focus, and to contract as it receded from it, their relative distances from each other, and from the margin of the shadow continuing invariable. In attempting to account for the formation of the exterior fringes, M. Fresnel found it necessary to reject the supposition of Dr. Young, that they were owing to light reflected from the edge of a body. He not only ascertained that the real place of the fringe was the seventeenth hundredth of a millimetre different from what it should be on that supposition, but he found that the fringes preserved the same intensity of light, whether the inflecting body had a round or a sharp edge, and even when the edge was such as not to afford sufficient light for their production. From this difficulty the undulatory theory speedily released him, and he was led by its indications to consider the exterior fringes as produced by an infinite number of elementary waves of light emanating from a primitive wave when partly interrupted by an opaque body.

The various phenomena of inflexion, which had so long resisted every effort to generalize them, having thus received so beautiful and satisfactory an explanation from the undulatory doctrine, they must of course be regarded as affording to that doctrine the most powerful support, while the Newtonian hypothesis of the materiality of light, is proportionally thrown into the shade. It is impossible, indeed, even for national partiality, to consider the views of Newton as furnishing any explanation of the facts discovered by Fresnel, and, as no attempt has been made by the small, though able phalanx of his disciples, to stay the decision with which, on this account at least, the doctrine of emission has been threatened;—we shall venture to suggest some principles by which the refracting phenomena may perhaps be yet brought within the poles of the Newtonian theory.

That the particles of light, like those of heat, are endowed with a repulsive force, which prevents them from accumulating, when in a state of condensation, or when they are detained by the absorptive action of opaque bodies, will be readily admitted. By this power, a beam of light radiating from a luminous point, has, in every azimuth, the same degree of intensity at the same distance from its centre of divergence; but if we intercept a portion of such a beam by an opaque body, the repulsive force of the light which formerly occupied its shadow, is withdrawn, and consequently the rays which pass near the body, will be repelled into the shadow, and will form by their interference with those similarly repelled on the other side, the interior fringes, which are parallel to the edge of the body. The rays which pass at a greater distance, will in like manner be bent towards the body, but with less force, and interfering with those rays which retain their primitive direction, from the state of their fits, or the position of their poles, they will form the exterior fringes. When the inflecting body is placed near the point of divergence, the greater proximity of the rays will produce a greater repulsive force, and consequently, a greater inflexion of the passing light; while the removal of the body from the point of divergence, will be accompanied with an increased distance of the particles—an inferior repulsive force and a feebler inflexion. As the phenomena of inflexion, considered under this aspect, arise from a property of the light itself, it follows that they will remain invariable, whatever be their nature or density of the body, or the form of the edge which acts upon the passing rays.

Before concluding our account of Sir Isaac Newton's optical discoveries, it is necessary to notice some of his minor researches which, though of inferior importance in the science of light, have exercised an influence over the progress of discovery, or been associated with the history of other branches of knowledge.

One of the most curious of these inquiries related to the connection between the refractive powers and the chemical composition of bodies. Having measured the refractive powers and the densities of twenty-two substances, he found that the forces which reflect and refract light are very nearly proportional to the densities of the same bodies. In this law, however, he noticed a remarkable exception in the case of unctuous and sulphureous bodies, such as camphor, olive oil, linseed oil, spirits of turpentine, and diamond, which have their refractive powers two or three times greater in respect of their densities than the other substances in the table, while among themselves their refractive powers are proportional to their densities, without any considerable variation. Hence he concluded that diamond "is an unctuous substance coagulated,"—a sagacious prediction, which has been since verified in the discoveries of modern chemistry. The connection between a high degree of inflammability, and a great refracting force, has been still more strongly established by the high refractive power which has been detected in phosphorus, and which was discovered in hydrogen by M. M. Biot and Arago.

There is no part of the optical labours of Newton which is less satisfactory than that which relates to the double refraction of light. In 1690 Huygens published his admirable treatise on light, in which he has given the law of double refraction in calcareous spar, as deduced from his theory of light, and as confirmed by direct experiment. Viewing it probably as a theoretical deduction, Newton seems to have regarded it as incorrect; and though he has given Huygens the credit of describing the phenomena more correctly than Bartholinus, yet, without assigning any reason, he rejected the law of the Dutch philosopher, and substituted another its place. These observations of Sir Isaac form the subject of the twenty-fifth and twenty-sixth queries at the end of his Optics, which was published fourteen years after the appearance of Huygen's work. The law adopted by Newton is not accompanied with any of the experiments from which it was deduced; and though he has given it without expressing any doubt of its accuracy, it is, nevertheless, entirely incompatible with observation, and has been rejected by all succeeding philosophers.

In his speculations respecting the successive disappearance and re-appearance of two of the four images which are formed when a luminous object is viewed through two rhombs of calcareous spar, one of which is made to revolve upon the other, Newton has been more successful. He concluded from these phenomena that every ray of light has two opposite sides originally endued with the property, on which the universal refraction depends, and other two opposite sides not endued with that property, and he suggested it as a subject for further inquiry, whether there are not more properties of light by which the sides of the rays differ, and are distinguished from one anothor. This is the first occasion on which the idea of polarity in the rays of light has been suggested.

From the various optical inquiries in which Newton was engaged he was strongly impressed with the belief that light consists of small material particles emitted from shining substances, and that these particles could again be combined into solid matter, so that "gross bodies and light were convertible into one another." He conceived also that the particles of solid bodies and of light exerted a mutual action upon each other, the former being agitated aud heated by the latter, and the latter being attracted and repelled by the former, with forces depending on the inertia of the luminous particles. These forces he regarded as insensible at all measurable distances, and he conceived that the distances between the particles of bodies was very small when compared with the extent of their sphere of attraction and repulsion.

With the exception of Hooke, Huygens, and Euler, almost all the contemporaries and successors of Newton maintained the doctrine of the materiality of light. It was first successfully assailed by Dr. Thomas Young, and since that time it has been shaken to its foundation by those great discoveries which have illustrated the commencement of the present century. The undulatory theory which has triumphed in its turn, is still subject to grave difficulties, and we fear another century must elapse before a final decision can be pronounced on this long disputed question.

The most important of the optical discoveries of Newton, of which we have given a general history, were communicated to the Royal Society in detached papers; but the numerous disputes in which they had involved their author, made him hesitate about the publishing of his other discoveries. Although he had drawn up a connected view of his labours, under the title of "Optics, or a Treatise on the Reflexions, Refractions, Inflexions, and Colours of Light," yet he formed the resolution of not publishing this work during the life of Hooke, by whose rival jealousy his tranquillity had been so frequently interrupted. Hooke, however, died in 1702, and the Optics of Newton appeared in English in 1704. Dr. Samuel Clark proposed a Latin edition of it, which accordingly appeared in 1706, and he was generously presented by Sir Isaac with £500, (as a gift of £100 each for his five children,) as a token of the approbation and gratitude of the author. Both the English and Latin editions have been frequently reprinted both in England and on the Continent, and there perhaps never has been a work of profound science so widely circulated.

From the optical labours of Sir Isaac Newton, we will now proceed to his astronomical discoveries—those transcendant deductions of human reason by which he has secured to himself an immortal name, and vindicated the intellectual dignity of his species. Pre-eminent as his triumphs have been, it would be unjust to affirm that they were achieved by himself alone. The torch of many a preceding age had thrown its light into the strongholds of the material universe, and the grasp of many a powerful hand had pulled down the strongest of its defences. An alliance, indeed, of many kindred spirits had been long struggling in this great cause, and Newton was only the leader of this glorious band—the fortunate general who won the victory, and therefore wears its laurels.

The history of science presents us with no example of an individual mind throwing itself far in advance of its contemporaries. It is only in the career of crime and ambition that reckless man takes the start of his species, and, uncurbed by moral and religious restraint, erects an unholy dynasty upon the ruins of ancient and venerable institutions. The achievements of intellectual power, though often begun by one mind and completed by another, have ever been the results of combined exertions. Slow in their growth, they gradually approximate to a more perfect condition;—the variety in the phenomena of nature call forth a variety of intellectual gifts;—the powers of analysis and combination are applied to the humbler labours of observation and experiment, and in the ordeal of rival inquiry truth is finally purified from error. How different is it with those systems which the imagination rears—those theories of wild import which are directed against the consciences and hopes of man. The fatal upas tree distils its poison in the spring as well as in the autumn of its growth, but the fruit which sustains life must have its bud prepared before the approach of winter, its blossom expanded in the spring, and its juices elaborated by the light and the heat of the summer and the autumnal sun.

In the century which preceeded the birth of Newton, the science of astronomy advanced with the most rapid strides. Emerging from the darkness of the middle ages, the human mind seemed to rejoice in its new-born strength, and to apply itself with elastic vigour to unfold the mechanism of the heavens. The labours of Hipparchus and Ptolemy had indeed furnished many important epochs and supplied many valuable data; but the cumbrous appendages of cycles and epicycles with which they explained the stations and retrogadations of the planets, and the vulgar prejudice which a false interpretation of Scripture had excited against a belief in the motion of the earth, rendered it difficult even for great minds to escape from the trammels of authority, and appeal to the simplicity of nature.

The generous and noble-minded sovereign of Castile, Alphonso, had long before proscribed the rude expedients of his predecessors; and when he declared that if the heavens were thus constituted, he could have given the Deity good advice, he must not only have felt the absurdity of the prevailing system, but must have obtained some foresight of a more simple arrangement. But neither he nor the astronomers whom he so liberally patronised seem to have established a better system, and it was left to Copernicus to enjoy the dignity of being the restorer of astronomy.

This eminent man, a native of Thorn, in Prussia, following his father's profession, began his career as a Doctor of Medicine, but an accidental attendance on the mathematical lectures of Brudzevius excited a love for astronomy, which became the ruling passion of his life. Quitting a profession uncongenial to such pursuits, he went to Bologna to study astronomy under Dominic Maria, and after having enjoyed the friendship and instruction of that able philosopher, he established himself at Rome in the humble capacity of a teacher of mathematics. Here he made many astronomical observations which served him as the basis of future researches; but an event soon occurred, which, though it interrupted for a time his important studies, placed him in a situation for pursuing them with increased zeal. The death of one of the canons enabled his uncle, who was Bishop of Ermeland, to appoint him to a canonry in the chapter of Frauenberg, where, in a house situated on the brow of a mountain he continued in peaceful seclusion, to carry on his astronomical observations. During his residence at Rome his talents had been so well appreciated, that the Bishop of Fossombrona, who presided over the council for reforming the calendar, solicited the aid of Copernicus in this desirable undertaking. At first he entered warmly into the views of the council, and charged himself with the determination of the length of the year and of the month, and of the other motions of the sun and moon that seemed to be required; but he found the task too irksome, and probably felt that it would interfere with those interesting discoveries which had already begun to dawn upon his mind.

Copernicus is said to have commenced his inquiries by a historical examination of the opinions of ancient authors on the system of the universe; but it is more likely that he sought for the authority of their great names to countenance his peculiar views, and that he was more desirous to present his own theory as one that he had received, rather than as one which he had invented. His mind had been long imbued with the idea, that simplicity and harmony should characterize the arrangements of the planetary system, and, in the complication and disorder which reigned in the hypothesis of Ptolemy, he saw insuperable objections to its being regarded as a representation of nature. In the opinions of the Egyptian sages, in those of Pythagoras, Philolaus, Aristarchus, and Nicetas, he recognised his own earliest conviction that the earth was not the centre of the universe; but he appears to have considered it as still possible that our globe might perform some function in the system more important than that of the other planets: and his attention was much occupied with the speculation of Martianus Capella, who placed the sun between Mars and the Moon, and made Mercury and Venus revolve round him as a centre; and with the system of Apollonius Pergœus, who made all the planets revolve round the sun, while the sun and moon were carried round the earth in the centre of the universe. The examination, however, of these hypotheses gradually dispelled the difficulties with which the subject was beset, and after the labours of more than thirty years, he was permitted to see the true system of the heavens. The sun he considered as immoveable in the centre of the system, while the earth revolved between the orbits of Venus and Mars, and produced by its rotation about its axis all the diurnal phenomena of the celestial sphere. The precession of the equinox was thus referred to a slight motion of the earth's axis, and the stations and the retrogadations of the planets were the necessary consequence of their own motions combined with that of the earth about the sun. These remarkable views were supported by numerous astronomical observations; and, in 1530, Copernicus brought to a close his immortal work on the Revolutions of the Heavenly Bodies.

But while we admire the genius which triumphed over so many difficulties, we cannot fail to commend the extraordinary prudence with which he ushered his new system into the world. Aware of the prejudices, and even of the hostility with which such a system would be received, he determined neither to startle the one nor provoke the other. He allowed his opinions to circulate in the slow current of personal communication. The points of opposition which they presented to established doctrines were gradually worn down, and they insinuated themselves into reception among to ecclesiastical circles by the very reluctance of their author to bring them into notice. In the year 1534, Cardinal Schonberg, Bishop of Capua, and Gyse, Bishop of Culm, exerted all their influence to induce Copernicus to lay his system before the world; but he resisted their solicitations; and it was not till 1539 than an accidental circumstance contributed to alter his resolution. George Rheticus, Professor of Mathematics at Wirtenberg, having heard of the successful labours of Copernicus, resigned his chair and proceeded to Frauenberg to make himself master of his discoveries. This zealous disciple prevailed upon his master to permit the publication of his system; and they seem to have arranged a plan for giving it to the world without alarming the vigilance of the church, or startling the prejudices of individuals. Under the disguise of a student of mathematics, Rheticus published in 1540 an account of the manuscript volume of Copernicus. This pamphlet was received without any disapprobation, and its author was encouraged to reprint it at Basle in 1541, with his own name. The success of these publications, and the flattering manner in which the new astronomy was received by several eminent writers, induced Copernicus to place his manuscripts in the hands of Rheticus. It was accordingly printed at the expense of Cardinal Schonberg, and appeared at Nuremberg, in 1543. Its illustrious author, however, did not live to peruse it. A complete copy was handed to him in his last moments, and he saw and touched it a few hours before his death. This great work was dedicated to the Holy Pontiff, in order, as Copernicus himself says, that the authority of the head of the church might silence the calumnies of individuals who had attacked his views by arguments drawn from religion. Thus introduced, the Copernican system met with no ecclesiastical opposition, and gradually made its way in spite of the ignorance and prejudice of the age.

Among the astronomers who preceded Newton and provided the materials of his philosophy, the name of Tycho Brahe merits a conspicuous place. Descended from an ancient Swedish family, he was born at Knudstorp, in Norway, in 1546, three years after the death of Copernicus. The great eclipse of the sun, which happened on the 26th of August, 1560, while he was at the university of Copenhagen, attracted his notice; and when he found that all its phenomena had been accurately predicted, he was seized with the most irresistible passion to acquire the knowledge of a science so infallible in its results. Destined for the profession of the law, his friends discouraged the pursuit which now engrossed his thoughts, and such were the reproaches, and even persecutions to which he was exposed, that he quitted his country with the design of travelling through Germany. At the very commencement of his journey, however, an event occurred in which the impetuosity of his temper had nearly cost him his life. At a wedding-feast in Rostock, a questionable point in geometry involved him in a dispute with a Danish nobleman of the same temperament as himself, and the two mathematicians resolved to settle the difference by the sword. Tycho, however, seems to have shared worst in the conflict, for he lost the greater part of his nose, and was forced to supply the deficiency by a substitute of gold, silver, and wax, so skilfully made that it could scarcely be distinguished from nature. During his stay at Augsburg he inspired the burgomaster of the city, Peter Hainzell, with a love of astronomy. This public spirited citizen erected an excellent observatory at his own expense, and here Tycho began that distinguished career which has placed him in the first rank of practical astronomers.

Upon his return to Copenhagen, in 1570, he was received with every mark of respect. The king invited him to court, and persons of all ranks harassed him with their attentions. At Herritzvold, near the place of his birth, the house of his maternal uncle afforded him a retreat from the gaieties of the capital, and he was there offered every accommodation for the prosecution of his astronomical studies. Here, however, the passion of love and the pursuits of alchemy distracted his thoughts; but though the peasant-girl of whom he was enamoured was of easier attainment than the philosopher's stone, the marriage produced an open quarrel with his relations, which became so violent, that the royal authority was necessary to effect a reconciliation. In the tranquillity of domestic happiness Tycho resumed his study of the heavens; and, in 1573 he enjoyed the singular good fortune of observing, through all its variations, the new star in the constellation, Cassiopeia, which appeared with such extraordinary splendour as to be visible in the daytime, and which gradually disappeared in the following year.

Dissatisfied with his residence in Denmark, Tycho resolved to settle in some distant country; and having travelled as far as Venice in search of a suitable abode, he at at last fixed upon Basle, in Switzerland. The King of Denmark, however, had learned his intention from the Prince of Hesse; and when Tycho returned to Copenhagen to remove his family and his instruments, his sovereign announced to him his resolution to detain him in his kingdom. He presented him with the canonry of Roschild, with an income of two thousand crowns yearly. To this he added a pension of one thousand crowns; and he promised to give him the island of Huen, with a complete observatory erected under his own eye. This generous offer was instantly accepted. The celebrated observatory of Uranibourg was established at the expense of £20,000; and in this magnificent retreat Tycho continued for twenty-one years to enrich astronomy with the most valuable observations. Admiring disciples crowded to this sanctuary of the sciences to acquire the knowledge of the heavens; and kings* and princes

* When James I. went to Denmark to visit his future spouse, the daughter of Frederic II., he spent a week under the roof of Tycho.—Besides making some considerable presents, and composing a set of Latin verses in favour of the astronomer, he gave him his royal license for the publication of his works in England.

felt themselves honoured by becoming the guests of the great astronomer of the age.

One of the principal discoveries of Tycho was that of the inequalities of the moon's motion, called the Variation. He detected also the annual equation which affects the place of her apogee and nodes, and he determined the greatest and the least inclination of the lunar orbit. His observations on the plantes were numerous and precise, and have formed the data of the the present generalizations in astronomy. Though thus skilful in the observation of phenomena, his mind was little suited to investigate their cause; and it was probably owing to this defect that he rejected the system of Copernicus. The vanity of giving his own name to another system was not likely to actuate a mind such as his; and it is more probable that he was led to adopt the immobility of the earth, and to make the sun, with all his attendant planets, circulate round it, from the great difficulty which still presented itself by comparing the apparent diameter of the stars with the annual parallax of the earth's orbit.

The death of Frederic II., in 1588, proved a severe calamity to Tycho, and to the science which he cultivated. During the first years of the minority of Christian IV., the regency continued the royal patronage to the Observatory of Uranibourg; and in 1592, the young king paid a visit of some days to Tycho, and left him a gold chain in token of his favour. The astronomer, however, had made himself enemies at court, and the envy of his high reputation had probably added fresh malignity to the irritation of personal feelings. Under the ministry of Walchendorf, a name for ever odious to science, Tycho's pension was stopped; he was even, in 1597, deprived of the canonry of Roschild, and was thus forced, with his wife and children, to seek an asylum in a foreign land. His friend, Henry Rantzan, of Wansbeck, under whose roof he found a hospitable shelter, was fortunately acquainted with the Emperor Rodolph II., who, to his love of science, added a passion for alchemy and astrology. The reputation of the astronomer having already reached the imperial ear, the recommendation of Rantzau was scarcely necessary to insure him his warmest friendship. Invited by the emperor, he repaired, in 1599, to Prague, where he met with the warmest reception. A pension of three thousand crowns was immediately settled upon him, and a commodious observatory erected for his use in the vicinity of that city. Here the exiled astronomer renewed with delight his interrupted labours, and the gratitude which he cherished for the favour increased the satisfaction which he felt in having so unexpectedly found a resting place for approaching age. These prospects of latter days were enhanced by the good fortune of receiving two such men as Kepler and Longomontanus for his pupils: but the fallacy of human anticipation was here, as in so many other cases, strikingly displayed. Tycho was not aware of the inroads which both his labours and his disappointments had made upon his constitution. Though surrounded with affectionate friends and admiring disciples, he was still an exile in a foreign land. Though his country had been base in its ingratitude, it was yet the land which he loved—the scene of his earliest affections—the theatre of his scientific glory. These feelings continually preyed upon his mind, and his insulted spirit was ever hovering among his native mountains. In this condition he was attacked with a disease of the most painful kind—being dining at the house of a lord named Rosenberg, he had drank more than usual, and not wishing to disturb the company he remained with them and retained his urine, notwithstanding the pressing necessity to the contrary, in consequence of which he was seized with the disorder which terminated his life in twelve days afterwards. He saw that death was approaching, and implored his pupils to persevere in their scientific labours. He conversed with Kepler on some of the most profound points of astronomy, and with these secular occupations he mingled frequent acts of piety and devotion. In this happy condition he expired on the 24th of October, 1601, without pain, at the age of fifty-five, unquestionably the unfortunate victim of the councils of Christian IV.

Notwithstanding the accessions which astronomy had received from the labours of Copernicus and Tycho Brahe, no progress was yet made in developing the general laws of the system, and scarcely an idea had been formed of the power by which the planets were retained in their orbits. The labours of assiduous observers had supplied the materials for this purpose, and Kepler arose to lay the foundations of physical astronomy.

John Kepler was born at Wiel, in Wirtenberg, in 1571. He was educated for the church, and discharged even some of the clerical functions; but his devotion to science withdrew him from the study of theology. Having received mathematical instruction from the celebrated Mæstlinus, he had made such progress in the science, that he was invited in 1594 to fill the mathematical chair of Gratz in Styria. Endowed with a fertile imagination, his mind was ever intent upon subtle and ingenious speculations. In the year 1596, he published his peculiar views in a work on the Harmonies and Analogies of Nature. In this singular production, he attempts to solve what he calls the great cosmographical mystery of the admirable proportion of the planetary orbits; and by means of the six regular geometrical solids, he endeavours to assign a reason why there are six planets; and why the dimensions of their orbits, and the time of their periodical revolutions, were such as Copernicus had found them. If a cube, for example, was inserted in a sphere, of which Saturn's orbit was one of the great circles, it would, he supposed, touch by its six planes the lesser sphere of Jupiter; and, in like manner, he proposes to determine, by the aid of the other geometrical solids, the magnitude of the spheres of the other planets. A copy of this work was presented by its author to Tycho Brahe, who had been too long versed in the severe realities of observation to attach any value to such wild theories. He advised his young friend "first to lay a solid foundation for his views by actual observation, and then, by ascending from these, to strive to reach the causes of things;" and there is reason to think, that, by the aid of the whole Baconian philosophy thus compressed by anticipation into a single sentence, he abandoned for a while his visionary inquiries.

In the year 1598, Kepler suffered persecution for his religious principles, and was compelled to quit Gratz; but though he was recalled by the States of Styria, he felt his situation insecure, and accepted of a pressing invitation from Tycho Brahe to settle at Prague, and assist him in his calculations. Having arrived in Bohemia in 1600, he was introduced by his friends to the Emperor Rodolph, from whom he ever afterwards received the kindest attention. On the death of Tycho in 1601, he was appointed mathematician to the emperor, a situation in which he was continued during the successive reigns of Matthias and Ferdinand; but what was of more importance to science, he was put in possession of the valuable collection of Tycho's observations. These observations were remarkably numerous; and as the orbit of Mars was more oval than that of any of the other planets, they were peculiarly suitable for determining its real form. The notions of harmony and symmetry in the construction of the solar system, which had filled the mind of Kepler, naturally led him to believe that the planets revolved with an uniform motion in circular orbits. So firm, indeed, was this conviction, that he made several attempts to represent the observations of Tycho by this hypothesis. The deviations were too great to be ascribed to errors of observation; and in trying various other curves he was led to the discovery, that Mars revolved round the sun in an eliptical orbit, in one of the foci of which the sun itself was placed. The same observations enabled him to determine the dimensions of the planet's orbit; and by comparing together the times in which Mars passed over different portions of its orbit, he found that they were to one another as the areas described by the lines drawn from the centre of the planet to the centre of the sun, or, in more technical terms, that the radius vector describes equal arcs in equal times. These two remarkable discoveries, the first that were ever made in physical astronomy, were extended to all the other planets of the system, and were communicated to the world in 1609, in his "Commentaries on the Motions of the Planet Mars, as deduced from the Observations of Tycho Brahe."

Although Kepler was conducted to these great laws by the patient examination of well-established facts, his imagination was ever hurrying him among the wilds of conjecture. Convinced that the mean distances of the planets from the sun bore to one another some mysterious relation, he not only compared them with the regular geometrical solids, but also with the intervals of musical tones; an idea which the ancient Pythagoreans had suggested, and which had been adopted by Archimedes himself. All these comparisons were fruitless; and Kepler was about to abandon an inquiry of about seventeen years' duration, when on the 8th of March, 1618, he conceived the idea of comparing the powers of the different numbers which express the planetary distances, in place of the numbers themselves. He compared the squares and the cubes of the distances with the same powers of the periodic times; nay, he tried even the squares of the times with the cubes of the distances; but his hurry and impatience led him into an error of calculation, and he rejected this law as having no existence in nature! On the 15th May, his mind again reverted to the same notion, and upon making the calculations anew, and free from error, he discovered the great law, that the squares of the periodic times of any two planets are to one another as the cubes of their distances from the sun. Enchanted with this unexpected result, he could scarcely trust his calculations; and, to use his own language, he at first believed that he was dreaming, and had taken for granted the very truth of which he was in search. This brilliant discovery was given to the world in 1619 in his "Harmony of the World," a work dedicated to James VI. of Scotland. Thus were established what have been termed the three laws of Kepler—the motion of the planets in elliptical orbits—the proportionality between the areas described and their times of description—and the relations between the squares of the periodic times and the cubes of the distances.

The relation of the movements of the planets to the sun, as the general centre of all their orbits, could not fail to suggest to Kepler, that some power resided in that luminary by which these various motions were produced; and he went so far as to conjecture, that this power diminishes as the square of the distance of the body on which it was exerted; but he immediately rejects this law, and prefers that of the simple distance. In his work on Mars, he speaks of a gravity as a mutual and corporeal affection between similar bodies. He maintained that the tides were occasioned by the moon's attraction, and that the irregularities of the lunar motions, as detected by Tycho Brahe, were owing to the joint actions of the sun and the earth; but the relation between gravity, as exhibited on the earth's surface, and as conducting the planets in their orbits, required more patience of thought than he could command, and was accordingly left for the exercise of higher powers.

The misery in which Kepler lived forms a painful contrast with the services which he performed to science. The pension on which he subsisted was always in arrears, and, though the three emperors whose reigns he adorned, directed their ministers to be more punctual in its payments, the disobedience of their commands was a source of continued vexation to the astronomer. When he retired to Sagan, in Silesia, to spend in retirement the remainder of his days, his pecuniary difficulties became still more harassing. Necessity at last compelled him to apply personally for the arrears which were due; and he accordingly set out in 1630, for Ratisbon; but in consequence of the great fatigue which so long a journey on horseback produced, he was seized with a fever, which carried him off, on the 30th of November, 1630, in the fifty-ninth year of his age.

While Kepler was laying the foundation of physical astronomy, Galileo was busily employed in extending the boundaries of the solar system. This distinguished philosopher was born at Pisa, in 1564. He was the son of a Florentine nobleman, and was educated for the medical profession; but a passion for geometry took possession of his mind, and called forth all his powers. Without the aid of a master he studied the writings of Euclid and of Archimedes, and such were his acquirements, that he was appointed by the Grand Duke of Tuscany to the Mathematical chair of Pisa, in the twenty-fifth year of his age. His opposition to the Aristotelian philosophy gained him many enemies, and at the end of three years he quitted Pisa, and accepted of an invitation to the Professorship of Mathematics at Padua. Here he continued for eighteen years adorning the university by his name, and diffusing around him a taste for the physical sciences. With the exception of some contrivances of inferior importance, Galileo had distinguished himself by no discovery till he had reached the forty-fifth year of his age. In the year 1609, the same year in which Kepler published his celebrated commentary on Mars, Galileo paid a visit to Venice, where he heard, in the course of conversation, that a Dutchman, of the name of Jansens had constructed and presented to Prince Maurice, an instrument through which he saw distant objects magnified and rendered more distinct, as if they had been brought nearer to the observer. This report was credited by some, and disbelieved by others; but, in the course of a few days, Galileo received a letter from a friend in Paris, which placed beyond a doubt the existence of such an instrument. The idea instantly took possession of his mind as one of the utmost importance to science; and so thoroughly was he acquainted with the properties of lenses, that he not only discovered the principle of its construction, but was able to complete a telescope for his own use. Into one end of a leaden tube he fitted a spectacle-glass, plane on one side and convex on the other, and in the other end he placed another spectacle-glass, concave on one side and plane on the other. He then applied his eye to the concave glass, and saw objects "pretty large and pretty near him." They appeared three times nearer, and nine times larger in surface, than to the naked eye. He soon after made another, which represented objects above sixty times larger; and sparing neither labour nor expense, he finally constructed an instrument so excellent, as "to show things almost a thousand times larger, and above thirty times nearer to the naked eye."

There is perhaps no invention that science has presented to man so extraordinary in its nature, and so boundless in its influence, as that of the telescope. To the uninstructed mind, the power of seeing an object at the incredible distance of a thousand miles, as large and nearly as distinct as if it was brought within a mile of the observer, must seem almost miraculous; and to the philosopher, even, who thoroughly comprehends the principles upon which it acts, it must ever appear one of the most elegant applications of science. To have been the first astronomer in whose hands such a gift was placed, was a preference to which Galileo owed much of his future fame.

No sooner had he completed his telescope than he applied it to the heavens, and on the 7th of January, 1608, the first day of its use, he saw, round Jupiter three bright little stars lying in a line parallel to the ecliptic, two to the east, and one to the west of the planet. Regarding them as ordinary stars, he never thought of estimating their distances. On the following day, when he accidentally directed his telescope to Jupiter, he was surprised to see the three stars to the west of the planet. To produce this effect it was requisite that the motion of Jupiter should be direct, though, according to calculation, it was actually retrograde. In this dilemma he waited with impatience for the evening of the 9th, but unfortunately the sky was covered with clouds. On the 10th he saw only two stars to the east—a circumstance which he was no longer able to explain by the motion of Jupiter. He was therefore compelled to ascribe the change to the stars themselves; and upon repeating his observations on the 11th he no longer doubted that he had discovered three planets revolving round Jupiter. On the 13th of January he for the first time saw the fourth satellite.

This discovery, though of the utmost importance in itself, derived an additional value from the light which it threw on the true system of the universe.

While the earth was the only planet enlightened by a moon, it might naturally be supposed that it alone was habitable, and was therefore entitled to the pre-eminence of occupying the centre of the system; but the discovery of four moons round a much larger planet deprived this argument of its force, and created a new analogy between the earth and the other planets. When Kepler received the "Sidereal Messenger," the work in which Galileo announced his discovery in 1610, he perused it with the deepest interest; and while it confirmed and extended his substantial discoveries, it dispelled at the same time some of those harmonic dreams which still hovered among his thoughts. In the "Dissertation" which he published on the discovery of Galileo, he expresses his hope that satellites will be discovered round Saturn and Mars—he conjectures that Jupiter has a motion of rotation about his axis—and states his surprise, that, after what has been written on the subject of telescopes by Baptista Porta, they had not been earlier introduced into observatories.

In continuing his observations Galileo applied his telescope to Venus, and in 1610 he discovered the phases of that planet, which exhibited to him the various forms of the waxing and waning moon. This fact established beyond a doubt that the planet revolved round the sun, and thus gave an additional blow to the Ptolemaic system. In his observations on the sun Galileo discovered his spots, and deduced from them the rotation of the central luminary. He observed that the body of Saturn had handles attached to it, but he was unable to detect the form of its ring, or render visible its minute satellites. On the surface of the moon he discovered her mountains and vallies, and determined the curious fact of her libration, in virtue of which parts of the margin of her disc occasionally appear and disappear. In the Milky Way he descried numerous minute stars which the unassisted eye was unable to perceive; and as the largest fixed stars, in place of being magnified by the telescope, became actually minute brilliant points, he inferred their immense distance as rendered necessary by the Copernican hypothesis. All his discoveries, indeed, furnished fresh arguments in favour of the new system; and the order of the planets, and their relation to a central sun, may now be considered as established by incontrovertible evidence.

While Galileo was occupied with these noble pursuits at Pisa, to which he had been recalled in 1611, his generous patron Cosmo II., Grand Duke of Tuscany, invited him to Florence, that he might pursue with uninterrupted leisure his astronomical observations, and carry on his correspondence with the German astronomers. His fame had now resounded through all Europe;—the strongholds of prejudice and ignorance were unbarred;—and the most obstinate adherents of ancient systems acknowledged the meridian power of the day star of science. Galileo was ambitious of propagating the great truths which he contributed so powerfully to establish. He never doubted that they would be received with gratitude by all—by the philosopher as the consummation of the greatest efforts of human genius—and by the Christian as the most transcendant displays of Almighty power. But he had mistaken the disposition of his species, and the character of the age. That same system of the heavens which had been discovered by the humble ecclesiastic of Frauenberg, which had been patronised by the kindness of a bishop, and published at the expense of a cardinal, and which the pope himself had sanctioned by the warmest reception, was, after the lapse of a hundred years, doomed to the most violent opposition, as subversive of the doctrines of the Christian faith. On no former occasion has the human mind exhibited such a fatal relapse into intolerance. The age itself had improved in liberality;—the persecuted doctrines themselves had become more deserving of reception;—the light of the Reformed faith had driven the Roman Catholics from some of their most obnoxious positions; and yet, under all these circumstances, the Church of Rome unfurled her banner of persecution against the pride of Italy—against the ornament of his species, and against truths immutable and eternal.

In consequence of complaints laid before the Holy Inquisition, Galileo was summoned to appear at Rome, in 1615, to answer for the heretical opinions which he had promulgated. He was charged with "maintaining as true the false doctrine held by many, that the sun was immoveable in the centre of the world, and that the earth revolved with a diurnal motion;—with having certain disciples to whom he taught the same doctrine;—with keeping up a correspondence on the subject with several German mathematicians;—with having published letters on the solar spots, in which he explained the same doctrine as true;—and with having glossed over, with a false interpretation, the passages of Scripture which were urged against it." The consideration of these charges came before a meeting of the Inquisition which assembled on the 25th of February, 1616, and the court declaring their disposition to deal gently with the prisoner, pronounced the following decree:—"That the Cardinal Bellarmine should enjoin Galileo to renounce entirely the above recited false opinions; that on his refusal to do so, he should be commanded by the commissary of the Inquisition to abandon the said doctrine, and to cease to teach and defend it; and that if he did not obey this command, he should be thrown into prison." On the 26th of February, Galileo appeared before Cardinal Bellarmine, and after receiving from him a gentle admonition, he was commanded by the commissary, in the presence of a notary and witnesses, to desist altogether from his erroneous opinions; and it was declared to be unlawful for him in future to teach them in any way whatever, whether orally or in his writings. To these commands Galileo promised obedience, and was dismissed from the Inquisition.

The mildness of this sentence was no doubt partly owing to the influence of the Grand Duke of Tuscany, and other persons of rank and power at the Papal Court, who took a deep interest in the issue of the trial. Dreading, however, that so light a punishment might not have the effect of putting down the obnoxious doctrines, the Inquisition issued a decree denouncing the new opinions as false, and contrary to the sacred writings, and prohibiting the sale of every book in which they should be mentioned.

Thus liberated from his persecutors, Galileo returned to Florence, where he pursued his studies with his wonted diligence and ardour. The recantation of his astronomical opinions was so formal and unreserved, that ordinary prudence, if not a sense of personal honour, should have restrained him from unnecessarily bringing them before the world. No anathema was pronounced against his scientific discoveries; no interdict was laid upon the free exercise of his genius. He was prohibited merely from teaching a doctrine which the Church of Rome considered as injurious to its faith. We might have expected, therefore, that a philosopher, conspicuous in the eyes of the world, would have respected the prejudices, however base, of an institution whose decrees formed part of the law of the land, and which possessed the power of life and death within the limits of its jurisdiction. Galileo, however, thought otherwise. A sense of degradation seems to have urged him to retaliate; and before six years had elapsed, he began to compose his "Cosmical System, or Dialogues on the two greatest Systems in the World, the Ptolomean and the Copernican," the concealed object of which is to establish the opinions which he had promised to abandon. In this work, the subject is discussed by three speakers, Sagredo, Salviatus, and Simplicius, a peripatetic philosopher, who defends the system of Ptolemy with much skill against the overwhelming arguments of the rival disputants. Galileo hoped to escape notice by this indirect mode of propagating the new system, and he obtained permission to publish his work, which appeared at Florence, in 1632.

The Inquisition did not immediately summon Galileo to their presence. Nearly a year elapsed before they gave any indications of their design; and, according to their own statement, they did not even take the subject into consideration till they saw that the obnoxious tenets were every day gaining ground, in consequence of the publication of the Dialogues. They then submitted the work to a careful examination, and having found it to be a direct violation of the injunction which had been formerly intimated to its author, they again cited him before their tribunal, in 1633. The venerable sage, now in his seventieth year, was thus compelled to repair to Rome; and when he arrived, he was committed to the apartments of the Fiscal of the Inquisition. The unchangeable friendship, however, of the Grand Duke of Tuscany obtained a remission of this severity, and Galileo was allowed to reside at the house of the Tuscan ambassador, during the two months which the trial occupied. When brought before the Inquisition, and examined on oath, he acknowledged that the Dialogues were written by himself, and that he obtained permission to publish them, without notifying to the person who gave it that he had been prohibited from holding, defending, or teaching the heretical opinions. He confessed also that the Dialogues were composed in such a manner, that the arguments in favour of the Copernican system, though given as partly false, were yet managed in such a manner, that they were more likely to confirm than overturn its doctrines; but that this error, which was not intentional, arose from the natural desire of making an ingenious defence of false propositions, and of opinions that had the semblance of probability.

After receiving these confessions and excuses, the Inquisition allowed Galileo a proper time for giving in his defence; but this seems to have consisted solely in bringing forward a certificate from Cardinal Bellarmine, which made no allusion to the promise under which Galileo had come, never to defend, nor teach in any way whatever, the Copernican doctrines. The court held this defence to be an aggravation of the crime rather than excuse for it, and proceeded to pronounce a sentence which will be ever memorable in the history of the human mind.

Invoking the name of our Saviour, they declare, that Galileo had made himself liable to the suspicion of heresy, by believing the doctrine, contrary to Scripture, that the sun was the centre of the earth's orbit, and did not move from east to west; and by defending as probable the opinion, that the earth moved, and was not the centre of the world; and that he had thus incurred all the censures and penalties which were enacted by the church against such offences;—but that he should be absolved from these penalties, provided he sincerely abjured and cursed all the errors and heresies contained in the formula of the church, which should be submitted to him. That so grave and pernicious a crime should not pass altogether unpunished, that he might become more cautious in future, and be an example to others to abstain from such offences, they decreed that his Dialogues should be prohibited by a formal edict, that he should be condemned to the prison of the Inquisition during pleasure, and that during the three following years, he should recite once a week the seven penitentiary psalms.

This sentence was subscribed by seven cardinals;

and on the 22nd of June, 1633, Galileo signed an abjuration, humiliating to himself and degrading to philosophy. At the age of seventy, on his bended knees, and with his right hand resting on the holy Evangelists, did this patriarch of science avow his present and his past belief in all the dogmas of the Romish Church, abandon as false and heretical the doctrine of the earth's motion and of the sun's immobility, and pledge himself to denounce to the Inquisition any other person who was even suspected of heresy. He abjured, cursed, and detested, those eternal and immutable truths which the Almighty had permitted him to be the first to establish. What a mortifying picture of moral depravity and intellectual weakness! If the unholy zeal of the assembly of Cardinals has been branded with infamy, what must we think of the venerable sage whose gray hairs were entwined with the chaplet of immortality, quailing under the fear of man, and sacrificing the convictions of his conscience, and the deductions of his reason, at the altar of a base superstition? Had Galileo but added the courage of the martyr to the wisdom of the sage—had he carried the glance of his indignant eye round the circle of his judges—had he lifted his hands to heaven, and called the living God to witness the truth and immortality of his opinions, the bigotry of his enemies would have been disarmed, and science would have enjoyed a memorable triumph.

The great truths of the Copernican system, instead of being considered as heretical, had been actually adopted by many pious members of the Roman Church, and even some of its dignitaries did not scruple to defend it openly. Previous to the first persecution of Galileo in 1615, a Neapolitan nobleman, Vincenzio Caraffa, a person equally distinguished by his piety and birth, had solicited Paul Anthony Foscarinus, a learned Carmelite monk, to illustrate and defend the new system of the universe.

With this request the ecclesiastic speedily complied; and in the pamphlet which he completed on the 6th of January, 1615, he defends the Copernican system with much boldness and ingenuity; he reconciles the various passages of Scripture with the new doctrine, and he expresses the hope that such an attempt, now made for the first time, will prove agreeable to the philosophers, but particularly to those very learned men, Galileo Galilei, John Kepler, and all the members of the Lyncean Academy, who, he believes, entertain the same opinion. This remarkable production, written from the convent of the Carmelites at Naples, is dedicated to the very reverend Sebastian Fantoni, general of the order of Carmelites, and was published at Florence, with the sanction of the ecclesiastical authorities in 1630, three years before the second persecution of Galileo.

It would be interesting to know the state of public feeling in Italy when Galileo was doomed to the prisons of the Inquisition. No appeal seems to have been made against so cruel a sentence, and neither in remonstrance nor in derision does an individual voice seem to have been raised. The master of the spirits of the age looked with sullen indifference on the persecution of exalted genius; and Galileo lay in chains deserted and unpitied. This unrebuked triumph of his enemies was perhaps favourable to the object of their vengeance. Resistance might have heightened the rigour of a sentence, which submission seems to have alleviated. The interference of some eminent individuals of Rome, among whom we have no doubt that the Grand Duke of Tuscany was the most influential, induced the Pope not only to shorten the period, but to soften the rigour of Galileo's imprisonment. From the dungeon of the Inquisition, where he had remained only four days, he was removed to the ambassador's palace; and when his health began to suffer, he was permitted to leave Rome; and would have been allowed to return to Florence, but as the plague raged in that city, he was sent to the residence of the Archbishop Piccolimini, where he carried on and completed his valuable investigations respecting the resistance of solids. Here he continued five months, when, in consequence of the disappearance of the plague at Florence, he was allowed to retire to his villa at Bellosguardo, and afterwards to that of Arcetri, in the vicinity of Florence.

Though Galileo was now, to a certain degree, liberated from the power of man, yet the afflicting dispensations of Providence began to fall thickly around him. No sooner had he retired to Arcetri, than his favourite daughter was seized with a dangerous illness, which quickly terminated in her death. He was himself attacked with a complication of disorders, and the most oppressive melancholy; and though he solicited permission to repair to Florence for medical assistance, yet this was denied him. In 1631, however, the Pope permitted him to pay a visit to Florence, and his friend, Father Castelli, was allowed to visit him, attended by an officer of the Inquisition. But this indulgence was soon withdrawn, and at the end of a few months he was ordered to return to Arcetri. The sight of his right eye had begun to fail him in 1636; in 1637, his left eye was similarly attacked, so that in a few months he was affected with total and incurable blindness. Before this calamity had supervened, he had noticed the curious phenomenon of the moon's libration, in consequence of which, parts of her visible disc that are exposed to view at one time are withdrawn at another. He succeeded in explaining two of the causes of this curious phenomenon, viz., the different distances of the observer from the line joining the centre of the earth and the moon, which produces the diurnal libration, and the unequal motion of the moon in her orbit, which produces the libration in longitude. It was left, however, to Helvetius to discover the libration in latitude, which arises from the inclination of her axis being a little less than a right angle to the ecliptic; and Lagrange to discover the spheroidal libration, or that which arises from the action of the earth upon the lunar spheroid.

The sorrows with which Galileo was now beset, seem to have disarmed the severity of the Inquisition. He was freely permitted to enjoy the society of his friends, who now thronged around him to express their respect and their sympathy. The Grand Duke of Tuscany was his frequent visitor, and Gassendi, Deodati, and our celebrated countryman, Milton, went to Italy for the purpose of visiting him. He entertained his friends with the warmest hospitality, and though simple and abstemious in his diet, yet he was fond of good wine, and seems even in his best days to have paid particular attention to the excellence of his cellar.

Although Galileo had nearly lost his hearing as well as sight, yet his intellectual faculties remained unimpaired; and while his mind was occupied in considering the force of percussion, he was seized with fever and palpitation of the heart, which after two months' illness, terminated his life on the 8th of January, 1642.

Among the predecessors of Newton in astronomical research we must not omit the names of Bouillaud, Borelli, and Dr. Hooke. Bouillaud, the author of several valuable astronomical works, was a native of France, and has derived more reputation from a single sentence in his "Astronomica," published in 1645, than from all the rest of his labours. He was not a believer in the doctrine of attraction; but in speaking of that power as the cause of planetary motions, he remarks, "that if attraction existed, it would decrease as the square of the distance." The influence was still more distinctly developed by Borelli, a Neapolitan philosopher, who, in 1666, published a work on the satellites of Jupiter. In this work he maintains, that all the planets perform their motions round the sun according to a general law; that the satellites of Jupiter and Saturn move round their primary planets in the same manner as the moon does round the earth, and that they all revolve round the sun, which is the only source of any virtue, and that this virtue attaches them, and unites them so that they cannot recede from their centre of action.

Dr. Hooke seems to have devoted much of his attention to the cause of the planetary motions. On the 21st March, 1666, he read to the Royal Society an account of a series of experiments for determining if bodies experience any deviation in their weight at different distances from the centre of the earth. His experiments were by no means satisfactory to himself, and hence he was led to the ingenious idea of measuring the force of gravity, by observing, at different altitudes, the rate of a pendulum clock. About two months afterwards, he exhibited to the Society an approximate representation of the forces which maintain the planets in their orbits, in the paths described by a circular pendulum impelled with different degrees of force; but though this experiment illustrated the production of a curvilinear motion, by combining a tangential force with a central force of attraction, yet it was only an illustration, and could not lead to the true cause of the planetary motions. Eight years after, however, he resumed the subject, and published a dissertation, entitled "an attempt to prove the motion of the earth from observation;" which has been considered as a remarkable production by the philosophers of every country.

# CHAPTER V.

Newton's first speculations on Gravity. He discovers the true law of Gravity and the cause of the Planetary Motions. His principles of Natural Philosophy. His "Principia" appears in 1687. Account of it and the Discoveries it contains. They meet with great opposition. The reception of the Newtonian Philosophy. Doctrine of Infinite Quantities. Discovers the Binomial Theorem. Doctrine of Fluxions. His Mathematical Tracts. His Universal Arithmetic. Account of the celebrated Controversy respecting the Invention of Fluxions.

When the plague had driven Newton from Cambridge, in 1666, he was sitting alone in the garden at Woolsthorpe, and reflecting on the nature of gravity, that remarkable power which causes all bodies to descend to the centre of the earth. As this power is not found to suffer any sensible diminution at the greatest distance from the earth's centre to which we can reach, being as powerful at the summit of the highest mountain as at the bottom of the deepest mine, he conceived it highly probable that it must extend much farther than was usually supposed. No sooner had this happy conjecture occurred to his mind than he considered what would be the effect of its extending as far as the moon. That her motion must be influenced by such a power, he did not for a moment doubt; and a little reflection convinced him that it might be sufficient for retaining that luminary in her orbit round the earth. Though the force of gravity suffers no sensible diminution at those small distances from the earth's centre at which we can place ourselves, yet he thought it very possible that, at the distance of the moon, it might differ much in strength from what it is on the earth. In order to form some estimate of the degree of its diminution, he considered that if the moon be retained in her orbit by the force of gravity, the primary planets must also be carried round the sun by the same power; and by comparing the periods of the different planets, with their distances from the sun, he found that if they were retained in their orbits by any power like gravity, its force must decrease in the duplicate proportion, or as the squares of their distances from the sun. In drawing this conclusion, he supposed the planets to move in orbits perfectly circular, and having the sun in their centre. Having thus obtained the law of the force by which the planets were drawn to the sun, his next object was to ascertain if such a force emanating from the earth, and directed to the moon, was sufficient, when diminished in the duplicate ratio of the distance, to retain her in her orbit. In performing this calculation, it was necessary to compare the space through which heavy bodies fall in a second of time at a given distance from the earth, viz., at its surface, with the space through which the moon, as it were, falls to the earth in a second of time while revolving in a circular orbit. Being at a distance from books when he made this computation, he adopted the common estimate of the earth's diameter, then in use among geographers and navigators, and each degree of latitude contained 60 English miles. In this way he found that the force which retains the moon in her orbit, as deduced from the force which occasions the fall of heavy bodies to the earth's surface, was one-sixth greater than what is actually observed in her circular orbit. This difference threw a doubt upon all his speculations; but unwilling to abandon what seemed to be otherwise so plausible, he endeavoured to account for the difference of the two forces, by supposing that some other cause must have been united with the force of gravity in producing so great a velocity of the moon in her circular orbit. As this new cause, however, was beyond the reach of observation, he discontinued all further inquiries into the subject, and concealed from his friends the speculations in which he had been employed.

After his return to Cambridge his attention was occupied with those optical discoveries which we have already noticed; but he had no sooner brought them to a close than his mind reverted to the subject of the planetary motions. Upon the death of Oldenburg, in 1678, Doctor Hooke was appointed secretary to the Royal Society; and as this distinguished body had requested the opinion of Newton about the system of physical astronomy, he addressed a letter to the secretary, on the 28th of November, 1679, in which he proposed a direct experiment for verifying the motion of the earth, namely, by observing whether or not bodies that fall from a considerable height descend in a vertical direction, for if the earth were at rest, the body would describe a vertical line, whereas if it revolved round its axis, the falling body must deviate from the vertical line towards the east. The Royal Society attached great value to this idea, and appointed Dr. Hooke to put it to the test of experiment. Being thus led to consider the subject more attentively, he wrote to Newton, that where ever the direction of gravity was oblique to the axis on which the earth revolved, that is, in every part of the earth except the equator, falling bodies should approach to the equator, and the deviation from the vertical, in place of being exactly to the east, as Newton maintained, should be to the south-east of the point from which the body began to move. Newton acknowledged that this conclusion was correct in theory, and Dr. Hooke is said to have given an experimental demonstration of it before the Royal Society, in 1679. Newton had erroneously concluded, that the path of the falling body would be a spiral; but Dr. Hooke, on the same occasion on which he made the preceding experiment, read a paper to the Society in which he proved that the path of the body would be an eccentric ellipse in the vacuo, and an elliptic spiral, if the body moved in a resisting medium.

This correction of Newton's error, and the discovery that a projectile would move in an elliptical orbit when under the influence of a force varying in the inverse ratio of the square of the distance, led Newton to discover "the theorem by which he afterwards examined the ellipses," and to demonstrate the celebrated proposition, that a planet acted upon by an attractive force varying inversely as the squares of the distances, will describe an elliptical orbit in one of whose foci the attractive force resides.

But though Newton had thus discovered the true cause of all the celestial motions, he did not yet possess any evidence that such a force actually resided in the sun and planets. The failure of his former attempt, to identify the law of falling bodies, at the earth's surface, with that which guided the moon in her orbit, threw a doubt over all his speculations, and prevented him from giving any account of them to the public.

An accident, however, of a very interesting nature, induced him to resume his former inquiries, and enabled him to bring them to a close. In June, 1682, when he was attending a meeting of the Royal Society of London, the measurement of a degree of meridian, executed by M. Picard, became the subject of conversation. Newton took a memorandum of the result obtained by the French astronomer, and having deduced from it the diameter of the earth, he immediately resumed his former calculations, and began to repeat it with these new data, In the progress of the calculation, he saw that the result which he had formerly expected, was likely to be produced, and he was thrown into such a state of nervous irritability, that he was unable to carry it on. In this state of mind, he entrusted it to one of his friends, and he had the high satisfaction of finding his former views amply realized. The force of gravity which regulated the fall of bodies at the earth's surface, when diminished as the square of the moon's distance from the earth, was found to be almost exactly equal to the centrifugal force of the moon, as deduced from her observed distance and velocity.

The influence of such a result upon such a mind may be more easily conceived than described. The whole material universe was spread out before him;—the sun with all his attending planets—the planets with all their satellites—the comets wheeling in every direction in their eccentric orbits, and the systems of the fixed stars stretching to the remotest limits of space. All the varied and complicated movements of the heavens, in short, must have been at once presented to his mind as the necessary result of that law which he had established in reference to the earth and the moon.

After extending this law to the other bodies of the system, he composed a series of propositions on the motion of the primary planets about the sun, which were sent to London about the end of 1683, and were soon afterwards communicated to the Royal Society.

About this period other philosophers had been occupied with the same subject. Sir Christopher Wren had many years before endeavoured to explain the planetary motions "by the composition of a descent towards the sun, and an impressed motion; but he at length gave it over, not finding the means of doing it. In January, 1684, Dr. Halley had concluded that the centrepital force decreased in the reciprocal proportion of the squares of the distances, and having one day met Sir Christopher Wren and Dr. Hooke, the latter affirmed that he had demonstrated upon that principle all the laws of the celestial motions. Dr. Halley confessed that his attempts were unsuccessful, and Sir Christopher, in order to encourage the inquiry, offered to present a book of forty shillings value, to either of the two philosophers who should, in the space of two months, bring him a convincing demonstration of it. Hooke persisted in the declaration, that he possessed the method, but avowed it to be his intention to conceal it for some time.

In August, 1684, Dr. Halley went to Cambridge for the express purpose of consulting Newton on this interesting subject. Newton assured him that he had brought this demonstration to perfection, and promised him a copy of it. This copy was received in November, by the Doctor, who made a second visit to Cambridge, in order to induce its author to have it inserted in the register-book of the Royal Society. On the 10th of December, Dr. Halley intimated to the Society, that they had seen at Cambridge Mr. Newton's treatise, "De Motu Corporum," which he had promised to send to the Society to be entered upon their register; and Dr. Halley was desired to unite with Mr. Paget, Master of the Mathematical School in Christ's Hospital, in reminding Mr. Newton of his promise, "for securing the invention to himself, till such time as he can be at leisure to publish it." On the 25th of February the secretary communicated a letter from Mr. Newton, in which he expresses his willingness "to enter in the register his notions about motion, and his intentions to fit them suddenly for the press." The progress of his work was interrupted, however, by a visit of some weeks which he made in Lincolnshire; but he proceeded with such diligence on his return, that he was able to transmit the manuscript to London before the end of April. This manuscript, entitled Philosophiæ Naturalis Principia Mathematica, and dedicated to the Society, was presented by Dr. Vincent on the 28th of April, 1686, when Sir John Hoskins, the vice-president, and the particular friend of Dr. Hooke, was in the chair. Dr. Vincent passed a just encomium on the novelty and dignity of the subject; and another member added, that "Mr. Newton had carried the thing so far, that there was no more to be added." Dr. Hooke took offence at these remarks, and blamed Sir John for not having mentioned "what he had discovered to him;" but the vice-president did not recollect any such communication, and the consequence was, that "these two, who, till then, were inseparable cronies, have since scarcely seen one another, and are utterly fallen out." After the breaking up of the meeting the Society adjourned to the coffee-house, when Dr. Hooke stated, that he not only had made the same discovery, but had given the first hint of it to Newton.

An account of these proceedings was communicated to Newton through two different channels. In a letter dated May 22nd, Dr. Halley wrote to him, "that Dr. Hooke has some pretensions upon the invention of the rule of the decrease of gravity being reciprocally at the squares of the distances from the centre. He says you had the notion from him, though he owns the demonstration of the curves generated thereby to be wholly your own. How much of this is so, you know best, as likewise what you have to do in this matter. Only Mr. Hooke seems to expect you would make some mention of him in the preface, which it is possible you may see reason to prefix."

This communication from Dr. Halley induced our author, on the 20th of June, to address a long letter to him, in which he gives a minute and able refutation of Hooke's claims; but before this letter was despatched, another correspondent, who had received his information from one of the members who had been present, informed Newton, "that Hooke made a great stir, pretending that he had all from him, and desiring they would see that he had justice done him." This fresh charge seems to have ruffled the tranquillity of Newton; and he accordingly added an angry and satirical postscript, in which he treats Hooke with very little ceremony, and goes so far as to conjecture that he might have acquired his knowledge of the law from a letter of his own to Huygens, directed to Oldenburg, and dated January 14th, 1672-3. "My letter to Hugenius was directed to Mr. Oldenburg, who used to keep the originals. His papers came into Mr. Hooke's possession, Mr. Hooke, knowing my hand, might have the curiosity to look into that letter, and there take the notion of comparing the forces of the planets arising from their circular motion; and so what he wrote to me afterwards about the rate of gravity might be nothing but the fruit of my own garden."

In replying to this letter, Dr. Halley assured him that Hooke's "manner of claiming the discovery had been represented to him in worse colours than it ought, and that he neither made public application to the Society for justice, nor pretended that you had all from him." The effect of this assurance was to make Newton regret that he had written the angry postscript to his letter; and in replying to Halley, he not only expressed his regret, but recounts the different new ideas which he had acquired from Hooke's correspondence, and suggests it as the best method of "compromising the present dispute," to add a scholium, in which Wren, Hooke, and Halley are acknowledged to have independently deduced the law of gravity from the second law of Kepler.

At the meeting at which the manuscript of the "Principia" was presented to the Royal Society, it was agreed that the printing of it should be referred to the Council, that a letter of thanks should be written to its author; and at a meeting of the Council it was resolved that the work should be printed at the Society's expense, and that Dr. Halley should superintend it while going through the press. These resolutions were communicated by Dr. Halley, and in Newton's reply he makes the following observations:

"The proof you sent me I like very well. I designed the whole to consist of three books; the second was finished last summer, being short, and only wants transcribing, and drawing the cuts fairly. Some new propositions I have since thought on, which I can as well let alone. The third wants the theory of comets. In autumn last I spent two months in calculation to no purpose for want of a good method, which made me afterwards return to the first book, and enlarge it with diverse propositions, some relating to comets, others to other things found out last winter. The third I now design to suppress. Philosophy is such an importinently letigious lady that a man had as good be engaged in law-suits as have to do with her. I found it so formerly, and now I can no sooner come near her again but she gives me warning. The two first books without the third, will not so well bear the title of "Philosophiæ Naturalis Principia Mathematica;" and therefore I had altered it to this, "De Motu Corporum Libri duo." But after second thoughts I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours."

In replying to this letter, Dr. Halley regrets that Newton's tranquillity should have been thus disturbed by envious rivals; and implores him in the name of the Society not to suppress the third book. He says "I must again beg you not to let your resentment run so high as to deprive us of your third book, wherein your applications of your mathematical doctrine to the theory of comets, and several curious experiments which, as I guess by what you write ought to compose it, will undoubtedly render it acceptable to those who will call themselves philosophers without mathematics, which are much the greater number."

To these solicitations Newton seems to have readily yielded. His second book was sent to the Society and presented on the 2nd March, 1686-7.

The third book was also transmitted, and presented on the 6th April, and the whole work was completed and published in May, 1687.

Such is a brief account of the publication of a work which is memorable not only in the annals of one science or of one country, but which will form an epoch in the history of the world, and will ever be regarded as the brightest page in the records of human reason.

The "Principia" consists of three books. The two first, which occupy three fourths of the work, are entitled, "On the Motion of Bodies;" and the third bears the title, "On the System of the World." The first and second books contain the mathematical Principles of Philosophy viz., the laws and conditions of motions and forces; and they are illustrated with several philosophical scholia which treat of some of the most general and best established points in philosophy, such as the density and resistance of bodies, spaces void of matter, and the motion of sound and light. The object of the third book is to deduce from these principles the constitution of the system of the world; and this book has been drawn up in as popular a style as posssible, in order that it may be generally read.

The great discovery which characterises the Principia is that of the principle of universal gravitation, as deduced from the motion of the moon, and from the three great facts or laws discovered by Kepler. This principle is "that every particle of matter is attracted by, or gravitated to, every other particle of matter with a force inversely proportional to the squares of their distances."

From the first law of Kepler, namely, the proportionality of the areas to the times of their description, Newton inferred that the force which kept the planet in its orbit was always directed to the sun; and from the second law of Kepler, that every planet moves in an ellipse with the sun in one of its foci, he drew the still more general inference, that the force by which the planet moves round that focus varies inversely as the square of its distance from the focus. As this law was true in the motion of satellites round their primary planets, Newton deduced the equality of gravity in all the heavenly bodies towards the sun, upon the supposition that they are equally distant from its centre, and in the case of terrestrial bodies, he succeeded in verifying this truth by numerous and accurate experiments.

By taking a more general view of the subject, Newton demonstrated that a conic section was the only curve in which a body could move when acted upon by a force varying inversely as the square of the distance; and he established the conditions depending on the velocity and the primitive position of the body, which were requisite to make it describe a circular, an elliptical, a parabolic, or a hyperbolic orbit.

Notwithstanding the generality and importance of these results, it still remained to be determined whether the focus resided in the centres of the planets, or belonged to each individual particle of which they were composed. Newton removed this uncertainty by demonstrating, that, if a spherical body acts upon a distant body with a force varying as the distance of this body from the centre of the sphere, the same effect will be produced, as if each of its particles acted upon the distant body according to the same law. And hence it follows that the spheres, whether they are of uniform diversity, or consist of concentric layers, with densities varying according to any law whatever, will act upon each other in the same manner as if their force resided in their centres alone. But as the bodies of the solar system are very nearly spherical, they will act upon one another, and upon bodies placed on their surface, as if they were so many centres of attraction; and therefore we obtain the law of gravity which subsists between spherical bodies, namely, that one sphere will act upon another with a force directly proportioned to the quantity of matter, and inversely as the square of the distance between the centres of the spheres. From the equality of action and reaction, to which no exception can be found, Newton concluded that the sun gravitated to the planets, and the planets to their satellites; and the earth itself to the stone which falls upon its surface, and, consequently, that the two mutually gravitating bodies approached to one another with velocities inversely proportional to their quantities of matter.

Having established this universal law, Newton was enabled not only to determine the weight which the same body would have at the surface of the sun and the planets, but even to calculate the quantity of matter in the sun, and in all the planets that had satellites, and even to determine the density or specific gravity of the matter of which they were composed. In this way he found that the weight of the same body would be twenty-three times greater at the surface of the sun than at the surface of the earth, and that the density of the earth was four times greater than that of the sun, the planets increasing in density as they receded from the centre of the system.

If the peculiar genius of Newton has been displayed in his investigation of the law of universal gravitation, it shines with no less lustre in the patience and sagacity with which he traced the consequences of this fertile principle.

The discovery of the spheroidal form of Jupiter by Cassini had probably directed the attention of Newton to the determination of its cause, and consequently to the investigation of the true figure of the earth. The spheroidal form of the planets had been ascribed by Copernicus to the gravity, or natural appetency of their parts; but upon considering the earth as a body revolving upon its axis, Newton quickly saw that the figure arising from the mutual attraction of its parts must be modified by another force arising from its volition. When a body revolves upon an axis, the velocity of rotation increases from the poles, where it is nothing, to the equator, where it is a maximum. In consequence of this velocity, the bodies on the earth's surface have a tendency to fly off from it, and this tendency increases with the velocity. Hence arises a centrifugal force which acts in combination with the force of gravity, and which Newton found to be the 289th part of the force of gravity at the equator, and decreasing, as the cosine of the latitude, from the equator to the poles. The great predominance of gravity over the centrifugal force prevents the latter from carrying off any bodies from the earth's surface, but the weight of all bodies is diminished by the centrifugal force, so that the weight of any body is greater at the poles than it is at the equator. If we now suppose the waters at the pole to communicate with those of the equator by means of a canal, one branch of which goes from the pole to the centre of the earth, and the other from the centre of the earth to the equator, then the polar branch of the canal will be heavier than the equatorial branch, in consequence of its weight not being diminished by the centrifugal force; and, therefore that the two columns may be in equilibrio, and the equatorial one may be lengthened. Newton found that the length of the polar must be to that of the equatorial canal as 229 to 230, or that the earth's polar radius must be seventeen miles less than its equatorial radius—that is, that the figure of the earth is an oblate spheroid, formed by the revolution of an ellipse round its lesser axis. Hence it follows that the intensity of gravity at any point of the earth's surface is in the inverse ratio of the distance of that point from the centre, and, consequently, that it diminishes from the equator to the poles—a result which he confirmed by the fact, that clocks required to have their pendulums shortened in order to beat true time, when carried from Europe towards the equator.

The next subject to which Newton applied the principle of gravity was the tides of the ocean. The philosophers of all ages had recognised the connection between the phenomena of the tides, and the position of the moon. The College of Jesuits, at Coimbra, and subsequently Antonio de Dominis and Kepler, distinctly referred the tides to the attraction of the earth by the moon; but so imperfect was the explanation which was thus given of the phenomenon, that Galileo ridiculed the idea of lunar attraction, and substituted for it a fallacious explanation of his own. That the moon is the principal cause of the tides, is obvious from the well-known fact, that it is high-water at any given place about the time when she is in the meridian of that place; and that the sun performs a secondary part in their production may be proved from the circumstance, that the highest tides take place when the sun, the moon, and the earth are in the same straight line—that is, when the force of the sun conspires with that of the moon; and that the lowest tides take place when the lines drawn from the sun and moon to the earth are at right angles to each other—that is, when the force of the sun acts in opposition to that of the moon. The most perplexing phenomenon in the tides of the ocean, and one which is still a stumbling-block to persons slightly acquainted with the theory of attraction, is the existence of high water on the side of the earth opposite to the moon, as well as on the side next the moon. To maintain that the attraction of the moon at the same instant draws the waters of the ocean towards herself, and also draws them from the earth in an opposite direction, seems, at first sight paradoxical; but the difficulty vanishes when we consider the earth, or rather the centre of the earth, and the water on each side of it as three distinct bodies placed at different distances from the moon, and consequently attracted with forces inversely proportional to the squares of their distances. The water nearest the moon will be much more powerfully attracted than the centre of the earth, and the centre of the earth more powerfully attracted than the water farthest from the moon. The consequence of this must be, that the waters nearest the moon will be drawn away from the centre of the earth, and will consequently rise from their level, while the centre of the earth will be drawn away from the waters opposite the moon, which will, as it were, be left behind, and consequently be in the same situation as if they were raised from the earth in a direction opposite to that in which they are attracted by the moon. Hence the effect of the moon's motion upon the earth is to draw its fluid parts into the form of an oblong spheroid, the axis of which passes through the moon. As the action of the sun will produce the very same effect, though in a smaller degree, the tide at any place will depend on the relative position of these two spheroids, and will be always equal either to the sum or to the difference of the effects of the two luminaries. At the time of new and full moon the two spheroids will have their axis coincident, and the height of the tide, which will then be a *spring* one, will be equal to the sum of the elevations produced in each spheroid considered separately, while at the first and third quarters the axis of the spheroids will be at right angles to each other, and the height of the tide, which will then be a *neap* one, will be equal to the difference of the elevations produced in each separate spheroid. By comparing the spring and neap tides, Newton found that the force with which the moon acted upon the waters of the earth was to that with which the sun acted upon them as 4.48 to 1; that the force of the moon produced a tide of 8.63 feet, that of the sun of 1.93 feet; and both of them combined, one of 10 feet and a half French measure,—a result which in the open sea does not deviate much from observation. Having thus ascertained the force of the moon upon the waters of our globe, he found that the quantity of matter in the moon was to that in the earth as 1 to 40, and the density of the moon to that of the earth as 11 to 9.

The motions of the moon, so much within the reach of our own observation, presented a fine field for the application of the theory of universal gravitation. The irregularities exhibited in the lunar motions had been known in the time of Hipparchus and Ptolemy. Tycho Brahe had discovered the great inequality called the variation, amounting to 37 minutes, and depending on the alternate acceleration and retardation of the moon in every quarter of a revolution, and he had also ascertained the existence of the annual equation. Of these two inequalities Newton gave a most satisfactory explanation. The action of the sun upon the moon may be always resolved into two, one acting in the direction of the line joining the moon and the earth, and consequently tending to increase or diminish the moon's gravity to the earth, and the other in a direction at right angles to this, and consequently tending to accelerate or retard the motion in her orbit. Now, it was found by Newton that this last force was reduced to nothing, or vanished at the syzigies or quadratures, so that at these four points the moon described areas proportionable to the times. The instant, however, that the moon quits these positions, the force under consideration, which we may call the tangential force, begins, and it reaches its maximum in the four octants. The force, therefore, compounded of these two elements of the solar force, or the diagonal of the parallelogram which they form, is no longer directed to the earth's centre, but deviates from it at a maximum about 30 minutes, and therefore affects the angular motion of the moon, the motion being accelerated in passing from the quadratures to the syzigies, and retarded in passing from the syzigies to the quadratures. Hence the velocity is in its mean state in the octants, a maximum in the syzigies, and a minimum in the quadratures.

Upon considering the influence of the solar force in diminishing or increasing the moon's gravity to the earth, Newton saw that her distance and her periodic time must from this cause be subject to change, and in this way he accounted for the annual equation observed by Brahe. By the application of similar principles, he explained the cause of the motion of the apsides, or of the greater axis of the moon's orbit, which has an angular progressive motion of three degrees four minutes nearly in the course of one lunation, and he showed that the retrogadation of the nodes, amounting to three minutes ten seconds daily, arose from one of the elements of the solar force being exerted in the plane of the ecliptic, and thus cause the line of the nodes, or the intersection of these two planes, to move in a direction opposite to that of the moon. The lunar theory thus blocked out by Newton, required for its completion the labours of another century. The imperfections of the fluxionary calculus prevented him from explaining the other inequalities of the moon's motions, and it was reserved to Euler, D'Alembert, Clairant, Mayer, and Laplace to bring the lunar tables to a high degree of perfection, and to enable the navigator to determine his longitude at sea with a degree of precision which the most sanguine astronomer could scarcely have anticipated.

By the consideration of the retrogade motion of the moon's nodes, Newton was led to discover the cause of the remarkable phenomenon of the procession of the equinoctial points which moved fifty seconds annually, and completed the circuit of the heavens in

25,920 years. Kepler declared himself unable to assign any cause for this motion, and we do not think any other person ever made the attempt. From the spheroidal form of the earth, it may regarded as a sphere with a spheroidal ring surrounding its equator, one half of the ring being above the plane of the ecliptic, and the other half below. Considering this excess of matter as a system of satellites adhering to the earth's surface, Newton saw that the combined actions of the sun and moon upon these satellites tended to produce a retrogadation with the nodes of the circles which they described in their diurnal rotation, and that the sum of all the tendencies being communicated to the whole mass of the planet, ought to produce a slow retrogadation of the equinoxial points. The effect produced by the motion of the sun he found to be forty seconds, and that produced by the action of the moon ten seconds.

Although there could be little doubt that the comets were retained in their orbits by the same laws which regulated the motions of the planets, yet it was difficult to put this opinion to the test of observation. The visibility of comets only in a small part of their orbits rendered it difficult to ascertain their distance and periodic time, and as their periods were probably of great length, it was impossible to correct approximate results by repeated observation. Nowton, however, removed this difficulty, by showing how to determine the orbits of a comet, namely, the form and position of the orbit, and the periodic time, by three observations. By applying this method to the comet of 1680, he calculated the elements of its orbit, and from the agreement of the computed places with those which were observed, he justly inferred that the motion of comets were regulated by the same laws as those of the planetary bodies. This result was one of great importance; for as the comets enter our system in every possible direction, and at all angles with the ecliptic; and, as a great part of their orbits extend far beyond the limits of the solar system, it demonstrated the existence of gravity in spaces far removed beyond the planet, and proved that the law of the inverse ratio of the squares of the distance was true in every possible direction, and at very remote distances from the centre of our system.

Such is a brief view of the leading discoveries which the "Principia" first announced to the world. The grandeur of the subjects of which it treats, the beautiful simplicity which it unfolds, the clear and concise reasoning by which that system is explained, and the irresistible evidence by which it is supported, might have insured it the warmest admiration of contemporary mathematicians, and the most welcome reception in all the schools of philosophy throughout Europe. This, however, is not the way in which great truths are generally received. Though the astronomical discoveries of Newton were not assailed by the class of ignorant pretenders who attacked his optical writings, yet they were everywhere resisted by the errors and prejudices which had taken a deep hold even of the strongest minds. And, notwithstanding a few examples that might be quoted, we must admit the truth of the remark of Voltaire, that "though Newton survived the publication of the 'Principia' more than forty years, yet at the time of his death, he had not above twenty followers out of England."

With regard to the progress of the Newtonian philosophy in Great Britain, some difference of opinion has been entertained. Professor Playfair gives the following account of it. "In the universities of England, though the Aristotelian physics had made an obstinate resistance, they had been supplanted by the Cartesian, which became firmly established about the time when their foundation began to be sapped by the general progress of science, and particularly by the discoveries of Newton. For more than thirty years after the publication of these discoveries, the system of vortices kept its ground, and a translation from the French into Latin of the *Physics* of Rohault—a work entirely Cartesian, continued at Cambridge to be the text for philosophical instruction. About the year 1718, a new and more elegant translation of the same book was published by Dr. Samuel Clarke, with the addition of notes, in which that profound and ingenious writer explained the views of Newton on the principal objects of discussion, so that the notes contained virtually a refutation of the text; they did so, however, only virtually, all appearance of argument and controversy being carefully avoided. Whether this escaped the notice of the learned Doctor or not, is uncertain; but the new translation, from its better Latinity, and the name of the editor, was readily admitted to all the academical honours which the old one had enjoyed. Thus the stratagem of Dr. Clarke completely succeeded; the tutor might predict from the text, but the pupil would sometimes look into the notes, and error is never so sure of being exposed, as when the truth is placed close to it, side by side, without anything to alarm prejudice, or awaken from its lethargy the dread of innovation. Thus, therefore, the Newtonian philosophy first entered the university of Cambridge under the protection of the Cartesian." To this passage the learned professor adds the following as a note.

"The universities of St. Andrews and Edinburgh were, I believe, the first in Britain where the Newtonian philosophy was made the subject of the academical prelictions. For this distinction they are indebted to James and David Gregory, the first in some respects the rival, but both the friends of Newton. Whiston bewails, in the anguish of his heart, the difference, in this respect, between those universities and his own. David Gregory taught in Edinburgh for several years prior to 1690, when he removed to

Oxford; and Whiston says, 'he had already caused several of his scholars to keep acts, as we call them, upon several branches of the Newtonian philosophy, while we, at Cambridge, poor wretches, were ignominiously studying the fictitious hypothesis of the Cartesians.' I do not, however, mean to say, that from this date the Cartesian philosophy was expelled from these universities; the Physics of Rohault were still in use as a text-book—at least occasionally, to a much later period than this, and a great deal, no doubt, depended on the character of the individual. Professor Keill introduced the Newtonian philosophy in his lectures at Oxford in 1697; but the instructions of the tutors, which constitute the real and efficient system of the university were not cast into the mould till long afterwards." Mr. Dugald Stewart, adopting the same view of the subject, has stated, "That the philosophy of Newton was publicly taught by David Gregory at Edinburgh, and by his brother James Gregory, at St. Andrews, before it was able to supplant the vortices of Descartes, in the very university of which Newton was a member. It was in the Scottish universities that the philosophy of Locke, as well as that of Newton, was first adopted as a branch of academical education."

Anxious as we should have been to have awarded to Scotland the honour of having first adopted the Newtonian philosophy, yet a regard for historical truth compels us to take a different view of the subject. It is well known that Sir Isaac Newton delivered lectures on his own philosophy from the Lucasian chair before the publication of the *Principia;* and in the very page of Whiston's Life quoted by Professor Playfair, he informs us that he had heard him read such lectures in the public schools, though at that time he did not at all understand them. Newton continued to lecture till 1699, and occasionally, we presume, till 1703, when Whiston became his successor, having been appointed his deputy in 1699. In both of these capacities Whiston delivered in the public schools a course of lectures on astronomy, and a course of physico-mathematical lectures, in which the mathematical philosophy of Newton was explained and demonstrated; and both these courses were published, the one in 1707, and the other in 1710, "for the use of the young men in the university." In 1707, the celebrated blind mathematician, Nicholas Saunderson, took up his residence in Christ's College, without being admitted a member of that body. The society not only allotted to him apartments, but gave him the free use of their library. With the consent of Whiston he delivered a course of lectures "On the Principia, Optics, and Universal Arithmetic of Newton," and the popularity of these lectures was so great that Sir Isaac corresponded on the subject of them with their author; and on the ejection of Whiston from Lucasian chair in 1711, Saunderson was appointed his successor, and contined to teach the Newtonian philosophy till his death in 1739.

One of the most strenuous supporters of the Newtonian philosophy was Dr. Langton. In 1709-10, when he was proctor of the college, instead of appointing a moderator, he discharged the office himself, and devoted his most active exertions to the promotion of mathematical knowledge. Previous to this, he had even published a paper of questions on the Newtonian philosophy, which appear to have been used as theses for disputations; and such was his ardour and learning that they greatly contributed to the popularity of his college. Between 1706 and 1716, the celebrated Roger Cotes, the friend and disciple of Newton, filled the Plumian chair of astronomy and experimental philosophy at Cambridge. During this period he edited the second edition of the Principia, which he enriched with an admirable preface, and thus contributed by his writings as well as by his lectures, to advance the philosophy of his master. About the same time, the celebrated Dr. Bentley, who first made known the philosophy of his friend to the readers of general literature, filled the high office of Master of Trinity College, and could not have failed to have exerted his utmost influence in propagating doctrines which he so greatly admired. Had any opposition been offered to the introduction of the true system of the universe, the talents and influence of these individuals would have immediately suppressed it; but no such opposition seems to have been made; and though there may have been individuals at Cambridge, ignorant of mathematical science, who adhered to the system of Descartes, and studied the physics of Rohault, yet it is probable that similar persons existed in the Scottish universities; and we cannot regard their adherence to error as disproving the general fact, that the philosophy of Newton was quickly introduced into all the universities of Great Britain.

Dr. John Keill was the first person who publicly taught natural philosophy by experiments. Desaguliers informs us that this author "laid down very simple propositions, which he proved by experiments, till he had instructed his auditors in the laws of motion, the principles of hydrostatics and optics, and some of the chief propositions of Sir Isaac Newton, concerning light and colours. He began these courses in Oxford about the year 1704 or 1705, and in that way introduced the love of the Newtonian philosophy." When Dr. Keill left the university, Desaguliers began to teach the Newtonion philosophy by experiments. He commenced his lectures at Harthall in Oxford, in 1710, and delivered upwards of one hundred and twenty courses; and when he went to settle in London in 1713, he informs us that he found "the Newtonian philosophy generally received among persons of all ranks and professions, and even among the ladies by the help of experiments." Such were the steps by which the philosophy of Newton was established in Great Britain; and long before his death Sir Isaac enjoyed the high satisfaction of seeing his doctrine triumphant in his native land.

Previous to the time of Newton, the doctrine of infinite quantities had been the subject of the most anxious study. The ancients made the first step in this curious inquiry by a rude, though ingenious, attempt to determine the area of curves. The method of exhaustions which was used for this purpose consisted in finding a given rectilinear area to which the inscribed and circumscribed polygonal figures continually approached by increasing the number of their sides. This area was obviously the area of a curve, and in the case of the parabola it was found by Archimedes to be two-thirds of the area formed by multiplying the ordinate by the abscissa. Although the synthetical demonstration of the results was perfectly conclusive, yet the method itself was limited and imperfect. The celebrated Pappus of Alexandria followed Archimedes in the same inquiries, and in his demonstration of the property of the centre of gravity of a plane figure, by which we may determine the solid formed by its revolution, he has shadowed forth the discoveries of later times.

In his curious tract on Stareometry, published in 1615, Kepler made some advances in the doctrine of infinitesimals. Prompted to the task by a dispute with the seller of some casks of wine, he studied the measurement of solids formed by the revolution of a curve round any line whatever. In solving some of the simplest of these problems, he conceived a circle to be formed of an infinite number of triangles having all their vortices in the centre, and their infinitely small bases in the circumference of the circle, and by thus rendering familiar the idea of quantities infinitely great and infinitely small, he gave an impulse to this branch of mathematics. The failure of Kepler, too, in solving some of the more difficult of the problems which he himself proposed, roused the attention of geometers, and seems particularly to have attracted the notice of Cavaleri.

This ingenious mathematician was born at Milan in 1598, and was Professor of Geometry at Bologna. In his method of Indivisibles, which was published in 1635, he considered a line as composed of an infinite number of points, a surface of an infinite number of lines, and a solid of an infinite number of surfaces; and he lays it down as an axiom, that the infinite sums of such lines and surfaces have the same ratio when compared with the linear or superficial unit, as the surfaces and solids which are to be determined. As it is not true that an infinite number of infinitely small points can make a line, or an infinite number of infinitely small lines a surface, Pascal removed this verbal difficulty by considering a line as composed of an infinite number of infinitely short lines, a surface as composed of an infinite number of infinitely narrow parallelograms, and a solid of infinitely thin solids. But, independent of this correction, the conclusions deduced by Cavaleri are rigorously true, and his method of ascertaining the ratios of areas and solids to one another, and the theorems which he deduced from it, may be considered as forming an era in mathematics.

By the application of this method, Roberval and Toricelli showed that the area of the cycloid is three times that of its generating circle, and the former extended the method of Cavaleri to the case where the powers of the terms of the arithmetical progression to be summed were fractional.

In applying the doctrine of infinitely small quantities to determine the tangents of curves, and the maxima and minima of their ordinates, both Roberval and Fermat made a near approach to the invention of Fluxions—so near indeed, that both Lagrange and Laplace have pronounced the latter to be the true inventor of the differential calculus.

The labours of Peter Fermat, a counsellor of the parliament of Thoulouse, approached still nearer to the fluxionary calculus. In his method of determining the maxima and minima of the ordinates of curves, he substitutes $x+e$ for the independent variable $x$ in the function which is to become a maximum, and as these two expressions should be equal when $e$ becomes infinitely small or $o$, he frees this equation from surds and radicals, and after dividing the whole by $e$ $e$ is made $=o$, and the equation for the maximum is thus obtained. Upon a similar principle he founded his method of drawing tangents to curves. But though the methods thus used by Fermat are in principle the same with those which connect the theory of tangents and of maxima with the analytical method of exhibiting the differential calculus, yet it is a singular example of national partiality, to consider the inventor of these methods as the inventor of the method of fluxions.

The discoveries of Fermat were improved and simplified by Hudde, Huygens, and Barrow, and by the publication of the "Arithmetic of Infinites" by Dr. Wallis, Savilian Professor of Geometry at Oxford, mathematicians were conducted to the very entrance of a new and untrodden field of discovery. This distinguished author had effected the quadrature of all curves whose ordinates can be expressed by any direct integral powers; and though he had extended his conclusions to the cases where the ordinates are expressed by the inverse or fractional powers, yet he failed in its application. Nicolas Mercator (Kauffmann) surmounted the difficulty by which Wallis had been baffled, by the continued division of the numerator by the denominator to infinity, and the applying Wallis's method to the resulting positive powers. In this way he obtained, in 1667, the first general quadrature of the hyperbola, and, at the same time, gave the regular development of a function in series.

In order to obtain the quadrature of the circle, Dr. Wallis considered that if the equations of the curves of which he had given the quadrature were arranged in a series, beginning with the most simple, these areas would form another series. He saw also that the equation of the circle was intermediate between the first and second terms of the first series, or between the equation of a straight line and that of a parabola, and hence he concluded, that by interpolating a term betwen the first and second term of the second series, he would obtain the area of the circle. In pursuing this singularly beautiful thought, Dr. Wallis did not succeed in obtaining the indefinite quadrature of the circle, because he did not employ general exponents; but he was led to express the entire area of the circle by a fraction, the numerator and the denominator of which are each obtained by the multiplication of a certain series of numbers.

Such was the state of this branch of mathematical science when Newton, at an early age, directed his attention to it. At the commencement of his mathematical studies, when the works of Dr. Wallis fell into his hands, he was led to consider how he could interpolate the general values of the areas in the second series of that mathematician. With this view he investigated the mathematical law of the co-efficients of the series, and obtained a general method of interpolating not only the series above referred to, but also other series. These were the first steps taken by Newton; and, as he himself informs us, in his letter to the secretary of the Royal Society, October 24, 1676, they would have entirely escaped from his memory, if he had not a few weeks before found the notes which he had made upon the subject. When he had obtained this method, it occurred to him that the very same process was applicable to the ordinates, and by following out this idea, he discovered the general method of reducing radical quantities, composed of several terms into infinite series, and was thus led to the discovery of the celebrated "Binomial Theorem." He now entirely neglected his methods of interpolation, and employed that theorem alone as the easiest and most direct method for the quadratures of curves, and in the solution of many questions which had not even been attempted by the most skilful mathematicians.

After having applied the Binomial theorem to the rectification of curves, and to the determination of the surfaces and contents of solids, and the position of their centres of gravity, he discovered the general principle of deducing the areas of curves from the ordinate, by considering the area as a nascent quality increasing by continual fluxion in the proportion of the length of the ordinate, and supposing the abscissa to increase uniformly in proportion to the time. In imitation of Calverius, he called the momentary increment of a line a point, though it is not a geometrical point, but an infinitely short line; and the momentary increment of an area or surface he called a line, though it is not a geometrical line, but an infinitely narrow space. By thus regarding lines as generated by the motion of points, surfaces by the motions of lines, and solids by the motion of surfaces, and by considering that the ordinates, abscissare, &c., of curves thus formed, vary according to a regular law depending on the equation of the curve, he deduces from this equation the velocities with which these quantities are generated; and by the rules of infinite series he obtains the ultimate value of the quantity required. To the velocities with which every line or quantity is generated, Newton gave the name of *Fluxions*, and to the lines or quantities themselves that of *Fluents*. This method constitutes the doctrine of fluxions which Newton had invented previous to 1666, when the breaking out of the plague at Cambridge drove him from that city, and turned his attention to other subjects.

But though Newton had not communicated this great invention to any of his friends, he composed his treatise "Analysis per equationes numero terminorum infinitas," in which the principle of fluxions and its numerous applications are clearly pointed out. In the month of June, 1669, he communicated this work to Dr. Barrow, who mentions it in a letter to Mr. Collins, dated the 20th June, 1699, as the production of a friend of his residing at Cambridge, who possesses a fine genius for such inquiries. On the 31st of July he transmitted the work to Collins; and having received his approbation of it, he informs him that the name of the author was Newton, a fellow of his own college, and a young man who had only two years before taken his degree of M.A. Collins took a copy of this treatise, and returned the original to Dr. Barrow; and this copy having been found among Collins's papers by his friend Mr. William Jones, and compared with the original manuscript borrowed from Newton, it was published with the consent of Newton in 1711, nearly half a century after it was written.

Though the discoveries contained in this treatise were not at first given to the world, yet they were made generally known to mathematicians by the correspondence of Collins, who communicated them to James Gregory; to Bertel and Vernon, in France; to Sluisius, in Holland; to Borelli, in Italy; and to Strode, Townsend, and Oldenburgh, in letters dated between 1669 and 1672.

Hitherto the method of fluxions was known only to the friends of Newton and their correspondents; but in the first edition of the "Principia," which appeared in 1687, he published, for the first time, the fundamental principle of the fluxionary calculus, in the second lemma of the second book. No information, however, is here given respecting the algorithm, or notation of the calculus, and it was not till 1693 that it was communicated to the mathematical world in the second volume of Dr. Wallis's works, which were published in that year. This information was extracted from two letters written by Newton, in 1692.

Some time in the year 1672, Newton had undertaken to publish an edition of Kinckhuysen's Algebra, with notes and additions. He therefore drew up a treatise, entitled "A Method of Fluxions," which he proposed as an introduction to that work; but the fear of being involved in disputes about this new discovery, or perhaps the wish to render it more complete, or to have the sole advantage of employing it in his physical researches, induced him to abandon this design. At a later period of his life he again resolved to give it to the world; but it did not appear till after his death, when it was translated into English, and published in 1736, with a commentary by Mr. John Colson, Professor of Mathematics in Cambridge.

To the first of Newton's Optics, which appeared in 1704, there were added two mathematical treatises, "entitled "Tractatus duo de speciebus et magnitudine figurarum curvilinearum," the one bearing the title of "Tractatus de Quadratura Curvarum," and the other "Enumeratio linearum tertii ordinis." The first contains an explanation of the doctrine of fluxions, and of its application to the quadrature of curves; and the second a classification of seventy-two curves of the third order, with an account of their properties. The reason for publishing these two tracts in his Optics (in the subsequent editions of which they are omitted) is thus stated in the advertisement:—"In a letter written to M. Leibnitz, in the year 1679, and published by Dr. Wallis, I mentioned a method by which I had found some general theorems about squaring curvilinear figures on comparing them with the conic sections, or other the simplest figures with which they might be compared. And some years ago I lent out a manuscript containing such theorems; and having since met with some things copied out of it, I have on this occasion made it public, prefixing to it an introduction, and joining a scholium concerning that method. And I have joined with it another small tract concerning the curvilineal figures of the second kind, which was also written many years ago, and made known to some friends, who have solicited the making it public."

In the year 1707, Mr. Whiston published the algebraical lectures which Newton had, during nine years, delivered at Cambridge, under the title of "Arithmetica Universalis, sive de Compositione et Resolutione Arithmetica Liber." It is not known how Mr. Whiston obtained possession of this work; but it is stated by one of the editors of the English edition, that "Mr. Whiston, thinking it a pity that so noble and useful a work should be doomed to a college confinement, obtained leave to make it public." It was soon after translated into English by Mr. Ralphson; and a second edition of it, with improvements by the author, was published at London in 1712 by Dr. Machin, then secretary to the Royal Society. With the view of stimulating mathematicians to write annotations on this admirable work, the celebrated S'Gravesande published a tract entitled "Specimen Commentarii in Arithmeticam Universalem;" and Maclaurin's Algebra seems to have been drawn up in consequence of this appeal.

Among the mathematical works of Newton, we must not omit to enumerate a small tract entitled "Methodus Differentialis," which was published with his consent in 1711. It consists of six propositions, which contain a method of drawing a parabolic curve through any given number of points, and which was useful for constructing tables by the interpolation of series, and for solving problems depending the quadrature of curves.

Another mathematical treatise of Newton's was published for the first time in 1779, in Dr. Horsley's edition of his works. It is entitled "Artis Analyticæ Specimina, vel Geometria Analytica." In editing this work, which occupies about one hundred and thirty quarto pages, Dr. Horsley used three manuscripts, one of which was in the handwriting of the author; another, written in an unknown hand, was given by Mr. William Jones to the Hon. Charles Cavendish; and a third, copied from this by Mr. James Wilson, the editor of Robins's works, was given to Dr. Horsley by Mr. John Nourse, bookseller to the king. Dr. Horsley has divided it into twelve chapters, which treat of infinite series; of the reduction of affected equations; of the specious resolution of equations; of the doctrine of fluxions; of maxima and minima; of drawing tangents to curves; of the radius of curvature; of the quadrature of curves; of the area of curves which are comparable with the conic sections; of the construction of mechanical problems, and on finding the lengths of curves.

In enumerating the mathematical works of Sir Isaac Newton, we must not overlook his solutions of the celebrated problems proposed by Bernouilli and Leibnitz. On the Kalends of January, 1697, John Bernouilli addressed a letter to the most distinguished mathematicians in Europe, challenging them to solve the two following problems:—

1. To determine the curve like connecting two given points which are at different distances from the horizon, and not in the same vertical line, along which a body passing by its own gravity, and beginning to move at the upper point, shall descend to the lower point in the shortest time possible.

2. To find a curve line of this property, that the two segments of a right line drawn from a given point through the curve, being raised to any given power,

and taken together, may make everywhere the same sum.

On the day after receiving these problems, Newton addressed to the President of the Royal Society a solution of them both. He announced that the curve required in the first problem must be a cycloid, and he gave a method of determining it. He solved also the second problem, and he showed that by the same method other curves might be formed which shall cut off three or more segments having the like proportions. Leibnitz, who was struck with the beauty of the problem, requested the author, who had allowed six months for its solution, to extend the period to double that time. This delay was readily granted, and solutions were obtained from Newton, Leibnitz, and the Marquis de L'Hopital; and although that of Newton, yet Bernouilli recognised in it his powerful mind, "as the lion is known by his claw."

The last mathematical effort of Sir Isaac was made with his usual success in solving a problem which Leibnitz proposed in 1716, in a letter to the Abbe Conty, "for the purpose of feeling the pulse of the English analysts," as he expressed it. The object of this problem was to determine the curve which should cut at right angles an infinity of curves of a given nature, but expressible by the same equation. Newton received this problem about five o'clock in the afternoon, and though the problem was extremely difficult, and he himself much fatigued at the time, yet he finished the solution of it before he went to bed.

Such is a short account of the mathematical writings of Newton, not one of which as is reported, were voluntarily communicated to the world by himself. The publication of his "Universal Arithmetic," is said to have been a breach of confidence on the part of Whiston; and however this may be, it was, as is evident, an unfinished work never designed for the public. The in consequence of plagiarisms from the manuscripts of publication of his "Quadrature of Curves," and of his "Enumeration of Curve Lines," was rendered necessary them which he had lent to his friends, and the rest of his analytical writings did not appear till after his death. It is impossible to penetrate into the motives by which Sir Isaac was on these occasions actuated. If his object was to keep possession of his discoveries till he had brought them to a higher degree of perfection, we may approve of the propriety, though we can by no means admire the prudence of such a step. If he wished to retain to himself his own methods, iu order that he alone might have the advantage of them, in prosecuting his inquiries, we cannot reconcile so selfish a measure with that openness and generosity of character which marked the whole of our author's life. If he withheld his labours from the world in order to avoid the disputes and contentions to which they might give rise, he adopted the very worst method of securing his tranquillity. That this was the leading motive under which he acted, there can be little doubt. The early delay in the publication of his method of fluxions, after the breaking out of the plague at Cambridge, was probably owing to his not having completed the algorithm of that calculus; but no apology can be made for the imprudence of withholding it any longer from the public. Had he published this noble discovery even previous to 1623, when his great rival had not even entered upon these studies which led him to the same method, he would have secured to himself the undivided honour of the invention; and Leibnitz could have aspired to no other fame but that of an improver of the doctrine of fluxions. But he unfortunately acted otherwise. He announced to his friends that he possessed a method of great generality and power; he communicated to them a general account of its principles and applications; and the information which was thus conveyed directed the attention of mathematicians to subjects to which they might not otherwise have applied their powers. In this way, the discoveries which he had previously made, were made subsequently by others; and Leibnitz, in place of appearing in the theatre of science as the disciple and follower of Newton, stood forth with all the dignity of a rival; and, by the early publication of his discoveries, had nearly placed himself on the throne which Newton was destined to ascend.

A brief account of the dispute between Newton and Leibnitz, respecting the invention of fluxions, is all that we can admit here from the popular nature of our volume.

In the beginning of 1673, Leibnitz came to London in the suite of the Duke of Hanover, and he became acquainted with the great men who then adorned the capital of the British Empire. Among these was Oldenburg, a fellow countryman, who was the secretary to the Royal Society. About the beginning of March in the same year, Leibnitz went to Paris, where, with the assistance of Huygens, he devoted himself to the study of the higher geometry. In the month of July he renewed his correspondence with Oldenburg, and he communicated to him some of the discoveries which he had made relative to a series, particularly the series for a circular arc in terms of the tangent. Oldenburg informed him in return of the discoveries of Newton and Gregory on series; and in 1676, Newton communicated to him, through Oldenburg, a letter of fifteen closely printed quarto pages, containing many of his analytical discoveries, and stating, that he possessed a general method of drawing tangents, which he thought it necessary to conceal in two sentences of transposed characters.

In this letter neither the method of fluxions nor any of its principles are communicated, but the superiority of the method over all others is so full described, that Leibnitz could scarcely fail to discover that Newton possessed that secret of which geometers had so long been in quest.

Had Leibnitz, at the time of receiving this letter, been entirely ignorant of his own differential method, the information thus conveyed to him by Newton could not fail to stimulate his curiosity, and excite his mighty efforts to obtain possession of so great a secret. That this new method was intimately connected with the subject of series was clearly indicated by Newton; and as Leibnitz was deeply versed in this branch of analysis, it is far from improbable that a mind of such strength and acuteness might attain his object by direct investigation. That this was the case may be inferred from his letter to Oldenburg, (to be communicated by Mr. Newton), of the 21st of June, 1677, where he mentions he had for some time been in possession of a method of drawing tangents more general than that of Sluisius, namely, by the differences of ordinates. He then proceeds with the utmost frankness to explain the method, which was no other than the differential calculus. He describes the algorithm which he had adopted, the formation of differential equations, and the application of the calculus to various geometrical analytical questions. No answer seems to have been returned to this letter, either by Newton or Oldenburg, and with the exception of a short letter from Leibnitz to Oldenburg, 12th of July, 1677, no farther correspondeuce seems to have taken place. This may be accounted for by the death of Oldenburg in the month of August, when the two rivals pursued their researches with all the zest which the greatness of the subject was so well calculated to inspire.

In the hands of Leibnitz the different calculus made rapid progress. In the *Acta Eruditorum*, which was published at Leipsic, in November, 1684, he gave the first account of it, describing its algorithm in the same manner as he had done in his letter to Oldenburg, and pointing out its application to the drawing of tangents, and the determination of maxima and minima. He makes a remote reference to the *similar calculus* of Newton, but lays no claim to the sole invention of the differential method. In the same work for June, 1686, he resumes the subject, and when Newton had not published a single word upon fluxions, and had not even made known his notation, the differential calculus was making rapid advances on the continent; and in the hands of the brothers, Bernouilli, had proved the means of solving some of the most important and difficult problems.

The silence of Newton was at last broken, and in the second lemma of the second book of the Principia, he explained the fundamental principle of the fluxionary calculus. His explanation, which was very short, concluded with the following scholium. "In a correspondence which took place about ten years ago between that very skilful geometer, G. G. Leibnitz, and myself, I announced to him that I possessed a method of determining maxima and minima, of drawing tangents, and of performing similar operations which was equally applicable to rational and irrational quantities, and concealed the same in transposed letters involving this sentence "(data equatione quoterneque fluentes quantitates involvente fluxiones invenire et vice versa.)" This illustrious man replied that he had also fallen on a method of the same kind, and he communicated to me his method which scarcely differed from mine except in the notation, and in the idea of the generation of quantities." This celebrated scholium, which is so often referred to in the present controversy, has, in our opinion, been much misapprehended. While Biot considers it as "eternalizing the rights of Leibnitz by recognising them in the Principia," Professor Playfair regards it as containing "a highly favourable opinion on the subject of the discoveries of Leibnitz." To us it appears to be nothing more than the simple statement of the fact, that the method communicated by Leibnitz was nearly the same as his own; and this much he might have said whether he believed that Leibnitz had seen the fluxionary calculus among the papers of Collins, or was the independent inventor of his own. It is more than probable, indeed, that when Newton wrote this scholium, he regarded Leibnitz as a second inventor; but when he found that Leibnitz and his friends had shown a willingness to believe, and had even ventured to throw out the suspicion, that he himself had borrowed the doctrine of fluxions from the differential calculus, he seems to have altered the opinion which he had formed of his rival, and to have been willing in his turn to retort the charge.

This change of opinion was brought about by a change of circumstances over which he had no control. Duillier, a Swiss mathematician, resident in London, communicated to the Royal Society in 1699, a paper on the line of quickest descent, which contains the following observations:—"Compelled by the evidence of facts, I hold Newton to have been the first inventor of this calculus, and the earliest by several years; and whether Leibnitz, the 'second inventor,' has borrowed any thing from the other, I would prefer to my own judgment that of those who have seen the letters and other copies of the same manuscripts of Newton." This imprudent remark, which by no means amounts to a charge of plagiarism, for Leibnitz is actually designated the 'second inventor,' may be considered as showing that the English mathematicians had been cherishing suspicions unfavourable to Leibnitz, and there can be no doubt that a feeling had long prevailed that this mathematician either had, or might have, seen among the papers of Collins the Analysis per Equationes &c., which contained the principles of the fluxionary method. Leibnitz replied to the remark of Duillier with much good feeling. He appealed to the facts as exhibited in his correspondence with Oldenburg; he referred to Newton's scholium as a testimony in his favour, and, without disputing or acknowledging the priority of Newton's claim, he asserted his own right to the invention of the differential calculus. Duillier transmitted a reply to the "Leipsic Acts;" but the editor refused to insert it. The dispute therefore terminated, and the feelings of the concluding parties continued for some time in a state of repose, though ready to break out on the slightest provocation.

When Newton's Optics was published, in 1704, accompanied by his treatise on the Quadrature of Curves, and his Enumeration of lines of the third order, the editor of the Leipsic Acts (who Newton supposed to be Leibnitz himself) took occasion to review the first of these tracts. After giving an imperfect analysis of its contents, he compared the method of fluxions with the differential calculus, and, in a sentence of some ambiguity, he states that Newton employed fluxions in place of the differences of Leibnitz, and made use of them in his *Principia* in the same manner as Fabri, in his Synopsis of Geometry, had substituted progressive motion in place of the indivesibles of Cavaleri. As Fabri, therefore, was not the inventor of the method which is here referred to, but borrowed it from Cavaleri, and only changed the mode of its expression, there can be no doubt that the artful insinuation contained in the above passage was intended to convey the impression that Newton had *stole* his method of fluxions from Leibnitz. The indirect character of this attack, in place of mitigating its severity, renders it doubly odious; and we are persuaded that no candid reader can peruse the passage without a strong conviction that it justifies the indignant feelings which it excited among the English philosophers. If Leibnitz was the author of the review, or if he was in any way a party to it, he merited the full measure of rebuke which was dealt out to him by the friends of Newton, and deserved those severe reprisals which doubtless embittered the rest of his days. He who dared to accuse a man like Newton, or, indeed, any man holding a fair character in society, with the crime of plagiarism, placed himself without the pale of the ordinary courtesies of life, and deserved to have the same charge thrown back upon himself.

Dr. Keill, as the representative of Newton's friends, could not brook this base attack upon his countryman. In a letter inserted in the Philosophical Transactions, for 1708, he maintained that Newton was, beyond the possibility of a doubt, the first inventor of fluxions. He referred for a direct proof of this, to his letters published by Wallis; and he asserted that the same calculus was afterwards published by Leibnitz, the name and the notation being changed. If we are to consider this passage as retorting the charge of plagiarism upon Leibnitz, we must admit that the mode of its expression is neither so coarse nor so insidious as that which is used by the writer of the Leipsic Acts. In a letter to Hans Sloane, in March, 1711, Leibnitz complained to the Royal Society of the treatment he had received. He expressed his conviction that Keill had erred more from rashness of judgment than from any improper motive, and that he did not regard the imputation as a calumny; and he requested that the Society would oblige Mr. Keill to disown publicly the injurious sense which his words might bear. When this letter was read, Mr. Keill justified himself to Sir Isaac and the other members, by showing them the obnoxious review of the Quadrature of Curves in the Leipsic Acts. They all agreed in attaching the same injurious meaning to the passage which we formerly quoted, and authorised Keill to explain and defend his statement. He accordingly addressed a letter to

Sir Hans Sloane, which was read to the Society, 24th of May, 1711, and a copy of which was ordered to be sent to Leibnitz. In this letter he declares that he never meant to state that Leibnitz knew either the name of Newton's method or the form of notation, and that the real meaning of the passage was "that Newton was the first inventor of fluxions or of the differential calculus, and that he had given in two letters to Oldenburg, which had been transmitted to Leibnitz, indications of it sufficiently intelligible to an acute mind, from which Leibnitz derived, or at least might derive the principles of his calculus."

The charge of plagiarism which Leibnitz thought was implied in the former letter of his antangonist is here greatly modified, if not altogether denied. Keill expresses only an opinion that the letter seen by Leibnitz contained intelligible indications of the fluxionary calculus. Even if this opinion was correct, it is no proof that Leibnitz either saw these indications or availed himself of them, or if he did perceive them, it might have been in consequence of his having previously been in possession of the differential calculus, or having enjoyed some distant view of it. Leibnitz should therefore, have accepted of this explanation of Kiell's, and allowed the dispute to terminate here; for no ingenuity on his part could affect an opinion which any other person, as well as Keill was entitled to maintain.

Leibnitz, however, thought otherwise, and wrote a letter to Sir Hans Sloane, which excited new feelings, and involved him in new embarrassments. Insensible to the mitigation which had been kindly impressed upon the supposed charge against his honour, he alleges that Keill had attacked his candour and sincerity more openly than before;—that he acted without any authority from Sir Isaac Newton, who was the party most interested;—and that it was in vain to justify his proceedings by referring to the provocation in the

Leipsic Acts, because in that journal, "no injustice had been done to any party, as every one had received only his due." He styled Keill as an upstart, and totally unqualified to judge of the case; he called upon the members of the society to silence his unjust clamours, which he was certain was contrary to the opinion of Sir Isaac himself, who was perfectly acquainted with all the facts, and who, he had no doubt, would willingly give his opinion on the matter.

This letter was, without doubt, the cause of all the rancour which so speedily followed, and it placed his antagonist in a new and more favourable position. It was not true that Keill acted without the authority of Newton, because Keill's letter was approved of, and transmitted by the Royal Soeiety, of which Newton, at the time, was president, and therefore became the act of that body. The obnoxious part, however, of Leibnitz's letter consisted in his appropriating to himself the opinions of the reviewer in the Leipsic Acts, by declaring that in a review which charged Newton with plagiarism, every person had got what was his due. The controversy was now changed in every feature. Leibnitz places himself in the position of the person who had first disturbed the tranquillity of science by maligning its most brilliant ornament; and the Royal Society was compelled to throw all the light they could upon a transaction which had exposed their respected president to so vile a charge. The Society, likewise, had become a party to the question, by their approbation and transmission of Keill's letter, and were on that account bound to vindicate the step they had taken.

When Leibnitz's letter was read, Keill appealed to the Society for the proofs of what he had advanced; Sir Isaac also expressed his displeasure at the obnoxious passage in the Leipsic Review, and at the defence of it by Leibnitz; and he left it to the members to act as they thought proper. A committee was therefore appointed, who were instructed to examine the registers of the Society, to inquire into the dispute, and to produce such documents as they should find, and report their own opinions on the case. The committee produced the following report:—

"We have consulted the letters and letter-books in the custody of the Royal Society, and those found among the papers of Mr. John Collins, dated between the years 1669 and 1677, inclusive; and showed them to such as knew and avouched the hands of Mr. Barrow, Mr. Collins, Mr. Oldenburg, and Mr. Leibnitz; and compared those of Mr. Gregory with one another, and with some of them taken in the hand of Mr. Collins; and have extracted from them what relates to the matter referred to us; all which extracts herewith delivered to you, we believe to be genuine and authentic. And by these letters and papers we find,

"1. That Mr. Leibnitz was in London in the beginning of the year 1673; and went thence, in or about March, to Paris, where he kept a correspondence with Mr. Collins, by means of Mr. Oldenburg, till about September, 1676, and then returned by London and Amsterdam to Hanover; and that Mr. Collins was very free in communicating, to able mathematicians, what he had received from Mr. Newton and Mr. Gregory.

"2. That when Mr. Leibnitz was the first time in London, he contended for the invention of another differential method, properly so called; and notwithstanding that he was shown by Dr. Pell, that it was Newton's method, persisted in maintaining it to be his own invention, by reason that he had found it by himself, without knowing what Newton had done before, and had much improved it. And we find no mention of his having any other differential method than Newton's, before his letter of the 21st of June, 1677, which was a year after a copy of Mr. Newton's letter had been sent to Paris to be communicated to him; and above four years after, Mr. Collins began to communicate that letter to his correspondent; in which letter the method of fluxions was sufficiently described to any intelligent person.

"3. That by Mr. Newton's letter of the 13th of June, 1676, it appears that he had the method of fluxions above five years before the writings of that letter. And by his "Analysis pre Æquationes numero Terminorum Infinitas," communicated by Dr. Barrow to Mr. Collins, in July, 1669, we find that he had invented the method before that time.

"4. That the differential method is one and the same with the method of fluxions, excepting the name and mode of notation; Mr. Leibnitz calling these quantities differences, which Mr. Newton calls moments or fluxions; and marking them with the letter *d*—a mark not used by Mr. Newton.

"And therefore we take the proper question to be, not who invented this or that method, but who was the first inventor of the method. And we believe, that those who have reputed Mr. Leibnitz the first inventor knew little or nothing of his correspondence with Mr. Collins and Mr. Oldenburg long before; nor of Mr. Newton's having that method above fifteen years before Mr. Leibnitz began to publish it in the "Acta Eruditorum" of Leipsic.

"For which reason we reckon Mr. Newton the first inventor, and are of opinion, that Mr. Keill, in asserting the same, has been no ways injurious to Mr. Leibnitz. And we submit to the judgment of the Society whether the extract and papers now presented to you, together with what is extant to the same purpose, in Dr. Wallis's third volume, may not deserve to be made public."

This report being read, the Society without a dissenting voice, ordered the collection of letters and manuscripts to be printed, under the superintendence of Dr. Halley, Mr. Jones, and Mr. Machin. Complete copies of it, under the title of "Commercium Epistolicum D. Johannis Collins et aliorum de analysi promata," were laid before the Society on the 8th of January, 1713, and Sir Isaac Newton as president, ordered a copy to be delivered to each member of the Committee, to examine it before its publication.

Leibnitz received information of the appearance of this publication when he was at Vienna, and, as he expresses himself, "being satisfied that it must contain 'malicious falsehoods,' I did not think proper to send for it by post, but wrote to M. Bernouilli to give me his sentiments. M. Bernouilli wrote me a letter, dated at Basle, June 7th, 1713, in which he said, 'that it appeared probable that Sir Isaac Newton had formed his calculus after having seen mine.'" This letter was published by a friend of Leibnitz, with reflections, in a loose sheet, entitled "Charta Volans," and dated July 29th, 1713. It was widely circulated without either the name of the author, printer, or place of publication, and was communicated to the "Journal Literaire" by another friend of Leibnitz, who added remarks of his own, and stated that when Newton published the Principia in 1687, "he did not understand the true differential method; and that he took his fluxions from Leibnitz."

In this state of the controversy, Mr. Chamberlayne conceived the design of reconciling these two distinguished philosophers; and in a letter, dated April 28th, 1714, he addressed himself to Leibnitz, who was still at Vienna. In his reply to this letter, Leibnitz declared that he had given no occasion for the dispute; "That Newton procured a book to be published, which was written purposely to discredit him, and sent it to Germany as in the name of the Society;" and he also stated, "that there was room to doubt whether Newton knew his invention before he had it of him." Mr. Chamberlayne communicated this letter to Sir Isaac Newton, who replied, that Leibnitz had attacked his reputation, in 1705, by intimating that he had borrowed from him the method of fluxions; and that if Mr. Collins would point out to him anything in which he had injured Mr. Leibnitz, he would give him satisfaction; that he would not retract things which he had known to be true; and that he believed that the Royal Society had done no injustice by the publication of the Commercium Epistolicum.

The Royal Society, having learned that Leibnitz complained of their having condemned him, inserted a declaration in their Journals on the 20th of May, 1714, that they did not pretend that the report of their committee should pass for a decision of the Society. Mr. Chamberlayne sent a copy of this to Leibnitz, along with Sir Isaac's letter, and Dr. Keill's answer to the papers inserted in the "Journal Literaire." After perusing these documents, Leibnitz replied:—"that Sir Isaac's letter was written with very little civility; that he was not in a humour to put himself in a passion against such people; that there were other letters among those of Oldenburg and Collins which should have been published; and that on his return to Hanover, he would be able to publish a 'Commercium Epistolicum,' which would be of service to the History of Learning." When this letter was read to the Royal Society, Sir Isaac remarked, that the last part of it injuriously accused the Society of having made a partial selection of papers for their publication; that he did not interfere in any way in the publishing of the "Commercium Epistolicum," and had even withheld from the committee two letters, one from Leibnitz in 1693, and another from Wallis in 1695, which were highly favourable to his claim. He stated that he did not think it proper for Mr. Leibnitz, himself, but that, if he had letters to produce in his favour, that they might be published in the Philosophical Transactions, or in Germany.

About this time the Abbe Conti, a noble Venetian, came to England. He was a correspondent of Leibnitz, and in a letter, which he had received soon after his arrival, he enters upon his dispute with Newton. He charges the English "with wishing to pass for almost the only inventors." He declared "that Bernouilli has judged right in saying that Newton did not possess before him the infinitessimal characteristic and algorithm." He remarks that Newton preceded him only in series; and he confesses that during his second visit to England, "Collins showed him part of his correspondence," or, as he afterwards expresses it, he saw "some of the letters of Newton at Mr. Collins's." He then attacks Sir Isaac's philosophy, particularly his opinions about gravity and vacuum, the intervention of God for the preservation of his creatures; and he accuses him of reviving the occult qualities of the schools. But the most remarkable passage in this letter is the following:—"I am a great friend of experimental philosophy, but Newton deviates much from it, 'when he pretends that all matter is heavy,' or that each particle of matter attracts every other particle."

The above letter to the Abbe Conti was generally shown in London, and came to be much talked of at court, in consequence of Leibnitz having been counsellor to the Elector of Hanover, when that prince ascended the throne of England. Many persons of distinction urged Newton to reply to Leibnitz's letter, but he declined. One day, however, the king inquired when Sir Isaac Newton's answer to Leibnitz would appear; and when Sir Isaac was informed of this, he addressed a long reply to the Abbe Conti. This letter written with dignified severity, is a complete refutation of the allegations of his adversary; and the following passage deserves to be quoted, as connected with that branch of the dispute which relates to Leibnitz's having seen part of Newton's letters to Mr. Collins. "He complains of the committee of the Royal Society, as if they had acted partially in omitting what made against me; but he fails in proving the accusation. For he instances in a paragraph concerning my ignorance, pretending that they omitted it, and yet you will find it in the Commercium Epistolicum, p. 547, lines 2, 3, and I am not ashamed of it. He saith that he saw this paragraph in the hands of Mr. Collins when he was in London the second time, that is in October, 1676. It is in my letter of the 24th of October, 1676, and therefore he then saw the letter. And in that and some other letters writ before that time, I described my method of fluxions; and in the same letter I described also two general methods of series, one of which is now claimed from me by Mr. Leibnitz." The letter concludes with the following paragraph: "But as he has lately attacked me with an accusation which amounts to plagiary, if he goes on to accuse me, it lies upon him by the laws of all nations to prove his accusations, on pain of being accounted guilty of calumny. He hath hitherto written letters to his correspondents full of affirmations, complaints, and reflections, without proving any thing. But he is the aggressor, and it lies upon him to prove the charge."

In transmitting this letter to Leibnitz, the Abbe Conti informed him that he had himself read with great attention, and without prejudice, the Commercium Epistolicum, and the little piece that contains the extract; that he had also seen at the Royal Society the original papers of the Commercium Epistolicum, and some other original pieces relating to it. "From all this," he says, "I infer, that, if all digressions are cut off, the only point is, whether Sir Isaac Newton had the method of fluxions or infinitesimals before you, or whether you had it before him. You published it first, it is true, but you have owned also that Sir Isaac Newton had given many hints of it in his letters to Mr. Oldenburg and others. This is proved very largely in the Commercium, and in the extract of it. What answer do you give? This is still wanting to the public, in order to form an exact judgment of the affair." The Abbe adds, that Leibnitz's own friends waited for his answer with great impatience, and that they were of opinion that he could not refuse answering, if not Dr. Keill, at least Sir Isaac Newton, who had given him a challenge in express terms.

Leibnitz was not long in complying with his request. He addressed a letter to the Abbe Conti, 9th April, 1716, but he sent it through M. Ramond, at Paris, to communicate it to others. When it was received by the Abbe, Newton wrote observations upon it, which were communicated only to some of his friends, and which, while he placed his defence on the most impregnable basis, at the same time threw much light on the early history of his mathematical discoveries.

The death of Leibnitz on the 14th November, 1716, put an end to this controversy, and Newton some time afterwards published the correspondence with the Abbe Conti, which had hitherto been only privately circulated among the friends of the disputants.

In viewing this controversy, at this distance of time, it is not difficult to form a correct estimate of the conduct and claims cf the two rival philosophers. It has been decided by the unanimous verdict of all nations, that Newton invented fluxions at least ten years before Leibnitz. Some of the letters of Newton which bore reference to this great discovery were perused by the German mathematician, according to his own confession; but there is no evidence that he borrowed his differential calculus from these letters. Newton was therefore the *first* inventor, and Leibnitz the *second*. It was impossible that the former could have been a plagiarist; but it was possible for the latter.

Although an attempt has been since made to place the conduct of Leibnitz on the same level with that of Newton, yet the circumstances of the case will by no means justify such a comparison. The conduct of Newton was at all times dignified and just. He knew his rights and he boldly claimed them. Conscious of his integrity, he spurned with indignation the charge of plagiarism with which an ungenerous rival had so falsely branded him; and if there is one step in his whole procedure which posterity can blame, it is his omission in the third edition of the Principia, of the references to the differential calculus of Leibnitz. This omission, however, was perfectly just. The scholium which he had left out was a mere historical statement of the fact, that the German mathematician had sent him a method which was the same as his own, and when he found that this simple assertion had been held by Leibnitz and others as a recognition of his independent claim to the invention, he was bound either to omit it altogether, or to enter into explanations which might have involved him in a new controversy.

The conduct of Leibnitz was different. That he was the aggressor no one can deny. That he first dared to accuse Newton of plagiarism, and that he frequently referred to it has been sufficiently apparent: and when arguments failed him he had recourse to threats. All this is now matter of history; and we may find some apology for it in his excited feelings, and in the insinuations which were occasionally thrown out against the originality of his discovery; but for other parts of his conduct we seek in vain for an excuse. When he assailed the philosophy in his letters to the Abbe

Conti, he exhibited perhaps only the petty feelings of a rival; but when he ventured to caluminate that great man in his correspondence with the Princess of Wales; when he dared to represent the philosophy of Newton as physically false, and as dangerous to religion; and when he founded these accusations on passages in the Principia and the Optics, glowing with all the fervour of genuine piety, he cast a blot upon his own name, which all his talents as a philosopher, and all his virtues as a man, will never be able to efface.

## CHAPTER VI.

**The privileges of the University attacked by James II. Newton chosen one of the delegates to resist this encroachment. Elected a member of the Convention Parliament. Burning of his manuscript. Supposed derangement of mind. Refutation of the statement. Friendship between Newton and Charles Montague. Newton appointed Master of the Mint. Newton elected Associate of the Academy of Sciences. Member for Cambridge. President of the Royal Society. Queen Anne confers upon him the honour of Knighthood.**

An event now occurred which drew Newton from his studies and placed him upon the theatre of public life. James II., desirous of re-establishing the Roman Catholic faith in its former supremacy, had begun to assail the rights and privileges of his Protestant subjects. Among other tyrannical acts, he sent his letter of mandamus to the University of Cambridge to order Father Francis, an illiterate Benedictine monk, to be received as Master of Arts, and to enjoy all the privileges of this degree, without taking the oaths of allegiance and supremacy. The University at once perceived the consequences which might arise from such a measure. Independent of the encroachment upon their vested rights which such an order involved, it was evident that the highest interests of the University were endangered, and that Roman Catholics might soon become a majority in the convocation. They, therefore, unanimously refused to pay the least attention to the royal mandate, and this they did with a firmness of purpose which greatly irritated the regal despot. He reiterated his commands, and accompanied them with the severest threatenings in case of disobedience. The Catholics were not idle in supporting the views of the sovereign. The honorary degree of M.A., which conveys no civil rights to its possessor, having been, upon one occasion given to the secretary of the ambassador from Morocco, it was triumphantly blazoned forth that the members of the University of Cambridge had a greater respect for an infidel than for a Roman Catholic, and were more obsequious to the Mohammedan ambassador than to their lawful sovereign. Though this reasoning might impose upon the ignorant, it produced but little effect upon the members of the University. A few weak-minded individuals, however, were disposed to yield a reluctant consent to the tyrant's wishes. They proposed to confer the degree, but at the same time to resolve that it should not be regarded as a precedent in future. To this it was replied, that the very act of submission in one case would be a stronger argument for continuing the practise than any such resolution would be against its repetition. The University, therefore, remained firm in their original decision. The Vice-Chancellor was summoned before the ecclesiastical commission to answer for this act of contempt. Newton was among the number of those who most strenuously resisted the command of the sovereign, and he was consequently chosen one of the nine delegates who were appointed to defend the independence of the University. These delegates appeared before the High Court. They maintained that not a single precedent could be found to justify so extraordinary a measure; and they showed that Charles II. had, under similar circumstances, been pleased to withdraw his mandamus. This representation had its full weight, and the king was compelled to abandon his design.

The part which Newton had taken in this affair, and the character which he now held in the scientific world, induced his friends to propose him as Member of Parliament for the university. He was accordingly elected in 1688, though by a majority of only five, and he sat in the convention Parliament till its dissolution. In the years 1688 and 1689, Newton was for the greater portion of the time absent from Cambridge, owing to his attendance in parliament; but it appears from the books of the university that from 1690 to 1695, he was seldom absent, and must, therefore, we presume, have renounced his parliamentary duties.

During his residence in the metropolis he must have experienced the inadequacy of his finances to the new circumstances in which he was placed, and it is probable that this was the cause of his confining himself to Cambridge. His income was certainly limited, and but little suited to the generosity of his disposition. Demands were, no doubt, made upon it by some of his less wealthy relatives; and there is every reason to believe, that he himself, as well as his influential friends, had been looking forward to some act of liberality on the part of the government.

An event, however, occurred, which will ever form an epoch in the history of this distinguished philosopher; and it is a most singular fact that this incident was for more than a century unknown to his own countrymen, and was only accidentally brought to light by the examination of the various manuscripts belonging to Huygens. This event has been magnified into a temporary aberration of mind, which is said to have arisen from a cause scarcely adequate to its production.

One winter morning while he was attending divine service in the chapel of the university, he had left in his study a favourite little dog called Diamond. Upon returning from his devotions he found that this animal had overturned a lighted candle on his desk, which set fire to several papers on which he had recorded the results of some optical experiments. These papers are said to have contained the labours of many years, and it has been stated that when the philosopher discovered the magnitude of his loss, he exclaimed "Oh, Diamond, Diamond, little do you know the mischief you have done me!" It is a curious circumstance that Newton never refers to the experiments which he is said to have lost on this occasion, and Mr. Conduit, the husband of his favourite niece, makes no allusion to the accident itself. The distress, however, which it occasioned, is said to have been so deep as to affect even the mighty powers of his understanding.

This extraordinary effect was first communicated to the world in the life of Newton by M. Biot, who says he received the following account of it from the celebrated M. Van Swinden:—

"There is among the manuscripts of the celebrated Huygens, a small journal in folio, in which he used to note down different occurrences. The following extract is written by Huygens himself, with whose handwriting I am well acquainted, having had occasion to peruse several of his manuscripts and autograph letters. "On the 29th of May, 1694, M. Colin, a Scotsman, informed me that, 18 months ago, the illustrious geometer, Isaac Newton, had become insane, either in consequence of his too intense application to his studies, or from excessive grief at having lost, by fire, his chemical laboratory and several manuscripts. When he came to the Archbishop of Cambridge, he made some observations which indicated an alienation of mind. He was immediately taken care of by his friends, who confined him to his house and applied remedies, by means of which he had now so far recovered his health that he began to understand the Principia." Huygens mentioned this circumstance to Leibnitz in a letter, dated June 8th, 1694, to which Leibnitz replies on the 23rd, "I am very glad that I received information of the cure of Mr. Newton at the same time that I first heard of his illness, which doubtless must have been very alarming. 'It is to men like you and him, Sir, that I wish a long life.'"

The first announcement of the preceeding statement created a great sensation among the friends and admirers of Newton. They could not easily be made to believe in the prostration of that intellectual strength which had unbarred the strongholds of the universe. The unbroken equanimity of Newton's mind, the purity of his moral character, his temperate and even abstemious life, his ardent and unaffected piety, and the weakness of his imaginative powers, all indicated a mind which was not likely to be overset by any affliction to which it could be exposed. The loss of a few experimental records could never have disturbed the equilibrium of a mind like his. If they were the records of discoveries, the discoveries themselves, indestructible, would have been afterwards given to the world. If they were merely the details of experimental results, a little time could have easily reproduced them. Had these records contained the first fruits of early genius—of obscure talent, on which fame had not yet shed its rays, we might suppose that the first blight of such early ambition would have unsettled the stability of an untried mind. But Newton was satiated with fame. His mightiest discoveries were completed, and diffused over all Europe, and he must have felt himself placed on the loftiest pinnacle of earthly ambition. The incredulity which such views could not fail to encourage, was increased by the novelty of the information. No English biographer had ever alluded to the existence of such a malady. History and tradition were alike silent, and it was not easy to believe that the Lucasian Professor of Mathematics at Cambridge, a member of the English parliament, and the most celebrated philosopher in Europe could have lost his reason without the distressing fact being known to his own countrymen.

But if the friends of Newton were surprised by the nature of the intelligence, they were distressed at the view which was taken of it by foreign philosophers. While one maintained that the intellectual exertions of Newton had terminated with the publication of the Principia, and that the derangement of his mind was the cause of his abandoning the science, others indirectly questioned the sincerity of his religious opinions, and ascribed to the aberration of his mind, those Theological pursuits which gilded his declining years. "But the fact," says Biot, "of the derangement of his intellect, whatever may have been the cause of it, will explain why, after the publication of the Principia, in 1687, Newton, though only forty-five years of age, never more published a new work on any branch of science, but contented himself with giving to the world those which he had composed long before that epoch, confining himself to the completion of those parts which might require development. We may also remark, that even these developments appear, always, to be derived from experiments and observations formerly made, such as the additions to the second edition of the Principia, published in 1713, the experiments on thick plates, those on diffraction, and the chemical queries placed at the end of the Optics in 1704; for in giving an account of these experiments, Newton distinctly says, that they were taken from ancient manuscripts which he had formerly composed; and he adds, that though he felt the necessity of extending them, on reading them more perfectly, he was not able to resolve to do this, these matters being no longer in his way. Thus it appears that though he had recovered his health sufficiently to understand all his researches, and even in some cases, to make additions to them, and useful alterations, as apears from the second edition of the Principia, for which he kept up a very active mathematical correspondence with Mr. Cotes, yet he did not wish to undertake new labours in those departments of science where he had done so much, and where he distinctly saw what remained to be done." Under the influence of the [same opinion, Mr. Biot finds "it extremely probable that his dissertation on the scale of heat, was written before the fire in his laboratory;" he describes Newton's conduct respecting the longitude bill as "almost puerile on so solemn an occasion, and one which might lead to the strongest conclusions, particularly if we refer it to the fatal accident which Newton had suffered in 1695."

The celebrated Marquis de la Place viewed the illness of our philosopher in a light more painful to his friends. He strenuously maintained that he never recovered the vigour of his intellect, and he was persuaded that Newton's theological inquiries did not commence till after that afflicting period of his life. He even commissioned Professor Gautier, of Geneva, to make inquiries on this particular subject during his visit to England, as if it concerned the interests of truth and justice, to show that Newton became a Christian and a theological writer, only after the decay of his strength and the eclipse of his reason.

Such having been the consequences of the disclosure of Newton's illness by the manuscript of Huygens, Sir David Brewster considered it to be a sacred duty to the memory of that distinguished individual, to the feelings of his countrymen, and to the interests of Christianity itself, to inquire into the nature and history of that indisposition which appears to have been so much misrepresented and misapplied. From the ignorance of so extraordinary an event, which has prevailed for such a long period in England, it might have been urged with some plausibility, that Huygens had mistaken the real import of the information that was conveyed to him; or that the individual from whom he received it had propagated an idle and a groundless rumour. But we are fortunately not confined to this very reasonable mode of defence. There still exists at Cambridge a manuscript journal written by Mr. Abraham de la Pryme, who was a student in the university while Newton was a Fellow of Trinity.

This MS. is entitled "Ephemeris Vitæ, or Diary of my own Life, containing an account likewise of the most observable and remarkable things that I have taken notice of from my youth up hitherto." Mr. Pryme was born in 1671, and begins his diary in 1685. This manuscript is in the possession of his collateral descendant, George Pryme, Esq., Professor of Political Economy at Cambridge, from whom Sir David Brewster obtained the following extract:—

"1692, February 3rd.—What I heard to-day I must relate. There is one Mr. Newton, (whom I have very oft seen,) Fellow of Trinity College, that is mighty famous for his learning, being a most excellent mathematician, philosopher, divine, &c. He has been Fellow of the Royal Society these many years; and amongst other very learned books and tracts he's written one upon the mathematical principles of philosophy, which has got him a mighty name, he having received, especially from Scotland, abundance of congratulatory letters for the same; but of all the books that he ever wrote, there was one of colours and light, established upon thousands of experiments which he had been twenty years of making, and which had cost him many hundred of pounds. This book, which he valued so much, and which he so much talked of, had the ill luck to perish, and be utterly lost just when the learned author was almost at putting a conclusion at the same, after this manner: In a winter's morning, leaving it amongst his other papers on his study table whilst he went to chapel, the candle, which he had unfortunately left burning there too, catched hold by some means of other papers and they fired the aforesaid book, and utterly consumed it and several other valuable writings; and which is most wonderful, did no further mischief. But when Mr. Newton came from chapel, and had seen what was done, every one thought he would have run mad, he was so troubled thereat, that he was not himself for a month after.

A long account of this, his system of light and colours, you may find in the Transactions of the Royal Society, which he had sent up to them long before this sad mischance happened unto him."

From this extract we are enabled to fix the approximate date of the accident by which Newton lost his papers. It must have been previous to the 3rd of January, 1692, a month before the date of the extract; but if we fix it by the dates in Huygen's manuscript, we should place it about the 29th of November, 1692, eighteen months previous to the conversation between Collins and Huygens. The manner in which Mr. Pryme refers to Newton's state of mind is that which is daily used when we speak of the loss of tranquillity which arises from the ordinary afflictions of life; and the meaning of the passage amounts to nothing more than that Newton was very much troubled by the destruction of his papers, and did not recover his serenity, and return to his usual occupations for more than a month. The very phrase that every person thought he would have run mad, is in itself a sufficient proof that no such melancholy effect was produced; and, whatever degree of indisposition may be implied in the expression "he was not himself for a month after," we are entitled to infer that one month was the period of its duration, and that, previous to the 3rd of February, 1692, the date of Mr. Pryme's memorandum, "Newton was himself again."

These facts and dates cannot be reconciled with those in Huygens's manuscript. It appears from that document, that, so late as May, 1694, Newton had only so far recovered his health as to begin again to understand the Principia. His supposed malady, thererefore, was in force from the 3rd of January, 1692, till the month of May, 1694,—a period of more than two years. Now, it is a most important circumstance, which M. Biot must have known, that in the very middle of this period Newton wrote his four celebrated letters to Dr. Bentley on the Existence of a Deity,—letters which evince a power of thought and a serenity of mind absolutely incompatible even with the slightest aberration of his intellect. No man can peruse these letters without the conviction that their author possessed his full vigour of reason, and was capable of understanding the most profound parts of his writings. The first of these letters was written on the 10th December, 1692, the second on the 17th January, 1693. His mind was therefore strong and vigorous during the composition of these letters, and as they were written at the express request of Dr. Bentley, who had been appointed to deliver the lecture founded by Mr. Boyle for vindicating the fundamental principles of Natural and Revealed Religion, we must consider such a request as showing his opinion of the strength and originality of his friend's mental powers.

In 1692, Newton, at the request of Dr. Wallis, transmitted to him the first proposition of his book on quadratures, with examples of it in first, second, and third fluxions. These examples were written in consequence of an application from his friend; and the author of the review of the Commercium Epistolicum, in which this fact is quoted, draws the conclusion, that he had not at that time forgotten his method of second fluxions. It appears also from the second book of the Optics, that in the month of June, 1692, he had been occupied with the subject of halos, and had made accurate observations both on the colours and the diameters of the rings in a halo which he had then seen round the sun.

But, though these facts stand in direct contradiction to the statement recorded by Huygens, the reader will be naturally anxious to know the real nature and extent of the indisposition to which it refers. The following letters written by Newton himself to Mr. Pepys, Secretary to the Admiralty, and Mr. Millington, of Magdalene College, Cambridge, will throw much light upon the subject. Lord Braybrooke published these letters in the Life of Pepys.

As will be presently seen, Newton had fallen into a bad state of health some time in 1692, in consequence of which both his sleep and his appetite were greatly affected. About the middle of September, 1693, he had been kept awake for five successive nights by this nervous disorder, and in this condition he wrote the following letter to Mr. Pepys :—

"September 13, 1693.

"SIR,

"Some time after Mr. Millington had delivered your message, he pressed me to see you the next time I went to London. I was averse; but upon his pressing consented, before I considered what I did, for I am extremely troubled at the embroilment I am in, and have neither ate nor slept well this twelvemonth, nor have my former consistency of mind. I never designed to get anything by your interest, nor by King James's favour, but am now sensible that I must withdraw from your acquaintance, and see neither you nor the rest of my friends any more, if I may but leave them quietly. I beg your pardon for saying I would see you again, and rest your most humble and most obedient servant,

"IS. NEWTON."

From this letter it is evident that his complaint had continued for a twelvemonth, and during that period neither ate nor slept well, nor enjoyed *his former consistency of mind.* It is not easy to understand exactly what is meant by this phrase; but whatever may be its import, it is certain that he was in a state of mind so sound as to enable him to compose his four famous letters to Bentley, all of which were written in the course of the twelvemonth here referred to.

On receiving this letter, Mr. Pepys appears to have written to his friend, Mr. Millington, to inquire after Mr. Newton's health; but the inquiry having been made in a vague manner, an answer equally vague was returned. Mr. Pepys, however, who seems to have been deeply anxious respecting Newton's health, addressed the following more explicit letter to Mr. Millington:—

"September 26th, 1693.

"SIR,

"After acknowledging your many old favours, give me leave to do it a little more particularly upon occasion of the new one conveyed to me by my nephew Jackson. Though, at the same time, I must acknowledge myself not at the ease I would be glad to be at in reference to the excellent Mr. Newton, concerning whom (methinks) your answer labours under the same kind of restraint which (to tell you the truth) my asking did. For I was loth at first dash to tell you, that I had lately received a letter from him so surprising to me for the inconsistency of every part of it as to be put into great disorder by it, from the concernment I have for him, lest it should arise from that which, of all mankind, I should least dread from him and most lament—I mean a discomposure in head, or mind, or both. Let me, therefore, beg of you, Sir, having now told you the true ground of the trouble I lately gave you, to let me know the very truth of the matter, as far at least as comes within your knowledge. For I own too great an esteem for Mr. Newton, as for a public good, to be able to let any doubt in me of this kind concerning him be a moment uncleared, where I can have any hopes of helping it.—I am, with great truth and respect, Dear Sir, your most humble and most affectionate servant,

"S. PEPYS."

Mr. Millington made the following reply:—

"Coll. Magd. Camb.,
"Sept. 30th, 1693.

"HONOR'D SIR,

"Coming home from a journey on the 28th instant, at night, I met with your letter, which you were pleased to honour me with of the 26th. I am much troubled I was not at home in time for the post, that I might as soon as possible put you out of your generous payne that you are in for the worthy Mr. Newton. I was, I must confess, very much surprised at the inquiry you were pleased to make by your nephew about the message that Mr. Newton made the ground of his letter to you, for I was very sure I never either received from you or delivered to him any such; and therefore I went immediately to wayt upon him, with a design to discourse him about the matter, but he was out of town, and since I have not seen him, till upon the 28th I met him at Huntingdon, where, upon his own accord, and before I had time to ask him any questions, he told me that he had writt to you a very odd letter, at which he was much concerned; and added, that it was in a distemper that much seized his head, and that it kept him awake for above five nights together, which upon occasion he desired I would represent to you, and beg your pardon, he being very much ashamed he should be so rude to a person for whom he hath so great an honour. He is now very well, and, though I fear he is under some small degree of melancholy, yet I think there is no reason to suspect that it hath at all touched his understanding, and I hope never will; and so I am sure all ought to wish who love learning or the honour of our nation, 'which it is a sign how much it is looked after, when such a person as Mr. Newton lyes so neglected by those in power.' And thus, honoured sir, I have made you acquainted with all I know of the cause of such inconsistencys in the letter of so excellent a person; and I hope it will remove the doubts and fears you are, with so much compassion and publicness of spirit, pleased to entertain about Mr. Newton; but if I should have been wanting in anything tending to the more full satisfaction, I shall, upon the least notice, endeavour to amend it with all gratitude and truth.—Honored Sir, your most faithfull and most obedient servant,

"JOH. MILLINGTON."

That this information was perfectly satisfactory to Mr. Pepys appears from the following letter:—

"October 3rd, 1693.

"Sir,

"You have delivered me from a fear that indeed gave me much trouble, and from my very heart I thank you for it—an evil to Mr. Newton being what every good man must feel for his own sake as well as his. God grant it may stop there. And for the kind reflection hee has since made upon his letter to mee, I dare not take upon mee to judge what answer I should make him to it, or whether any or no; and therefore pray that you will bee pleased either to bestow on mee what directions you see fitt for my own guidance towards him in it, or to say to him in my name but your own pleasure, whatever you may think may be most welcome to him upon it, and most expressive of my regard and affectionate esteem of him, and concernment. Dear Sir, your most humble and most faithful servant,

"S. PEPYS."

It does not appear from the memoirs of Mr. Pepys, whether he ever returned any answer to the "odd letter" of Mr. Newton, which occasioned this correspondence; but we there find that in less than two months after the date of the preceding letter, an opportunity presented itself of introducing to him a Mr. Smith, who wished to have his opinion on some problem in the doctrine of chances. This letter from Mr. Pepys is dated November 22nd, 1693. Sir Isaac replied to it on the 26th November, and wrote to Pepys aggain on the 16th December: and in both these letters he enters fully into the discussion of the mathematical question which had been submitted to his judgment.

It is obvious, from Mr. Newton's letter to Mr. Pepys, that the subject of receiving some favour from the government had been a matter of anxiety with himself, and of discussion among his friends. Mr. Millington was no doubt alluding to this anxiety, when he represents Newton as an honour to the nation, and expresses his surprise "that such a person should be so neglected by those in power." And we find the same subject distinctly referred to in two letters written to Mr. Locke during the preceding year. In one of these he says, "Being fully convinced that Mr. Montague, upon an old grudge, which I thought had been worn out, has been false to me, I have done with him, and intend to sit still, unless my Lord Monmouth be still my friend." Mr. Locke appears to have assured him of the continued friendship of this nobleman, and Newton still referring to the same topic remarks, "I am very glad Lord Monmouth is still my friend, but intend not to give his lordship any further trouble. My inclinations are to sit still." In a later letter to Mr. Locke, dated September, 1693, which is given below, he asks his pardon for saying or thinking that there was a design to sell him an office. In these letters Newton no doubt referred to some appointment in London which he was solicitous to obtain, and which Mr. Montague and his other friends may have failed in procuring. This opinion is confirmed by the letter of Mr. Montague, announcing to him his appointment to the wardenship of the Mint, in which he says that he can *at last* give him good proof of his friendship.

In the same month in which Newton wrote to Mr. Pepys, we find him in correspondence with Mr. Locke. Displeased with his opinions respecting innate ideas, he had rashly stated that they struck at the root of all morality, and that he regarded the author of such doctrines as a Hobbist. Upon reconsidering their opinions, he addressed the following remarkable letter to Mr. Locke, written only three days after his "odd letter" to Mr. Pepys, and consequently during the illness under which he then laboured :—

"Sir,

"Being of opinion that you endeavoured to embroil me with women, and by other means, I was so much affected with it, that when I was told you were sickly, and would not live, I answered, 'twere better if you were dead.' I desire you will forgive me this uncharitableness ; for I am now satisfied that what you have done is just, and I beg your pardon for having hard thoughts of you for it, and for representing that you struck at the root of morality, in a principle you laid down in your book of ideas, and designed to pursue in another book, and that I took you for a Hobbist. I beg your pardon also for saying or thinking that there was a design to sell me an office, or to embroil me.—I am your most humble and unfortunate servant,

"Is. Newton."

"At the Bull, in Shoreditch, London,
September 16th, 1693."

To this letter Locke returned the following answer, so nobly distinguished by philosophical magnanimity and Christian charity :—

"Oates, Oct. 5th, 1693.

"Sir,

"I have been, ever since I first knew you, so entirely and sincerely your friend, and I thought you so much mine, that I could not have believed what you tell me of yourself, had I had it from anybody else. And though I cannot but be mightily troubled that you should have had so many wrong and unjust thoughts of me, yet next to the return of good offices, such as from a sincere good will I had ever done you, I receive your acknowledgment of the contrary as the kindest thing you have done me, since it gives me hopes I had not lost a friend I so much valued.—After what your letter expressss, I shall not need to say anything to justify myself to you. I shall always think your own reflection on my carriage, both to you and to all mankind, will sufficiently do that. Instead of that, give me leave to assure you, that I am more ready to forgive you than you can be to desire it; and I do it so freely and fully, that I wish for nothing more than the opportunity to convince you that I truly love and esteem you, and that I have the same good will for you as if nothing of this had happened. To confirm this to you more fully, I should be glad to meet you anywhere, and the rather, because the conclusion of your letter makes me apprehend it would not be wholly useless to you. But whether you think it fit or not, I leave wholly to you. I shall always be ready to serve you to my utmost, in any way you shall like, and shall only need your commands or permission to do it.

"My book is going to press for a second edition; and, though I can answer for the design with which I write it, yet, since you have so opportunely given me notice of what you have said of it, I should take it as a favour if you would point out to me the places that gave occasion to that censure, that by explaining myself better, I may avoid being mistaken by others, or unawares doing the least prejudice to truth or virtue. I am sure you are so much a friend to them both, that, were you none to me, I could expect this from you. But I cannot doubt that you would do a great deal more than this for my sake, who, after all, have all the concern of a friend for you, wish you extremely well, and am, without compliment, &c."

Newton made the following reply to this friendly epistle:—

"Cambridge, Oct. 5th, 1693.

"Sir,

"The last winter, by sleeping too often by my fire, I got an ill habit of sleeping; and a distemper, which this summer has been epidemical, put me farther out of order, so that when I wrote to you, I had not slept an hour a night for a fortnight together, and for five days together not a wink. I remember I wrote to you, but what I said of your book I remember not. If you please to send me a transcript of that passage I will give you an account of it if I can.—I am your most humble servant,

Is. Newton."

Although the first of these epistles shows the existence of a nervous irritability which could not fail to arise from the reasons assigned, yet it is evident that its author was in full possession of his mental powers. The answer of Mr. Locke is obviously written upon that supposition; and it is worthy of remark, that the celebrated Dugald Stewart, who first presented a portion of these letters to the public, never imagined for a moment that Newton was labouring under any mental alienation; and no man could be a better judge.

The notion entertained by Laplace, that Newton devoted his attention to theology only in the latter part of his life, may be considered as deriving some countenance from the fact, that the celebrated general scholium, at the end of the second edition of the Principia, published in 1713, did not appear in the first edition of that work. This argument has been ably denied by Dr. J. C. Gregory, of Edinburgh, on the authority of a manuscript of Newton, which seems to have been transmitted to his ancestor, Dr. David Gregory, between the years 1687 and 1698. This manuscript which consists of twelve folio pages in Newton's handwriting, contains, in the form of additions, and scholia to some propositions in the third book of the Principia, an account of the opinions of the ancient philosophers on gravitation and motion, and on natural theology, with various quotations from their works. Attached to this manuscript are three very curious paragraphs. The two first appear to have been the original draught of the general scholium already referred to; and the third relates to the subject of an ethereal medium, respecting which he maintains an opinion diametrically opposite to that which he afterwards published at the end of his Optics. Dr. Gregory concludes his account of this manuscript in the following words:—"I do not know whether it is true, as stated by Huygens, 'Newtonium incidisse, in Phrenitim;' but I think every gentleman who examines this manuscript will be of opinion that he must have thoroughly recovered from his phrenitis before he wrote either the Commentary on the Opinions of the Ancients, or the Sketch of his own Theological and Philosophical Opinions which it contains."

In the middle of the year 1694, about the time Newton is pronounced to be beginning to understand the Principia, we find him occupied with the difficult and profound subject of the lunar theory. In order to procure observations for verifying the equations which he had deduced from the theory of gravity, he paid a visit to Mr. Flamstead, at the Royal Observatory of Greenwich, on the 1st September, 1694, when he received from him a series of lunar observations. On the 7th October he wrote to Flamstead that he had compared the observations with his theory, and had satisfied himself that by both together "the moon's theory may be reduced to a good degree of exactness, perhaps to the exactness of two or three minutes." He wrote to him again on the 24th of October, and the correspondence was continued for about four years, Newton making constant application for observations, to compare with his theory of the planetary motions; while Flamstead, not sufficiently aware of the importance of the inquiry, received his requests as if they were idle intrusions in which the interests of science were but slightly concerned. In one of his letters to Newton, he says, "Upon hearing occasionally that you had sent a letter to Dr. Wallis about the parallax of the fixed stars to be printed, and that you had mentioned me therein with respect to the theory of the moon, I was concerned to be publicly brought upon the stage about what, perhaps, will never be fitted for the public, and thereby the world put into expectation of what perhaps they are never likely to have. I do not love to be printed upon every occasion, much less to be dunned by foreigners about mathematical things, or to be thought by your own people to be trifling away my time when I should be about the king's business.

In receiving the details which we have now given respecting the health and occupations of Newton, from the commencement of 1692 till 1695, it is impossible to come to any other conclusion than that he possessed a sound mind, and was quite capable of pursuing his mathematical, his metaphysical, and his astronomical inquiries. During the period of bodily indisposition, his mind, though in a state of nervous irritability, and disturbed by want of rest, was capable of putting forth its highest powers. At the request of Dr. Wallis, he drew up an example of one of his propositions on the quadrature of curves in second fluxions. At the desire of Dr. Bentley, he composed his profound and beautiful letters on the existence of the Deity. He was requested by Locke to reconsider his opinions on the subject of innate ideas; and we find him grappling with the difficulties of the lunar theory.

But with all these proofs of a vigorous mind, a diminution of his mental powers has been rashly inferred from the cessation of his great discoveries, and from his unwillingness to enter upon new investigations. The facts, however, here assumed, are as incorrect as the inference drawn from them. The ambition of fame is a youthful passion which is softened, if not subdued by age. Success diminishes its ardour, and early pre-eminence often extinguishes it. Before the middle period of his life, Newton was invested with all the insignia of immortality; but endowed with a native humility of mind, and animated with those hopes which teach us to form a moderate estimate of human greatness, he was satisfied with the laurels which he had won, and he sought only to perfect and complete his labours. His mind was principally bent on the improvement of his Principia; but he occasionally diverged into new fields of scientific research,—he solved problems of great difficulty, which had been proposed to try his strength—and he devoted much time to profound inquiries in chronology and theological literature.

The powers of his mind were therefore in full requisition; and when we take into our consideration that he was called to the discharge of high official functions which forced him into public life, and compelled to direct his genius into new channels,—we can scarcely be surprised that he ceased to produce any original works on abstract science. In the direction of the affairs of the Mint, and of the Royal Society, to which we shall now follow him, he found ample occupation for his time, while he devoted the leisure of his declining years to those exalted studies in which philosophy yields to the supremacy of faith, and hope administers to the aspirations of genius.

We have hitherto viewed Newton as a faithful philosopher leading a life of seclusion within the walls of a college, and either engaged in the duties of his professorship, or occupied in mathematical and scientific inquiries. He had now reached the fifty-third year of his age, and while those of his own standing at the university had been receiving high appointments in the church, or more lucrative offices under the government, he still remained without any mark of the respect or gratitude of his country. All Europe, it is true, had been offering incense to his name, and Englishmen themselves boasted of him as the pride of their country, and the ornament of mankind, but he was left in comparative poverty; so much was this the case indeed, that the council of the Royal Society excused him from making the usual payment of one shilling weekly, "on account of his low circumstances, as he represented." He had no other income than the salary of his professorship, and the trifling rental of his paternal inheritance. Such disregard of the greatest genius, dignified by the most exemplary virtue, could only have taken place in England, and we should have been ready to have ascribed this carelessness to the turbulence of the age in which he lived, had we not seen, in the history of another century, that the successive governments which preside over the destinies of Britian, have never been able to feel or to recognize the true nobility of soul, while the people of the country are reduced to a state of misery and privation, on account of the enormous sums granted for the support of the aristocracy, who never have done, nor, in all likelihood, ever will do an action worthy of being handed down to posterity;

but these are the men—and women too,—whom the British government delights to honour.

Among his friends of Cambridge Newton had the honour of numbering Charles Montague, grandson of the Earl of Manchester, a young man of high promise, and every way worthy of his friendship. Though devoted to literary pursuits, and twenty years younger than Newton, he cherished for the philosopher all the veneration of a disciple, and his affection gathered new strength as he rose to the highest honours and offices of the state. In the year 1684, we find him co-operating with Newton in the establishment of a Philosophical Society at Cambridge, but though both of them had made personal application to different individuals to become members, yet the plan failed as Newton expresses it from the want of persons willing to try useful experiments.

Mr. Montague sat along with Newton in the Convention Parliament, and such were the powers he displayed in that assembly, as a public speaker, that he was appointed a commissioner of the treasury, and soon afterwards a privy councillor. In these situations his talents and knowledge of business were highly conspicuous, and in 1694, he was appointed Chancellor of the Exchequer. The current coin of the kingdom having been much adulterated and debased, one of his earliest designs was to recoin it, and to restore it to its intrinsic value. This scheme, however, met with great opposition, being characterised as a wild project, unsuited to a period when the nation was engaged in war, as highly injurious to the interests of commerce, and as likely to sap the foundation of the government. But he had studied the subject too deeply, and had entrenched himself behind opinions too impartial and too well founded, to be driven from a measure which the best interests of his country seemed to require.

The persons whom Mr. Montague had consulted about the re-coinage, were Newton, Locke, and Halley, and in consequence of Mr. Overton, the warden of the Mint, having been appointed a Commissioner of Customs, he embraced the opportunity which was thus offered, of serving his friend and his country, by recommending Newton to that important office. The notice of his appointment was conveyed to Newton in the following letter:—

London, 19th March, 1693.

"Sir,

"I am very glad that, at last, I can give you a good proof of my friendship, and the esteem the king has of your merit. Mr. Overton, the Warden of the Mint, is made one of the Commissioners of the Customs, and the king has promised me to make Mr. Newton Warden of the Mint. The office is the most proper for you. 'Tis the chief office in the Mint, 'tis worth five or six hundred pounds per annum, and has not too much business to require more attendance than you can spare. I desire you will come up as soon as you can, and I will take care of your warrant in the meantime. Let me see you as soon as you come to town, that I may carry you to kiss the king's hand. I believe you may have a lodging near me.

"I am, &c., &c.

"Charles Montague."

In this new situation the mathematical and chemical knowledge of Newton was of great service to the nation, and he became eminently useful in carrying on the re-coinage, which was completed in the space of two years. In the year 1699, he was promoted to the Mastership of the Mint,—a situation which was worth £1500 per annum, and which he held during the remainder of his life. While he was yet Master of the Mint, he wrote an official report on the coinage which was published; and he likewise drew up a table of essays of foreign coins, which was printed at the end of Dr. Arbuthnot's Tables of Ancient Coins, Weights, and Measures, and appeared in 1727.

Newton retained his professorship at Cambridge till his promotion to the Mastership in 1699, when he appointed Mr. Whiston to be his deputy, with all the emoluments of the office; and when he resigned the chair in 1703, he succeeded in getting him nominated his successor.

The appointment of Newton to the Mastership of the Mint must have been peculiarly gratifying to the members of the Royal Society, and it was probably from a feeling of gratitude to Mr. Montague, more than from a regard for his talents, that this able statesman was unanimously elected president of that learned body. This office he held for three years, and Newton had the satisfaction of addressing to him his solution of the celebrated problems proposed by John Bernouilli.

Charles Montague was created Earl of Halifax in 1700; and after the death of his first wife he conceived an ardent attachment for the niece of Newton, who was the widow of Colonel Barton. This lady was young, gay, and beautiful, and though she did not escape the censures of the world, she was regarded by all who knew her as a woman of the strictest honour and virtue. What were the causes which prevented her union with the Earl of Halifax, we have no means of knowing, but so great was the esteem and affection which he bore her, that in his will in which he left £100 to Mr. Newton, he bequeathed to Mrs. Barton a very large portion of his fortune. This distinguished statesman died in 1715, in the fifty-fourth year of his age. Himself, a poet and an elegant writer, he was the liberal patron of genius, and he numbered among his intimate friends Congreve, Halley, Prior, Tickel, Steele, and Pope. His conduct to Newton will be for ever remembered in the annals of science. The sages of every nation and of every age will pronounce with affection the name of Charles Montague, and the persecuted science of England will continue to deplore that he was the first and the last English minister who honoured genius by his friendship and rewarded it by his patronage.

The elevation of Mr. Newton to the highest offices in the mint, was followed by other marks of honour. The Royal Academy of Sciences at Paris having been empowered by a New Charter, granted in 1699, to admit a very small number of foreign associates, Newton was elected a member of that distinguished body. In the year 1701, on the assembling of a new parliament, he was re-elected one of the members for the University of Cambridge. In 1703, he was chosen President of the Royal Society of London, to which office he was annually re-elected during the twenty-five remaining years of his life. On the 16th of April, 1705, when Queen Anne was living at the royal residence of Newmarket, she went with Prince George of Denmark, and the rest of the court to visit the University of Cambridge. After the meeting of the Regia Consilia, her majesty held a court at Trinity Lodge, the residence of Dr. Bentley then master of Trinity, where the honour of knighthood was conferred upon Mr. Newton, Mr. John Ellis, the vice-chancellor, and Mr. James Montague, the university counsel.

On the dissolution of the parliament, which took place in 1705, Sir Isaac was again a candidate for the representation of the university, but notwithstanding the recent expression of the royal favour, he lost his election by a very great majority. There were four candidates, of whom the highest had 182 votes, while Sir Isaac, the lowest, had only 117. This singular result was perhaps owing to the loss of that personal influence which his residence in the university could not fail to command, though it is more probable that the ministry preferred the candidates of a more obsequious character, and that the electors looked for advantages which Newton was not able to procure for them.

Although the first edition of the Principia had been for some time sold off, and copies had become scarce, yet Sir Isaac's attention was so much occupied with his professional avocations, that he could not find leisure for preparing a new edition. Dr. Bentley, who had frequently urged him to undertake the task, at last succeeded by engaging Roger Cotes, Plumian Professor of Astronomy at Cambridge, to superintend its publication at the university press. In June, 1709, Sir Isaac committed this important trust to his young friend; and about the middle of July, he promised to send him in the course of a fortnight, his own revised copy of the work. Business, however, seems to have intervened and that gentleman was obliged to remind Sir Isaac of his promise, which he did in the following letter :—

"Cambridge, Aug. 18th, 1709.

"SIR,

"The earnest desire I have to see a new edition of your "Principia," makes me somewhat impatient till we receive your copy of it, which you were pleased to promise me about the middle of last month you would send down in about a fortnight's time. I hope you will pardon me for this uneasiness. from which I cannot free myself, and for giving you this trouble to let you know it. I have been so much obliged by yourself and your book, that (I desire you to believe me,) I think myself bound in gratitude to take all the care that I properly can that it shall be correct. Your obliged servant, "ROGER COTES."

"For Sir Isaac Newton, at his house,
in Jermyn Street, near St. James's
Church, Westminster."

This was the commencement of that lengthened correspondence, consisting of nearly three hundred letters, in which Sir Isaac and Mr. Cotes discussed the various improvements which were thought necessary in a new edition of the Principia. The revised copy was forthwith sent to Cambridge, and Mr. Cotes gladly undertook the labour of superintending the printing of it.

When the work was ready for publication, Mr. Cotes expressed a wish that Dr. Bentley should write a preface to it, but it was the opinion both of Sir Isaac and the Doctor, that the preface should come from the pen of Mr. Cotes himself. This he engaged to do; but previous to writing it he addressed the following letter to Dr. Bentley, in order to learn from Sir Isaac the particular view with which it should be written.

"March 10th, 1712-13.

"SIR,

"I received what you wrote to me in Sir Isaac's letter. I will set about the index in a day or two. As for the preface, I should be glad to know from Sir Isaac with what view he thinks proper to have it written. You know the book has been received abroad with some disadvantage, and the cause of it may be easily guessed at. The "Commercium Epistolicum," lately published by order of the Royal Society, gives such indubitable proofs of Mr. Leibnitz's want of candour, that I shall not scruple in the least to speak out the full truth of the matter, if it be thought convenient. There are some pieces of his looking this way, which deserve a censure, as his "Tentamen de motuum cælestium causis." If Sir Isaac is willing that something of this nature may be done, I should be glad if, whilst I am making the index, he would consider of it, and put down a few notes of what he thinks most material to be insisted on. This I say upon supposition that I write the preface myself. But I think it will be much more advisable that you, or he, or both of you, should write it whilst you are in town. You may depend upon it I will own it, and defend it as well as I can, if hereafter there should be occasion. I am, Sir, &c.

We have no means of learning the instructions which were given to Mr. Cotes; but it appears from the preface itself that Sir Isaac had prohibited any personal reference to the conduct of Leibnitz.

In the year 1714 several captains and owners of merchant vessels petitioned the House of Commons to consider the propriety of bringing in a bill to reward inventions for promoting the discovery of the longitude at sea. A committee was appointed to investigate the subject, and Mr. Ditton and Mr. Whiston, having thought of a new method of finding the longitude, submitted it to the committee. Four members of the Royal Society viz., Sir Isaac Newton, Dr. Halley, Mr. Cotes, and Dr. Clarke, were examined on the subject, along with Messrs. Ditton and Whiston. The three last of these philosophers stated their opinions verbally. Mr. Cotes considered the proposed scheme as correct in theory and on shore, and both he and Dr. Halley were of opinion that expensive experiments would be requisite. Newton, when called upon for his opinion, read the following memorandum, which is worthy of being recorded:—

"For determining the longitude at sea there have been several projects, true in theory, but difficult to execute.

"1. One is by a watch to keep time exactly; but by reason of the motion of the ship, the variation of heat and cold, wet or dry, and the difference of gravity in different latitudes, such a watch hath not yet been made.

"2. Another is by the eclipses of Jupiter's satellites; but, by reason of the length of telescopes requisite to observe them, and the motion of a ship at sea, those eclipses cannot yet be there observed.

"3. A third is by the place of the moon; but her theory is not yet exact for that purpose; it is exact enough to determine the longitude within two or three degrees, but not within a degree.

"4. A fourth is Mr. Ditton's project, and this is rather for keeping an account of the longitude at sea, than for finding it, if at any time it should be lost, as it may easily be in cloudy weather. How far this is practicable, and with what charge, they that are skilled in sea affairs are best able to judge. In sailing by this method, whenever they are to pass over very deep seas, they must sail due east or west; they must first sail into the latitude of the next place to which they are going beyond it, and then keep due east or west, till they come at that place. In the three first three ways there must be a watch regulated by a spring, and rectified every visible sunrise and sunset, to tell the hour of the day or night. In the fourth way such a watch is not necessary. In the first way there must be two watches, this and the other above mentioned. In any of the three first ways, it may be of some service to find the longitude within a degree, and of much more service to find it within forty minutes, or half a degree if it may be, and the success may deserve rewards accordingly. In the fourth way, it is easier to enable seamen to know their distance and bearing from the shore, 40, 60, or 80 miles off, than to cross the seas; and some part of the reward may be given, when the first is performed on the coast of Great Britain, for the safety of ships coming home: and the rest, when seamen shall be enabled to sail to an assigned remote harbour without losing their longitude if it may be."

The committee brought up their report on the 11th of June, and recommended that a bill should be introduced into parliament for the purpose of rewarding inventions or discoveries connected with the determination of the longitude. The bill passed the House of Commons on the 3rd of July, and was agreed to by the Lords on the 8th of the same month.

Mr. Whiston, in giving an account of this transaction, in his "Longitude Discovered," states that nobody understood Sir Isaac's paper, and that after sitting down he obstinately kept silence, though he was much pressed to explain himself more distinctly. At last seeing that the scheme was likely to be rejected, Whiston ventured to say that Sir Isaac did not wish to explain more through fear of compromising himself, but that he really approved of the plan. Sir Isaac, he goes on to say, repeated every word that he had said. This is the part of Newton's conduct which M. Biot has described as puerile, and "tending to confirm the fact of the aberration of his intellect in 1693." We must be satisfied with the correctness of Whiston's statement before we can admit such a censure. The paper read by Newton is perfectly plain, and we may easily understand how he might have approved of Mr. Ditton's plan as ingenious and practicable under particular circumstances, though he did not think it of that paramount importance which would have authorized the House of Commons to distinguish it by a parliamentary reward. The conflict between public duty and a disposition to promote the interests of Messrs. Whiston and Ditton, was undoubtedly the cause of that embarrassment of manner which the former of these mathematicians has so ungratefully brought before the public.

# CHAPTER VII.

Respect in which Newton is held at the court of George I. The Princess of Wales delighted with his conversation. She obtains a manuscript copy of his system of Chronology. She allows the Abbe Conti to take a copy of it on the promise of secrecy. He publishes it surreptitiously in French. Sir Isaac's defence of his system. Theological studies of Sir Isaac. Origin of Newton's Theological studies. Analogy between the Book of Nature and that of Revelation. Minor Discoveries of Newton. His Reflecting Sextant. Reflecting Microscope. Prismatic Reflector. His method of varying the Magnifying power of Newtonian Telescopes. His Experiments on Impressions on the Retina.

Sir Isaac Newton became an object of interest at the British court on the accession of George I., to the throne of these dominions in 1714. His high situation under government—his splendid reputation—his spotless character—and above all, his unaffected piety, attracted the attention of the Princess of Wales. This lady who possessed a highly cultivated mind, derived the greatest pleasure from conversing with Newton and corresponding with Leibnitz. In all her difficulties she applied to Sir Isaac, from whom she received that information and assistance which she had elsewhere sought in vain; and she was frequently heard to declare that she considered herself as fortunate in living at a period which enabled her to enjoy the conversation of so great a genius. But while Newton was thus esteemed by the house of Hanover, Leibnitz, his great rival, endeavoured to weaken and undermine his influence. In his correspondence with the princess he represented the Newtonian philosophy as, not only physically false, but as injurious to the interests of religion. He asserted that natural religion was rapidly declining in England, and he supported this assertion by referring to the works of Locke, and to the beautiful and pious sentiments contained in the 28th query at the end of the Optics. He represented the principles of these great men as precisely the same with those of the materialists, and thus endeavoured to degrade the character of English philosophers.

These attacks of Leibnitz became subjects of conversation at court, and when they reached the ear of the king, his majesty expressed his expectation that Sir Isaac would draw up a reply. He accordingly entered the lists on the mathematical part of the controversy, and left the philosophical part of it to Dr. Clarke, who was more than a match for the German philosopher. The correspondence which ensued was carefully perused by the princess, and from the estimation in which Sir Isaac continued to be held we may infer that the views of the English philosopher were not very remote from her own.

One day, when Sir Isaac was conversing with her royal highness on some portion of ancient history, he was led to mention to her, and to explain, his new system of chronology which he composed during his residence at Cambridge, when he was in the habit, as he himself expresses it, "of refreshing himself with history and chronology when he was weary with other studies." The princess was so much pleased with this ingenious system, that she afterwards sent a message by the Abbe Conti to Sir Isaac, requesting him to speak with her, and she, when he came, earnestly entreated him to favour her with a copy of the interesting work which contained his system of chronology. Sir Isaac informed her, that it existed merely in separate papers, which were not only in a state of confusion, but which contained a very imperfect view of the subject, and he promised, in a few days to draw up an abstract of it for her own private use, and on the condition that it should not be communicated to any other person whatever. Some time after the princess received the manuscript, she requested that the Abbe Conti might be allowed to have a copy of it. Sir Isaac granted this request, and the Abbe was informed that he received a copy of the manuscript with Sir Isaac's permission, at the princess's request, and that it was to be kept secret. The manuscript which was thus confidingly put into the hands of a foreigner, was entitled "A short Chronicle from the first Memory of Things in Europe to the Conquest of Persia by Alexander the Great." It consists of about twenty four quarto printed pages, with an introduction of four pages, in which Sir Isaac states that he "does not pretend to be exact to a year, that there may errors of five or ten years, and sometimes twenty, but not much above."

The Abbe Conti kept his promise of secresy so long as he remained in England, but he no sooner reached Paris, than he communicated it to M. Freret, a learned antiquarian, who not only translated it, but drew up observations upon it for the purpose of refuting some of its principal results. Sir Isaac was unacquainted with this transaction till he was informed of it by the French bookseller, M. Cavalier, in whose hands it had been placed, who requested his leave to publish it, and charged one of his friends in London to procure Sir Isaac's answer, which was speedily given as follows:—

"I remember that I wrote a Chronological Index for a particular friend, on condition that it should not be communicated. As I have not seen the manuscript which you have under my name, I know not whether it be the same. That which I wrote was not all done with design to publish it. I intend not to meddle with that which hath been given you under my name, nor to give any consent to the publishing of it. I am your very humble servant.

"IS. NEWTON."

London, May 27th, 1725, O.S.

Before this letter was written, M. Cavalier had received the royal sanction for printing the work; and when it was completed he sent a copy of it in a present to Sir Isaac, who received it on the 11th November, 1725. It was entitled "Abrege de Chronologie de M. Le Chevalier Newton, fait par lui-meme, et traduit sur le manuscript Anglais," and was accompanied with observations by M. Freret, the object of which was to contradict the leading points of the system. An advertisement was prefixed to it, in which M. Cavalier defends himself for publishing it without the author's permission, on the ground that he had written three letters to obtain his consent, and had declared that he would take Sir Isaac's silence as a tacit permission. When Sir Isaac received this work, he drew up a paper entitled "Remarks on the Observations made on a Chronological Index of Sir Isaac Newton, translated into French by the Observator, and published at Paris." These remarks were published in the 33rd volume of the Philosophical Transactions for 1725. He gives a complete history of the transaction; charges the Abbe Conti with deceit, and blames M. Cavalier for having asked his permission to publish the translation without sending him a copy for his perusal, without acquainting him with the name of the translator, and without announcing his intention of publishing along with it a refutation of the original. The observations made by the translator against the conclusions deduced by the author were founded on an imperfect knowledge of Sir Isaac's system; and they are so plausible that Dr. Halley confesses that he was at first prejudiced in favour of the observations, taking the calculations for granted, and not having seen the original.

To all the observations of Freret, Sir Isaac returned an explicit answer. This presumptuous antiquary had ventured to state at the end of his observations, "that he believed he had stated enough concerning the epochs of the Argonauts, and the length of generations, to make people cautious about the rest; for these are the two foundations of all this new system of chronology." He founds his arguments against the epochs of the Argonauts, as fixed by our author, on the supposition that Sir Isaac places the vernal equinox at the time of the Argonautic expedition *in the middle of the sign of Aries*, whereas Sir Isaac places it *in the middle of the constellation*—a point corresponding with the middle of the back of Aries, or eight degrees from the first star of Aries. This position of the colure is assigned on the authority of Eudoxus, as given by Hipparchus, who says that the colure passed over the back of Aries. Setting out with this mistake, M. Freret concludes that the Argonautic expedition took place 532 years earlier than Sir Isaac allowed. His second objection relates to the length of generations, which he asserts is made in this new system only 18 or 20 years. On the contrary, Sir Isaac reckons a generation at 33 years, or 3 generations at 100; and it was the length of the reigns of kings that he made 18 or 20 years. This deduction he founds on the reigns of 64 French kings. Now the ancient Greeks and Egyptians reckoned the length of a reign equal to that of a generation; and it was by correcting this mistake, and adopting a measure founded on fact, that Sir Isaac placed the Argonautic expedition 44 years after the death of Solomon, and fixed some of the other points of his system.

Sir Isaac's answer to these objections called into the field a fresh antagonist, Father Souciet, who published no fewer than five dissertations on the new chronology. These dissertations were written in a style disgraceful to a scholar; and the friends of Sir Isaac being afraid that the manner in which his system was attacked, would be more hurtful to his feelings than the arguments themselves, prevailed upon a friend to draw up an abstract of Souciet's objections, stripped of their offensive language. The perusal of these arguments only convinced Sir Isaac of the shameful ignorance of their author; and he was induced to read the original work, which only confirmed him in opinion.

In consequence of these attacks and discussions, Sir Isaac was prevailed upon, in order to set the matter at rest, to prepare his larger work for publication. He had nearly completed it at the time of his death, and it was published in 1728, under the title of "The Chronology of Ancient Kingdoms amended, to which is prefixed, a Short Chronicle from the first memory of Things in Europe, to the Conquest of Persia, by Alexander the Great." It was dedicated to the Queen, by Mr. Conduit, and consists of six chapters; 1. On the Chronology of the Greeks; 2. Of the Empire of Egypt; 3. Of the Assyrian Empire; 4. Of the two contemporary Empires of the Babylonians and Medes; 5. A Description of the Temple of Solomon; 6. Of the Empire of the Persians. The sixth chapter was not copied out with the other five, which makes it doubtful whether it was intended for publication or not, but as it was found among his papers, and appeared to be a continuation of the same work, it was thought proper by the friends of the deceased author to add it to the other five chapters.

After the death of Newton, Dr. Halley, who had not yet seen the larger work, felt himself called upon, both as astronomer-royal and as the personal friend of the author, to reply to the first and last dissertation of Father Souciet, which were chiefly astronomical; and in two papers printed in the 34th and 35th volumes of the Philosophical Transactions, he has done this in a most convincing and learned argument, showing the total incapacity of Souciet for the task which he had so zealously undertaken.

Among the supporters of the views of Newton, we may enumerate Dr. Reid, Nauze, and some other writers; and among its opponents who were far more numerous, M. Freret, who left at his death a posthumous work on the subject, M. Fourmond, Mr. A. Bedford, Dr. Shuckford, Dr. Middleton, Whiston, and M. Delambre. The object of Fourmond is to show the uncertainty of the astronomical argument, arising on the one hand from the vague account of the ancient sphere as given by Hipparchus; and, on the other, from the extreme rudeness of ancient astronomical observations. Delambre has taken a similar view of the subject. He regards the observations of ancient astronomers as too incorrect to form the basis of a system of chronology; and he maintains, that if we admit the accuracy of the details in the sphere of Eudoxus, and suppose them all to belong to the same epoch, all the stars which it contains ought at that epoch to be found in the place where they are marked, and we might thence verify the accuracy, and ascertain the state of the observations. It follows, however, from such an examination, that the sphere would indicate almost as many different epochs as it contains stars. Some of them even had not, in the time of Eudoxus, arrived at the position which had been for a long time attributed to them, and will not even reach it for 300 years to come, and on this account he considers it impossible to deduce any chronological conclusions from such a rude mass of errors.

But however well founded these observations may be, we heartily concur in the opinion of M. Danou "that they are not sufficient to establish a new system, and we must regard the system of Newton, as a great fact in the history of chronological science, and as confirming the observation of Varro, the stage of history does not commence till the first Olympiad."

While upon this part of our subject, we must not forget to enumerate, among the chronological writings of Sir Isaac Newton, his "Letter to a person of distinction, who had desired his opinion of the learned Bishop Lloyd's hypothesis of the most ancient year." This hypothesis was sent by the Bishop of Worcester to Dr. Prideaux. Sir Isaac says, that it is filled with many excellent observations on the ancient year; but he does not find it proved that any ancient nations used a year of 12 months and 360 days, without correcting it from time to time by the luminaries, to make the months keep to the course of the moon, and the year to the course of the sun, and returns of the seasons and fruits of the earth." After examining the years of all the nations of antiquity, he concludes, "that no other years are to be met with among the ancients, but such as were either luni-solar, or solar or lunar, or the calendars of these years. A practical year," he adds, "of 360 days, "is none of these.—The beginning of such a year would have run round the four seasons in seventy years; and such a notable revolution would have been mentioned in history, and is not to be asserted without proving it."

We will now turn our attention to the history of the theological studies of Sir Isaac Newton, which must ever be regarded as one of the most interesting portions of his life. That he who among all the individuals of his species possessed the highest intellectual powers, was not only a learned and profound divine, but a firm believer in the great doctrines of religion, is one of the proudest triumphs of the Christian faith. Had he distinguished himself only by an external respect for the offices and duties of religion, and had he left merely in his last words an acknowledgment of his faith, his piety might have been ascribed to the decay or the extinction of his transcendant powers. But Isaac Newton had been a Christian from his early youth, and though never intended to be a preacher of the gospel, yet he interchanged the study of the Scriptures with that of the laws of the material universe; and from the examination of the works of the Creator, he found it to be no abrupt transition to investigate the revelation of his will, and to calculate the immortal destinies of mankind.

But when the religious professions of Sir Isaac Newton could not be ascribed to an ambition of popularity, to the influence of weak health, or to the force of professional impulse, it became necessary for the apostles of infidelity to refer it to some extraordinary cause. His supposed aberration of intellect was therefore eagerly seized upon by some as a plausible origin for his religious principles: while others, without any view of supporting the cause of scepticism, ascribed his theological researches to the habits of the age in which he lived, and to a desire of promoting political freedom, by turning against the abettors of despotism those powerful weapons which the Holy Scriptures supplied. The anxiety of Laplace to refer his religious writings to a late period of his life, seems to have also been felt by Biot, who has gone so far as to fix the date of one of his most important works, and thus to establish the erroneous suspicions of his colleague.

In his observations on Sir Isaac's "Historical Account of two notable Corruptions of the Scriptures," he says, "From the nature of the subject, and from certain indications which Newton seems to give at the beginning of his dissertation, we may conjecture with probability that he composed it at the time when the errors of Whiston, and a work of Dr. Clarke on the same subject, drew upon them the attacks of all the theologians of England, which would place the date between the years 1712 and 1719. It would then be truly a prodigy to remark, that a man of from seventy-two to seventy-five years of age was able to compose, *rapidly*, as he leads us to believe, so extensive a piece of sacred criticism, of literary history, and even of bibliography, where an erudition the most vast, the most varied, and the most ready, always supports an argument well arranged and powerfully combined. * * * At this epoch of the life of Newton, the reading of religious books had become one of his most habitual occupations; and after he had performed the duties of his office, they formed, along with the conversation of his friends, his principal amusement. He had then almost ceased to care for the sciences; and as we have already remarked, since the fatal epoch of 1693, he gave to the world only three really new scientific productions."

M. Biot has, however, adopted 1712-19 as the date of this critical dissertation, notwithstanding the prodigy which it involves;—it is regarded as the composition of a man of seventy-two or seventy-five;—the reading of religious books is stated to have become one of his most habitual occupations, and such reading to have been one of his principal amusements;—and all this is associated with "the fatal epoch of 1693," as if his illness at that time had been the cause of his abandoning science and betaking himself to theology. Carrying on the same views, Mr. Biot asks, in reference to Sir Isaac's works on Prophecy, "How a mind of the character and force of Newton's, so habituated to the severity of mathematical considerations, so exercised in the observation of real phenomena, and so well aware of the conditions by which truth is to be discovered, could put together such a number of conjectures without noticing the extreme improbability of his interpretations from the infinite number of arbitrary postulates on which he has founded them?" We would apply the same question to the reasoning by which Biot fixes the date of the critical dissertation; and we would ask how so eminent a philosopher could hazard such frivolous conjectures upon a subject on which he had not a single fact to guide his inquiries. The obvious tendency, though not the design of the conclusion at which he arrives, is injurious to the memory of Sir Isaac, as well as to the interests of religion; and these considerations might have checked the temerity of speculation even if it had been founded on better data. The Newtonian interpretation of the Prophecies, and especially that part which Biot characterizes as unhappily stamped with the spirit of prejudice, has been adopted by men of the soundest and most unprejudiced minds; and, in addition to the moral and historical evidence by which it is supported, it may yet be exhibited in all the fullness of demonstration. But the speculation of Biot respecting the date of Newton's theological works was never maintained by any other person than himself, and is capable of being disproved by the most incontrovertible evidence.

We have previously seen in the extract from Mr. Pryme's manuscript journal, that before the year 1692, when a shade is supposed to have obscured his powerful mind, Newton was well known as an "excellent divine"—a character which it was impossible he could have acquired without the attention of many years to theological researches; but important as this argument would have been, we are fortunately not left to so general a defence. The correspondence of Newton with Locke, published by Lord King, places it beyond the possibility of a doubt that he had begun his researches respecting the prophecies before the year 1691—before the 49th year of his age, and previous to "the fatal epoch of 1693." That he had previously discussed this interesting subject is evident from the following letter to his friend:—

"Cambridge, Feb. 7th, 1690-91.

"Sir,

"I am sorry your journey proved to so little purpose, though it delivered you from the trouble of the company the day after. You have obliged me by mentioning me to my friend at London, and I must thank both you and my Lady Masham for your civilities at Oates, and for not thinking that I made a long stay there. I hope we shall meet again in due time, and then I should be glad to have your judgment upon some of my mystical fancies. The Son of Man, Dan. vii., I take to be the same with the Word of God upon the White Horse in Heaven, Apoc. xii., for both are to rule the nations with a rod of iron; but whence are you certain that the Ancient of Days is Christ? Does Christ any where sit upon the throne? If Sir Francis Masham be at Oates, present, I pray my service to him with his lady, Mrs. Cudworth and Mrs. Masham. Dr. Covel is not in Cambridge.—I am your affectionate and humble servant,

"Is. Newton."

"Know you the meaning of Dan. x. 21. There is none that holdeth with me in these things, but Mich. the Prince."

Having thus determined the date of those investigations which constitute his "Observations on the Prophecies of Holy Writ," particularly the prophecies of Daniel and the Apocalypse, we shall proceed to fix the latest date of his "historical account of two notable corruptions of the Scripture, in a letter to a friend."

This work seems to have been a very early production of Newton's. It was written in the form of a letter to Mr. Locke, and at the time, Sir Isaac appears to have been anxious for its publication. Afraid, however, of being again led into a controversy, and dreading the intolerance to which he might be exposed, he requested his friend, who was at that time intending to proceed to Holland, to get it translated into French, and published on the continent. Having abandoned his design of visiting Holland, Locke transmitted the manuscript, without the author's name, to his learned correspondent, M. Le Clerc, in Holland; and it appears from a letter of Le Clerc to Mr. Locke, that he had received the manuscript previous to April 11th, 1691. M. Le Clerc, however, delayed taking any steps regarding its publication for a long time; and it was till the 20th of January, 1692, that he announced to Locke, his intention of publishing the tract in Latin. When this plan was communicated to Newton, he became alarmed at the risk of detection, and resolved to stop the publication. This resolution he intimated to Mr. Locke in the following letter :—

"Cambridge, Feb. 16th, 1691-2.

"Sir,

"Your former letters came not to my hand, but this I have. I was of opinion my papers had lain still, and am sorry to hear there is news about them. Let me entreat you to stop their translation and impression so soon as you can; for I design to suppress them. If your friend hath been at any pains and charge, I will repay it, and gratify him. I am very glad my Lord Monmouth is still my friend, but intend not to give his lordship and you any farther trouble. My intentions are to sit still. I am to beg his lordship's pardon for pressing into his company the last time I saw him, and had not done it, but that Mr. Paulin pressed me into the room. Miracles, of good credit, continued in the church for about two or three hundred years. Gregorius Thaumaturgus had his name from thence, and was one of the latest who was eminent for that gift; but of their number and frequency I am not able to give you a just account. The history of those ages is very imperfect. Mr.

Paulin told me you had writ for some of Mr. Boyle's red earth, and by that I know you had the receipt.

"Your most affectionate and obedient servant,

"Is. Newton."

We have here positive evidence that this celebrated treatise which M. Biot asserts to have been written between the years 1712 and 1719, was actually in the hands of Le Clerc in Holland, at least twenty-one years previously, viz, April, 1691; and consequently before the time of its author's supposed insanity. Mr. Locke lost no time in obeying the request of Newton, and Le Clerc instantly stopped the publication of the letter; but, as he had never understood who was the author, he deposited the manuscript, which was in the handwriting of Locke, in the library of the Remonstrants, where it was afterwards found, and was published at London, in 1754, under the title of "Two Letters from Sir Isaac Newton to M. Le Clerc" —a form which had never been given to it by its author. The copy thus published was very imperfect, as it wanted both the beginning and the end, and erroneous in many places; but Dr. Horsley has published a genuine edition, which is in the form of a single letter to a friend, and was printed from a manuscript in the handwriting of Sir Isaac Newton, in the possession of the Rev. Dr. Ekins, Dean of Carlisle.

Having thus determined the dates of the principal theological writings of Sir Isaac, with as much accuracy as possible, we shall now proceed to give some account of their contents.

"The Observations on the prophecies of Daniel, and the Apocalypse of St. John" were published in London, in 1733, in one quarto volume. The work is divided into two parts, the first of which treats of the Prophecies of Daniel, and the second of the Apocalypse of St. John. It begins with an account of the different books which compose the Old Testament, and, as the author considers Daniel to be the most distinct in the order of time, and the easiest to be understood, he makes him the king to all the prophetic books in those matters which relate to the *last time.* He next considers the figurative language of the prophets, which he regards as taken "from the analogy between the world natural, and an empire or kingdom considered as a world political;" the heavens, and the things therein, the inferior people; and the lowest parts of the earth the most miserable of the people. The sun is put for the whole race of kings, the moon for the body of the common people, and the stars for subordinate princes and rulers. In the earth, the dry land and the waters are put for the people of several nations. Animals and vegetables are also put for the people of several regions. When a beast or man is put for a kingdom, his parts and qualities are put for the analogous parts and qualities of the kindom; and when a man is taken in a mystical sense, his qualities are often signified by his actions, and by the circumstances and things about him. In applying these principles he commences with the vision of the image composed of four different metals. This image he considers as representing a body of four great nations which should reign in succession over the earth, viz., the people of Babylonia, the Persians, the Greeks, and the Romans, while the stone cut out without hands is a new kingdom which should arise after the four, conquer all those nations, become very great, and continue to the end of time.

The vision of the four beasts is the prophecy of the four empires repeated, with several new editions. The lion with eagle's wings was the kingdom of Babylon and Media, which overthrew the Assyrian power. The beast like a bear was the Persian empire, and its three ribs were the kingdoms of Sardis, Babylon, and Egypt. The third beast, like a leopard was the Greek empire, and its four heads and four wings were the kingdoms of Cassander, Lysimachus, Ptolemy, and and Seleucus. The fourth beast, with its great iron teeth was the Roman empire, and its ten horns were the ten kingdoms into which it was broken in the reign of Theodosius the Great.

In the fifth chapter the author treats of the kingdoms represented by the feet of the image composed of iron and clay which did not stick to one another, and which were of different strength. These were the Gothic tribes called Ostrogoths, Visigoths, Vandals, Gepidae, Lombards, Burgundians, Alans, &c., all of whom had the same manners and customs, and spoke the same language, and who, about 416 years before Christ, were all quietly settled in several kingdoms within the empire, not only by conquest, but by grants of the Emperor.

In the sixth chapter Sir Isaac treats of the ten kingdoms represented by the ten horns of the fourth beast, into which the western empire became divided about the time when Rome was besieged and taken by the Goths. These kingdoms were

1. The kingdom of the Vandals and Alans in Spain and Africa.
2. The kingdom of Suevians in Spain.
3. The kingdom of the Visigoths.
4. The kingdom of the Alans in Gaul.
5. The kingdom of the Burgundians.
6. The kingdom of the Franks.
7. The kingdom of the Britons.
8. The kingdom of the Huns.
9. The kingdom of the Lombards.
10. The kingdom of Ravenna.

Some of these kingdoms at length fell and new ones sprung up; but whatever was their subsequent number, they still retain the name of the ten kings from their first number.

The eleventh horn of Daniel's fourth beast is shown in the seventh chapter to be the Church of Rome in its triple character of a seer, a prophet, and a king, and its power to change times and laws is copiously illustrated in the eighth chapter.

In the ninth chapter our author treats of the kingdom represented in Daniel by the ram and the he-goat, the ram indicating the kingdom of the Medes and Persians from the beginning of the four empires, and the he-goat the kingdom of the Greeks to the end of them.

The prophecy of the seventy weeks, which had hitherto been restricted to the first coming of our Saviour, is shown to be a prediction of all the main periods relating to the coming of the Messiah, the times of his birth and death, the time of his rejection by the Jews, the duration of the Jewish war, by which he caused the city and sanctuary to be destroyed, and the time of his second coming.

In the eleventh chapter our author treats, with great sagacity and acuteness, of the time of our Saviour's birth and passion—a subject which had exceedingly perplexed all preceding commentators.

After explaining in the twelfth chapter the last prophecy of Daniel, viz., that of the scripture of truth, which he considers as a commentary on the vision of the ram and he-goat, he proceeds in the thirteenth chapter to the prophecy of the king who did according to his will, and magnified himself above every god, and honoured Mahuzzims, and regarded not the desire of women. He shows that the Greek empire, after the division of the Roman empire into the Greek and Latin empires, became the king who in matters of religion did according to his will, and in legislation exalted and magnified himself above every god.

In the second part of the work on the Apocalypse of

St. John, Sir Isaac treats, 1st, of the time when the prophecy was written, which he conceives to have been during John's exile in Patmos, and before the epistles to the Hebrews and to Peter were composed, which in his opinion have a reference to the Apocalypse; 2ndly, of the scene of the vision, and the revelation which the Apocalypse has to the book of the law of Moses and to the worship of God in the temple; and, 3rdly, of the relation which the Apocalypse has to the prophecies of Daniel, and of the subject of the prophecy itself.

Sir Isaac Newton regards the prophecies of the Old and New Testament not as given to gratify men's curiosity, by enabling them to foreknow things, but that, after they were fulfilled, they might be interpreted by the event, and afford convincing arguments that the world is governed by Providence. He considers that there is so much of this prophecy already fulfilled, as to afford to the diligent student sufficient instances of God's Providence; and he adds, that "amongst the interpreters of the last age there is scarce one of note who hath not made some discovery worth knowing, and thence it seems one may gather that God is about opening these mysteries. The success of others put me upon considering it, and if I have done anything which may be useful to following writers, I have my design."

Such is a brief abstract of this truly valuable and ingenious work, which is characterised throughout by great learning, and marked with the sagacity of its distinguished author. The same qualities of his gifted mind are equally conspicuous in his "Historical Account of Two Notable Corruptions of Scripture."

This praiseworthy treatise relates to two texts in the Epistles of St. John and St. Paul. The first of these is in 1 John v. 7:—"For there are three that bear record in heaven, the Father, the Son, and the Holy Ghost, and these three are one." This text he considers as a gross corruption of Scripture, which had its origin among the Latins, who interpreted the Spirit, Water, and Blood, to be Father, Son, and Holy Ghost, in order to prove them one. With the same view Jerome inserted the Trinity in express words in his version. The Latins marked his variations in the margins of their books; and in the twelfth and following centuries, when the disputations of the schoolmen were at their height, the variation began to creep into the text in transcribing. After the invention of printing, it crept out of the Latin into the printed Greek, contrary to the authority of all the Greek manuscripts and ancient versions; and from the Venetian press it went soon after to Greece. After proving these positions, Sir Isaac gives the following paraphrase of this remarkable passage:—

" 'Who is he that overcometh the world, but he that believeth that Jesus is the Son of God,' that Son spoken of in the Psalms, where he saith, 'thou art my Son, this day have I begotten thee.' 'This is he that,' after the Jews had long expected him, *came*, first in a mortal body, *by* baptism of *water*, *and* then in an immortal one, *by* shedding his *blood* upon the cross and rising again from the dead; 'not by water only, but by water and blood;' being the Son of God as well by his resurrection from the dead (Acts xiii. 33) as by his supernatural birth of the virgin (Luke i. 35). 'And it is the Spirit' also *that*, together with the water and blood, 'beareth witness' to the truth of his coming; 'because the Spirit is truth;' and so a fit and unexceptionable witness, 'For there are three that bear record' of his coming; 'the Spirit,' which he promised to send, and which was since shed forth upon us in the form of cloven tongues, and in various gifts; 'the' baptism of 'water,' wherein God testified 'this is my beloved Son;' 'and the' shedding of his 'blood,' accompanied with his resurrection, whereby he became the most faithful martyr, or witness, of this truth. 'And these three,' the Spirit, the baptism, and passion of Christ, 'agree in' witnessing 'one' and the same thing, (namely, that the Son of God is come;) and therefore their evidence is strong: for the law requires but two consenting witnesses, and here we have three: 'and if we receive the witness of men,' the threefold 'witness of God,' which he bare of his Son, by declaring at his baptism, 'this is my beloved Son;' by raising him from the dead, and by pouring out his Spirit on us, 'is greater;' and, therefore, ought to be more readily received."

While the Latin Church was corrupting the preceding text, the Greek Church was doing the same to St. Paul's 1st Epistle to Timothy, iii. 16—"Great is the Mystery of Godliness, God manifest in the Flesh." According to Sir Isaac, after proving by a learned and ingenious examination of ancient manuscripts that this has been altered from the original, he concludes that the reading should be, "Great is the Mystery of Godliness, who (namely, our Saviour) was manifest in the flesh."

As this ingenious dissertation had the effect of depriving the zealous defenders of the doctrine of the Trinity of the aid of two leading texts, Sir Isaac Newton has been regarded as an Anti-trinitarian; but such a conclusion is not warranted by anything he has published. Even Mr. Biot has remarked, "that there is absolutely nothing in the writings of Newton to justify, or even to authorise, the idea that he was an Anti-trinitarian." Sir Isaac himself distinctly warns his readers, that his object was solely to "purge the truth of things spurious." For our own part, we are disposed to think that he declares his belief in the doctrine of the Trinity, when he says, "In the eastern nations, and for a long time in the western, the *faith* subsisted without this text; and it is rather a danger to religion, than an advantage to make *it now* lean upon a bruised reed. There cannot be better service done to the truth, than to purge it of things spurious; and, therefore, knowing your prudence and calmness of temper, I am confident I shall not offend you by telling you my mind plainly; especially since it is no article of faith, no point of discipline, nothing but a criticism concerning a text of scripture which I am going to write about." The word *faith* in this passage cannot mean faith in the scriptures in general, but faith in the particular doctrine of the Trinity; for it is this article of faith only to which Sir Isaac refers, when he deprecates *its* leaning on a bruised reed. But whatever may be the meaning of this passage, we know that Newton greatly offended Mr. Whiston for having represented him as an Arian; and so much did he resent the conduct of his friend in ascribing to him heretical opinions, that he prevented him from being elected a Fellow of the Royal Society while he was President.

The only other religious works which were composed by Sir Isaac Newton were his "Lexicon Propheticum," to which was added a Dissertation on the sacred cubit of the Jews, and "Four Letters addressed to Dr. Bentley, containing some arguments in proof of a Deity."

The Lexicon Propheticum was left incomplete, and has never been published; but the Latin Dissertation which was appended to it, was published in 1737, among the Miscellaneous Works of Mr. John Greaves. In this treatise he proves that the ancient cubit was about twenty-six and a half Roman unciæ.

Upon the death of the Honourable Robert Boyle, on the 30th December, 1691, it was found by a codicil to his will, that he had left the sum of £50 per annum for the purpose of establishing a lectureship, in which eight discourses were to be preached annually in one of the churches of the metropolis, in illustration of the evidences of Christianity, and in opposition to the wide-spreading principles of infidelity. Dr. Bentley, though at that time a very young man, was appointed to preach the first course of sermons; and the manner in which he discharged this important duty gave the highest satisfaction, not only to the trustees of the lectureship, but to the public in general. In the first six lectures the Doctor exposed the folly of atheism even in reference to the present life, and derived powerful arguments for the existence of a Supreme Being from the faculties of the soul, and the structure and functions of the human frame. In order to complete his plan, he proposed to devote his seventh and eighth lectures to the demonstration of a divine Providence from the physical constitution of the universe, as established in the Principia. In order to qualify himself for this task, he received from Newton written directions respecting a list of books necessary to be perused previous to the study of that work; and having made himself master of the system which it contained, he applied it with irresistible force of argument to establish the existence of an overruling mind. Previous to the publication of these lectures, Bentley encountered a difficulty which he was not able to solve, and he prudently transmitted to Sir Isaac during 1692 a series of queries on the subject. This difficulty occurred in an argument urged by Lucretius, to prove the eternity of the world from a hypothesis of deriving the frame of it by mechanical principles from matter endowed with an innate power of gravity, and evenly scattered throughout the heavens. Sir Isaac with great pleasure entered upon the consideration of the subject, and returned his sentiments to Dr. Bentley in the four letters which we have already noticed in the preceding chapter.

In the first of these letters, dated December 10th, 1692, Sir Isaac mentions that when he wrote his treatise about our system, namely, the Third Book of the Principia, "he had an eye upon such principles as might work with considering men for the belief of a deity, and he expressed his happiness that it has been found useful for that purpose. In answering the first query of Dr. Bentley, the exact import of which we do not know, he states that if matter were evenly diffused through a finite space, and endowed with innate gravity, it would fall down into the middle of the space, and form one great spherical mass; but if it were diffused through an infinite space, some of it would collect into one mass, and some into another, so as to form an infinite number of great masses. In this manner the sun and stars might be formed if the matter were of a lucid nature. But he thinks it inexplicable by natural causes, and to be ascribed to the counsel and contrivance of a voluntary agent, that the matter should divide itself into two sorts, part of it composing a shining body like the sun, and part an opaque body like the planets. Had a natural and blind cause, without continuance and design, placed the earth in the centre of the moon's orbit, and Jupiter in the centre of his system of satellites, and the sun in the centre of the planetary system, the sun would have been a body like Jupiter and the earth, that is without light and heat, and consequently he knows no reason why there is only one body qualified to give light and heat to all the rest, but because the author of the system thought it convenient, and because one was sufficient to warm and enlighten all the rest.

To the second query of Dr. Bentley he replies that the motions which the planets now have could not spring from any natural cause alone, but were impressed by an intelligent agent. "To make such a system with all its motions, required a cause which understood, and compared together the quantities of matter in the several bodies of the sun and planets, and the gravitating powers resulting from thence, the several distances of the primary planets from the sun, and of the secondary ones from Saturn, Jupiter, and the Earth, and the velocities with which those planets could revolve about those quantities of matter in the central bodies; and to compare and adjust all these things together in so great a variety of bodies, argues that cause to be not blind and fortuitous, but very well skilled in mechanics and geometry."

In the second letter, 17th Jan., 1692-3, he admits that the spherical mass formed by the aggregation of the particles would effect the figure of the space in which the matter was diffused, provided the matter descends directly downwards to that body, and the body has no diurnal rotations; but he states that by earthquakes loosening the parts of this solid, the protuberance might sink a little by the weight, and the mass by degrees approach a sperical figure. He then proceeds to correct an error of Dr. Bentley's in supposing that all infinites are equal. He admits that gravity might put the planets in motion, but he maintains that, without the Divine power, it could never give them such a circulating motion as they have about the sun, because a proper quantity of a transverse motion is necessary for this purpose; and he concludes that he is compelled to ascribe the frame of this system to an intelligent agent.

The third letter contains opinions confirming or correcting several positions which Dr. Bentley had laid down, and he concludes it with a curious examination of the opinion of Plato, that the motion of the planets is such as if they had all been created by God in some region very remote from our system, and let fall from thence towards the sun, their falling motion being turned aside into a traverse one whenever they arrived at their several orbits. Sir Isaac shows that there is no common place such as that conjectured by Plato, provided the gravitating power of the sun remains constant; but that Plato's affirmation is true if we suppose the gravitating power of the sun to be doubled at that moment of time when they all arrive at their several orbits. "If we suppose," he says, "the gravity of all the planets towards the sun to be of such a quantity as it really is, and that the motions of the planets are turned upwards, every planet will ascend to twice its height from the sun. Saturn will ascend till he be twice as high from the sun as he is at present, and no higher; Jupiter will ascend as high again as he is at present, that is, a little above the orb of Saturn; Mercury will ascend to twice his present height, that is, to the orb of Venus; and so of the rest; and then, by falling down again from the places to which they ascended, they will arise again at their several orbs with the same velocities they had at first, and with which they now revolve.

"But if so soon as their motions by which they revolve are turned upwards, the gravitating power of the sun, by which their descent is perpetually retarded, be diminished by one half, they will now ascend perpetually, and all of them, at all equal distances from the sun, will be equally swift. Mercury, when he arrives at the orb of Venus, will be as swift as Venus; and he and Venus, when they arrive at the orb of the earth, and so of the rest. If they begin all of them to ascend at once, and ascend in the same line, they will consequently, in ascending, become nearer and nearer together, and their motions will constantly approach to an equality, and become at length slower than any motion assignable. Suppose, therefore, that they ascended till they were almost contiguous, and their motions inconsiderably little, and that all their motions were at the same moment of time, turned back again, or, which comes almost to the same thing, that they were only deprived of their motions, and let fall at that time, they would all at once arrive at their several orbs, each with the velocity it had at first; and if their motions were then turned sideways, and at the same time the gravitating power of the sun doubled, that it might be strong enough to retain them in their orbs, they would revolve in them as before their ascent. But if the gravitating power of the sun was not doubled, they would go away from their orbs into the highest heavens in parabolical lines."

In the fourth letter, he states that the hypothesis that matter is at first evenly diffused through the universe, is in his opinion inconsistent with the hypothesis of innate gravity without a supernatural power to reconcile them, and therefore it infers a Deity; "For if there be innate gravity," he says, "it is impossible now for the matter of the earth and all the planets and stars to fly up from them, and become evenly spread throughout all the heavens without a supernatural power."

These letters, of which we have endeavoured to give a brief summary, will repay the most attentive perusal by the philosopher as well as the divine. They are written with much perspicuity of language, and great power of thought, and they contain results which incontestibly prove that their author was fully master of his noblest faculties, and comprehended the profoundest parts of his own writings.

The logical acuteness, the varied erudition, and the absolute freedom from all prejudice which shine throughout the theological writings of Newton, might have protected them from the charge of having been written in his old age, and at a time when a failure of mind was supposed to have unfitted him for his mathematical studies. But it is fortunate for his reputation, as well as for the interests of Christianity, that we have been able to prove the falsity of such base insinuations, and to produce the most undoubted evidence that all the theological writings of Newton were composed when his mental powers were in their greatest vigour, and before the crisis of that bodily indisposition which is supposed by some to have affected his reason. The able letters to Dr. Bentley were even written in the middle of that period when want of sleep and appetite had disturbed the serenity of his mind, which is a sufficient proof that this disturbance never affected the higher functions of his understanding.

When a mind of great and acknowledged power first directs its energies to the study of the material universe, no indications of order attract his notice, and no proofs of design call forth his admiration. In the starry firmament he sees no bodies of stupendous magnitude, and no distances of immeasurable span. The two great luminaries appear vastly inferior in magnitude to many objects around him, and the greatest distances in the heavens seem even inferior to those which his own eye can embrace on the surface of the earth. The planets, when observed with care, are seen to have a motion among the fixed stars, and to vary in their magnitude and distances, but these changes appear to follow no law. Sometimes they move to the east, sometimes to the west, sometimes towards the north, and sometimes towards the south, and at other times they are absolutely stationary. No system, in short, appears, and no general law seems to direct their motions. By the observations and inquiries of astronomers, however, during successive ages, a regular system has been recognised in this chaos of moving bodies, and the magnitudes, distances, and revolution of every planet which composes it has been determined with the most extraordinary accuracy. Minds fitted and prepared for this species of inquiry are capable of understanding the great variety of evidence by which the truth of the planetary system is established; but thousands of individuals, who are even distinguished in other branches of knowledge, are incapable of such researches, and view with a sceptical eye the great and irrefragable truths of astronomy.

That the sun is stationary in the centre of our system—that the earth moves round the sun, and round its own axis—that the earth is eight thousand miles in diameter, and the sun one hundred and ten times as large; that the earth's orbit is one hundred and ninety millions of miles in breadth; and that if this immense space were filled with light, it would appear only like a luminous point at the nearest fixed star—are positions absolutely unintelligible and incredible to all who have not carefully studied the subject. To millions of mankind, then, the Great Book of Nature is absolutely sealed, though it is in the power of all to unfold its pages, and to peruse those glowing passages which proclaim the power and wisdom of its Almighty Author.

The Book of Revelation exhibits to us the same peculiarities as that of Nature. To the ordinary eye it presents no immediate indication of its divine origin. Events apparently insignificant—supernatural interferences seemingly unnecessary—doctrines almost contradictory—and prophecies nearly unintelligible of moral and physical evil—the prediction of a Messiah—the actual advent of our Saviour—his instruction—his miracles—his death—his resurrection—and the subsequent propagation of his religion by the unlettered fishermen of Galilee, are each a stumbling-block to the wisdom of this world. The youthful and vigorous mind when first called upon to peruse the sacred writings, turns from them with disappointment. It recognizes in them no profound science—no secular wisdom—no divine eloquence—no disclosure of Nature's secrets—no direct impress of an Almighty hand. Bnt though the system of revealed truth which this book contains is like that of the universe concealed from common observation, yet the labours of centuries have established its divine origin, and developed in all its order and beauty the great plan of human restoration. In the chaos of its incidents, we discover the whole history of our species, whether it is delineated in events that are past, or shadowed forth in those which are to come,—from the creation of man and the origin of evil, to the extinction of his earthly dynasty, and the commencement of his immortal career.

The antiquity and authenticity of the books which comprise the sacred canon—the fulfilment of its prophecies—the miraculous works of its founder—his death and resurrection, have been demonstrated to all who are capable of appreciating the force of historical evidence; and in the poetical and prose writings of the inspired authors, we discover a system of doctrine, and a code of morality traced in characters as distinct and legible as the most unerring truths in the material world. False systems of religion have indeed been deduced from the Scriptures, as false systems of the universe have sprung from the study of the Book of Nature; but the very prevalence of a false system proves the existence of one that is true; and though the two classes of facts necessarily depend on different kinds of evidence, yet we have no hesitation in saying that the Copernican system is not more demonstrably true than the system of theological truth contained in the Bible. If men of high powers, then, are still found, who are insensible to the evidence which sustains the system of the universe, need we wonder that there are others whose minds are shut against the clear evidence which entrenches the strongholds of our faith.

If such, then, is the character of the Christian faith, we need not be surprised that it was embraced and expounded by such a genius as Sir Isaac Newton. Cherishing its doctrines, and leaning on its promises, he felt it his duty, as it was his pleasure, to apply to it that intellectual strength which had successfully surmounted the difficulties of the material universe. The fame which that success procured him, he could not but feel to be the breath of popular applause which administered only to his personal feelings; but the investigation of the sacred mysteries, while it prepared his own mind for its final destiny, was calculated to promote the spiritual interests of millions.

This noble impulse he did not hesitate to obey, and by thus uniting philosophy with religion, he dissolved the league which genius had formed with scepticism, and added to the host of witnesses the brightest name of ancient or modern times.

Having thus given an account of the principal labours of Sir Isaac Newton, we consider it our duty to notice several of his minor discoveries and inventions, which could not properly be introduced earlier.

The most important of these, perhaps, are his chemical researches, which he seems to have pursued with more or less diligence from the time when he first witnessed the practical operations of chemistry during his residence at the apothecary's at Grantham. His first chemical experiments were, in all likelihood, made on the alloys of metals, for the purpose of obtaining a good metallic composition for the specula of reflecting telescopes. In his paper on thin plates, he treats of the combination of solids and fluids; but he enters more largely on these and other subjects in the queries published at the end of his Optics.

One of his most important chemical treatises is his *Tabula quantitatum et graduum caloris*, which was published in the Philosophical Transactions. This short paper contains a comparative scale of temperature from that of melting ice to that of a small kitchen coal fire. The following are the principal points of the scale, the intermediate degrees of heat having been determined with great care.

| Degrees of heat. | Equal Parts of heat. | |
|---|---|---|
| 0 | 0 | Freezing point of water. |
| 1 | 12 | Blood heat. |
| 2 | 24 | Heat of melting wax. |
| 3 | 48 | Melting point of equal parts of tin and bismuth. |
| 4 | 96 | Melting point of lead. |
| 5 | 192 | Heat of a small coal fire. |

The first column of this table contains the degrees of heat in arithmetical progression; and the second contains the degrees of heat in geometrical progression, the second degree being twice as great as the first, and so on. It is obvious from the above table, that the heat at which equal parts of tin and bismuth melt is four times greater than that of blood heat, the heat of melting lead eight times greater; and the heat of a small coal fire sixteen times greater.

This table was constructed by the help of a thermometer, and of red hot iron. By the former he measured all heats as far as that of melting tin; and by the latter he measured all the higher heats. For the heat which heated iron loses in a given time is as the total heat of the iron; and therefore, if the times of cooling are taken equal, the heats will be in a geometrical progression, and may therefore be easily formed by a table of logarithms.

He found by a thermometer constructed with linseed oil, that if the oil, when the thermometer was placed in melting snow, occupied a space of 1000 parts, the same oil, rarefied with one degree of heat, or that of the human body, occupied a space of 10,256; in the heat of water beginning to boil, a space of 10,705; in the heat of water boiling violently, 10,725; in the heat of melted tin beginning to cool, and putting on the consistency of an amalgam 11,516, and when the tin had become solid 11,496. Hence the oil was rarefied in the ratio of 40 to 39 by the heat of the human body; of 15 to 14 by the heat of boiling water; of 15 to 13 in the heat of melting tin beginning to solidify; and of 23 to 20 in the same tin when solid. The rarefaction of air was with the same heat, ten times greater than that of oil; and the rarefaction of oil fifteen times greater than that of spirit of wine. By making the heat of oil proportioned to its rarefaction, and by calling the heat of the human body 12 parts, we obtain the heat of water beginning to boil, 33; of water boiling violently, 34; of melted tin beginning to solidify, 72; and of the same become solid, 70.

Sir Isaac then heated a sufficiently thick piece of iron till it was red hot; and having fixed it in a cold place, when the wind blew uniformly, he put upon it small pieces of different metals and other fusible bodies, and noted the times of cooling, till all the particles having lost their fluidity grew cold, and the heat of the iron was equal to that of the human body. Then, by assuming that the excesses of the heats of the iron and of the solidified particles of metal, above the heat of the atmosphere, were in geometrical progression when the times were in arithmetical progression, all the heats were obtained. The iron was placed in a current of air, in order that the air heated by the iron might always be carried away by the wind, and that cold air might replace it with a uniform motion; for thus equal parts of the air were heated in equal times, and received a heat proportional to that of the iron. But the heats thus found had the same ratio to one another with the heats found by the thermometer; and hence he was right in assuming, that the rarefactions of the oil were proportional to its heats.

Another short chemical treatise by Sir Isaac Newton has been published by Dr. Horsley. It is entitled "De Natura Acidorum," but is principally occupied with a number of brief opinions on chemical subjects. This paper was written later than 1687, as it bears a reference to the "Principia;" and the most important facts which it contains seems to have been more distinctly reproduced in the queries at the end of the Optics.

The most important of these queries relate to fire, flame, and electric attractions, and as they were revised in the year 1716 and 1717, they may be regarded as containing the most matured opinions of their author. Fire he regards as a body heated so hot as to emit light copiously, and flame as a vapour, fume, or exhalation, heated so hot as to shine. In his long query on elective attractions he considers the small particles of bodies as acting upon one another at distances so minute as to escape observation. When salt of tartar deliquesces, he supposes that this arises from an attraction between the saline particles and the aqueous particles held in solution in the atmosphere, and to the same attraction he ascribes it that the water will not distil from the salt of tartar without great heat. For the same reason sulphuric acid attracts water powerfully, and parts with it with great difficulty. When this attractive force becomes very powerful, as in the union between sulphuric acid and water, so as to make the particles "coalesce with violence," and rush towards one another with an accelerated motion, heat is produced by the mixture of the two fluids. In like manner, he explains the production of flame from the mixture of cold fluids—the action of fulminating powders,—the combination of iron filings with sulphur,—and all the other chemical phenomena of precipitation, combination, solution, and crystallization, and the mechanical phenomena of cohesion and capillary attraction. He ascribes hot springs, volcanoes, fire-damps, mineral coruscations, earthquakes, hot suffocating exhalations, hurricanes, lightning, thunder, fiery meteors, subterraneous explosions, land-slips, ebullitions of the sea, and water-spouts, to sulphureous steams abounding in the bowels of the earth, and fermenting with minerals, or escaping into the atmosphere, when they ferment with acid vapours fitted to promote fermentation.

In explaining the structure of solid bodies, Sir Isaac is of opinion "that the smallest particles of matter may cohere by the strongest attractions and compose bigger particles whose virtue is still weaker; and so on for divers successions, until the progression ends in the biggest particles on which the operations in chemistry, and the colours of natural bodies, depend, and which, by adhering, compose bodies of a sensible magnitude. If the body is compact, and bends or yields inward to pression, without any sliding of its parts, it is hard and elastic, returning to its figure with a force rising from the mutual attraction of its parts. If the parts slide upon one another, the body is malleable or soft. If they slip easily, and are of a fit size to be agitated by heat, and the heat is big enough to keep them in agitation, the body is fluid; and if it be apt to stick to things, it is humid; and the drops of every fluid affect a round figure by the mutual attraction of their parts, as the globe of the earth and sea affects a round figure, by the mutual attraction of its parts by gravity."

Sir Isaac then supposes that, as the attractive force of bodies can but reach to a small distance from them, "a repulsive virtue ought to succeed;" and he considers such a virtue as following the reflexion of the rays of light, the rays being repelled by the immediate contact of the reflecting body, and also from the emission of light, the ray, as soon as it is shaken off from a shining body by the vibrating motion of the parts of the body getting beyond the reach of attraction, and being driven away with exceeding great velocity by the force of reflexion.

Many of the chemical views which Newton thus published in the form of queries, were illustrated and confirmed by Stephen Hall, in his book on "Vegetable Statics," a work which contains the germ of some of the most valuable discoveries in modern chemistry.

Although there is little reason to suppose that Sir Isaac Newton was a believer in the doctrines of alchemy, yet we are informed, by the Rev. Mr. Law, that he had been a diligent student of Jacob Behmen's writings, and that there were found among his papers copious extracts from them. He also states that Sir Isaac, together with Dr. Newton, his relation, in the earlier part of his life, set up furnaces, and were for several months in quest of the philosopher's tincture.

Sir Isaac communicated to the Royal Society a paper entitled, "An Hypothesis explaining properties of Light," in which he, for the first time, introduces his opinions respecting ether, and employs them to explain the nature of light and the cause of gravity.—"He was induced to do this," he says, "because he had observed the heads of some great virtuosos to run much upon hypotheses, and he therefere gave one which he was obliged to consider as the most probable, if he were inclined to adopt one.

This hypothesis seems to have been afterwards a subject of discussion between him and Mr. Boyle, to whom he promised to communicate his opinion more fully in writing. He accordingly addressed to him a long letter, in which he explains his views respecting ether, and employs them to account for the refraction of light—the cohesion of two polished pieces of metal in an exhausted receiver—the adhesion of quicksilver to glass tubes—the cohesion of the parts of all bodies—the cause of filtration—the phenomena of capillary attraction—the action of menstrua on bodies—the transmutation of gross compact substances into aerial ones—and the cause of gravity. From the language used in this paper, we should be led to suppose that Newton had entirely forgotten that he had formerly treated the general subject of ether, and applied it to the explanation of gravity. "I shall set down," he says, "one conjecture more which came into my mind now as I was writing this letter; it is about the cause of gravity," which he goes on to explain; and he concludes by saying that "he has so little fancy to things of this nature, that, had not your encouragement moved me to it, I should never, I think, thus far have set pen to paper about them."

These opinions, however, about the existence of ether, Newton seems to have subsequently renounced;

for in the manuscript, which we mentioned already as being in the possession of Dr. J. C. Gregory, which was written previous to 1702, he states, that ether is neither obvious to our senses, nor supported by any argument, but is a gratuitous assumption, which, if we are to trust to reason and to our senses, must be banished from the nature of things; and he goes on to establish, by various arguments, the validity of this opinion. This renunciation of his former hypothesis probably arose from his having examined more carefully some of the phenomena which he endeavoured to explain by it. Those of capillary attraction, for example, he had ascribed to the other "standing rarer in the very sensible cavities of the capillary tubes than without them;" whereas he afterwards discovered their true cause, and ascribed them to the reciprocal attraction of the tube and the fluid. But, however, this may be, there can be no doubt that he resumed his early opinions before the publication of his "Optics," which may be considered as containing his views upon this subject.

The queries which contain these opinions are the 18th,—24th, all of which appeared for the first time in the second English edition of the Optics. If a body is either heated or loses it heat when placed in vacuo, he ascribes the conveyance of heat in both cases "to the vibration of a much subtiler medium than air," and he considers this medium as the same with that by which light is refracted and reflected, and by whose vibrations light communicates heat to bodies and is put into fits of easy reflexion and transmission.

This ethereal medium, according to Sir Isaac, is exceedingly more rare and more elastic than air. It pervades all bodies, and is expanded through all the heavens. It is much rarer within the dense bodies of the sun, stars, planets, and comets, than in the celestial spaces between them, and also more rare within glass, water, &c., than in the free and open spaces void of air and other grosser bodies. In passing out of glass, water, &c., and other dense bodies, into empty space, it grows denser and denser by degrees, and this gradual condensation extends to some distance from the bodies. Owing to its great elasticity, and, consequently its efforts to spread in all directions, it presses against itself, and, consequently, against the solid particles of bodies, so as to make them continually approach one another, the body being impelled from the denser parts of the medium towards the rarer with all that power which we call gravity.

In employing this medium to explain the nature of light, Newton does not suppose that light is nothing more than the impression of those undulations on the retina. He regards light as a peculiar substance composed of heterogeneous particles thrown off with great velocity, and in all directions, from luminous bodies; and he supposes that these particles while passing through the ether excite in it vibrations or pulses which accelerate or retard the particles of light, and thus throw them into their alternate fits of easy reflexion and transmission.

Hence, if a ray of light falls upon a transparent body, in which the ether consists of strata of variable density, the particles of light acted upon by the vibrations which they create will be urged with an accelerated velocity in entering the body, while their velocity will be retarded in quitting it. In this manner he conceives the phenomena of refraction to be produced, and he shows how in such a case the refraction would be regulated by the law of the sines.

In order that the ethereal medium may produce the fits of easy reflexion and transmission, he conceives that its vibrations must be swifter than light. He computes its elasticity to be four hundred and ninety billion times greater than that of air in proportion to its density, and about six hundred million times more rare than water, from which he infers that the resistance which it would oppose to the motion of the planets would not be sensible in ten thousand years. He considers that the functions of vision and hearing may be performed chiefly by the vibrations of this medium, executed in the bottom of the eye, or in the auditory nerve by the rays of light, and propagated through the solid, pellucid, and uniform capillamenta of the optic or auditory nerves into the place of sensation; and he is of opinion that animal motion may be performed by the vibrations of the same medium, excited in the brain by the power of the will, and propagated from thence by the solid, pellucid, and uniform capillamenta of the nerves into the muscles for contracting and dilating them.

In the registers of the Royal Society there exist several letters on the excitation of electricity, which were occasioned by an experiment of this kind having been mentioned in Sir Isaac's hypothesis of light. The Society had ordered the experiment to be tried at their meeting on the 16th December, 1675; but in order to secure its success, Mr. Oldenburg, the secretary wrote to Newton for a more particular account of it. Sir Isaac being "thus put upon recollecting himself a little farther about it," remembers that he made the experiment with a glass fixed at the distance of one third of an inch from one end of a brass hoop, and only one-eighth of an inch from the other. Small pieces of thin paper were then laid upon the table; when the glass was laid above them and rubbed, the pieces of paper leapt from one part of the glass to the other, and twirled about in the air. Notwithstanding the explicit account of the experiment, it entirely failed at the Royal Society, and the secretary was desired to request the loan of Sir Isaac's apparatus, and to inquire whether or not he had secured the papers from being removed by the air, which might have somewhere got in. Sir Isaac, in his answer, recommended to the Society to rub the glass "with stuff whose threads may rake its surface, and, if that will not do, to rub it with the fingers' ends to and fro, and knock them as often upon the glass." The directions enabled the Society to succeed with the experiment, by using a scrubbing brush of hog's bristles, and the help of a knife made with whalebone.

Among the minor inventions of Sir Isaac Newton, we must not omit his reflecting instrument for observing the moon's distance from the fixed stars at sea. The description of this instrument was communicated by Dr. Halley, in the year 1700; but, either from having mislaid the manuscript, or from attaching no value to the invention, he did not communicate it to the Royal Society, and it remained among his papers till after his death, in the year 1742, when it was published in the Philosophical Transactions.

"By this instrument," says Sir Isaac, "the distance of the moon from any fixed star is thus observed:—view the star through the perspicil by the direct light, and the moon by the reflexed, (or on the contrary); and turn the index till the star touch the limb of the moon, and the index shall show on the brass limb of the instrument the distance of the star from the moon's limb; and though the instrument shake by the motion of the ship at sea, yet the moon and star will shake together as if they did really touch one another in the heavens; so that an observation may be made as exactly at sea as at land.

"And by the same instrument may be observed exactly the altitudes of the moon and stars, by bringing them to the horizon; and thereby the latitude and times of observation may be determined more exactly than by the ways now in use.

"In the time of the observation if the instrument move angularly about the axis of the telescope, the star will move in a tangent of the moon's limb or of the horizon; but the observation may, notwithstanding, be made exactly by noting when the line, described by the star, is a tangent to the moon's limb or to the horizon.

"To make the instrument useful, the telescope ought to take in a large angle; and, to make the observation true, let the star touch the moon's limb, not on the outside, but on the inside."

This ingenious contrivance is obviously the same invention as that which Mr. Hadley produced in 1731, and which, under the name of Hadley's Quadrant, has been of so great service in navigation. The merit of its first invention must therefore be transferred to Sir Isaac Newton.*

In the year 1672, Newton communicated to Oldenburg his design for a microscope, which he considered to be as capable of improvement as the telescope, and perhaps more so, because it requires only one speculum; and in another letter to the same gentleman, in the same year, he suggests another improvement in microscopes, which is to "illuminate the object in a darkened room with the light of any convenient colour not too much compounded, for by that means the microscope will, with distinctness, bear a deeper charge and larger aperture, especially if its construction be such as I may hereafter describe.

Among the minor and detached labours of Sir Isaac Newton, we must mention his curious experiments on the action of light upon the retina. Locke seems to have wished his opinion respecting a fact stated in Boyle's Book on Colours, and in a letter from Cambridge, dated January 30th, 1691, he communicated to his friend the following very remarkable observations made by himself:—

"The observation you mention in Mr. Boyle's Book of Colours, I once made upon myself with the hazard of my eyes. The manner was this:—I looked a very little while upon the sun in the looking-glass with my right eye, and then turned my eyes into a dark corner of my chamber and winked to observe the impression made, and the circles of colours which encompassed it, and how they decayed by degrees, and at last vanished. This I repeated a second and a third time. At the third time when I saw the phantasm of light and colours about it were almost vanished, intending my fancy upon them to see their last appearance, I found to my amazement that they began to return, and by little and little become as lively and vivid as when I had newly looked upon the sun. But when I ceased to intend my fancy upon them, they vanished again. After this I found, that, as often as I went into the dark, and intended my mind upon them, as when a man looks earnestly to see anything which is difficult to be seen, I could make the phantasm return without looking any more upon the sun; and the oftener I made it return, the more easily I could make it return again. And at length, by repeating this without looking any more upon the sun, I made such an impression on my eye that if I looked upon the clouds, or a book, or any bright object, I saw upon it a round bright spot of light like the sun, and which is still stranger, though I looked upon the sun with my right eye only, and not with my left, yet my fancy began to make an impression upon my left eye, as well as upon my right. For if I shut my right eye or looked upon a book or the clouds with my left eye I could see the spectrum of the sun almost as plain as with my right eye, if I did but intend my fancy a little while upon it; for at first, if I shut my right eye, and looked with my left, the spectrum of the sun did not appear till I intended my fancy upon it; but by repeating, this appeared more easily. And now, in a few hours time, I had brought my eyes to such pass, that I could look upon no bright object with either eye but I saw the sun before me, so that I durst neither write nor read; but to recover the use of my eyes, shut myself up in my chamber made dark, for three days together, and used all means to divert my imagination from the sun. For if I thought upon him I presently saw his picture, though I was in the dark. But by keeping in the dark and employing my mind about other things, I began in three or four days to have some use of my eyes again; and by forbearing to look upon bright objects recovered them pretty well, though not so well but that for some months after the spectrum of the sun began to return as often as I began to meditate upon the phenomenas even though I lay in bed at midnight with my curtains drawn. But now I have been very well for many years, though I am apt to think, if I durst venture my eyes, I could still make the phantasm return by the power of my fancy. This story I tell you to let you understand, that in the observations related by Mr. Boyle, the man's fancy probably concurred with the impression made by the sun's light, to produce that plantasm of the sun which he constantly saw in bright objects. And so your question about the cause of this phantasm involves another about the power of fancy which, I must confess, is too hard a knot for me to untie. To place this effect in a constant motion is hard; because the sun ought then to appear perpetually. It seems rather to consist in a disposition of the sensorum to move the imagination strongly, and to be easily moved both by imagination and by the light, as often as bright objects are looked upon."

These observations possess in many respects a high degree of interest. The fact of the transmission of the impression from the retina of the one eye to that of the other, is particularly important; and it deserves to be remarked as a singular coincidence that Dr. Brewster had occasion to observe, and to describe the same phenomena long before the observations of Sir Isaac were communicated to the scientific world.

## CHAPTER VIII.

Newton's acquaintance with Dr. Pemberton, who edits the third edition of the "Principia." Sir Isaac's first attack of ill health. His recovery. Is again taken ill. His death. His Funeral. A medal struck in honour of him. Division of his property. Permanence of Newton's reputation. Character of his Genius. His social character. Simplicity of his character. Religious and moral character. Hospitality and mode of life. His absence. Personal appearance. Memorials.

About the year 1722, Newton was desirous of publishing a third edition of his Principia, and the premature death of Mr. Cotes, who had superintended the second edition, having deprived him of his valuable assistance, he was fortunate enough to become acquainted with Dr. Pemberton, a young and accomplished physician, who had cultivated mathematical learning with considerable success. M. Poleni, an emiment Professor in the university of Padua, having endeavoured, on the authority of a new experiment, to overturn the common opinion respecting the force of bodies in motion, and to establish that of Leibnitz in its place, Dr. Pemberton transmitted to Dr. Mead, a demonstration of its inaccuracy. Dr. Mead communicated this paper to Sir Isaac, who not only highly approved of it, but added a demonstration of his own, drawn from another consideration of the subject; and this was printed without his name, as a postscript to Pemberton's paper, when it appeared in the Philosophical Transactions in 1722.

In a short time after the commencement of their acquaintance, Sir Isaac engaged Dr. Pemberton to superintend the new edition of the Principia. In discharging this duty the doctor had occasion to make many remarks on this work, which the author always received with the utmost good-nature, and the third edition appeared with numerous alterations in 1726. On the occasions upon which Pemberton had personal intercourse with Sir Isaac, and which were necessarily numerous, he endeavoured to learn his opinions on various mathematical subjects, and to obtain some historical information respecting his inventions and discoveries. Sir Isaac entered freely into all these topics; and during the conversations which took place, and while they were reading together Dr. Pemberton's popular account of Sir Isaac's discoveries, he obtained the most satisfactory evidence, that though his memory was much decayed, yet he was fully able to understand his own writings.

During the last twenty years of Newton's life, which he spent in London, the charge of his domestic concerns devolved upon his beautiful and accomplished niece, Mrs. Catherine Barton, the widow of Colonel Barton, for whom, as we already observed, the Earl of Halifax had conceived the warmest affection. This lady, who had been educated at the expense of her uncle, married Mr. Conduit, and continued to reside with her husband in Sir Isaac's house till the time of his death.

In the year 1722, when he had reached the eightieth year of his age, he was seized with an incontinence of urine, which was ascribed to stone in the bladder, and was considered incurable. By means of a strict regime, however, and other precautions, he was enabled to alleviate his complaint, and to procure long intervals of ease. At this time, he gave up the use of his carriage, and always went out in a chair. He declined all invitations to dinner, and at his own house he had only small parties. In his diet he was extremely temperate. Though he took a little butcher's meat, yet the principal articles of his food were broth, vegetables, and fruit, of which he always ate very heartily. In spite of all his precautions, however, he experienced a return of his old complaint, and in August, 1724, he passed a stone, the size of an ordinary pea, which came away in two pieces, the one at the distance of two days from the other. After some months of tolerable good health, he was seized in January, 1725, with a violent cough and inflammation of the lungs; and, in consequence of this attack, he was prevailed upon with some difficulty, to take up his residence at Kensington, where his health experienced a decided improvement. In February, 1725, he was attacked in both his feet, with a fit of the gout, of which he had felt slight symptoms a few years before, and the effect of this new complaint was to produce a great and beneficial change in his general health. On the 7th of March, when his head was clearer and his memory stronger than his nephew had known it for a long time, he entered into a long conversation on various subjects, particularly in astronomy. He explained to Mr. Conduit how comets might be formed out of the light of vapours discharged from the sun, and the fixed stars as the centres of systems. He conceived that these luminaries were replenished by the same comets being again returned to them; and upon this principle he explained the extraordinary lights which were seen among the fixed stars by Hipparchus, Tycho, Brahe, and Kepler's disciples, and which he supposed to arise from the additional fuel which they received.

We here give this conversation as originally copied from Mr. Conduit's handwriting. It is entitled

"A remarkable and curious conversation between Sir Isaac Newton and Mr. Conduit.

"I was on Sunday night, the 7th of March, 1724-5, at Kensington, with Sir Isaac Newton, in his lodgings, just after he was come out of a fit of the gout, which he had had in both his feet, for the first time in the eighty-third year of his age. He was better after it, and his head clearer, and memory stronger, than I had known them for some time. He then repeated to me by way of discourse, very distinctly, though rather in answer to my queries than in one continued narration, what he had often hinted to me before, viz., that it was his conjecture (he would affirm nothing,) that there was a sort of revolution in the heavenly bodies; that the vapours and light omitted by the sun, which had their sediment as water, and other matter had gathered themselves by degrees into a body and attracted more matter from the planets, and at last made a secondary planet (viz. one of those that go round another planet) and then by gathering to them, and attracting more matter, became a primary planet; and then by increasing still became a comet, which after several revolutions, by coming nearer and nearer to the sun, had all its volatile parts condensed, and became a matter fit to recruit and replenish the sun (which must waste by the constant heat and light it emitted) as a faggot would this fire if put into it, (we were sitting by a wood fire) and that that would probably be the effect of the comet of 1680, sooner or later, for, by the observations made upon it, it appeared, before it came near the sun, with a tail only two or three degrees long; but by the heat it contracted in going so near the sun, it seemed to have a tail of thirty or forty degrees when it went from it; that he could not say when this comet would drop into the sun; it might perhaps have five or six revolutions more first, but whenever it did, it would so much increase the heat of the sun that this earth would be burnt, and no animals could live. That he took the three phenomena seen by Hipparchus, Tycho Brahe, and Kepler's disciples to have been of this kind, for he could not otherwise account for an extraordinary light as those were, appearing all at once among the fixed stars (all which he took to be suns enlightening other planets as our sun does ours) as big as Mercury or Venus seems to us, and gradually diminishing for sixteen months, and then sinking into nothing. He seemed to doubt whether there were not intelligent beings superior to us who superintended these revolutions of the heavenly bodies by the direction of the Supreme Being. He appeared also to be very clearly of opinion that the inhabitants of this world were of a short date, and alleged as one reason for that opinion, that all arts, as letters, ships, printing, needle, &c., were discovered with in the memory of history, which could not have happened if the world had been eternal; and that there were visible marks of ruin upon it which could not be effected by a flood only.

"When I asked him how this earth could have been re-peopled if ever it had undergone the same fate it was threatened with hereafter by the comet of 1680, he answered, that required the power of a Creator. He said he took all the planets to be composed of the same matter with this earth, viz., earth, water, stones, &c., but variously concocted.

"I asked him why he would not publish his conjectures as conjectures, and instanced that Kepler had communicated his; and though he had not gone near so far as Kepler, yet Kepler's guesses were so just and happy, that they had been proved and demonstrated by him."

"His answer was, 'I do not deal in conjectures.'"

"But upon my talking to him about the four observations that had been made of the comet of 1680, at 574 years' distance, and asking him the particular times, he opened his 'Principia' which laid on the table, and shewed me the particular periods, viz., 1st, the Julium Sidus, in the time of Justinian, in 1106, and in 1680."

"And I, observing that he said there of that comet 'incidet in corpus solis,' and in the next paragraph adds, 'stellæ fixæ refici possurit,' told him I thought he owned there what we had been talking about, viz., that the comet would drop into the sun, and that fixed stars were recruited and replenished by comets when they dropt into them, and, consequently, that the sun would be recruited too; and asked him why he would not own as freely what he thought of the sun as well as what he thought of the fixed stars."

"He said, 'that concerned us more;' and laughing, added, 'that he had said enough for people to know his meaning.'"

Notwithstanding the improvement which his health had experienced, his indisposition was still sufficiently severe to prevent him from discharging his duties at the Mint; and, as his deputy was at the same time confined with the dropsy, he was desirous, in 1725, of resigning his office in favour of Mr. Conduit. Difficulties were probably experienced in making this arrangement, but his nephew discharged for him all the duties of his office; and during the last year of his life he hardly ever went to the Mint.

But, though every kind of motion was calculated to aggravate his complaint, and though he had derived from absolute rest, and from the air at Kensington, the highest benefit, yet great difficulty was experienced in preventing him from occasionally going to town. Feeling himself able for the journey, he went to London on Tuesday, the 28th of February, 1727, to preside at a meeting of the Royal Society. On the following day Mr. Conduit considered him better than he had been for many years, and Sir Isaac was himself so sensible of this improvement in his health, that he assured his nephew, that on the Sunday preceding he had slept from eleven o'clock at night till eight o'clock next morning without waking. He had undergone great fatigue, however, in attending the meeting of the Royal Society, and in paying and receiving visits, and the consequence of this was a violent return of his former complaint. He returned to Kensington on Saturday, the 4th of March, and was attended by Dr. Mead and Dr. Cheselden, who pronounced his disease to be stone, and held out no hopes of his recovery. From the time of his last journey to London he had experienced violent fits of pain with very short intermissions; and though the drops of sweat run down his face during these severe paroxysms, yet he never uttered a cry or a complaint, or displayed the least marks of peevishness or impatience; but during the short intervals of relief which occurred, he smiled and conversed with his usual gaiety and cheerfulness. On Wednesday, the 15th of March, he seemed a little better; and slight, though groundless, hopes were entertained of his recovery. On the morning of Saturday, the 18th, he read the newspapers, and carried on a pretty long conversation with Dr. Mead, when all his senses and faculties were strong and vigorous; but at six o'clock of the same evening he became insensible, and he continued in that state during the whole of Sunday, and till Monday the 20th, when he expired between one and two o'clock in the morning, in the eighty-fifth year of his age.

His remains were removed to London, and on Tuesday, the 28th of March, they lay in state in the Jerusalem Chambers, and were thence conveyed to Westminster Abbey, where they were buried near the entrance into the choir on the left hand. The pall was supported by the Lord High Chancellor, the Dukes of Roxburgh and Montrose, and the Earls of Pembroke, Sussex, and Macclesfield, who were Fellows of the Royal Society. The Honourable Sir Michael Newton, Knight of the Bath, was chief mourner, and was followed by some other relations, and several distinguished characters who had been intimately acquainted with the deceased. The funeral service was performed by the Bishop of Rochester, attended by the prebend and choir.

Sensible of the high honour which they derived from their connection with so distinguished a philosopher, the relations of Sir Isaac Newton who inherited his personal estate, agreed to devote £500 to the erection of a monument to his memory, and the Dean and Chapter of Westminster appropriated for it a place in the most conspicuous part of the Abbey, which had been frequently refused to the greatest of the English nobility. This monument was erected in 1731. On the front of a sarcophagus resting on a pedestal, are sculptured, in basso-relievo, youths bearing in their hands the emblems of Sir Isaac's principal discoveries. One carries a prism, another a reflecting telescope, a third is weighing the sun and planets with a steel-yard, a fourth is employed about a furnace, and two others are loaded with money newly coined. On the sarcophagus is placed the figure of Sir Isaac, in a recumbent position, with his elbow resting on several of his works. Two youths stand before him with a scroll, on which is drawn a remarkable diagram relative to the solar system, and above that is a converging series. Behind the sarcophagus is a pyramid, from the middle of which rises a globe in mezzo-relievo, upon which several of the constellations are drawn, in order to show the path of the comet of 1681, whose period Sir Isaac had determined, and also the position of the solstitial colure mentioned by Hipparchus, and by means of which Sir Isaac had, in his Chronology, fixed the time of the Argonautic expedition. A figure of Astronomy, as Queen of the Sciences, sits weeping on the globe, with a sceptre in her hand, and a star surmounts the summit of the pyramid. The following epitaph is inscribed on the monument :—

Hic situs est
ISAACUS NEWTON, Eques Auratus,
Qui animi vi prope divina
Planetorum Motus, Figuras,
Cometarum semitas, Oceanique Æstus,
Sua Mathesi facem preferente,
Primus demonstravit.
Radiorum Lucis dissimilitudines,
Quas nemo antea vel suspicatus erat, pervestigavit,
Naturæ, Antiquitates, S. Scripturæ,
Sedulus, sagax, fidus Interpres,
Dei Opt. Max. Majestatem philosophia asserust,
Evangelii simplicitatem moribus expressit.
Libi gratulentur Mortales, tale tantumque extitisse,
HUMANI GENERIS DECUS.
Natus XXV. Decemb. MDCXLII. Obiit. XX. Mar.
MDCCXXVII.

Of which the following is a literal translation:—

Here lies
Sir Isaac Newton, Knight,
Who, by a vigour of mind almost supernatural,
First demonstrated
The Motions and Figures of the Planets,
The Paths of the Comets, and the Tides of the Ocean.
He diligently investigated
The different refrangibilities of the Rays of Light,
And the properties of the Colours to which they give rise.
An assiduous, Sagacious, and Faithful Interpreter
Of Nature, Antiquity, and the Holy Scriptures,
He asserted in his Philosophy the Majesty of God,
And exhibited in his conduct the simplicity of the Gospel.
Let Mortals rejoice
That there has existed such and so great
AN ORNAMENT OF HUMAN NATURE.
Born 25th December, 1642. Died 20th March, 1727.

In the beginning of 1731, a medal was struck at the Tower, in honour of Sir Isaac Newton. It had on one side the head of the philosopher, with the motto, "Felix cognoscere causas," and on the reverse a figure representing the Mathematics.

On the 4th February, 1755, a magnificent full-length statue of Sir Isaac Newton in white marble, was erected in the antechapel of Trinity College. He is represented standing on a pedestal in a loose gown, holding a prism, and looking upwards with an expression of the deepest thought. On the pedestal is the following inscription :—

> Qui genus humanum ingenio superavit.
>
> Who surpassed all men in genius.

This statue, executed by Roubilliac, was erected at the expense of Dr. Robert Smith, the author of the "Compleat System of Optics," and Professor of Astronomy and Experimental Philosophy at Cambridge.—It has been thus described by a modern poet:—

> Hark! where the organ, full and clear,
> With loud hosannahs charms the ear;
> Behold, a prism within his hands,
> Absorbed with thought, great Newton stands;
> Such was his brow and looks serene,
> His serious gait and musing mien,
> When taught on eagle wings to fly,
> He traced the wonders of the sky;
> The chambers of the sun explored,
> Where tints of thousand hues were stored.

Dr. Smith likewise bequeathed the sum of £500 for executing a painting on glass, for the window at the south end of Trinity College, Cambridge. The subject represents the presentation of Sir Isaac Newton to his Majesty, who is seated under a canopy with a laurel chaplet in his hand, and attended by the British Minerva, apparently advising him to reward merit in the person of the great philosopher. Below the throne, the Lord Chancellor Bacon is preparing to register the reward about to be conferred upon Sir Isaac. The original drawing of this ridiculous picture was executed by Cipriani, and cost one hundred guineas.

The personal estate of Sir Isaac Newton, which was worth about £32,000, was divided among the eight grandchildren of his mother, by the Rev. Mr. Smith.

The family estates of Woolsthorpe and Sustern, he bequeathed to John Newton, the heir-at-law, whose great-grandfather was Sir Isaac's uncle. This gentleman does not appear to have placed any great value on the bequest, for, a few years afterwards, he sold them to Mr. Turnor, of Stoke Rocheford. A short time before his death, Sir Isaac gave away an estate in Berkshire, to the sons and daughter of a brother of his niece, Mrs. Conduit, who, in consequence of their father dying before Sir Isaac, had no share in the personal estate; and he also gave an estate of the same value, which he purchased at Kensington, to Catherine, the only daughter of Mr. Conduit, who afterwards married Mr. Wallop, the eldest son of Lord Lymington. Sir Isaac was succeeded as Master and Warden in the Mint by his nephew, John Conduit, Esq., who wrote a treatise on the gold and silver coin, and who died in 1737, leaving behind him his wife and daughter, the former of whom died in 1739, in the fifty-ninth year of her age.

Such was the latter end of Sir Isaac Newton, and such were the laurels which were dedicated to his memory. Many discoveries have, since his day, been added to science; but astonishing as these discoveries are, they have not obliterated the minutest of his labours, and have served only to brighten the halo which encircles his name. The achievements of genius, like the source from which they spring, are indestructible. Acts of legislation and deeds of war may confer a high celebrity, but the reputation which they bring is only local and transient; and while they are hailed by the nation which they benefit, they are reprobated by the people whom they ruin or enslave. The labours of science on the contrary bear along with them no counterpart of evil. They are the liberal bequests of great minds to every individual of their race, and wherever they are welcomed and honoured, they become the solace of private life, and the ornament and bulwark of the commonwealth.

The importance of Sir Isaac Newton's discoveries has been sufficiently exhibited in the preceding pages. The peculiar character of his genius, and the method which he pursued in his inquiries, can only be gathered from an attentive perusal of his works, and from the history of his individual labours. Were we to judge of the qualities of his mind from the early age at which he made his principal discoveries, and from the rapidity of their succession, we should be led to ascribe to him that quickness of penetration, and that exuberance of invention, which is more characteristic of poetical than philosophical genius. But we must bear in mind that Newton was placed in the most favourable circumstances for the development of his powers. The flower of his youth, and the vigour of his manhood, were entirely devoted to science. No injudicious guardian controlled his ruling passion, and no ungenial studies or professional toils interrupted the continuity of his pursuits. His discoveries, therefore, were the fruit of persevering and unbroken study; and he himself declared, that whatever service he had done to the public was not owing to any extraordinary sagacity, but solely to industry and patient thought.

Initiated early into the abstractions of Geometry, he was deeply imbued with her cautious spirit; and if his acquisitions were not made with the rapidity of intuition, they were at least firmly secured; and the grasp which he took of his subject was proportional to the mental labour which it had exhausted. Overlooking what was trivial, and separating what was extraneous, he bore down with instinctive sagacity on the prominences of his subject; and having thus grappled with its difficulties, he never failed to entrench himself in its strongholds.

To the highest powers of invention, Newton added what so seldom accompanies them—the talent of simplifying and communicating his profoundest speculations. This valuable faculty characterises all his writings. In the economy of her distributions, Nature is seldom thus lavish of her intellectual gifts.—The inspired genius which creates is rarely conferred along with the matured judgment which combines; and yet, without the exertion of both, the fabric of human wisdom could never have been reared. Though a ray from heaven kindled the vestal fire, yet an humble priesthood was required to keep alive the flame.

The method of investigating truth by observation and experiment, so successfully pursued in the Principia, has been ascribed by more than one modern writer of celebrity to Lord Bacon, and Sir Isaac Newton is represented as having owed all his discoveries to the application of the principles of that distinguished writer. One of the greatest admirers of Lord Bacon has gone so far as to characterise him as a philosopher who has had no rival in the times which are past, and as likely to have none in those which are to come. In a eulogy so overstrained as this, we feel that the language of panegyric has passed into that of idolatry; and we are desirous of weighing the force of arguments which tend to depose Newton from the high-priesthood of nature, and to unsettle the proud destinies of Copernicus, Galileo, and Kepler.

That Bacon was a man of powerful genius, and endowed with varied and profound talent—the most skilful logician—the most nervous and eloquent writer of the age which he adorned, are points which we have not the hardihood to deny. The study of ancient systems had early impressed him with the conviction that experiment and observation were the only sure guides in physical inquiries; and, ignorant though he was of the methods, the principles, and the details of the mathematical sciences, his ambition prompted him to aim at the construction of an artificial system by which the laws of nature might be investigated, and which might direct the inquiries of philosophers in all future ages. The necessity of experimental research, and of advancing gradually from the study of facts to the determination of their cause, though the groundwork of Bacon's method, is a doctrine which was not only inculcated, but successfully followed by preceding philosophers. In a letter from Tycho Brahe to Kepler, this industrious astronomer urges his pupil "to lay a solid foundation for his views by actual observation, and then by ascending from these, to strive to reach the causes of things;" and it was no doubt under the influence of this advice that Kepler submitted his wildest fancies to the test of observation, and was conducted to his most splendid discoveries.—The reasonings of Copernicus, who preceded Bacon by more than a century, were all founded upon the most legitimate induction. Dr. Gilbert had exhibited, in his treatise on the magnet, the most perfect specimens of physical research. Leonardo da Vinci had described, in the clearest manner, the proper method of philosophic investigation; and the whole scientific career of Galileo was one continued example of the most sagacious application of observation and experiment to the discovery of general laws. The names of Paracelsus, Helmont, and Cardan have been ranged in opposition to this host of great names; and while it is admitted that even they had thrown off the yoke of the schools, and had succeeded in experimental research, their credulity and their pretensions have been adduced as a proof, that, to the greater number of philosophers, the method of induction was unknown. The fault of this argument consists in the conclusion being infinitely more general than the fact. The errors of the men were not founded on their ignorance, but on their presumption. They wanted the patience of philosophy, and not her methods. An excess of vanity, a waywardness of fancy, and an insatiable appetite for that species of passing fame which is derived from eccentricity of opinion, moulded the reasonings and disfigured the writings of these ingenious men; and it can scarcely admit of a doubt, that, had they lived in the present age, their philosophical character would have reeived the same impress from the peculiarity of their tempers and dispositions.—This is an experiment, however, which it is impossible now to make; but the history of modern science supplies the defect, and the experience of every man furnishes a proof that in the present age there are many philosophers of elevated talents and inventive genius, who are as impatient of experimental research as Paracelsus, as fanciful as Cardan, and as presumptuous as Van Helmont.

Having thus shown that the distinguished philosophers, who flourished previous to the time of Bacon, were perfect masters both of the principles and practice of inductive research, it becomes interesting to inquire whither or not the philosophers who succeeded him acknowledged any obligation to his system, or derived the slightest advantage from his precepts. If Bacon constructed a method to which modern science owes its existence, we shall find its cultivators grateful for the gift, and offering the richest incense at the shrine of a benefactor whose labours conducted them to immortality. No such testimonies, however, are to be found. Nearly two centuries have passed away, teeming with the richest fruits of human ingenuity, and no grateful disciple has made his appearance to vindicate the rights of the alleged legislator of science. Even Newton, who was born and educated after the publication of the "Novum Organon," never mentions the name of Bacon or his system in any of his works; and the amiable and indefatigable Boyle treated him with the same disrespectful silence. When we are told, therefore, that Newton owed all his discoveries to the method of

Bacon, nothing more can be meant than that he proceeded in that path of observation and experiment which had been so warmly recommended in the "Novum Organon;" but it ought to have been added, that the same method had been used by his predecessors,—that Newton possessed no secret that had not been used by Galileo and Copernicus—and that he would have enriched science with the same splendid discoveries if the name and the writings of Bacon had never been heard of.

From this view of the subject we shall now proceed to examine the Baconian process itself, and consider if it possesses any merit as an artificial method of discovery, or if it is at all capable of being employed for this purpose even in the humblest walks of scientific inquiry.

The process of Lord Bacon was, we believe, never tried by any philosopher but himself. As the subject of its application he selected that of heat. With his usual erudition, he collected all the facts that science could supply,—he arranged them in tables—he cross-questioned them with all the sagacity of a special pleader,—he combined them with all the sagacity of a judge—and he conjured with them by all the magic of his exclusive processes. But, after all this display of physical logic, Nature thus interrogated was still silent. The oracle, which he had himself established, refused to give his responses, and the ministering spirit was driven with discomfiture from his own shrine. This example, in short, of the application of his system, will remain to future ages as a memorable instance of the absurdity of attempting to fetter discovery by any artificial rules.

Nothing, even in mathematical science, can be more certain than that a collection of scientific facts are of themselves incapable of leading to discovery, or to the determination of general laws, unless they contain the predominating fact or the relation in which the discovery mainly resides. A vertical column of arch-stones possesses more strength than the same materials arranged as an arch without the key-stone. However nicely they are arranged, and however nobly the arch may spring, it never can possess either equilibrium or stability. In this comparison all the facts are supposed to be necessary to the final result; but, in the inductive method, it is impossible to ascertain the relative importance of any facts, or even to determine if the facts have any value at all, till the master fact which constitutes the discovery has crowned the zealous efforts of the aspiring philosopher. The mind then returns to the dark and barren waste over which it has been hovering; and by the guidance of this single torch it embraces under the comprehensive grasp of general principles, the multifarious and insulated phenomena which had formerly neither value nor connection. Hence, it must be obvious to the most superficial thinker, that discovery consists, either in the detection of some concealed relation—some deep-seated affinity which baffles ordinary research, or in the discovery of some simple fact which is connected by slender ramifications with the subject to be investigated; but which, when once detected, carries us back by its divergence to all the phenomena which it embraces and explains.

In order to give additional support to these views, it would be interesting to ascertain the general character of the process by which a mind of acknowledged power actually proceeds in the path of successful inquiry. The history of science does not furnish us with much information on this head, and if it is to be found at all it must be gleaned from the biographies of eminent men. Whatever this process may be in its details, if it has any, there cannot be the slightest doubt that in its generalities at least it is the very reverse of the method of induction. The impatience of genius spurns the restraints of mechanical rules, and never will submit to the plodding drudgery of inductive discipline. The discovery of a new fact unfits even a patient mind for deliberate inquiry. Conscious of having added to science what had escaped the sagacity of former ages, the ambitious spirit invests its new acquisition with an importance which by no means belongs to it. He imagines innumerable benefits to follow from his discovery. He forms a thousand theories to explain it, and he exhausts his fancy in trying all its possible relations to recognized difficulties and unexplained facts. The reins, however, thus freely given to his imagination, are speedily drawn up. His wildest conceptions are all subjected to the rigid test of experiment, and he has thus been hurried by the excursions of his own fancy into new and fertile paths, far removed from ordinary observation. Here the peculiar character of his own genius displays itself by the invention of methods of trying his own speculations, and he is thus often led to new discoveries far more important and general than that by which he began his inquiry. For a confirmation of these opinions, we may refer to the history of Kepler's Discoveries; and if we do not recognize them to the same extent in the labours of Newton, it is because he kept back his discoveries till they were nearly perfected, and therefore withheld from our knowledge the successive steps of his inquiries.

The social character of Newton was such as might have been expected from his intellectual attainments. He paid and received frequent visits, he assumed no superiority in conversation; he was candid, cheerful, and affable; his society was therefore much sought, and he submitted to intrusions on his valuable time without a murmur; but by early rising, and by a methodical distribution of his hours, he found leisure to study and compose, and every moment which he could command he passed with a pen in his hand and a book before him. He had none of the eccentricities of genius, but suited himself to every company, and always spoke of himself and others in such a manner that he was never suspected of vanity. Dr. Pemberton remarks, "But this I immediately discovered in him, which at once both surprised and charmed me. Neither his extreme great age, nor his universal reputation, had rendered him stiff in opinion, or in any degree elated. Of this I had occasion to have almost daily experience. The remarks I continually sent him by letters on the Principia were received with the utmost goodness. These were so far from being any way displeasing to him, that on the contrary it occasioned him to speak many kind things of me to my friends, and to honour me with a public testimony of his good opinion."

The modesty of Sir Isaac Newton, in reference to his great discoveries, was not founded on any indifference to the fame which they conferred, or upon any erroneous judgment of their importance to science. The whole of his life proves that he knew his place as a philosopher, and was ready to assert and vindicate his rights. His modesty arose from the depth and extent of his knowledge, which showed him what a small portion of nature he had been able to examine, and how much remained to be explored in the same field in which he had himself laboured. In the magnitude of the comparison he recognised his own littleness; and a short time before his death he uttered this memorable sentiment:—"I do not know what I may appear to the world; but to myself I seem like a boy playing on the sea shore and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay undiscovered before me." What a lesson to the vanity and presumption of philosophers—to those especially who have never once found the smoother pebble or the prettier shell! What a preparation for the latest inquiries and the last views of the decaying spirit,—for those inspired doctrines which alone can throw a light over the dark ocean of undiscovered truth!

The native simplicity of Sir Isaac Newton's mind is beautifully pourtrayed in the affecting letter to Mr. Locke, in which he acknowledges that he had thought and spoken of him uncharitably; and the humility and candour in which he asks the forgiveness of his friend, could only have emanated from a mind as noble as it was pure.

In his religious and moral character there is much to admire and to imitate. While he exhibited in his life and writings an ardent regard for the general interests of religion, he was at the same time a conscientious believer in Revelation. He was too deeply versed in the sacred writings, and too much imbued with their spirit, to judge rashly of other men who took different views of them from himself. He cherished the great principles of religious toleration, and never hesitated to express his detestation of persecution, even in its mildest form. Immorality and impiety he never permitted to pass without censure; and whenever Dr. Halley, who was not over scrupulous, ventured to say anything disrespectful of religion in his presence, he invariably checked him, and said, "I have studied these things—you have not."

After Sir Isaac Newton took up his residence in the metropolis; he lived in a very handsome style, and kept his carriage, with an establishment of three males and three female servants. In his own house he gave splendid entertainments, though without ostentation or vanity. His own diet was frugal, and his dress was always plain, if we except one occasion, when he opposed Mr. Annesley in 1705, as a candidate for the university of Cambridge, when he dressed himself in a suit of laced clothes.

He was generous and charitable, one of his maxims being, "that those who gave nothing before death, never, in fact gave at all"—a sentiment which ought to fall as a solemn admonition on the ear of those miserable-minded men who bequeath their property for such a purpose as may purchase a posthumous character for philanthropy. Though his wealth had become considerable by prudent economy, yet he had always a contempt for money, and he spent a considerable part of his income in relieving the poor—in assisting his relations—and in encouraging ingenuity and learning. The sums which he gave to his relations, at different times, were enormous. James Hutton, Esq., of Pimlico, a gentleman of his mother's family, informs us that "he was very kind to all the Ayscoughs. To one he gave £800, to another £200, and to a third £100, and many other sums; and other engagements did he enter into also for them. He was the ready assistant of all who were any way related to him—to their children and grandchildren." But his generosity was not confined to his own relations. In 1724, he wrote a letter to the Lord Provost of Edinburgh, offering to contribute £20 per annum, to a provision for Mr. Maclaurin, provided he acccepted the situation of assistant to Mr. James Gregory, who was professor of mathematics in the university.

The habits of deep meditation which Newton had acquired, though they did not show themselves in his ordinary intercourse with society, exercised their full influence over his mind when in the midst of his own family. Absorbed in thought he would often sit down on his bedside after he rose, and remain there for hours without dressing himself, occupied with some interesting investigation. Owing to the same absence of mind, he neglected to take the requisite quantity of nourishment, and it was therefore often necessary to remind him of his meals. The following anecdote is a proof of this. Dr. Stukely, his intimate friend, who had been deputy to Dr. Halley, as secretary to the Royal Society, on visiting Sir Isaac one day, was shown into the dining-room, where his dinner had been for some time served up. The Doctor waited for a considerable time, and getting impatient, he removed the cover from a chicken, which he ate, replacing the bones under the cover. In a short time after Sir Isaac entered the room, and after the usual compliments sat down to his dinner; but on taking off the cover, and seeing nothing but bones, he remarked, "How absent we philosophers are. I really thought that I had not dined."

Sir Isaac Newton is supposed to have had but little knowledge of the world, and to have been very ignorant of the usages of society. This opinion has, we think, been rashly deduced from a letter which he wrote in the twenty-seventh year of his age to his young friend, Francis Aston, Esq., who was about to set out on his travels. This letter is a highly interesting production; and while it shows much knowledge of the human heart, it throws a strong light upon the character and opinions of its author.

"Trinity College, Cambridge,
May 18th, 1669.

"Sir,

"Since in your letter you give me so much liberty of spending my judgment about what may be to your advantage in travelling, I shall do it more freely than perhaps otherwise have been decent. First, then, I will lay down some general rules, most of which, I believe, you have considered already; but, if any of them be new to you they may excuse the rest; if none at all, yet is my punishment more in writing than yours in reading.

"When you come into any fresh country, 1. Observe their humours. 2. Suit your own carriage thereto, by which insinuation you will make their converse more free and open. 3. Let your discourse be more in queries and doubtings than peremptory assertions or disputings, it being the design of travellers to learn, not to teach. Besides it will persuade your acquaintance that you have the greater esteem of them, and so make them more ready to communicate what they know to you; whereas nothing sooner occasions disrespect and quarrels than peremptoriness. You will find little or no advantage in seeming wiser, or much more ignorant than your company. 4. Seldom discommend any thing though never so bad, or do it but moderately lest you be unexpectedly forced to an unhandsome retraction. It is safer to commend any thing more than it deserves, than to discommend a thing so much as it deserves; for commendations meet not so often with oppositions, or, at least, are not usually so ill resented by men that think otherwise as discommendations; and you will insinuate into men's favour by nothing sooner than seeming to approve and commend what they like; but beware of doing it by a comparison. 5. If you be affronted, it is better, in a foreign country, to pass it by in silence, and with a jest, though with some dishonour, than to endeavour to revenge; for in the first case your credit is ne'er the worse when you return into England, or come into other company that have not heard of the quarrel. But, in the second case, you may bear the marks of the quarrel while you live, if you outlive it at all. But if you find yourself unavoidably engaged, 'tis best, I think, if you can command your passion and language, to keep them pretty evenly at some certain moderate pitch, not much heightening them to exasperate your adversary, or provoke his friends, nor letting them grow over much dejected to make him insult. In a word, if you can keep reason above passion, that and watchfulness will be your best defendants. To which purpose you may consider, that, though such excuses as this,—He provoked me so much I could not forbear,—may pass amongst friends, yet amongst strangers they are insignificant, and only argue a traveller's weakness.

"To these I may add some general heads for inquiries or observations, such as at present I can think on. As, 1. To observe the policies, wealth, and state affairs of nations, so far as a solitary traveller may conveniently do. 2. Their impositions upon all sorts of people, trades, or commodities that are remarkable. 3. Their laws and customs, how far they differ from ours. 4. Their trades and arts, wherein they excel or come short of us in England. 5. Such fortifications as you shall meet with, their fashion, strength, and advantages for defence, and other such military affairs as are considerable. 6. The power and respect belonging to their degrees of nobility or magistracy. 7. It will not be time mis-spent to make a catalogue of the names and excellencies of those men that are most wise, learned, or esteemed in any nation. 8. Observe the merchandise and manner of guiding ships. 9. Observe the products of nature in several places, especially in mines, with the circumstances of mining, and of extracting metals or minerals out of their ore, and of refining them; and if you meet with any transmutations out of their own species into another, (as out of iron into copper, out of any metal into quicksilver, out of one salt into another, or into an insipid body, &c.) those, above all, will be worth your noting, being the most luciferous, and many times luciferous experiments, too, in philosophy. 10. The prices of diet and other things. 11. And the staple commodities of places.

"These generals, (such as at present I could think of) if they will serve for nothing else, yet they may assist you in drawing up a model to regulate your travels by. As for particulars, these that follow are all that I can now think of, viz., Whether at Schemnitium, in Hungary, (where there are mines of gold, copper, iron, vitriol, antimony, &c.) they change iron into copper by dissolving it in a vitriolate water, which they find in cavities of rocks in the mines, and then melting the slimy solution in a strong fire, which in the cooling proves copper. The like is said to be done in other places, which I cannot now remember. Perhaps, too, it may be done in Italy. For about twenty or thirty years ago there was a certain vitriol came from thence, (called Roman vitriol) but of a nobler virtue than that which is now called by that name; which vitriol is not now to be gotten, because, perhaps, they make a better gain by some such trick as turning iron into copper with it than by selling it. 2. Whether in Hungary, Sclavonia, Bohemia, near the town Eila, or at the mountains of Bohemia near Silesia, there be rivers whose waters are impregnated with gold; perhaps the gold being dissolved by some corrosive waters like *aqua regis*, and the solution carried along with the stream that runs through the mines. And whether the practice of laying mercury in the rivers till it be tinged with gold, and then straining the mercury through leather, that the gold may stay behind, be a secret yet, or openly practised. 3. There is newly contrived, in Holland, a mill to grind glasses plane withal, and I think polishing them too. Perhaps it will be worth the while to see it. 4. There is in Holland one —— Borry, who some years since was imprisoned by the Pope, to have extorted from him secrets (as I am told) of great worth, both as to medicine and profit, but he escaped into Holland, where they have granted him a guard. I think he usually goes clothed in green. Pray inquire what you can of him, and whether his ingenuity be any profit to the Dutch. You may inform yourself whether the Dutch have any tricks to keep their ships from being all worm-eaten in their voyages to the Indies. Whether pendulum clocks do any service in finding out the longitude, &c.

"I am very weary, and shall not stay to part with a long compliment, only I wish you a good journey, and God be with you.

"Is. Newton."

Sir Isaac Newton's wonderful faculties were very little impaired, even in extreme old age; and his cheerful disposition, combined with temperance and a constitution naturally sound, preserved him from the usual infirmities of life. In his personal appearance he was of middle size, with a figure inclining to plumpness. According to Mr. Conduit, "he had a very lively and piercing eye, a comely and gracious aspect, with a fine head of hair as white as silver, without any baldness, and when his peruke was off was a venerable sight." Bishop Atterbury asserts, on the other hand, that the lively and piercing eye did not belong to Sir Isaac during the last twenty years of his life. He says, "Indeed in the whole air of his face and make there was nothing of that penetrating sagacity which appears in his compositions. He had something rather languid in his look and manner, which did not raise any great expectation in those who did not know him." This opinion of the Bishop is confirmed by Mr. Thomas Hearne, who says "that Sir Isaac was a man of no very promising aspect. He was a short well-set man. He was full of thought, and spoke very little in company, so that his conversation was not agreeable. When he rode in his coach, one arm would be out of his coach on the one side, and the other on the other." Sir Isaac's sight was preserved to the last, so that he never required spectacles; and he never "lost more than one tooth to the day of his death." The severe trial of this great man's bodily suffering was reserved for the last stage of his existence, and he supported it with characteristic resignation.

Besides the statue of Newton executed by Roubilliac, there is a bust of him by the same artist in the Library of Trinity College, Cambridge. Several good paintings of him are extant. Two of these are in the hall of the Royal Society of London, and have been often engraved. Another, by Vanderbank, is in the apartment of the Master's lodge in Trinity College, and has been engraved by Vertue. Another, by Valentine Ritts, is in the landing-place near the entrance to Trinity College Library; but the best was painted by Sir Godfrey Kneller, and is in the possession of Lord Egremont at Petworth. In the University Library there is preserved a cast taken from his face after death.

Every memorial of so great a man as Sir Isaac Newton has been preserved and cherished with peculiar veneration. His house at Woolsthorpe has been religiously protected by Mr. Turnor of Stoke Rocheford. The proprietor Dr. Stukely, who visited it in Sir Isaac's lifetime on the 13th of October, 1721, gives the following description of it in a letter to Dr. Mead, written in 1727. "'Tis built of stone as is the way of the country hereabouts, and a reasonable good one. They led me up stairs and showed me Sir Isaac's study, where I suppose he studied when in the country in his younger days, or perhaps when he visited his mother from the university. I observed the shelves were of his own making, being pieces of deal boxes which probably he sent his books and clothes down in on these occasions. There were some years ago two or three hundred books in it of his father-in-law, Mr. Smith, which Sir Isaac gave to Dr. Newton.

"When the house was repaired in 1798, a tablet of white marble was put up in the room where Sir Isaac was born, with the following inscription :—

"Sir Isaac Newton, son of John Newton, Lord of the Manor of Woolsthorpe, was born in this room on the 25th December, 1642."

Nature and Nature's laws lay hid in night,
God said "Let Newton be," and all was Light.

The following lines have been written upon the house:—

Here Newton dawned, here lofty wisdom woke,
And to a wondering world divinely spoke.
If Tully glowed, when Phædrus's steps he trode,
Or fancy formed Philosophy a God;
If sages still for Homer's birth contend
The sons of science at this dome must bend.
All hail the shrine! All hail the natal day,
Cam boasts his noon—this *Cot* his morning ray.

The house still contains the two dials made by Newton, but the styles of both are wanting. The celebrated apple-tree, the fall of one of the apples of which is said to have turned the attention of Newton to the subject of gravity, was destroyed by wind in the year 1827; but the proprietor has preserved it in the form of a chair.

The chambers which Sir Isaac inhabited at Cambridge are known by tradition. They are the apartments next to the great gate of Trinity College, and it is believed that they then communicated by a staircase with the Observatory in the Great Tower,—an observatory which was furnished by the contributions of Newton, Cotes, and others. His telescope is preserved in the library of the Royal Society in London, and his globe, his universal ring-dial, quadrant, compass, and a reflecting telescope said to have belonged to him, in the library of Trinity College. There is also in the same collection, a long and curled lock of his silver-white hair. The door of his bookcase, is in the Museum of the Royal Society of Edinburgh.

The manuscripts, letters, and other papers of Newton, have been preserved in different collections; but the great mass of his papers came into the possession of the Portsmouth family, through his niece, Lady Lymington, and have been safely preserved by that noble family.

# APPENDIX.

As every circumstance connected with Sir Isaac Newton must be interesting, we are enabled, through the industrious research of Dr. Brewster, to insert the following "Observations on the family of Sir Isaac Newton," which we trust will be gratifying to our readers :—

"In the year 1705, Sir Isaac gave into the Herald's Office an elaborate pedigree, stating, upon oath, *that he had reason to believe* that John Newton of Westby, in the County of Lincoln, was his great grandfather's father, and that this was the same John Newton who was buried in Basingthorpe church, on the 22d December, 1563. This John Newton had four sons, John, Thomas, Richard, and William Newton of Gunnerly, the last of whom was grandfather to Sir John Newton, Bart., of Hather. Sir Isaac considered himself as descended from these, 'he having, by tradition from his kindred, ever since he can remember, reckoned himself next of kin (among the Newtons) to Sir John Newton's family.'"

The pedigree founded upon these and other considerations was accompanied by a certificate from Sir John Newton, of Thorpe, Bart., who states that he had heard his father speak of Sir Isaac Newton "as of his relation and kinsman," and that "he himself believed that Sir Isaac was descended from John Newton, son to John Newton of Westby, but knoweth not in what particular manner."

The pedigree of Sir Isaac, as entered at the Herald's Office, does not seem to be satisfactory either to himself or to his successors, as it could not be traced with certainty beyond his grandfather; and it will be seen from the following interesting correspondence, that, upon making further researches, he had some reason to believe that he was of Scottish extraction:—

EXTRACT OF A LETTER FROM THE REV. DR. REID, OF GLASGOW, TO DR. GREGORY, OF EDINBURGH.

"Glasgow, 14th March, 1784.

"I send you on the other page an anecdote respecting Sir Isaac Newton, which I do not remember whether I ever happened to mention to you in conversation. If his descent is not clearly ascertained (as I think it is not in the books I have seen), might it not be worth while to inquire if evidence can be found to confirm the account which it is said he has given of himself. Sheriff Cross was very zealous about it when death put a stop to his inquiries.

"When I lived in Old Aberdeen, above twenty years ago, I happened to be conversing, over a pipe of tobacco, with a gentleman of that country, who had been lately at Edinburgh. He told me that he had often been in company with Mr. Hepburn, of Keith, with whom I had the honour of some acquaintance.— He said that, speaking of Sir Isaac Newton, Mr. Newton mentioned an anecdote which he had from Mr. James Gregory, professor of mathematics at Edinburgh, which was to this purpose:—

"Mr. Gregory being at London for some time after he resigned the mathematical chair, was often with

Sir Isaac Newton. One day Sir Isaac said to him, 'Gregory, I believe you don't know that I am connected with Scotland?' 'Pray how, Sir Isaac,' said Gregory. Sir Isaac said he was told that his grandfather was a gentleman of East Lothian; that he came to London with King James, at his accession to the crown of England, and then spent his fortune, as many more did at that time, by which his son (Sir Isaac's father) was reduced to mean circumstances. To this Gregory bluntly replied, 'Newton a gentleman of East Lothian; I never heard of a gentleman of East Lothian of that name.' Upon this Sir Isaac said, 'that being very young when his father died, [The reader will observe from the foregoing biography, that Sir Isaac was a posthumous child, his father having died before he was born,] he had it only by tradition, and it might be a mistake,' and immediately turned the conversation to another subject.

"I confess I suspected that the gentleman who was my author had given some colouring to this story, and therefore I never mentioned it for a good many years.

"After I removed to Glasgow, I came to be very intimate with Mr. Cross, then Sheriff of Lanark, and one day at his own house mentioned this story without naming my author, of whom I expressed some diffidence.

"The Sheriff immediately took it up as a matter worth being inquired into. He said he was well acquainted with Mr. Hepburn of Keith, (who was then alive) and that he would write him to know whether he ever heard Mr. Gregory say that he had such a conversation with Sir Isaac Newton. He said he knew that Mr. Keith, the ambassador, was also intimate with Mr. Gregory, and that he would write him to the same purpose.

"Some time after, Mr. Cross told me that he had answers from both the gentlemen above mentioned, and that both remembered to have heard Mr. Gregory mention the conversation between him and Sir Isaac Newton, to the purpose above narrated, and at the same time acknowledged that they had made no further inquiry about the matter.

"Mr. Cross, however, continued the inquiry, and a short time before his death, told me that all he had learned was, that there is, or was lately, a baronet's family of Newton in West Lothian, or Mid Lothian, (I have forgot which). That there is a tradition in that family, that Sir Isaac Newton wrote a letter to the old knight that then was (I think Sir John Newton, of Newton, was his name,) desiring to know what children, and particularly what sons he had, their age, and what professions they intended: That the old baronet never deigned to return an answer to this letter which his family was sorry for, as they thought Sir Isaac might have intended to do something for them."

Several years after this letter was written, a Mr. Barron, a relative of Sir Isaac Newton, seems to have been making inquiries respecting the family of his ancestor, and in consequence of this the late Professor Robison applied to Dr. Reid, to obtain from him a particular account of the remarkable conversation between Sir Isaac and Mr. James Gregory, referred to in the preceding letter. In answer to this request, Dr. Reid wrote the following letter, for which I was indebted to John Robison, Esq., Sec., R. S. E. who found it among his father's manuscripts.

LETTER FROM DR. REID TO PROFESSOR ROBISON, RESPECTING THE FAMILY OF SIR ISAAC NEWTON.

"Dear Sir,

"I am very glad to learn by yours of April 4, that a Mr. Barron, a near relation of Sir Isaac Newton, is anxious to inquire into the descent of that great man, as the family cannot trace it farther, with any certainty, than his grandfather. I, therefore, as you desire, send you a precise account of all I know; and am glad to have this opportunity, before I die, of putting this information in hands that will make the proper use of it, if it shall be found of any use.

"Several years before I left Aberdeen, (which I did in 1764) Mr. Douglas of Feckel, the father of Sylvester Douglas, now a barrister at London, told me, that having been lately at Edinburgh, he was often in company with Mr. Hepburn of Keith, a gentleman of whom I had some acquaintance, by his lodging a night at my house, at New Machar, when he was in the rebel army in 1745. That Mr. Hepburn told him, that he had heard Mr. James Gregory, professor of mathematics, Edinburgh, say, that being one day in familiar conversation with Sir Isaac Newton at London, Sir Isaac said, 'Gregory, I believe you don't know that I am a Scotchman.' 'Pray, how is that?' said Gregory. Sir Isaac said he was informed that his grandfather (or great-grandfather) was a gentleman of East (or West) Lothian; that he went to London with King James I., at his accession to the crown of England; and that he attended Court in expectation, as many others did, until he spent his fortune, by which means his family was reduced to low circumstances. At the time this was told me, Mr. Gregory was dead, otherwise I should have had his own testimony, for he was my mother's brother. I likewise thought at that time, that it had been certainly known that Sir Isaac had been descended from an old English family, as I think is said in his *eloge* before the Academy of Sciences at Paris, and therefore I never mentioned what I had heard for many years, believing that there must be some mistake in it.

"Some years after I came to Glasgow I mentioned (I believe for the first time) what I had heard to have been said by Mr. Hepburn to Mr. Cross, late Sheriff of this county, whom you will remember. Mr. Cross was moved by this account, and immediately said: 'I know Mr. Hepburn very well, and I know he was intimate with Mr. Gregory. I shall write him this same night, to know whether he heard Mr. Gregory say so or not.' After some reflection he added, 'I know that Mr. Keith, the ambassador, was also an intimate acquaintance of Mr. Gregory, and, as he is at present in Edinburgh, I shall likewise write to him this night.'

"The next time I waited on Mr. Cross he told me that he had written both to Mr. Hepburn and Mr. Keith, and had an answer from both, and that both of them testified that they had several times heard Mr. James Gregory say, that Sir Isaac Newton told him what is above expressed, but that neither they nor Mr. Gregory, as far as they knew, ever made any farther inquiry into the matter. This appeared very strange both to Mr. Cross and me, and he said he would reproach them for their indifference, and would make inquiry as soon as he was able."

"He lived but a short time after this, and in the last conversation I had with him upon the subject, he said, that all he had yet learned was that there was a Sir John Newton, of Newton, in one of the counties of Lothian, (but I have forgot which,) some of whose children were yet alive; that they reported that their father, Sir John, had a letter from Sir Isaac Newton, desiring to know the state of his family, what children he had, particularly what sons, and in what way they were. The old knight never returned an answer to this letter, thinking probably that Sir Isaac was some upstart, who wanted to claim a relation to his worshipful house. This omission the children regretted, conceiving that Sir Isaac might have had a view of doing something for their benefit."

"After this I mentioned occasionally in conversation what I knew, hoping that these facts might lead to some more certain discovery, but I found more coldness about the matter than I thought it deserved. I wrote an account of it to Dr. Gregory, your colleague, that he might impart it to any menber of the Antiquarian Society, who he judged might have the curiosity to trace the matter farther."

"In the year 1787, my colleague, Mr. Patrick Wilson, Professor of Astronomy, having been in London, told me on his return that he had met accidentally with a James Hutton, Esq., of Pimlico, Westminster, a near relation of Sir Isaac Newton, to whom he mentioned what he had heard from me with respect to Sir Isaac's descent, and that I wished much to know something more decisive on that subject. Mr. Hutton said, if I pleased to write to him he would give me all the information he could give. I wrote him accordingly, and had a very polite answer, dated at Bath, 25th December, 1787, which is now before me. He says, 'I shall be glad, when I return to London, if I can find, in some old notes of my mother, anything that may establish the certainty of Sir Isaac's descent. If he spoke so to Mr. James Gregory, it is most certain he spoke truth. But Sir Isaac's grandfather, not his great-grandfather, must be the person who came from Scotland with King James I. If I find anything to the purpose, I will take care it will reach you."

"In consequence of this letter I expected another from Mr. Hutton when he should return to London, but never had any. Mr. Wilson told me he was a very old man, and whether he be dead or alive I know not."

"This is all I know of the matter, and for the facts above-mentioned I pledge my veracity. I am much obliged to you, Dear Sir, for the kind expressions of your affection and esteem, which, I assure you, are mutual on my part, and I sincerely sympathise with you on your afflicting state of health, which makes you consider yourself as out of the world, and despair of seeing me any more."

"I have been long out of the world by deafness, and extreme old I age. hope, however, if we should not meet again in this world, that we shall meet and renew our acquaintance in another.—In the mean time, I am, with great esteem, Dear Sir, yours affectionately,

"THO. REID."

"Glasgow College,
"12th April, 1792."

This curious letter Dr. Brewster published in the Edinburgh Philosophical Journal for October, 1820. It excited the particular attention of the late George Chalmers, Esq., who sent the Doctor an elaborate letter on the subject; but as he was at that time in the expectation of obtaining some important information through other channels, this letter was not published. This hope, however, has been disappointed. A careful search was made through the charter-chest of the Newtons of Newton in East Lothian, by Mr. Richard Hay Newton, the representative of that family, but no document whatever could be found to throw the least light upon the matter. It deserves to be remarked, however, that Sir Richard Newton, the alleged correspondent of Sir Isaac, appears to have destroyed his correspondence; for though the charter-chest contains the letters of his predecessors for some generations yet there is not a single epistolary document either of his own or his lady's.

Hitherto the evidence of Sir Isaac's Scottish descent has been derived chiefly from his conversation with Mr. James Gregory; but Dr. Brewster was enabled, by the kindness of Mr. Robison, to corroborate this evidence by the following information, derived, as will be seen, from the family of the Newtons, of Newton. Among various memoranda in the handwriting of Professor Robison, who at one time proposed to write the life of Sir Isaac, are the following :—

"1st, Lord Henderland informed me in a letter dated March, 1794, than he had heard from his infancy that Sir Isaac considered himself as descended from the family of Newton of Newton. This he heard from his uncle, Richard Newton, of Newton, (who was third son of Lord William Hay, of Newhall). He said that Sir Isaac wrote to Scotland to learn whether any descendants of that family remained, and this (it was thought) with the view to leave some of his fortune to the family possessing the estate with the title of baronet. Mr. Newton not having this honour, and being a shy man, did not encourage the correspondence, because he did not consider himself as of kin to Sir Isaac Newton, &c."

"2nd, Information communicated to me by Hay Newton, Esq., of that Ilk, 18th August, 1800.

"The late Sir Richard Newton of Newton, Bart., chief of that name, having no male children, settled the estate and barony of Newton in East Lothian county upon his relation, Richard Hay Newton Esq., son of Lord William Hay. (This entail was executed in 1724, a year or two before Sir Richard's death.) It cannot be discovered how long the family of Newton have been in possession of the barony, there being no tradition concerning that circumstance farther than that they came originally from England at a very distant period and settled on these lands." "The celebrated Sir Isaac Newton was a distant relation of the family, and corresponded with the last baronet, the above mentioned Sir Richard Newton."

The preceding documents furnish the most complete evidence that the conversation respecting Sir Isaac Newton's family took place between him and Mr.

Gregory; and the testimony of Lord Henderland proves that his own uncle, Richard Newton, of Newton, the immediate successor of Sir Richard Newton, with whom Sir Isaac corresponded, was perfectly confident that such a correspondence took place.

All these circumstances prove that Sir Isaac Newton could not trace his pedigree with any certainty beyond his grandfather, and that there were two different traditions in his family, one of which referred his descent to John Newton, of Westby, and the other to a gentleman of East Lothian, who accompanied King James VI. of Scotland, to London, on his accession to the English throne. In the first of these traditions he seems to have placed most confidence in the year 1705, when he drew out his traditionary pedigree; but as the conversation with Professor James Gregory respecting his Scotch extraction took place twenty years afterwards, namely, between 1725 and 1727, it is probable that he had discovered the incorrectness of his first opinions, or at least was disposed to attach more importance to the other tradition, respecting his descent from a Scotch family.

In the letter addressed to Dr. Brewster by Mr. Chalmers, the following observations respecting the immediate relations of Sir Isaac are made. "The Newtons of Woolsthorpe," he says, "who were merely yeomen farmers, were not by any means opulent. The son of Sir Isaac's father's brother was a carpenter called John. He was afterwards appointed gamekeeper to Sir Isaac, as lord of the manor, and died at the age of sixty in 1725. This John had a son Robert (John ?) who was Sir Isaac's second cousin, and who became possessed of the whole land estates at and near Woolsthorpe, which belonged to the great Newton, as his heir-at-law. Robert (John ?) became a worthless and dissolute person, who very soon wasted this ancient patrimony, and falling down with a tobacco pipe in his mouth when he was drunk, it broke in his throat, and put an end to his life at the age of thirty years, in 1737."

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We conclude this volume with a short outline of Natural Philosophy, which, it is hoped, will not be considered inappropriate by the inquirer after scientific knowledge. It is abridged from an excellent article in the Dublin University Magazine; and is alike distinguished for the simplicity of its language and the utility of its object:—

The history of philosophy is one of the most interesting studies of the man of letters. The view of the progress of knowledge, from its earliest recorded development, to its present extended diffusion, offers to the reflecting mind a field of contemplation worthy of traversing. If we compare the feeble efforts of the most ancient philosophers to penetrate the veil of ignorance which then surrounded mankind, with the mighty power of the promoters of knowledge of the present day, we must be struck with the extraordinary progress of the human mind through the different stages of barbarism, semi-civilization, and recent advancement. Let us consider the kind of knowledge acquired by these philosophers on their travels.

Thales, of Miletus, visited Egypt, where he studied geometry, astronomy, and cosmogony. He was the founder of the Ionian sect of philosophy, upon his return to his native place. He appears to have taught the cause of the inequality of days and nights, and the theory of eclipses. He maintained that water is the principle of which all the bodies in the universe are composed—that the world was the work of God, and that God sees the most secret thoughts in the heart of man. It is related that he measured the height of the pyramids of Memphis by the extent of their shadows; and he is considered the first who employed the circumference of a circle in the measurement of angles.

Pythagoras studied geometry among the Egyptians. This science he improved by his subsequent discovery of several important propositions. He is the earliest recorded teacher of the true system of astronomy, and he made many important discoveries in the other physical sciences. He observed many curious phenomena on the surface of the earth, which must have led him to reason on the changes which this surface must have undergone in the lapse of ages. In the 15th book of Ovid's Metamorphoses, a number of these observations are mentioned, which are extremely curious, and testify, in a very remarkable manner, the superior mind of the philosopher.

Plato also travelled into the east, where he became versed in the learning of the Persians and the Egyptians. He wrote several works which treated chiefly of metaphysical subjects. He mingled together his doctrines of theogony and cosmogony, so that it is a difficult matter to separate his peculiar notions of the latter. The passage in his writings most interesting to the modern geologist, is that which treats of the Atlantic; recorded by Plato as a large continent beyond the Pillars of Hercules, and which had sunk under water, thereby giving place to the present Atlantic Ocean. He made many improvements in geometry; to him is ascribed the discovery of the mathematical bodies, called the regular solids. He conceived the world to be a figure shaped like one of these solids, called the "dodecahedron."

Of the opinions of Aristotle respecting the formation of the world, we have not any clear account. He wrote upon a variety of subjects, among which Natural history occupied a prominent place. "He regarded the matter of the heavens as ingenerate and eternal

—that mankind and all species of animals, have subsisted from everlasting by a perpetual course of generation without any original beginning or production; and that the earth has been for ever adorned with trees, plants, flowers, minerals, and other productions as we now see it to be." It is possible that he may have taken the eternity of the world from Lucans, a disciple of Pythagoras, who is the most ancient assertor of this idea, so different from the opinion of his master.

We thus find that the most eminent ancient philosophers indulged more or less in reveries respecting cosmogony. In studying other branches of learning, they must have been frequently led into considerations of the probable origin of the world which they inhabited; and they endeavoured to frame hypotheses, some of which were very ingenious, but more generally extremely absurd. Occasionally a master mind, like that of Pythagoras, made an approximation to the truth, which has astonished the learned of later times. Sometimes facts were related in corroboration of these hypotheses—sometimes they were distorted to explain the dreams of philosophic fancy. But among the ancients, the observation of natural facts was not made in a way to benefit science. We find many of the arts and sciences brought by the ancients to a considerable degree of perfection. In architecture, poetry, eloquence, and perhaps in some other branches, they excel the moderns. The progress made by them in geometry was admirable indeed; and they based that science upon a foundation fitted to bear the splendid superstructure raised by modern ingenuity. But in those branches that required a combination of the perceptive and reasoning powers, their progress was very limited. Chemistry and experimental philosophy are of modern origin: at least what was effected in them by the ancients, or rather what is recorded as having been effected, is very trifling. In botany, zoology, and mineralogy, a number of detached observations have been recorded in the writings of Aristotle, Theophrastes, and Pliny; but no classification was ever attempted, nor any extended train of reasoning from these facts adopted by the ancient philosophers.

If we turn onr attention to the state of knowledge among the Arabians, we shall find that the mathematical sciences were for the most part cultivated by them. They devoted, it is true, some attention to astronomy and to alchemy; but the former being studied for the purpose of contributing to their desire to penetrate the mysteries of fate, was merely a compound of truth and falsehood that has been denominated astrology; and the latter, ministering to the passions of most men for acquiring wealth, offered a strong temptation to its votaries, but was of no service in furthering the march of mind, although it discovered facts that were afterwards of service to the modern chemist. We may regard the Arabians more as the preservers of ancient science. Many of the inventions ascribed to them have been traced to the Indians, and were received either directly from that people, or through the medium of the Greek philosophers, whose works were translated into the Arabic language.

The conclusion to which we may arrive from a contemplation of the state of learning in the early and middle ages is, that geometry was the only science successfully cultivated, and handed down free of any error or absurdity; all the other branches of learning were more or less imbued with mistaken views, arising generally from imperfect data. There seemed to be little respect paid to knowledge acquired from the observations of facts of daily occurrence. Abstruse studies were most esteemed. Mathematics requiring a deep train of thought, and at the same time of correct reasoning, without needing the aid of experiment requisite in physical science, enabled the cultivators to improve without any danger of perversion.

In the history of natural philosophy a knowledge of mathematics is indispensable on the part of the student; if he venture beyond the vestibule. The votary of pure mathematics will be insensibly led to the application of his favourite science for the explanation of some of the phenomena in Nature; and if he be successful in solving any of their mysteries, he will be encouraged to pursue still further the research after physical truth. The ancients were thus led to apply their mathematical knowledge. The name of Archimedes is handed down as one of the brightest in ancient times. His discoveries form an important era in the history of science; and they arose from his extensive application of geometry to physics. Before his time there were no correct notions of the theory of mathematics, and he is the first who pointed out the specific gravity of bodies. He is the only one of the ancients who can bear comparison with the moderns as a natural philosopher.

When learning began, after the dark ages, to revive in Europe, the mathematical lore of the ancients was sought after with great avidity. What had been effected by them in the physical sciences received also its due appreciation. Both the truth and the absurdity of their astronomy, mechanics, and other branches, were swallowed at first without being questioned. But as the attention of the early moderns became more directed to scientific investigation, many of the errors of the ancients became manifest. Some of these were speedily corrected, while others, admitting of more discussion, remained for a longer period under the judgment of the new cultivators of knowledge. Every successive age, however, dispelled more or less of these errors. The study of the pure mathematics advanced with rapid strides. Their field was augmented with numberless new discoveries. Their application to physics became every day more general; and the impulse which science had now received, carried forward its votaries with a velocity never before known in its progress.

Natural philosophy had now become a science of great importance, from the additions made by its early modern cultivators to what was received by them from the ancients. Among the ancients, Archimedes was the first who applied geometry to physics, and then gave it a double power. Among the first revivors of learning in Europe was Descartes, who applied algebra to geometry, and thus put an engine of incalculable power into the hands of the cultivators of natural philosophy. The attention of the learned was now directed to experiment, a method, if investigated, but little appreciated or understood by the ancients. Galileo, by his invention of the telescope, opened, as it were, the gates of the heavens, into which rushed a host of ardent inquiriers after truth. A number of other illustrious men at the same time directed their attention to experiment in the other branches of physical science. The discovery of printing some time before enabled the new acquisitions to knowledge to be widely diffused, and enabled one nation to communicate its knowledge to another with a rapidity never before conceived. The discovery of America not only was a successful experiment on a large scale, but it added another proof to the true system of the world, and directed in a further degree the attention of men to the examination of natural phenomena. The Reformation promoted the freedom of discussion, and enabled the laity to take part in studies, almost previously attended to by the clergy alone. But what contributed in the greatest degree to the improvement of physical science, was the new path pointed out to its votaries by the illustrious Lord Bacon. His master mind discovered the causes of error in the philosophy of the ancients, and demonstrated that as long as their mode of reasoning was pursued, it was impossible for the moderns to frame a true system of science. He asked, "Wherein can arise such vagueness and sterility in all the physical systems which have hitherto existed in the world?" It is not certainly from anything in nature itself; for the steadiness and regularity of the laws by which it is governed clearly mark them out as objects of certain and precise knowledge. Neither can it arise from any want of ability in those who have pursued such inquiries, many of whom have been men of the highest talent and genius of the ages in which they lived; and it can therefore arise from nothing else but the perverseness and insufficiency of the methods that have been pursued. Men have sought to make a world from their own conceptions, and to draw from their own minds all the materials which they employed; but if, instead of doing so, they had consulted experience and observation, they would have had facts, and not opinions to reason about, and might have ultimately arrived at the knowledge of the laws which govern the material world."

The opinions of Bacon became gradually appreciated. The calm observation of facts became in every succeeding age more attended to, and natural philosophy based upon actual experiment, and not upon the wild conceptions of the imagination.

The science of chemistry may be said to be one of the offspring of the Baconian philosophy. It is a branch of physics—the first principles of which depend wholly upon experiment. Its progress testifies, in a remarkable degree, the importance of the inductive method of reasoning. It required a calm and patient examination of the changes produced on bodies by experiment, in order that a sufficient mass of facts might be accumulated to raise it to the rank of a science. It required its cultivators to divest themselves of all prejudices—to view things as they really are, and not to set out with a previous leaning to a particular set of opinions, and to torture the result of their experiment to prop these opinions up. It is impossible it could have become a regular science under the influence of the Aristoteleian philosophy. The more men reasoned from facts, the more did chemistry advance; and it now affords one of the most brilliant examples of the happy mode pointed out by Bacon, for the improvement of knowledge.

If we sum up the results of our observations on the progress of knowledge, we shall arrive at the conclusion, that the observation of facts was the very last object attended to in its march through successive ages—at least the calm and unprejudiced examination of facts. The learned of antiquity seemed more inclined to abstruse studies—to studies which required a precision of reasoning, which often testified their extreme ingenuity. If their first principles were correct, so were their conclusions. But that their first principles were, for the most part, erroneous, we have obdurate proof in many instances. In geometry their first data were correct, being self-evident truths, and their conclusions were therefore just. For a succession of ages, knowledge, derived from observation of the common objects in nature, was looked down upon as unworthy the regard of the philosopher. There was a consequent bar to the progress of physical knowledge.

After the revival of learning in Europe, it was not to be expected that the eyes of man could be all at once opened to the errors of the ancients. Both the truth and falsehood of the ancient philosophy were studied. The human mind was still imbued with prejudice. This, however, gradually wore away—mathematics became more extended—natural philosophy was extended and improved—chemistry was invented. We may gradually trace the progress of real knowledge from the first axioms of geometry, through the more advanced stages of mathematics—through the successive developments of natural philosophy—to a science resting solely upon a careful examination of facts, the science of chemistry.

Natural philosophy opened to man the field of space—it taught him to regard the motion of objects upon a grand scale—it enabled him to assign dimensions to this space, and to measure the relations of motion. Chemistry taught him to view the changes produced on bodies, by motions of their minute component particles, the measurement of which motions eludes our most subtle investigations. Natural philosophy instructed him in the external relations of the bodies in nature—chemistry in the internal. It led him, as it were, into the mind of inanimate matter.

During the last hundred years, the attention paid to an unprejudiced examination of facts has been continually on the increase. Philosophers have entered upon their investigations without bias to any particular opinions. Instead of commencing their researches by laying down a favourite hypothesis, and then distorting facts to accord with it, they commenced with an impartial examination of the facts themselves; and, following the suggestions of Bacon, they framed their theory by the inductive method of reasoning. By not attending to the advice of Bacon, men were inclined to imagine circumstances which have no existence in reality. They beheld nature through a medium that rarely presented her in her true form. So long as the field of physical science was limited, the liability to view nature in this manner continued; but, as discovery followed discovery, the disposition to prejudice became more and more removed. For a long period men adhered to certain dogmas that had been handed down through a succession of ages, and finding it difficult to reconcile many facts in nature with these dogmas, they had recourse to hypothesis, the frequent absurdity of which paved the way to the exploding of erroneous principles, and dispelled the illusion, although supported by the authority of antiquity. Knowledge derived from facts, spreading far and wide, carried with it the examples of its own importance. The arts of life received incalculable improvements—they, in return, aided science. They formed the passage from one branch to another—they were the illustrators of theory, by showing its practical application. The ancient philosopher would have disdained to lend his aid to the agriculturist, the mechanist, or the navigator. The modern man of science regards, as his proudest boast, the improvements given by him to the arts, thereby rendering man little inferior in power to the deities of the ancients.

It seems indeed strange that the impartial examination of facts is the result of a highly improved state of scientific knowledge. But so it is. It is only within a comparatively recent period that the point has been reached by the human mind. It has been often remarked, that the farther we advance in knowledge, the more deeply are we impressed with a sense of our own ignorance. Although this may not be exactly true, still we may acknowledge that we are less confident in broaching any new theory, in proportion to the advance of the march of intellect. When our opinions can be submitted to the test of an appeal to facts, we are more cautious in stating these opinions. It is our province to study the facts attentively, so that we may not be found in the wrong.

Geometry teaches us the relations of dimensions as conceived in the human mind, but which, in the abstract, has no real existence. Natural philosophy treats of the relations of existing bodies, viewed in a state of motion. Chemistry informs us of the changes produced in the bodies themselves. Mathematics, natural philosophy, and chemistry, frequently view conditions which have no existence in nature.

Let us now direct our attention to a branch of knowledge which treats of the objects in nature as they really appear—Natural History. The field of natural history is wide indeed : it includes all nature. But as we cannot penetrate beyond the confines of the earth which we inhabit, we must be content to become acquainted with what it contains on its surface or in its bosom. If we explore the animal, the vegetable, or the mineral kingdom—if we mount into the atmosphere, or descend into the mine—we are impressed with the imperative necessity of our acquaintance with the different branches of physics. This demonstrates the mutual dependence of one branch of knowledge on another, and in the consideration of this dependence a field of interesting and delightful occupation is opened to us. Let us enter this field ourselves, and let us view these mutual relations.

It will be sufficient for our present purpose, if we view the relations of natural history with the other branches of knowledge. The relations of these other branches among each other can be deduced from after consideration. It will also simplify our subject, if we select some individual part of natural science, and then show its dependence upon other kinds of knowledge. The consideration of any one point will lead to that of others, and these to more, so that we have merely to choose the particular place from which we are to start; we shall have no difficulty in our way of gathering materials for comparisons.

The part of natural history which we have selected for our purpose is geology. There is, perhaps, no portion of physical science so well suited for exemplifying the relations of its different branches with each other. It treats of, says Lyell, "the successive changes that have taken place in the organic and inorganic kingdoms of nature. It inquires into the causes of these changes, and the influence which they have exerted in modifying the surface and external structure of our planet." It is the science of the earth which we inhabit—it is a science that may, at all times, be studied. If we explore the mountain or the valley, the quarry or the mine, we may everywhere find matter for reflection; we may read the book of creation, written in characters not to be misunderstood. The language admits of no misinterpretation—it is the language of facts.

The division of natural history that presses closest upon the attention of the geologist, is mineralogy. The mineral masses of the globe are what at first arrest his attention. If he walk through a cultivated country, he does not find geological phenomena of mineral substances so interesting as in districts more in a state of wild and uncultivated nature. Still there is much to claim his attention. The soil in the fields will differ in many places, and furnish an index of the substratum. Thus he will distinguish the ferrugenous clayey soil that indicates the underlying of basaltic rocks—the calcareous clayey soil resting upon mountain limestone—the chalky soil—the granitic soil, and various others. If he examine the low grounds he will find the finer particles of clay accumulated in the lowest situations; along the slopes of the hills he will observe coarser parts of mineral substances, increasing in size as he approaches the rocky summits. If he enter into a mountain district, he will discover large masses of rocks in various states of disintegration, their harder parts withstanding the assaults of ages, their softer parts separated by various causes from the harder, and carried from these elevated regions to the lower parts of the country by the agency of rivers and torrents. In examining these mineral substances, he will find his knowledge of mineralogy brought into requisition. If his knowledge of it be extensive, he may be enabled to discover something that may not have been known before. If his knowledge of it be slight, he will find the examination of nature an excellent exercise and means of improvement.

An acquaintance of chemistry is also requisite in the study of every department of natural history. In zoology and botany we find it constantly referred to for the explanation of the vital functions and the composition of animal and vegetable substances. In mineralogy its importance is still greater, from the circumstance that the elementary bodies found in plants and animals, are very few in comparison with those composing minerals. In the view of the changes that have taken place in the globe, we are struck with the important part played by chemical agency. We can conceive no alteration produced either on the surface or in the interior of the globe in which this agency was not, in a greater or less degree, exerted. If we regard the action of existing causes, still going on, in altering the surface of the earth, we shall find at every step appeal made to chemical knowledge.

A knowledge of mechanical philosophy is also required by the student of geology. The laws of attraction and motion are always in action, and no change can take place in the globe at variance with these laws. The consideration of the subject of attraction is of great importance to the geologist; it leads him into the examination of the density of mineral masses, and of the globe itself. It will be many phenomena which at first sight may seem anomalous. The application of the laws of motion will be also a powerful auxiliary, whether we view the movement of great mineral masses, or of their fragments; or that of currents in the atmosphere or the ocean.

The connection of hydrostatics with geology is of the greatest importance. The vast ocean which covers so much of the earth's surface is subject to hydrostatical laws; so are the lakes and rivers. Many of the phenomena of the torrents that rush from the mountains upon the plains, and which are so important agents in modifying this surface, will be understood by applying the laws of hydrostatics. Many geologists have conceived the globe itself to have been at one time in a state of fluidity. How are we to criticise the opinions of these philosophers, but by assigning the truth of their conclusions to the standard of hydrostatics.

The atmosphere surrounding the globe may be said to fall within the province of the geologist. In any system of cosmogony that has been, or ever can be invented, the atmosphere will play a prominent part, as its original formation may be accounted for. It is the grand cause of most of the alterations that take place on the surface of the earth. It is the cause of the rain that fertilizes one portion of this surface while it gradually wears down another, and of the snow that caps the lofty mountains, which is the magazine for supplying rivers and lakes. It is the supporter of plants that cover the earth, and that modify its superficies in an infinite variety of ways; in fact, to dilate upon this subject would require a separate memoir.

The geologist must also take into consideration electrical phenomena. By some cosmogonists electricity is made to play a very prominent part in the formation of the earth; we must therefore be acquainted with it in order to combat them with their own weapons. The knowledge of the different electric states of bodies is also of the first importance in the explanation of many of the natural phenomena. At times of volcanic eruptions, or of earthquakes, these phenomena become most interesting, and will, if viewed by a well-informed observer, be turned to good account in elucidating the history of our planet. The discoveries made by some philosophers in galvanism have been applied to expound geology; several have conceived they had obtained a key for the solution of all difficulties in the action of oxygen upon the base of the alkalies and earths brought to light by galvanism. The science of magnetism is still in its cradle, but its connection with geology is most intimate. It is now under the fostering care of some of the most eminent philosophers, and will, I am sure, ere long, throw much light upon geology.

With the science of astronomy, geology is linked in the closest degree. In the early ages of knowledge, the attention of men was turned to the contemplation of the heavens, and many years of observation taught them to recognize the celestial bodies. Their imaginations invented many fables with regard to the stars and planets. This play of fancy descended to the earth, and from astronomy the mind of the ancient philosopher was led to consider the subject of cosmogony. In the ancient systems of astronomy the earth is regarded as a vast plain, and the ancient notions of its formation and alteration, especially by deluges, are all grounded upon the supposition of its being a plane superficies, with the exception of the doctrine of the Pythagoreans. That the earth is of a plane figure, would strike the mind more forcibly than its possessing a spherical shape. This latter opinion is the result of deep and laborious reflection. The causes of error among the early cosmogonists are in this case extremely manifest. An advanced state of the science of astronomy was requisite to prove to the satisfaction of all that the form of the earth is globular.

It has been supposed by some philosophers of the last century that there has been a change of climate in the different regions of the earth's surface, as the remains of organic beings are found in situations where, had they lived, it would have been impossible for them to have existed under the circumstances of the present temperature of those places. They have conceived a solution of this difficulty will be, that the poles of the globe have changed their positions, and still continue to do so—that at one time their points were in the present equator, and that the frigid regions were where the torrid now are. This supposition can only be refuted by a very refined train of astronomical investigation, and it was left to the immortal Laplace to demonstrate its incorrectness; and that, although there is a trifling shifting of the extremities of the terrestrial axis, these irregularities are confined within certain limits, and are the consequence of the extreme perfection of the system of the world. In many other particulars, the connection of geology and astronomy might be shown, but the investigation would lead us into a field that would require a separate paper to describe.

Although the connection of optics with geology may not at first strike us as being very manifest, its indirect relations are not very important. Of late years, very singular discoveries have been made in optics, particularly by Dr. Brewster. These discoveries refer to the action on the sight, of bodies having a crystalline form; a very remarkable connection has been observed between their chemical composition, crystalline structure, and optical phenomena. These discoveries will evidently improve the science of mineralogy, and the more it is improved, the better will geology be elucidated. From the intimate connection of astronomy and geology, and of astronomy with optics, especially with regard to the instruments for observing the heavenly bodies, the improvements in these instruments, by adding to our knowledge of the heavens, will enable us to extend our acquaintance with the earth. The invention of the microscope has unfolded the secret of organization; and as geology treats of the organic, as well as of the inorganic changes of the kingdoms of nature, the minuteness of which eludes the observation of the naked eye, it will be appreciated in the highest degree.

In the study of natural philosophy, we cannot proceed far without the aid of mathematics. In geology itself there are many points that require immediately the mathematical skill of the observer; so that, viewed both directly and indirectly, mathematics must be understood by the geologist. I have shown that the philosophers of antiquity who proposed systems of cosmogony, were all geometers, and among the moderns, many illustrious names—I need only quote that of Playfair for example—have been distinguished alike for mathematical and geological knowledge. An acquaintance with civil history is likewise required. As history teaches us the progress of human society, and informs us of the changes that have taken place in human institutions, so geology instructs us in the mutations of organic and inorganic nature. Where there are records of natural changes, preserved in the pages of history, its connection with geology becomes more intimate, and is often of the greatest value.

As knowledge becomes more cultivated by different nations, the languages spoken by those nations should be attended to by the student. Many of the natural appearances of a country cannot be described in a manner to bring them before the eye of the reader, so well as in the language of that country. Foreign literature is now so much extended that it would be both impossible and unprofitable to translate all the eminent publications that are almost daily issuing from the continental press. Periodical literature is an invention of modern times. Periodical publications must be read in the language in which they are originally written, in order that their full force and importance may be felt. It is also proper that the opinions of the ancients should be studied in their respective tongues. The difficulty of acquiring languages has been much decreased of late years, by the improvements in the mode of learning them, and the knowing of one or two will open to the student the gates of as many more as he may desire to study.

With intellectual philosophy, the connection of geology is very interesting. A true system of geology can be the result only of a highly refined train of reasoning. I have shown the imperfection of the ancient mode of reasoning, and its consequent influence on physical science. Bacon pointed out to me the true mode of observing the system of nature. In studying geology, we should be aware of the proper mode of conducting our reasonings from facts, and be alive to our liability to commit errors. The science of logic here lends its aid, and matter is aided by the powers of the mind.

We will readily arrive at the conclusion, that, if an improvement take place in any one department of knowledge, it cannot fail to be of use to all the others. The connection which I have endeavoured to show between geology and the other branches of philosophy, will furnish us constantly with illustrations. In fact, we cannot study any one circumstance in science in an isolated manner. We must view its relation to others in order to understand it.

Referring again to the definition of geology, that it treats both of the organic and inorganic changes of nature, we may recal an important observation which we made a little way before, that it requires a knowledge of the productions of the animal and vegetable kingdoms. If the attention of the geologist be confined to the consideration merely of the mineral masses of our globe, his exertions will be placed within narrow limits—his science will be incomplete—he must regard attentively the remains of organised beings enshrined in many of these mineral masses—he must compare them with existing species—he will discover many exuviae of beings very different from the races that now inhabit the earth—he will observe the bones of gigantic mammalia and reptiles, the types of which are not now to be found—of fishes, birds, and insects—an immense variety of fossil shells, scarcely any of which agree with existing species—he will discover the remains of plants, many of which he will in vain endeavour to reconcile with those that now clothe the surface of the earth. But the knowledge of these fossil remains is still in its infancy—he must be previously acquainted with those natural productions, both animal and vegetable, that are now found on the earth—he must study geology, in order to know the external forms of animals, their habits, and instincts, and anatomy, that he may be able to recognise the species of fossil bones. To understand the connection of the external forms of animals with their internal structure, he must be acquainted with physiology. The connection of conchology with geology is of the closest description. When an ignorant person observes another gathering shells upon the beach, he is inclined to despise him, considering the occupation as one belonging to children. When he sees him searching for shells imbedded in rocks, he thinks the sanity of the collector to be dubious! He little thinks that these imbedded shells are part of the language in which the history of the globe is recorded. They have been called the medals that illustrate its annals. The science of botany enables the geologist to investigate the remains of plants found in strata. To compare them with existing species is a most interesting task. The wonders of the antediluvian world are developed by this application of zoology and botany. This illustrates how intimately the different branches of natural history are linked with each other. The additions made to them within a very recent period are so great as to astonish when enumerated. At the present moment the utmost zeal is manifested to acquire a knowlegde of natural productions, both recent and fossil. The geologists of the present day vie with each other in their investigation of organic remains. The illustrious Cuvier applied his profound knowledge of anatomy to the examination of bones found in strata ; his steps have been followed by a number of other ardent inquirers. The vast variety of shells, corallines, and other remains of the lower animals, discovered in rocks, are undergoing a strict scrutiny by naturalists well versed in the knowledge of recent productions. Very lately the examination of fossil plants has excited the ardour of several of the students of natural science, and their labours are likely to be rewarded ere long with an abundant harvest of facts. Geology is thus becoming based upon a sure foundation—it is showing itself worthy of the attention of the philosopher. The more facts that may be discovered, the surer will be the foundation upon which it will rest. The improvements in the other departments of physical and intellectual knowledge will contribute to cement the superstructure, while the study of the science itself will form one of the greatest means of human happiness.

*FINIS.*

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