A PHILOSOPHICAL and MATHEMATICAL DICTIONARY.

A.

ABACIST, an Arithmetician. In this fenfe we find the word ufed by William of Malmefbury, in his Hiftory *de Geftis Anglorum*, written about the year 1150; where he fhews that one Gerbert, a learned monk of France, who was afterwards made pope of Rome in the year 998 or 999, by the name of Silvefter the 2d, was the firft who got from the Saracens the abacus, and that he taught fuch rules concerning it, as the Abacifts themfelves could hardly underftand.

ABACUS, *in Arithmetic*, an ancient inftrument ufed by moft nations for cafting up accounts, or performing arithmetical calculations: it is by fome derived from the Greek ἀβαξ, which fignifies a cupboard or beaufet, perhaps from the fimilarity of the form of this inftrument; and by others it is derived from the Phœnician *abak*, which fignifies duft or powder, becaufe it was faid that this inftrument was fometimes made of a fquare board or tablet, which was powdered over with fine fand or duft, in which were traced the figures or characters ufed in making calculations, which could thence be eafily defaced, and the abacus refitted for ufe. But Lucas Paciolus, in the firft part of his fecond diftinction, thinks it is a corruption of Arabicus, by which he meant their Algorifm, or the method of numeral computation received from them.

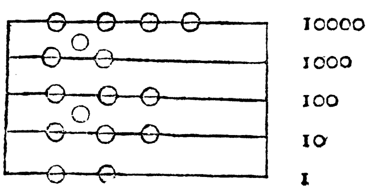
We find this inftrument for computation in ufe, under fome variations, with moft nations, as the Greeks, Romans, Germans, French, Chinefe, &c.

The Grecian abacus was an oblong frame, over which were ftretched feveral brafs wires, ftrung with little ivory balls, like the beads of a necklace; by the various arrangements of which all kinds of computations were eafily made. Mahudel, in Hift. Acad. R. Infcr. t. 3. p. 390.

The Roman Abacus was a little varied from the Grecian, having pins fliding in grooves, inftead of ftrings or wires and beads. Philof. Tranf. No. 180.

The Chinefe Abacus, or Shwan-pan, like the Grecian, confifts of feveral feries of beads ftrung on brafs wires, ftretched from the top to the bottom of the inftrument, and divided in the middle by a crofs piece from fide to fide. In the upper fpace every ftring has two beads, which are each counted for 5; and in the lower fpace every ftring has five beads, of different values, the firft being counted as 1, the fecond as 10, the third as 100, and fo on, as with us. See SHWAN-PAN.

The Abacus chiefly ufed in European countries, is nearly upon the fame principles, though the ufe of it is here more limited, becaufe of the arbitrary and unequal divifions of money, weights, and meafures, which, in China, are all divided in a tenfold proportion, like our fcale of common numbers. This is made by drawing any number of parallel lines, like paper ruled for mufic, at fuch a diftance as may be at leaft equal to twice the diameter of a calculus, or counter. Then the value of thefe lines, and of the fpaces between them, increafes, from the loweft to the higheft, in a tenfold proportion. Thus, counters placed upon the firft line, fignify fo many units or ones; on the fecond line 10's, on the third line 100's, on the fourth line 1000's, and fo on: in like manner a counter placed in the firft fpace, between the firft and fecond line, denotes 5, in the fecond fpace 50, in the third fpace 500, in the fourth fpace 5000, and fo on. So that there are never more than four counters placed on any line, nor more than one placed in any fpace, this being of the fame value as five counters on the next line below. So the counters on the Abacus, in the figure here below, exprefs the number or fum 47382.



Befides the above inftruments of computation, there have been feveral others invented by different perfons; as *Napier's rods* or *bones*, defcribed in his Rabdologia, which fee under the word Napier; alfo the *Abacus Rhabdologicus*, a variation of Napier's, which is defcribed in the firft vol. of *Machines et Inventions approuvées par l'Academie Royale des Sciences*. An ingenious and general one was alfo invented by Mr. Gamaliel Smethurft, and is defcribed in the Philofophical Tranfactions, vol. 46; where the inventor remarks that computations by it are much quicker and eafier than by the pen, are lefs burthenfome to the memory, and can be performed by blind perfons, or in the dark as well as in the light. A very comprehenfive inftrument of this kind was alfo contrived by the late learned Dr. Nicholas Saunderfon, by which he performed very intricate calculations: an account of it is prefixed to the firft volume of his Algebra, and it is there by the editor called *Palpable Arithmetic*: which fee.

Abacus, *Pythagorean*, fo denominated from its inventor, Pythagoras; a table of numbers, contrived for readily learning the principles of arithmetic; and was probably what we now call the multiplication-table.

Abacus, or Abaciscus, in *Architecture*, the upper part or member of the capital of a column; ferving as a crowning both to the capital and to the whole column. Vitruvius informs us that the *Abacus* was originally intended to reprefent a fquare flat tile laid over an urn, or a bafket; and the invention is afcribed to Calimachus, an ingenious ftatuary of Athens, who, it is faid, adopted it on obferving a fmall bafket, covered with a tile, over the root of an Acanthus plant, which grew on the grave of a young lady; the plant fhooting up, encompaffed the bafket all around, till meeting with the tile, it curled back in the form of fcrolls: Calimachus paffing by, took the hint, and immediately executed a capital on this plan; reprefenting the tile by the Abacus, the leaves of the acanthus by the volutes or fcrolls, and the bafket by the vafe or body of the capital. See Acanthus.

Abacus is alfo ufed by Scamozzi for a concave moulding in the capital of the Tufcan pedeftal. And the word is ufed by Palladio for other members which he defcribes. Alfo, in the ancient architecture, the fame term is ufed to denote certain compartments in the incruftation or lining of the walls of ftate-rooms, mofaic-pavements, and the like. There were *Abaci* of marble, porphyry, jafper, alabafter, and even glafs; varioufly fhaped, as fquare, triangular, and fuch like.

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Abacus *Logifticus* is a right angled triangle, whofe fides, about the right angle, contain all the numbers from 1 to 60; and its area the products of each two of the oppofite numbers. This is alfo called a *canon of fexagefimals*, and is no other than a multiplication-table carried to 60 both ways.

Abacus & *Palmulæ*, in the Ancient Mufic, denote the machinery by which the ftrings of the polyplectra, or inftruments of many ftrings, were ftruck, with a plectrum made of quills.

Abacus *Harmonicus* is ufed by Kircher for the ftructure and difpofition of the keys of a mufical inftrument, either to be touched with the hands or feet.

Abacus, in *Geometry*, a table or flate upon which fchemes or diagrams are drawn.

ABAS, a weight ufed in Perfia for weighing pearls; and is an eighth part lighter than the European carat.

ABASSI, a filver coin current in Perfia, deriving its name from Schaw Abbas II. King of Perfia, and is worth near eighteen pence Englifh money.

ABATIS, or Abattis, from the French *abattre*, to throw down, or beat down, in the Military Art, denotes a kind of retrenchment made by a quantity of whole trees cut down, and laid lengthways befide each other, the clofer the better, having all their branches pointed towards the enemy, which prevents his approach, at the fame time that the trunks ferve as a breaft-work before the men. The Abattis is a very ufeful work on moft occafions, efpecially on fudden emergencies, when trees are near at hand; and has always been practifed with confiderable fuccefs, by the ableft commanders in all ages and nations.

ABBREVIATE; to abbreviate fractions in arithmetic and algebra, is to leffen proportionally their terms, or the numerator and denominator; which is performed by dividing thofe terms by any number or quantity, which will divide them without leaving a remainder. And when the terms cannot be any farther fo divided, the fraction is faid to be in its leaft terms.

So $\frac{16}{24} = \frac{8}{12} = \frac{4}{6} = \frac{2}{3}$,

by dividing the terms continually by 2.

And $\frac{294}{504} = \frac{147}{252} = \frac{49}{84} = \frac{7}{12}$,

by dividing by 2, 3, and 7.

Alfo $\frac{3 \times 8 \times 5}{6} = \frac{8 \times 5}{2} = 4 \times 5 = 20$,

by dividing by 3 and by 2.

And $\frac{12abx^2}{4acx} = \frac{3bx}{c}$, by dividing by $4ax$.

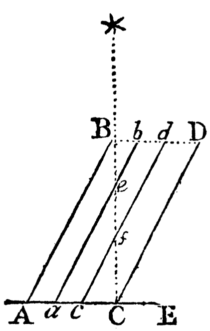
And $\frac{ab^2 + b^2x}{ax + x^2} = \frac{b^2}{x}$, by dividing by $a + x$.

ABBREVIATION, of fractions, in Arithmetic and Algebra, the reducing them to lower terms.

ABERRATION, in *Aftronomy*, an apparent motion of the celeftial bodies, occafioned by the progreffive motion of light, and the earth's annual motion in her orbit.

This effect may be explained a dnfamiliarized by thn motion

motion of a line parallel to itſelf, much after the manner that the compoſition and reſolution of forces are explained. If light have a progreſſive motion, let the proportion of its velocity to that of the earth in her orbit, be as the line B C to the line A C; then, by the compoſition of theſe two motions, the particle of light will ſeem to deſcribe the line B A or D C, inſtead of its real courſe B C; and will appear in the direction A B or C D, inſtead of its true direction C B. So that if A B repreſent a tube, carried with a parallel motion by an obſerver along the line A C, in the time that a particle of light would move over the ſpace B C, the different places of the tube being A B, *ab*, *cd*, C D; and when the eye, or end of the tube, is at A, let a particle of light enter the other end at B; then when the tube is at *a b*, the particle of light will be at *e*, exactly in the axis of the tube; and when the tube is at *c d*, the particle of light will arrive at *f*, ſtill in the axis of the tube; and laſtly, when the tube arrives at C D, the particle of light will arrive at the eye or point C, and conſequently will appear to come in the direction D C of the tube, inſtead of the true direction B C. And ſo on, one particle ſucceeding another, and forming a continued ſtream or ray of light in the apparent direction D C. So that the apparent angle made by the ray of light with the line A E, is the angle D C E, inſtead of the true angle B C E; and the difference, B C D or A B C, is the quantity of the aberration.



M. de Maupertuis, in his Elements of Geography, gives alſo a familiar and ingenious idea of the aberration, in this manner: "It is thus," ſays he, "concerning the direction in which a gun muſt be pointed to ſtrike a bird in its flight; inſtead of pointing it ſtraight to the bird, the fowler will point a little before it, in the path of its flight, and that ſo much the more as the flight of the bird is more rapid, with reſpect to the flight of the ſhot." In this way of conſidering the matter, the flight of the bird repreſents the motion of the earth, or the line A C, in our ſcheme above, and the flight of the ſhot repreſents the motion of the ray of light, or the line B C.

Mr. Clairaut too, in the Memoires of the Academy of Sciences for the year 1746, illuſtrates this effect in a familiar way, by ſuppoſing drops of rain to fall rapidly and quickly after each other from a cloud, under which a perſon moves with a very narrow tube; in which caſe it is evident that the tube muſt have a certain inclination, in order that a drop which enters at the top, may fall freely through the axis of the tube, without touching the ſides of it; which inclination muſt be more or leſs according to the velocity of the drops in reſpect to that of the tube: then the angle made by the direction of the tube and of the falling drops, is the aberration ariſing from the combination of thoſe two motions.

This diſcovery, which is one of the brighteſt that have been made in the preſent age, we owe to the accuracy and ingenuity of the late Dr. Bradley, Aſtronomer Royal; to which he was occaſionally led by the reſult of ſome accurate obſervations which he had made with another view, namely, to determine the annual parallax of the fixed ſtars, or that which ariſes from the motion of the earth in its annual orbit about the ſun.

The annual motion of the earth about the ſun had been much doubted, and warmly conteſted. The defenders of that motion, among other proofs of the reality of it, conceived the idea of adducing an inconteſtable one from the annual parallax of the fixed ſtars, if the ſtars ſhould be within ſuch a diſtance, or if inſtruments and obſervations could be made with ſuch accuracy, as to render that parallax ſenſible. And with this view various attempts have been made. Before the obſervations of M. Picard, made in 1672, it was the general opinion, that the ſtars did not change their poſition during the courſe of a year. Tycho Brahe and Ricciolus fancied that they had aſſured themſelves of it from their obſervations; and from thence they concluded that the earth did not move round the ſun, and that there was no annual parallax in the fixed ſtars. M. Picard, in the account of his *Voyage d'Uranibourg*, made in 1672, ſays that the pole ſtar, at different times of the year, has certain variations which he had obſerved for about 10 years, and which amounted to about 40″ a year: from whence ſome who favoured the annual motion of the earth were led to conclude that theſe variations were the effect of the parallax of the earth's orbit. But it was impoſſible to explain it by that parallax; becauſe this motion was in a manner contrary to what ought to follow only from the motion of the earth in her orbit.

In 1674 Dr. Hook publiſhed an account of obſervations which he ſaid he had made in 1669, and by which he had found that the ſtar γ Draconis was 23″ more northerly in July than in October: obſervations which, for the preſent, ſeemed to favour the opinion of the earth's motion, although it be now known that there could not be any truth or accuracy in them.

Flamſteed having obſerved the pole ſtar with his mural quadrant, in 1689 and the following years, found that its declination was 40″ leſs in July than in December; which obſervations, although very juſt, were yet however improper for proving the annual parallax; and he recommended the making of an inſtrument of 15 or 20 feet radius, to be firmly fixed on a ſtrong foundation, for deciding a doubt which was otherwiſe not ſoon likely to be brought to a concluſion.

In this ſtate of uncertainty and doubt, then, Dr. Bradley, in conjunction with Mr. Samuel Molineux, in the year 1725, formed the project of verifying, by a ſeries of new obſervations, thoſe which Dr. Hook had communicated to the public almoſt 50 years before. And as it was his attempt that chiefly gave riſe to this, ſo it was his method in making the obſervations, in ſome meaſure, that they followed; for they made choice of the ſame ſtar, and their inſtrument was conſtructed upon nearly the ſame principles: but had it not greatly exceeded the former in exactneſs, they might ſtill have continued in great uncertainty as to the parallax of the fixed ſtars. And this was chiefly owing to the accuracy of the ingenious Mr. George Graham, to whom the lovers of aſtronomy are alſo indebted for ſeveral other exact and convenient inſtruments.

The fuccefs then of the intended experiment, evidently depending very much on the accuracy of the inftrument, that leading object was firft to be well fecured. Mr. Molineux's apparatus then having been completed, and fitted for obferving, about the end of November 1725, on the third day of December following, the bright ftar in the head of Draco, marked γ by Bayer, was for the firft time obferved, as it paffed near the zenith, and its fituation carefully taken with the inftrument. The like obfervations were made on the fifth, eleventh, and twelfth days of the fame month; and there appearing no material difference in the place of the ftar, a farther repetition of them, at that feafon, feemed needlefs, it being a time of the year in which no fenfible alteration of parallax, in this ftar, could foon be expected. It was therefore curiofity that chiefly urged Dr. Bradley, being then at Kew, where the inftrument was fixed, to prepare for obferving the ftar again on the 17th of the fame month; when, having adjufted the inftrument as ufual, he perceived that it paffed a little more foutherly this day than it had done before. Not fufpecting any other caufe of this appearance, they firft concluded that it was owing to the uncertainty of the obfervations, and that either this, or the foregoing, was not fo exact as they had before fuppofed. For which reafon they propofed to repeat the obfervation again, to determine from what caufe this difference might proceed: and upon doing it, on the 20th of December, the doctor found that the ftar paffed ftill more foutherly than at the preceding obfervation. This fenfible alteration furprifed them the more, as it was the contrary way from what it would have been, had it proceeded from an annual parallax of the ftar. But being now pretty well fatisfied, that it could not be entirely owing to the want of exactnefs in the obfervations, and having no notion of any thing elfe that could caufe fuch an apparent motion as this in the ftar; they began to fufpect that fome change in the materials, or fabric of the inftrument itfelf, might have occafioned it. Under thefe uncertainties they remained for fome time; but being at length fully convinced, by feveral trials, of the great exactnefs of the inftrument; and finding, by the gradual increafe of the ftar's diftance from the pole, that there muft be fome regular caufe that produced it; they took care to examine very nicely, at the time of each obfervation, how much the variation was; till about the beginning of March 1726, the ftar was found to be 20″ more foutherly than at the time of the firft obfervation: it now indeed feemed to have arrived at its utmoft limit fouthward, as in feveral trials, made about this time, no fenfible difference was obferved in its fituation. By the middle of April it appeared to be returning back again towards the north; and about the beginning of June, it paffed at the fame diftance from the zenith, as it had done in December, when it was firft obferved.

From the quick alteration in the declination of the ftar about this time, increafing about one fecond in three days, it was conjectured that it would now proceed northward, as it had before gone fouthward, of its prefent fituation; and it happened accordingly; for the ftar continued to move northward till September following, when it became ftationary again; being then near 20″ more northerly than in June, and upwards of 39″ more northerly than it had been in March. From September the ftar again returned towards the fouth, till, in December, it arrived at the fame fituation in which it had been obferved twelve months before, allowing for the difference of declination on account of the preceffion of the equinox.

This was a fufficient proof that the inftrument had not been the caufe of this apparent motion of the ftar; and yet it feemed difficult to devife one that fhould be adequate to fuch an unufual effect. A nutation of the earth's axis was one of the firft things that offered itfelf on this occafion; but it was foon found to be infufficient; for though it might have accounted for the change of declination in γ Draconis, yet it would not at the fame time accord with the phenomena obferved in the other ftars, particularly in a fmall one almoft oppofite in right afcenfion to γ Draconis, and at about the fame diftance from the north pole of the equator: for though this ftar feemed to move the fame way, as a nutation of the earth's axis would have made it; yet changing its declination but about half as much as γ Draconis in the fame time, as appeared on comparing the obfervations of both made on the fame days, at different feafons of the year, this plainly proved that the apparent motion of the ftar was not occafioned by a real nutation; fince, had that been the cafe, the alteration in both ftars would have been nearly equal.

The great regularity of the obfervations left no room to doubt, but that there was fome uniform caufe by which this unexpected motion was produced, and which did not depend on the uncertainty or variety of the feafons of the year. Upon comparing the obfervations with each other, it was difcovered that, in both the ftars above mentioned, the apparent difference of declination from the *maxima*, was always nearly proportional to the verfed fine of the fun's diftance from the equinoctial points. This was an inducement to think that the caufe, whatever it was, had fome relation to the fun's fituation with refpect to thofe points. But not being able to frame any hypothefis, fufficient to account for all the phenomena, and being very defirous to fearch a little farther into this matter, Dr. Bradley began to think of erecting an inftrument for himfelf at Wanftead; that, having it always at hand, he might with the more eafe and certainty enquire into the laws of this new motion. The confideration likewife of being able, by another inftrument, to confirm the truth of the obfervations hitherto made with that of Mr. Molineux, was no fmall inducement to the undertaking; but the chief of all was, the opportunity he fhould thereby have of trying in what manner other ftars fhould be affected by the fame caufe, whatever it might be. For Mr. Molineux's inftrument being originally defigned for obferving γ Draconis, to try whether it had any fenfible parallax, it was fo contrived, as to be capable of but little alteration in its direction; not above feven or eight minutes of a degree: and there being but few ftars, within half that diftance from the zenith of Kew, bright enough to be well obferved, he could not, with his inftrument, thoroughly examine how this caufe affected ftars that were differently fituated, with refpect to the equinoctial and folfticial points of the ecliptic.

Thefe confiderations determined him; and by the contrivance and direction of the fame ingenious perfon, Mr.

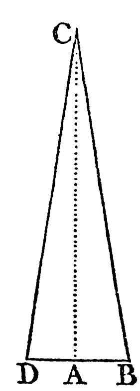
Mr. Graham, his inſtrument was fixed up the 19th of Auguſt 1727. As he had no convenient place where he could make uſe of ſo long a teleſcope as Mr. Molineux's, he contented himſelf with one of but little more than half the length, namely of 12 feet and a half, the other being 24 feet and a half long, judging from the experience he had already had, that this radius would be long enough to adjuſt the inſtrument to a ſufficient degree of exactneſs: and he had no reaſon afterwards to change his opinion; for by all his trials he was very well ſatisfied, that when it was carefully rectified, its ſituation might be ſecurely depended on to half a ſecond. As the place where his inſtrument was hung, in ſome meaſure determined its radius; ſo did it alſo the length of the arc or limb, on which the diviſions were made, to adjuſt it: for the arc could not conveniently be extended farther, than to reach to about $6\frac{1}{4}$ degrees on each ſide of his zenith. This however was ſufficient, as it gave him an opportunity of making choice of ſeveral ſtars, very different both in magnitude and ſituation; there being more than two hundred, inſerted in the Britiſh Catalogue, that might be obſerved with it. He needed not indeed to have extended the limb ſo far, but that he was willing to take in *Capella*, the only ſtar of the firſt magnitude that came ſo near his zenith.

His inſtrument being fixed, he immediately began to obſerve ſuch ſtars as he judged moſt proper to give him any light into the cauſe of the motion already mentioned. There was a ſufficient variety of ſmall ones, and not leſs than twelve that he could obſerve through all ſeaſons of the year, as they were bright enough to be ſeen in the day-time, when neareſt the ſun. He had not been long obſerving, before he perceived that the notion they had before entertained, that the ſtars were fartheſt north and ſouth when the ſun was near the equinoxes, was only true of thoſe ſtars which are near the ſolſticial colure. And after continuing his obſervations a few months, he diſcovered what he then apprehended to be a general law obſerved by all the ſtars, namely, that each of them became ſtationary, or was fartheſt north or ſouth, when it paſſed over his zenith at ſix of the clock, either in the evening or morning. He perceived alſo that whatever ſituation the ſtars were in, with reſpect to the cardinal points of the ecliptic, the apparent motion of every one of them tended the ſame way, when they paſſed his inſtrument about the ſame hour of the day or night; for they all moved ſouthward when they paſſed in the day, and northward when in the night; ſo that each of them was fartheſt north, when it came in the evening about ſix of the clock, and fartheſt ſouth when it came about ſix in the morning.

Though he afterwards diſcovered that the maxima, in moſt of theſe ſtars, do not happen exactly when they paſs at thoſe hours; yet, not being able at that time to prove the contrary, and ſuppoſing that they did, he endeavoured to find out what proportion the greateſt alterations of declination, in different ſtars, bore to each other; it being very evident that they did not all change their declination equally. It has been before noticed, that it appeared from Mr. Molineux's obſervations, that γ *Draconis* changed its declination above twice as much as the before-mentioned ſmall ſtar that was nearly oppoſite to it; but examining the matter more nicely, he found that the greateſt change in the declination of theſe ſtars, was as the ſine of the latitude of each ſtar reſpectively. This led him to ſuſpect that there might be the like proportion between the *maxima* of other ſtars; but finding that the obſervations of ſome of them would not perfectly correſpond with ſuch an hypotheſis, and not knowing whether the ſmall difference he met with might not be owing to the uncertainty and error of the obſervations, he deferred the farther examination into the truth of this hypotheſis, till he ſhould be farther furniſhed with a ſeries of obſervations made in all parts of the year; which would enable him not only to determine what errors the obſervations might be liable to, or how far they might ſafely be depended on; but alſo to judge, whether there had been any ſenſible change in the parts of the inſtrument itſelf.

When the year was completed, he began to examine and compare his obſervations; and having pretty well ſatisfied himſelf as to the general laws of the phenomena, he then endeavoured to find out the cauſe of them. He was already convinced that the apparent motion of the ſtars was not owing to a nutation of the earth's axis. The next that occurred to him, was an alteration in the direction of the plumb-line, by which the inſtrument was conſtantly adjuſted; but this, upon trial, proved inſufficient. Then he conſidered what refraction might do; but here alſo he met with no ſatisfaction. At laſt, through an amazing ſagacity, he conjectured that all the phenomena hitherto mentioned, proceeded from the progreſſive motion of light, and the earth's annual motion in her orbit: for he perceived, that if light were propagated in time, the apparent place of a fixed object would not be the ſame when the eye is at reſt, as when it is moving in any other direction but that of the line paſſing through the object and the eye; and that when the eye is moving in different directions, the apparent place of the object would be different.

He conſidered this matter in the following manner. He imagined C A to be a ray of light, falling perpendicularly upon the line B D: then, if the eye be at reſt at A, the object muſt appear in the direction A C, whether light be propagated in time, or in an inſtant. But if the eye be moving from B towards A, and light be propagated in time, with a velocity that is to the velocity of the eye, as A C to A B; then, light moving from C to A, whilſt the eye moves from B to A, that particle of it by which the object will be diſcerned, when the eye in its motion comes to A, is at C when the eye is at B. Joining the points B, C, he ſuppoſed the line B C to be a tube, inclined to the line B D in the angle D B C, and of ſuch a diameter as to admit of but one particle of light: then it was eaſy to conceive, that the particle of light at C, by which the object muſt be ſeen when the eye arrives at A, would paſs through the tube B C, ſo inclined to the line B D, and accompanying the eye in its motion from B to A; and that it would not come to



to the eye, placed behind ſuch a tube, if it had any other inclination to the line B D. If, inſtead of ſuppoſing B C ſo ſmall a tube, we conceive it to be the axis of a larger; then, for the ſame reaſon, the particle of light at C cannot paſs through that axis, unleſs it be inclined to B D in the ſame angle D B C.

In the like manner, if the eye move the contrary way, from D towards A, with the ſame velocity; then the tube muſt be inclined in the angle BDC. Although therefore the true or real place of an object, be perpendicular to the line in which the eye is moving, yet the viſible place will not be ſo; ſince that muſt doubtleſs be in the direction of the tube. But the difference between the true and apparent place, will be, *cæteris paribus*, greater or leſs, according to the different proportions between the velocity of light and that of the eye: ſo that if we could ſuppoſe light to be propagated in an inſtant, then there would be no difference between the real and viſible place of an object, although the eye were in motion; for in that caſe, A C being infinite with reſpect to A B, the angle A C B, which is the difference between the true and viſible place, vaniſhes. But if light be propagated in time, which was then allowed by moſt philoſophers, then it is evident from the foregoing conſiderations, that there will always be a difference between the true and viſible place of an object, except when the eye is moving either directly towards, or from the object. And in all caſes, the ſine of the difference between the true and viſible place of the object, will be to the ſine of the viſible inclination of the object to the line in which the eye is moving, as the velocity of the eye, is to the velocity of light.

If light moved only 1000 times faſter than the eye, and an object, ſuppoſed to be at an infinite diſtance, were really placed perpendicularly over the plane in which the eye is moving; it follows, from what has been ſaid, that the apparent place of ſuch object will always be inclined to that plane, in an angle of 89° 56′½; ſo that it will conſtantly appear 3′½ from its true place, and will ſeem ſo much leſs inclined to the plane, that way towards which the eye tends. That is, if A C be to A B or A D, as 1000 to 1, the angle A B C will be 89° 56′½, and the angle A C B 3′½, and B C D or 2 A C B will be 7′, if the direction of the motion of the eye be contrary at one time to what it is at another.

If the earth revolve about the ſun annually, and the velocity of light were to the velocity of the earth's motion in its orbit, as 1000 is to 1; then it is eaſy to conceive, that a ſtar really placed in the pole of the ecliptic, would to an eye carried along with the earth, ſeem to change its place continually; and, neglecting the ſmall difference on account of the earth's diurnal revolution on its axis, it would ſeem to deſcribe a circle about that pole, every where diſtant from it by 3′½. So that its longitude would be varied through all the points of the ecliptic every year, but its latitude would always remain the ſame. Its right aſcenſion would alſo change, and its declination, according to the different ſituation of the ſun in reſpect of the equinoctial points; and its apparent diſtance from the north pole of the equator, would be 7′ leſs at the autumnal, than at the vernal equinox.

The greateſt alteration of the place of a ſtar, in the pole of the ecliptic, or, which in effect amounts to the ſame, the proportion between the velocity of light and the earth's motion in its orbit, being known, it will not be difficult to find what would be the difference, on this account, between the true and apparent place of any other ſtar at any time; and, on the contrary, the difference between the true and apparent place being given, the proportion between the velocity of light, and the earth's motion in her orbit, may be found.

After the hiſtory of this curious diſcovery, related by the author nearly in the terms above, he gives the reſults of a multitude of accurate obſervations, made on a great number of ſtars, at all ſeaſons of the year. From all which obſervations, and the theory as related above, he found that every ſtar, in conſequence of the earth's motion in her orbit and the progreſſive motion of light, appears to deſcribe a ſmall ellipſe in the heavens, the tranſverſe axis of which is equal to the ſame quantity for every ſtar, namely 40″ nearly; and that the conjugate axis of the ellipſe, for different ſtars, varies in this proportion, namely, as the right ſine of the ſtar's latitude; that is, radius is to the ſine of the ſtar's latitude, as the tranſverſe axis to the conjugate axis: and conſequently a ſtar in the pole of the ecliptic, its latitude being there 90°, whoſe ſine is equal to the radius, will appear to deſcribe a ſmall circle about that pole as a centre, whoſe radius is equal to 20″. He alſo gives the following law of the variation of the ſtar's declination: if A denote the angle of poſition, or the angle at the ſtar made by two great circles drawn from it through the poles of the ecliptic and equator, and B another angle, whoſe tangent is to the tangent of A, as radius is to the ſine of the ſtar's latitude; then B will be equal to the difference of longitude between the ſun and the ſtar, when the true and apparent declination of the ſtar are the ſame. And if the ſun's longitude in the ecliptic be reckoned from that point in which it is when this happens; then the difference between the true and apparent declination of the ſtar, will be always as the ſine of the ſun's longitude from that point. It will alſo be found that the greateſt difference of declination that can be between the true and apparent place of the ſtar, will be to 20″, the ſemitranſverſe axis of the ellipſe, as the ſine of A to the ſine of B.

The author then ſhews, by the compariſon of a number of obſervations made on different ſtars, that they exactly agree with the theory deduced from the progreſſive motion of light, and that conſequently it is highly probable that ſuch motion is the cauſe of thoſe variations in the ſituation of the ſtars. From which he infers, that the parallax of the fixed ſtars is much ſmaller, than hath been hitherto ſuppoſed by thoſe, who have pretended to deduce it from their obſervations. He thinks he may venture to ſay, that in the ſtars he had obſerved, the parallax does not amount to 2″; nay, that if it had amounted to 1″, he ſhould certainly have perceived it, in the great number of obſervations that he made, eſpecially of γ Draconis; which agreeing with the hypotheſis, without allowing any thing for parallax, nearly as well when the ſun was in conjunction with, as in oppoſition to, this ſtar, it ſeems very probable

bable that the parallax of it is not fo much as one fingle fecond; and confequently that it is above 400000 times farther from us than the fun.

From the greateft variation in the place of the ftars, namely 40″, Dr. Bradley deduces the ratio of the velocity of light in comparifon with that of the earth in her orbit. In the preceding figure, A C is to A B, as the velocity of light to that of the earth in her orbit, the angle A C B being equal to 20″; fo that the ratio of thofe velocities is that of radius to the tangent of 20″, or of radius to 20″, fince the tangent has no fenfible difference from fo fmall an arc: but the radius of a circle is equal to the arc of $57^{\circ}\frac{3}{10}$ nearly, or equal to 206260″; therefore the velocity of light is to the velocity of the earth, as 206260 to 20, or as 10313 to 1.

And hence alfo the time in which light paffes over the fpace from the fun to the earth, is eafily deduced; for this time will be to one year, as A B or 20″ to 360° or the whole circle; that is, 360° : 20″ : : $365\frac{1}{4}$ days : $8^{m}\ 7^{s}$, namely, light will pafs from the fun to the earth in the time of 8 minutes, 7 feconds; and this will be the fame, whatever the diftance of the fun is.

Dr. Bradley having annexed to his theory the rules or formulæ for computing the aberration of the fixed ftars in declination and right afcenfion; thefe rules have been varioufly demonftrated, and reduced to other practical forms, by Mr. Clairaut in the Memoirs of the Academy of Sciences for 1737; by Mr. Simpfon in his Effays in 1740; by M. Fontaine des Crutes in 1744; and feveral other perfons. The refults of thefe rules are as follow: Every ftar appears to defcribe in the courfe of a year, by means of the aberration, a fmall ellipfe, whofe greater axis is 40″, and the lefs axis, perpendicular to the ecliptic, is equal to 40″ multiplied by the fine of the ftar's latitude, the radius being 1. The eaftern extremity of the longer axis, marks the apparent place of the ftar, the day of the oppofition; and the extremity of the lefs axe, which is fartheft from the ecliptic, marks its fituation three months after.

The greateft aberration in longitude, is equal to 20″ divided by the cofine of its latitude. And the aberration for any time, is equal to 20″ multiplied by the cofine of the elongation of the ftar found for the fame time, and divided by the cofine of its latitude. This aberration is fubtractive in the firft and laft quadrant of the argument, or of the difference between the longitudes of the fun and ftar; and additive in the fecond and third quadrants. The greateft aberration in latitude, is equal to 20″ multiplied by the fine of the ftar's latitude. And the aberration in latitude for any time, is equal to 20″ multiplied by the fine of the ftar's latitude, and multiplied alfo by the fine of the elongation. The aberration is fubtractive before the oppofition, and additive after it.

The greateft aberration in declination, is equal to 20″ multiplied by the fine of the angle of pofition A, and divided by the fine of B the difference of longitude between the fun and ftar when the aberration in declination is nothing. And the aberration in declination at any other time, will be equal to the greateft aberration multiplied by the fine of the difference between the fun's place at the given time and his place when the aberration is nothing. Alfo the fine of the latitude of the ftar is to radius, as the tangent of A the angle of pofition at the ftar, is to the tangent of B, the difference of longitude between the fun and ftar when the aberration in declination is nothing. The greateft aberration in right-afcenfion, is equal to 20″ multiplied by the cofine of A the angle of pofition, and divided by the fine of C the difference in longitude between the fun and ftar when the aberration in right afcenfion is nothing. And the aberration in right-afcenfion at any other time, is equal to the greateft aberration multiplied by the fine of the difference between the fun's place at the given time, and his place when the aberration is nothing. Alfo the fine of the latitude of the ftar is to radius, as the cotangent of A the angle of pofition, to the tangent of C.

ABERRATION *of the Planets*, is equal to the geocentric motion of the planet, the fpace it appears to move as feen from the earth, during the time that light employs in paffing from the planet to the earth. Thus, in the fun, the aberration in longitude is conftantly 20″, that being the fpace moved by the fun, or, which is the fame thing, by the earth, in the time of $8^{m}\ 7^{s}$, which is the time in which light paffes from the fun to the earth, as we have feen in the foregoing article. In like manner, knowing the diftance of any planet from the earth, by proportion it will be, as the diftance of the fun is to the diftance of the planet, fo is $8^{m}\ 7^{s}$ to the time of light paffing from the planet to the earth: then computing the planet's geocentric motion in this time, that will be the aberration of the planet, whether it be in longitude, latitude, right-afcenfion, or declination.

It is evident that the aberration will be greateft in the longitude, and very fmall in latitude, becaufe the planets deviate very little from the plane of the ecliptic, or path of the earth; fo that the aberration in the latitudes of the planets, is commonly neglected, as infenfible; the greateft in Mercury being only $4''\frac{1}{3}$, and much lefs in the other planets. As to the aberrations in declination and right-afcenfion, they muft depend on the fituation of the planet in the zodiac. The aberration in longitude, being equal to the geocentric motion, will be more or lefs according as that motion is; it will therefore be leaft, or nothing at all, when the planet is ftationary; and greateft in the fuperior planets Mars, Jupiter, Saturn, &c, when they are in oppofition to the fun; but in the inferior planets Venus and Mercury, the aberration is greateft at the time of their fuperior conjunction. Thefe maxima of aberration for the feveral planets, when their diftance from the fun is leaft, are as below: viz. for

Saturn - - -	27″·0
Jupiter - - -	29 ·8
Mars - - -	37 ·8
Venus - - -	43 ·2
Mercury - -	59 ·0
The Moon - -	$\frac{2}{3}$

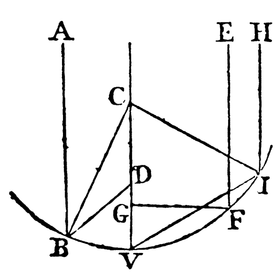
And between thefe numbers and nothing the aberrations of the planets, in longitude, vary according to their fituations. But that of the fun varies not, being conftantly 20″, as has been before obferved. And this may alter his declination by a quantity, which varies from 0 to near 8″; being greateft or 8″ about the equinoxes, and vanifhing in the folftices.

The

The methods of computing thefe, and the formulas for all cafes, are given by M. Clairaut in the Memoirs of the Academy of Sciences for the year 1746, and by M. Euler in the Berlin Memoirs, vol. 2, for 1746.

Optic ABERRATION, the deviation or difperfion of the rays of light, when reflected by a fpeculum, or refracted by a lens, by which they are prevented from meeting or uniting in the fame point, called the geometrical focus, but are fpread over a fmall fpace, and produce a confufion of images. Aberration is either lateral or longitudinal: the lateral aberration is meafured by a perpendicular to the axis of the fpeculum or lens, drawn from the focus to meet the refracted or reflected ray: the longitudinal aberration is the diftance, on the axis, between the focus and the point where the ray meets the axis. The aberrations are very amply treated in Smith's Complete Syftem of Optics, in 2 volumes 4to.

There are two fpecies of aberration, diftinguifhed according to their different caufes: the one arifes from the figure of the fpeculum or lens, producing a geometrical difperfion of the rays, when thefe are perfectly equal in all refpects; the other arifes from the unequal refrangibility of the rays of light themfelves; a difcovery that was made by Sir Ifaac Newton, and for this reafon it is often called the Newtonian aberration. As to the former fpecies of aberration, or that arifing from the figure, it is well known that if rays iffue from a point at a given diftance; then they will be reflected into the other focus of an ellipfe having the given luminous point for one focus, or directly from the other focus of an hyperbola; and will be varioufly difperfed by all other figures. But if the luminous point be infinitely diftant, or, which is the fame, the incident rays be parallel, then they will be reflected by a parabola into its focus, and varioufly difperfed by all other figures. But thofe figures are very difficult to make, and therefore curved fpecula are commonly made fpherical, the figure of which is generated by the revolution of a circular arc, which produces an aberration of all rays, whether they are parallel or not, and therefore it has no accurate geometrical focus which is common to all the rays. Let B V F reprefent a concave fpherical fpeculum, whofe centre is C; and let A B, E F be incident rays parallel to the axis CV. Becaufe the angle of incidence is equal to the angle of reflection in



all cafes, therefore if the radii C B, C F be drawn to the points of incidence, and thence B D making the angle C B D equal to the angle C B A, and F G making the angle C F G equal to the angle C F E; then B D, FG will be the reflected rays, and D, G, the points where they meet the axis. Hence it appears that the point of coincidence with the axis is equally diftant from the point of incidence and the centre: for becaufe the angle C B D is equal to the angle C B A, which is equal to the alternate angle B C D, therefore their oppofite fides C D, D B are equal: and in like manner, in any other, G F is equal to G C. And hence it is evident that when B is indefinitely near the vertex V, then D is in the middle of the radius C V; and the nearer the incident ray is to the axis C V, the nearer will the reflected ray come to the middle point D; and the contrary. So that the aberration D G of any ray E F G, is always more and more, as the incident ray is farther from the axis, or the incident point F from the vertex V; till when the diftance V I is 60 degrees, then the reflected ray falls in the vertex V, making the aberration equal to the whole length D V. And this fhews the reafon why fpecula are made of a very fmall fegment of a fphere, namely, that all their reflected rays may arrive very near the middle point or focus D, to produce an image the moft diftinct, by the leaft aberration of the rays. And in like manner for rays refracted through lenfes.

In fpherical lenfes, Mr. Huygens has demonftrated that the aberration from the figure, in different lenfes, is as follows.

1. In all plano-convex lenfes, having their plane furface expofed to parallel rays, the longitudinal aberration of the extreme ray, or that remoteft from the axis, is equal to $\frac{9}{2}$ of the thicknefs of the lens.

2. In all plano-convex lenfes, having their convex furface expofed to parallel rays, the longitudinal aberration of the extreme ray, is equal to $\frac{7}{6}$ of the thicknefs of the lens. So that in this pofition of the fame plano-convex lens, the aberration is but about one-fourth of that in the former; being to it only as 7 to 27.

3. In all double convex lenfes of equal fpheres, the aberration of the extreme ray, is equal to $\frac{5}{3}$ of the thicknefs of the lens.

4. In a double convex lens, the radii of whofe fpheres are as 1 to 6, if the more convex furface be expofed to parallel rays, the aberration from the figure is lefs than in any other fpherical lens; being no more than $\frac{15}{14}$ of its thicknefs.

But the foregoing fpecies of aberration, arifing from the figure, is very fmall, and eafily remedied, in comparifon with the other, arifing from the unequal refrangibility of the rays of light; infomuch that Sir Ifaac Newton fhews in his Optics, pa. 84 of the 8vo. edition, that if the object-glafs of a telefcope be plano-convex, the plane fide being turned towards the object, and the diameter of the fphere, to which the convex fide is ground, be 100 feet, the diameter of the aperture being 4 inches, and the ratio of the fine of incidence out of glafs into air, be to that of refraction, as 20 to 31; then the diameter of the circle of aberrations will in this cafe be only $\frac{961}{72000000}$ parts of an inch: while the diameter of the little circle, through which the fame rays are fcattered by unequal refrangibility, will be about the 55th part of the aperture of the object-glafs, which here is 4 inches. And therefore the error arifing from the fpherical figure of the glafs, is to the error arifing from the different refrangibility of the rays, as $\frac{961}{72000000}$ to $\frac{4}{55}$, that is as 1 to 5449.

So

So that it may feem ftrange that objects appear through telefcopes fo diftinct as they do, confidering that the error arifing from the different refrangibility, is almoft incomparably larger than that of the figure. Newton however folves the difficulty by obferving that the rays, under their various aberrations, are not fcattered uniformly over all the circular fpace, but collected infinitely more denfe in the centre than in any other part of the circle; and that, in the way from the centre to the circumference, they grow more and more rare, fo as at the circumference to become infinitely rare; and, by reafon of their rarity, they are not ftrong enough to be vifible, unlefs in the centre, and very near it.

In confequence of the difcovery of the unequal refrangibility of light, and the apprehenfion that equal refractions muft produce equal divergencies in every fort of medium, it was fuppofed that all fpherical object-glaffes of telefcopes would be equally affected by the different refrangibility of light, in proportion to their aperture, of whatever materials they might be conftructed: and therefore that the only improvement that could be made in refracting telefcopes, was that of increafing their length. So that Sir Ifaac Newton, and other perfons after him, defpairing of fuccefs in the ufe and fabric of lenfes, directed their chief attention to the conftruction of reflecting telefcopes.

However, about the year 1747, M. Euler applied himfelf to the fubject of refraction; and purfued a hint fuggefted by Newton, for the defign of making object-glaffes with two lenfes of glafs inclofing water between them; hoping that, by conftructing them of different materials, the refractions would balance one another, and fo the ufual aberration be prevented. Mr. John Dollond, an ingenious optician in London, minutely examined this fcheme, and found that Mr. Euler's principles were not fatisfactory. M. Clairaut likewife, whofe attention had been excited to the fame fubject, concurred in opinion that Euler's fpeculations were more ingenious than ufeful. This controverfy, which feemed to be of great importance in the fcience of optics, engaged alfo the attention of M. Klingenftierna of Sweden, who was led to make a careful examination of the 8th experiment in the fecond part of Newton's Optics, with the conclufions there drawn from it. The confequence was, that he found that the rays of light, in the circumftances there mentioned, did not lofe their colour, as Sir Ifaac had imagined. This hint of the Swedifh philofopher led Mr. Dollond to re-examine the fame experiment: and after feveral trials it appeared, that different fubftances caufed the light to diverge very differently, in proportion to their general refractive powers. In the year 1757 therefore he procured wedges of different kinds of glafs, and applied them together fo that the refractions might be made in contrary directions, that he might difcover whether the refraction and divergency of colour would vanifh together. The refult of his firft trials encouraged him to perfevere; for he difcovered a difference far beyond his hopes in the qualities of different kinds of glafs, with refpect to their divergency of colours. The Venice glafs and Englifh crown glafs were found to be nearly allied in this refpect: the common Englifh plate glafs made the rays diverge more; and the Englifh flint glafs moft of all. But without enquiring into the caufe of this difference, he proceeded to adapt wedges of crown glafs, and of white flint glafs, ground to different angles, to each other, fo as to refract in different directions; till the refracted light was entirely free from colours. Having meafured the refractions of each wedge, he found that the refraction of the white glafs was to that of the crown glafs, nearly as 2 to 3: and he hence concluded in general, that any two wedges made in this proportion, and applied together fo as to refract in contrary directions, would refract the light without any aberration of the rays.

Mr. Dollond's next object was to make fimilar trials with fpherical glaffes of different materials, with the view of applying his difcovery to the improvement of telefcopes: and here he perceived that, to obtain a refraction of light in contrary directions, the one glafs muft be concave, and the other convex; and the latter, which was to refract the moft, that the rays might converge to a real focus, he made of crown glafs, the other of white flint glafs. And as the refractions of fpherical glaffes are inverfely as their focal diftances, it was neceffary that the focal diftances of the two glaffes fhould be inverfely as the ratios of the refractions of the wedges; becaufe that, being thus proportioned, every ray of light that paffes through this compound glafs, at any diftance from its axis, will conftantly be refracted, by the difference between two contrary refractions, in the proportion required; and therefore the different refrangibility of the light will be entirely removed.

But in the applications of this ingenious difcovery to practice, Mr. Dollond met with many and great difficulties. At length, however, after many repeated trials, by a refolute perfeverance, he fucceeded fo far as to conftruct refracting telefcopes much fuperior to any that had hitherto been made; reprefenting objects with great diftinctnefs, and in their true colours.

Mr. Clairaut, who had interefted himfelf from the beginning in this difcovery, now endeavoured to afcertain the principles of Mr. Dollond's theory, and to lay down rules to facilitate the conftruction of thefe new telefcopes. With this view he made feveral experiments, to determine the refractive power of different kinds of glafs, and the proportions in which they feparated the rays of light: and from thefe experiments he deduced feveral theorems of general ufe. M. D'Alembert made likewife a great variety of calculations to the fame purpofe; and he fhewed how to correct the errors to which thefe telefcopes are fubject, fometimes by placing the object-glaffes at a fmall diftance from each other, and fometimes by ufing eye-glaffes of different refractive powers. But though foreigners were hereby fupplied with the moft accurate calculations, they were very defective in practice. And the Englifh telefcopes, made, as they imagined, without any precife rule, were greatly fuperior to the beft of their conftruction.

M. Euler, whofe fpeculations had firft given occafion to this important and ufeful enquiry, was very reluctant in admitting Mr. Dollond's improvements, becaufe they militated againft a pre-conceived theory of his own. At laft however, after feveral altercations, being convinced of their reality and importance by M. Clairaut,

aut, he aſſented; and he ſoon after received farther ſatisfaction from the experiments of M. Zeiher, of Peterſburgh.

M. Zeiher ſhewed by experiments that it is the lead, in the compoſition of glaſs, which gives it this remarkable property, namely, that while the refraction of the mean rays is nearly the ſame, that of the extreme rays conſiderably differs. And, by increaſing the lead, he produced a kind of glaſs, which occaſioned a much greater ſeparation of the extreme rays than that of the flint glaſs uſed by Mr. Dollond, and at the ſame time conſiderably increaſed the mean refraction. M. Zeiher, in the courſe of his experiments, made glaſs of minium and lead, with a mixture alſo of alkaline ſalts; and he found that this mixture greatly diminiſhed the mean refraction, and yet made hardly any change in the diſperſion: and he at length obtained a kind of glaſs greatly ſuperior to the flint glaſs of Mr. Dollond for the conſtruction of teleſcopes; as it occaſioned three times as great a diſperſion of the rays as the common glaſs, whilſt the mean refraction was only as 1·61 to 1.

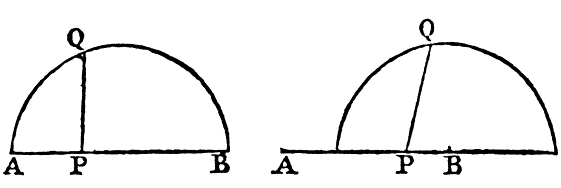
Other improvements were alſo made on the new or achromatic teleſcopes by the inventor Mr. John Dollond, and by his ſon Peter Dollond; which may be ſeen under the proper words. For various diſſertations on the ſubject of the aberration of light, colours, and the figure of the glaſs, ſee Philoſ. Tranſ. vols. 35, 48, 50, 51, 52, 55, 60; Memoirs of the Academy of Sciences of Paris, for the years 1737, 1746, 1752, 1755, 1756, 1757, 1762, 1764, 1765, 1767, 1770; the Berlin Ac. 1746, 1762, 1766; Swed. Mem. vol. 16; Com. Nov. Petripol. 1762; M. Euler's Dioptrics; M. d'Alembert's Opuſcules Math.; M. de Rochon Opuſcules; &c, &c.

ABRIDGING, *in Algebra*, is the reducing a compound equation, or quantity, to a more ſimple form of expreſſion. This is done either to ſave room, or the trouble of writing a number of ſymbols; or to ſimplify the expreſſion, either to eaſe the memory, or to render the formula more eaſy and general.

So the equation $x^3 - ax^2 + abx - abc = 0$, by putting $p = a$, $q = ab$, and $r = abc$, becomes $x^3 - px^2 + qx - r = 0$.

And the equation $x^2 + (a+b)x - \frac{ab}{c} = 0$, by putting $p = a + b$, and $q = \frac{ab}{c}$, becomes $x^2 + px - q = 0$.

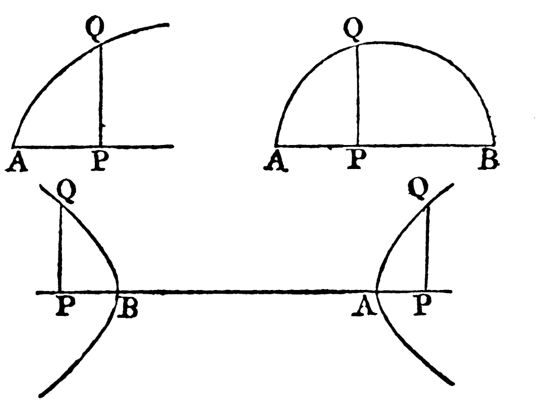
ABSCISS, Abscisse, or Abscissa, is a part or ſegment cut off a line, terminated at ſome certain point, by an ordinate to a curve; as A P or B P.



The abſciſs may either commence at the vertex of the curve, or at any other fixed point. And it may be taken either upon the axis or diameter of the curve, or upon any other line drawn in a given poſition.

Hence there are an infinite number of variable abſciſſes, terminated at the ſame fixed point at one end, the other end of them being at any point of the given line or diameter.

In the common parabola, each ordinate P Q has but



one abſciſs A P; in the ellipſe or circle, the ordinate has two abſciſſes A P, B P lying on the oppoſite ſides of it; and in the hyperbola the ordinate P Q has alſo two abſciſſes, but they lie both on the ſame ſide of it. That is, in general, a line of the ſecond kind, or a curve of the firſt kind, may have two abſciſſes to each ordinate. But a line of the third order may have three abſciſſes to each ordinate; a line of the fourth order may have four; and ſo on.

The uſe of the abſciſſes is, in conjunction with the ordinates, to expreſs the nature of the curves, either by ſome proportion or equation including the abſciſs and its ordinate, with ſome other fixed invariable line or lines. Every different curve has its own peculiar equation or property by which it is expreſſed, and different from all others: and that equation or expreſſion is the ſame for every ordinate and its abſciſſes, whatever point of the curve be taken. So, in the circle, the ſquare of any ordinate is equal to the rectangle of its two abſciſſes, or $AP \cdot PB = PQ^2$; in the parabola, the ſquare of the ordinate is equal to the rectangle of the abſciſs and a certain given line called the parameter; in the ellipſe and hyperbola, the ſquare of the ordinate is always in a certain conſtant proportion to the rectangle of the two abſciſſes, namely, as the ſquare of the conjugate to the ſquare of the tranſverſe, or as the parameter is to the tranſverſe axis; and ſo other properties in other curves.

When the natures or properties of curves are expreſſed by algebraic equations, any general abſciſs, as A P, is commonly denoted by the letter x, and the ordinate P Q by the letter y; the other or conſtant lines being repreſented by other letters. Then the equations expreſſing the nature of theſe curves are as follow; namely, for the

circle - - $dx - x^2 = y^2$, where d is the diameter A B;
parabola - $px = y^2$, where p is the parameter;
ellipſe - $t^2 : c^2 :: tx - x^2 : y^2$, } where t is the tranſverſe,
hyperbola $t^2 : c^2 :: tx + x^2 : y^2$, } & c the conjugate axis.

ABSIS, ABSIDES. See Apsis, Apsides.

ABSOLUTE