Cursory remarks on some parts of a work, entitled Studies of nature ; originally written by M. de Saint Pierre, and translated into English by ... Henry Hunter / [William Cole].

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CURSORY REMARKS

On some parts of a Work, entitled

STUDIES OF NATURE.

[PRICE TWO SHILLINGS.]

CURSORY REMARKS

On some parts of a Work, entitled

STUDIES OF NATURE;

ORIGINALLY WRITTEN BY

M. De SAINT PIERRE,

AND TRANSLATED INTO ENGLISH

By the Rev. HENRY HUNTER, D. D.

BY WILLIAM COLE.

Prove all things: hold fast that which is good.

ST. PAUL.

LONDON:

Printed by James Cundee, Ivy Lane;

FOR WILLIAMS AND SMITH, STATIONERS' COURT.

1807.

PREFACE.

WHEN the work entitled "STUDIES OF NATURE," *by* M. D. Saint Pierre, *first made its appearance in this country, the writer of the following pages received from a friend, a translation of that part which relates to the figure of the earth. From this communication he immediately perceived the ground of the author's mistake, and pointed it out to that friend, who also clearly saw the error, and the fallacy of the arguments advanced by* M. De Saint Pierre, *in support of it.*

When the publication of the translation was announced, it was expected that some remarks upon this subject would have been made by the translator, and annexed to the work, in additional notes under his own signature. This expectation, however, was dis-

appointed, and we find nothing more, upon this subject, than a disavowal of the sentiments.

A considerable time has elapsed since the translation first made its appearance, and it has now attained a degree of popularity that seldom falls to the lot of a philosophical publication. It is, therefore, somewhat extraordinary that no strictures, upon the objectional parts of this work, have as yet appeared; in expectation of which, the author of the following remarks has long waited, hoping that some one, better qualified for the work, would have undertaken it.

As no real mathematician can be misled by the speculations of this author, it is probable that they consider them as beneath their notice. It appears, however, that they may be injurious to some, who have made but little progress in scientific knowledge; it is therefore presumed, that a detection of some of the philosophical errors contained in that work, will

not be unacceptable to the public, especially to the younger part of it.

Knowing that errors in philosophy lead to errors in morality and religion, the writer of the remarks has undertaken to point out, and, in some measure, to rectify, some of the mistakes that are to be found in the Studies of Nature, *particularly those that are opposed to the Newtonian system. In the execution of this work he has endeavoured to adapt his explanations to the understanding of those who want either the leisure, or the inclination to enter into more abstruse mathematical speculations. For persons of this description these Remarks are intended: experienced mathematicians stand in no need of them.*

Of the necessity of some remarks of this kind, the writer was more fully convinced by a conversation that took place where he happened to be present. The subject of M. De Saint Pierre's *work being mentioned, as well as some other extravagant modern hypotheses, it was observed by a person in company that*

they might possibly be true. "Who can tell,' said he, 'but Sir Isaac Newton's theory may experience the same fate with those of his predecessors. The systems of Ptolemy, *and* Tycho Brahe, *had their day, but they fell before the superiority of Newton; and it is probable that his system, in its turn, may give way to some future genius." Unreasonable as this supposition appeared, it produced a conviction, that* M. De Saint Pierre *was not the only person who supposed that the Newtonian philosophy was founded upon the reputation of its author merely; and that trifling mistakes, undetected, undoubtedly lead to errors of a greater magnitude. Under the influence of this conviction the following remarks originated, and are now submitted to the candour of the public.*

CURSORY REMARKS,

&c. &c.

NOTWITHSTANDING the evidence upon which the philosophy of Sir Isaac Newton is founded, a year seldom passes, without producing some attempt to invalidate that evidence, and to bring his system into disrepute. Many of the hypotheses advanced in opposition to the Newtonian theory, are too futile to merit any regard, and are generally the instruments of their own refutation. Others carry with them a degree of plausibility, which, although they are beneath the notice of a mathematician, or a real philosopher, may tend to mislead, or perplex a student, or young practitioner in the sciences.

Some attempts of this kind are made professedly with a view to establish some particular doctrine, either in philosophy, or religion; or

even the truth of Christianity itself. And we doubt not, that, in many cases of this kind, the authors are sincere in their professions, and actually intend to promote the cause they have undertaken to support.

It is, however, to be lamented, that truth oftentimes suffers more by a weak and unfounded argument in its favour, than by an attack avowedly hostile. And thus it is that religion, as well as philosophy, is frequently "wounded inthe house of its friends." Specious arguments have a tendency, not only to deceive the superficial and incautious reader, but even to delude the authors themselves, who, not unfrequently, become the dupes of their own error.

We were led to these reflections by a perusal of the *Studies of Nature*, by *M. De Saint Pierre;* where amongst a variety of judicious and beautiful observations, we meet with an equal variety of crude ideas, and vague hypotheses. To examine the foundation, and to point out the fallacy of so meof these, is the design of the following pages; and, in this examination, we shall direct our attention, more particularly to those parts which

are opposed to the Newtonian system of philoso phy.

Notwithstanding the many errors which may be detected in this work, we cannot but entertain a high respect for its author. His avowed design is to vindicate the conduct of divine providence, and to promote the cause of virtue, and religion. His purpose we highly approve, and although in many instances he appears to be deceived, his sincerity cannot be doubted.

The character of the translator also, stands high in our estimation; we revere his abilities, and while we regret his loss, * we wish to pay a proper degree of respect to his memory. He has disavowed many of the sentiments of his author, and we give him full credit for his declaration; we cannot, however, but regret, that he has passed over unnoticed, some of the most palpable errors; while several passages, of less importance, are illustrated, or corrected in notes of his own.

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* Since these pages were written, the literary world has sustained an irretrievable loss by the death of Dr. Hunter.

To enter minutely into the examination of every subject contained in this celebrated work, would very far exceed the limits of our plan; it is sufficient, therefore, to point out, and endeavour to correct, some of the most prominent errors, and to subjoin a few such remarks as naturally arise from them.

The author of the *Studies of Nature*, seems to have mistaken the ground upon which the Newtonian philosophy is erected. This he ascribes to the authority of its supporters, or to the reputation of Newton himself. In his advertisement [Page IX. Vol. I.] he says, "The authority of great names, serves but as strong hold to error. It is thus that on the faith of a Maupertuis, or of a Condamine, Europe has till now believed, that the earth was flattened at the poles." "For my own part, I am perfectly convinced, that Newton himself would, at this day, renounce such an erroneous opinion." And farther on [Page XIV.] he tells us that "Modern Astronomers have, in their turn, suffered themselves to be seduced by the reputation of Newton." These assertions are evidently erroneous. The philosophy of Sir Isaac Newton is founded in fact, and supported by demonstration;

a kind of evidence which cannot be affected by the credit of great names and in opposition to which the authority even of Newton himself would be ineffectual. Had the elements of Euclid been built upon no firmer ground than his own reputation, they would have been buried in oblivion, ages long ago: and in like manner the Newtonian philosophy might have fallen before the first reputable opposer, had it rested upon such a sandy foundation. Imaginary systems will ever be insufficient to explain the phenomena of nature: these, therefore, were rejected by Sir Isaac Newton, and his philosophy established upon the solid bases of experiment and fact. If *M. De Saint Pierre* had pursued the same method, instead of following the *ignis fatuus* of mere hypothesis, he would not only have discovered the real ground of the Newtonian theory, but might have avoided many absurd conclusions, into which his fertile imagination has now led him.

Among the philosophical errors to be met with in this work, the most palpable is that which relates to the figure of the earth; and as this has laid the foundation of many other mistakes into which he has fallen, the consideration of it claims

the precedence in our remarks. In the passage before quoted [Advert. Page IX.] he says that "on the faith of a Maupertuis, and of a Condamine, Europe has till now believed, that the earth was flattened at the poles." He adds, "I demonstrate, after their own operations, in the explication of the plates, at the beginning of the first volume, that it is lengthened out at the poles." Here, with some degree of confidence, he asks, "What answer is it possible to give to the geometrical demonstration which I produce of it?" He farther declares [Page XIV.], "It is evident, that the degree being greater, and the curve longer, toward the north, Newton ought to have concluded that the earth was lengthened out at the poles, but he deduced the directly opposite conclusion, namely, that it was flattened there." And speaking of the supporters of the Newtonian system [Page XIII.], he says, "I have demonstrated their error on the principles of Geometry."

Let us now examine this pretended demonstration; and, to avoid the suspicion of misrepresentation, we will give his illustration of the subject in his own words, or rather those of his translator, at full length. [Page XXXII, et seq.]

" Certain celebrated Astronomers," says he, "have, it is true, laid it down as a fundamental principle, that the earth was flattened at the poles. Hear what the academician whom I last quoted [*Bouguer*] says on the subject. He had been employed, with some others, to measure a degree of the meridian, near the equator, which they found to contain 56748 fathoms." 'But,' " continues he," 'what is well worthy of attention, the terrestrial degrees have not been found of the same length in other regions where similar operations have been performed, and the difference is too great to be ascribed to the unavoidable errors in observation. The degree upon the polar circle is found to be 57422 fathoms. Accordingly it follows, beyond contradiction, that the earth is not perfectly round, and that it must be higher toward the equator than toward the poles, conformably to what other experiments indicate, which it is not necessary to detail. The curving of the earth is more sudden toward the equator, in the direction of north and south, as the degrees are smaller there. And the earth, on the contrary, is flatter toward the poles, because there the degrees are greater.' " *Bouguer's Navigation.*"

" I," says *M. De Saint Pierre,* " deduce without hesitation a conclusion diametrically opposite, from the observations of these academicians. I conclude that the earth is lengthened out at the poles, precisely for this reason, that the degrees of the meridian are greater there than under the equator. Here is my demonstration. If you place a degree of the meridian, at the polar circle, over a degree of the same meridian at the equator, the first degree, which is 57422 fathoms, would exceed the second, which contains only 56748 fathoms, by 674 fathoms, conformably to the operations of the academicians themselves."

" Consequently if you were to apply the whole arch of the meridian, which crowns the polar circle, and which contains 47 degrees, to an arch of 47 degrees of the same meridian near the equator, it would produce a considerable protuberance, its degrees being greater. This polar arch of the meridian could not extend in length over the equinoctial arch of the same meridian, because it contains the same number of degrees, and consequently a chord of the same length."

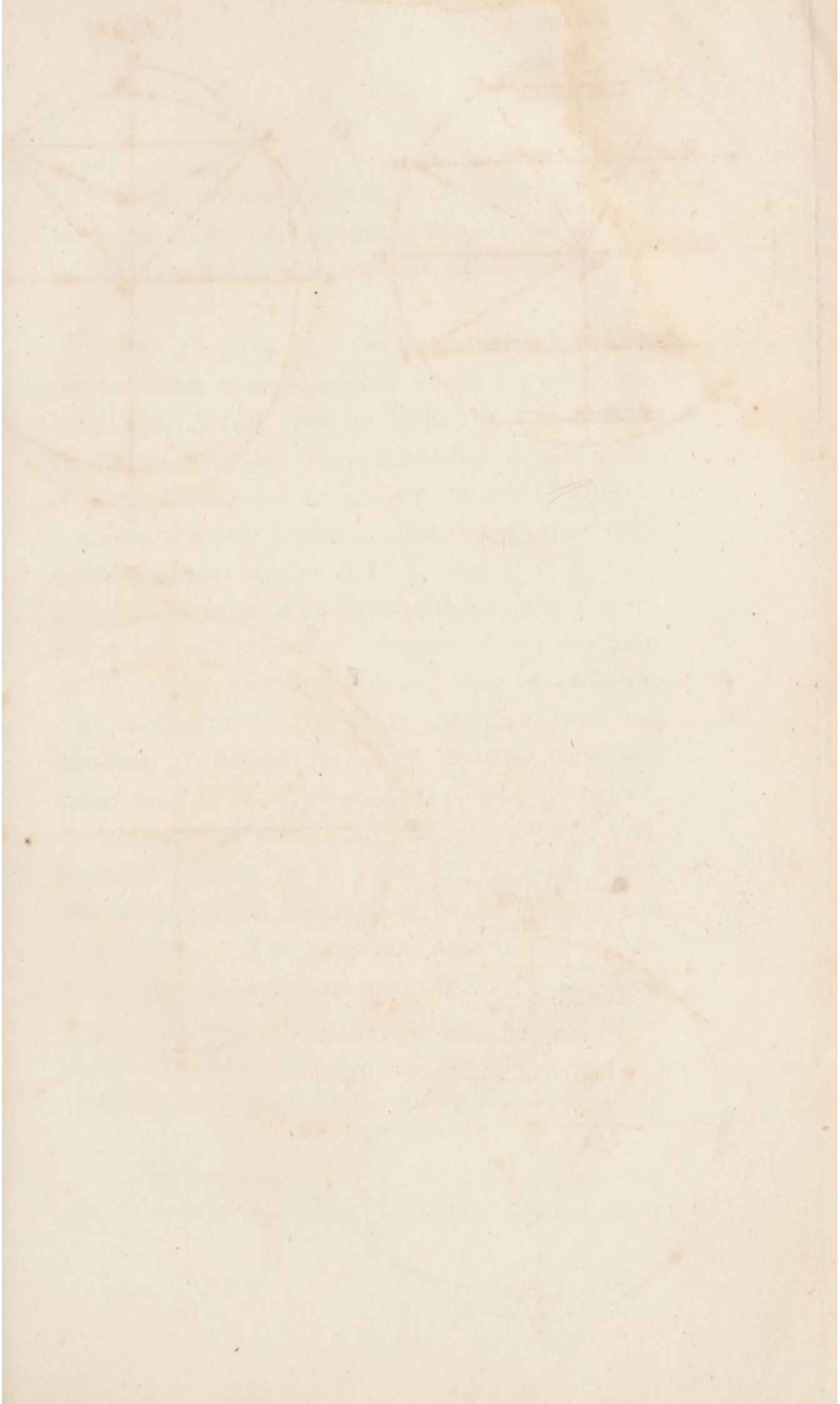


Fig. I.

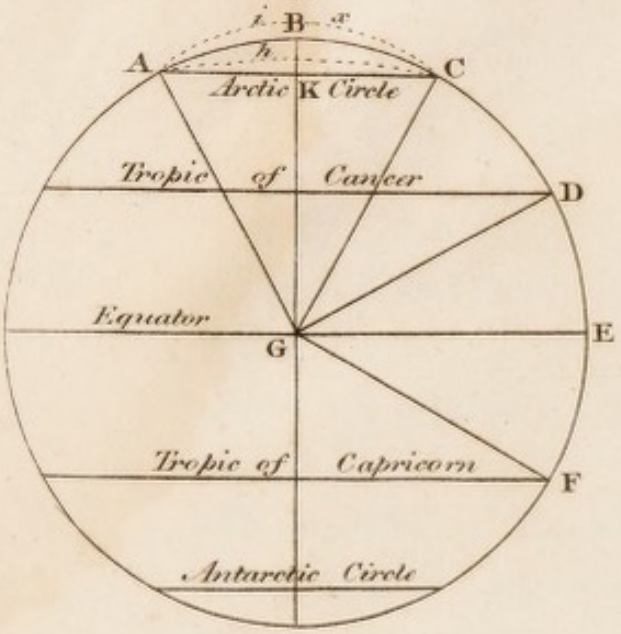


Fig. II.

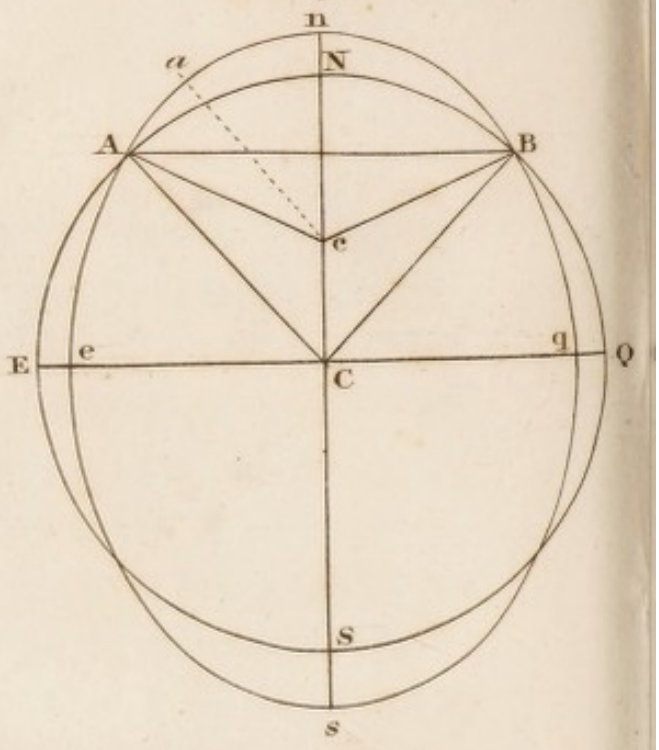


Fig. IV.

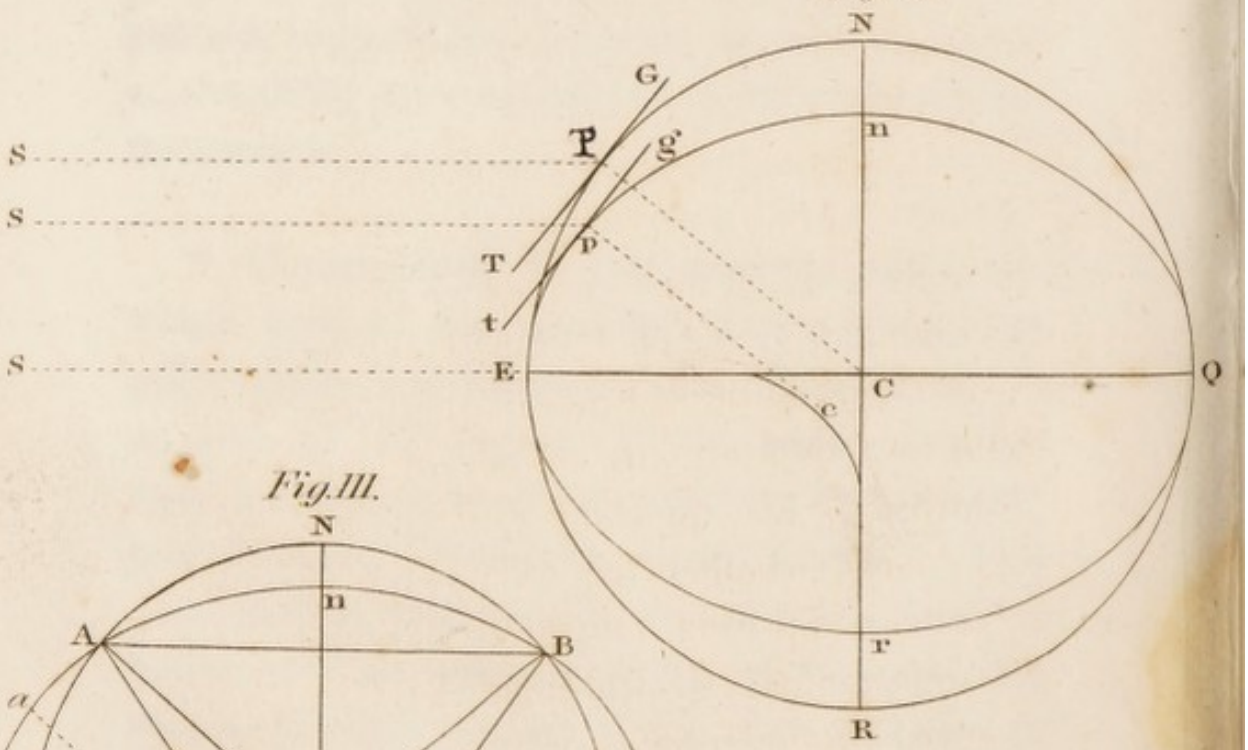
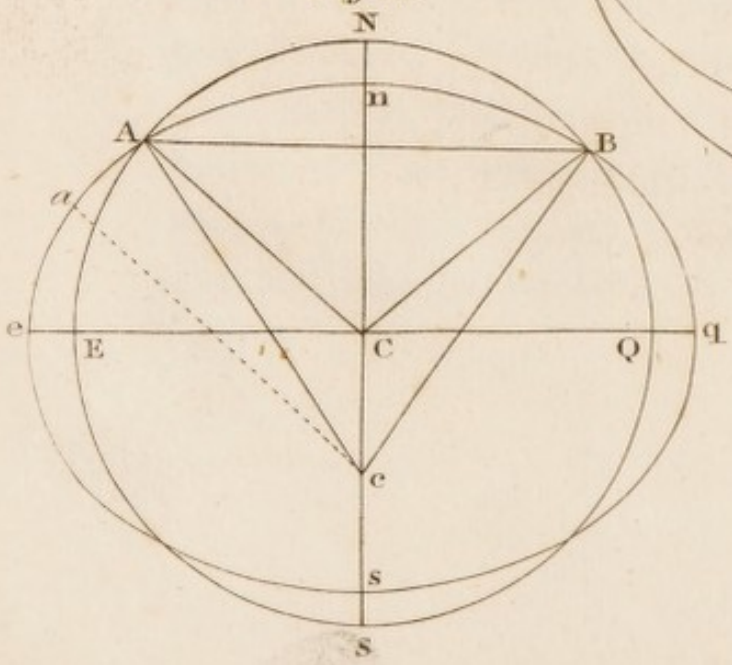


Fig. III.



" I here present a figure of the globe, which I have got engraved, in order to render the mistake of our Astronomers perceptible to every eye."

" Let *x*" [Fig. 1] " be the unknown arch of the meridian comprehended above the arctic polar circle, A B C, and let D E F be the arch of the same meridian comprehended between the tropics. These two arches are, it is well known, each equal to 47 degrees. But though they both subtend equal angles A G C, and D G F, they are by no means of equal expansion, for according to our astronomers, a degree of the meridian at the polar circle is greater, by 674 fathoms, than a degree of the same meridian near the equator. It follows therefore, that the unknown polar arch *x*, of 47 degrees, exceeds, in extent, the equinoctial arch D E F, which likewise contains 47 degrees, by 47 times 674 fathoms, which amount to 31678 fathoms, or twelve leagues and two thirds. The question now to be determined then is, whether this unknown polar arch *x* is contained within the circle, in the curve A *h* C, or coincides with it as A B C, or falls without its circumference, in the direction A *i* C.

" The unknown polar arch *x* cannot be contained within the globe, as A *h* C, as is pretend-

ed by our Astronomers, who will have it to be flattened there; for if it were contained, it would be evidently smaller than the spherical arch A B C which surrounds it, conformably to this axiom, 'that the thing contained is smaller than what contains it,' and the more this curve A *h* C shall be flattened, the less will be its extent, as it will approach nearer and nearer to its chord, that is, the straight line A K C.

"On the other hand, this polar arch *x* cannot coincide with the spherical arch A B C, for it exceeds it by twelve leagues and two thirds, it must belong therefore to a curve which falls without the circumference of the globe, as in the direction A *i* C. The globe of the earth then is lengthened out at the poles as the degrees of the meridian are greater there than at the equator. Astronomers have consequently erred in concluding from the magnitude of those degrees that the poles were flattened."

In this demonstration, as it is called, the author seems not to have understood, or not to have duly considered, the method of finding the the latitude of a place upon the earth's surface; if he had he would not have concluded that the chord of the polar arc, bounded by the arctic

circle, and that of the arc bounded by the two tropics, were equal to each other, or that they subtended equal angles at the center of the earth. It is not the angle at the center of magnitude but that at the center of curvature, that measures, or rather that is measured by, the degrees in the arc. The want of attention to this circumstance laid the foundation of his error, and has led to a variety of absurd consequences.

To determine the latitude, we cannot descend to the center of the earth, and from thence draw lines to the circumference, so that by the angle contained between them we might determine the degrees in the arc they include: but must content ourselves with such observations as we are capable of making upon the surface. And in conformity to this, the latitude of a place is determined, by observing the elevation of the sun, or of a star above the horizon, or tangent to the surface, at that place.

But it is necessary here to be a little more particular. And in order, in the first place, to point out the fallacy of *M. De Saint Pierre's* hypothesis, let it be remembered, that all angles are measured by the arc of a circle described

upon the angular point as a centre,—that the degrees of any arc contained between two radii, will be greater in proportion as the radius is increased,—but that the curvature of a circle, or the deflection of the circumference from the tangent, is greater in proportion as the radius is less.

This being premised, let EANBQS [Fig. 2] represent a section of the earth, supposing it a perfect sphere, in the plane of a meridian; in which let C be the centre, EQ the equatorial diameter, N the north pole, and NS the axis. Also let *e*A*n*B*qs* be a section of the earth, in the same plane, supposing it to be a prolate spheroid, or "lengthened out at the poles;" in which *eq* will be the equatorial diameter, *n* the north pole, and *ns* the axis. Let the periphery of the ellipsis cut that of the circle in the points A and B, and let AB, AC, and BC be drawn. Now in the sphere, the arc ANB will be the measure of the degrees in the angle ACB, and ACN, or $\frac{1}{2}$ACB, will truly represent the polar distance AN. But in the spheroid the curvature of the arc A*n*B being greater, or more sudden, than that of the spherical arc ANB, its radius is less than AC, or NC. Let it be *nc*, which is found by making A*c*, *nc*, and B*c* equal to each other.

Here let us suppose the elliptical arc A*n* B, to coincide with a circular arc, whose radius is *nc*, which in small arcs will not sensibly differ; then joining A*c* and B*c*, the arc A*n*B will not be the measure of the angle A C B, but of A *c* B, which is greater than ACB, consequently the arc, or polar distance, A*n*, contains a greater number of degrees than the arc AN. Draw *ca* parallel to CA, then will the angle *acn*, be equal to the angle ACN, and the arc, or polar distance *an*, will contain the same number of degrees as the arc AN; but the arc *an* is less than AN, because its radius *ac* is less than AC; therefore the degrees toward the pole, in the prolate spheroid, are less than those of the sphere.

We will now invert our demonstration, by supposing the earth to be an oblate spheroid, or "flattened at the poles." Let, as before, EANBQS [Fig. 3] represent a section of the sphere; and let *e*A*n*B*qs* represent a like section of the spheroid. Then the same preparation and notation being made as before, because the arc A*n*B is flatter, or has a less curvature, than ANB its radius A*c* is greater than AC; whence reasoning as in the former case, it will appear that the arc *an* contains the same number of degrees

as the arc AN; but *an* is greater than AN, therefore the degrees toward the pole in the oblate spheroid are greater than those of the sphere.

In order to accommodate our ideas to those of our author, and to take up the consideration of the subject in his own way, we have in the foregoing cases, supposed the periphery of the ellipsis to cut that of the circle, in the points A and B. This however, is contrary to the fact; for the equatorial diameter being the same in both, and the axis of the spheroid being less than that of the sphere, the periphery of the ellipsis must fall wholly within the circle, and they can only touch each other at the extremities of the equatorial diameter. Let us, therefore, endeavour to illustrate the subject in a different way, agreeably to the known method of finding the latitude of any place.

Here, let E N Q R [Fig. 4] represent a meridian on the sphere, and E *n* Q *r* a meridian on the spheroid, where the equatorial diameter of both will be represented by EQ, and the axis of the sphere and spheroid by NR, and *nr*, respectively. Let S represent the sun, or a known star, which in order to simplify the illustration, we

will suppose to be upon the meridian, and in the plane of the equator. Now by reason of their immense distance, all lines drawn from the sun or star, to any part of the earth, may be considered as parallel to each other; therefore take any point P upon the spherical meridian, and another point *p* upon the spheroidical meridian, such that the tangent *tpg* at the point *p*, may be parallel to the tangent TPG at the point P: then will the elevation of the sun, or star, above the horizon, at the points P and *p*, be represented by the angles SPT and *spt* respectively; which as the sun, or star, is supposed to be in the plane of the equator will be equal to the complement of the latitude. But the angles SPT and *spt* are equal, therefore the latitudes at the points P and *p* are the same.

Now, if the curvature of the arc E*p*, according to the Newtonian theory, be greater, or more sudden, than that of EP, the point *p* will fall within the circle; and the position of its tangent *tpg*, will vary quicker than that of the circle TPG, consequently E*p* will be less than E P wherever the point P is taken, or whether the arc EP represent one, or any number of degrees, not exceeding the quadrant EN.

It must, however, be observed, that if the

points P and *p* be supposed to move towards N and *n*, the radius of curvature of the ellipsis, and consequently the length of the degrees will increase, so that at a certain point, when the radius of curvature of the ellipsis becomes equal to PC the radius of the circle, a degree on the spheroid will be equal to a degree on the sphere ; and from that point to the pole, the degrees on the spheroid, still increasing, will be greater than those on the sphere, yet so that the whole elliptical arc E*p*, is always less than the circular arc EP.

But it is found by the observations of the French mathematicians; who measured the length of a degree, both at the equator, and at the polar circle, that in passing from the equator towards the pole, the degrees do so increase ; therefore the spheroidal surface of the earth falls within the sphere, and its figure is that of an oblate spheroid, or it is flattened at the poles.

Mr. Murdoch, in his Mercator's sailing, has calculated the arcs of the meridian, both to the spheroid and sphere, in minutes of the equator, for every degree of latitude. From this calculation it appears, that the first degree from the equator contains 58½ of those minutes—at about 55 degrees of latitude a degree of the sphe-

roid is equal to that of the sphere, or 60 minutes —that a degree at the pole is equal to $60\frac{7}{10}$ minutes,—and that the whole quadrantal arc, or extent from the equator to the pole on the spheroid is $5370\frac{2}{10}$ minutes, being $29\frac{8}{10}$, or near half a degree, less than that of the sphere. Hence also it appears, that the surface of the spheroid must fall within the sphere.

From the preceding remarks, the hypothesis of *M. De Saint Pierre* will, we apprehend, appear to be groundless, even to those who have neither leisure nor inclination to enter into more abstruse mathematical researches.*

We shall just mention an argument or two which he brings in support of his theory from collateral circumstances. One of these is drawn from two observations, said to be made at different times by *Kepler* and *Tycho Brahe*, upon two lunar eclipses; wherein the shadow of the earth was supposed to be lengthened in the direction of

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* Those who wish to see this subject treated in a more scientific manner, we would refer to Dr. Hutton's Mathematical and Philosophical Dictionary; or to Murdoch's Mercator's Sailing.

the axis. Admitting these observations to have been correct and just as *M. De Saint Pierre* has represented them; the phenomena would not warrant the inference, as the supposition of an elliptical shadow would not solve the difficulty. It is very probable that in those eclipses, the difference between the calculated and observed time of duration, was occasioned by the moon, at the the time of observation, being at a greater distance from the earth, than was supposed in the calculation.

He also lays considerable stress upon the authority of M. Cassini, who, contrary to the Newtonian Theory, maintained that the earth is a prolate spheroid; and in this he was supported by *De Mairan*, and others. This error of *Cassini* arose from his assuming false data, for, from the observations made by himself in *France*, he supposed that the degrees of the meridian increased in going towards the equator; but when, by subsequent observations, the contrary was found to be the fact, he saw, and, it is said, candidly acknowledged his error.

But our author, although he seems eagerly to catch at these trifling circumstances in favor of his

hypothesis, slightly passes over, or rather endeavors to evade, an argument of much greater weight on the opposite side of the question. We here allude to the observations made upon the vibrations of a pendulum, in different latitudes; which although they perfectly corroborate all the other circumstances,—although, independent of other observations, they furnish a convincing argument in favor of Sir IsaacNewton's theory, and indeed first led to the discovery, or even a suspicion of the spheroidal figure of the earth;—yet they are passed over by *M. De Saint Pierre,* as if they were beneath his notice. For speaking of the Academicians, and those who support the Newtonian system, he says [Page 27. Vol. III.] "They have I admit, made the vibrations of a pendulum to quadrate with it, but that experiment is liable to a thousand errors."

As he has advanced nothing that tends to invalidate this argument, it is unnecessary for us to say any thing in its support, especially as we have already, as we conceive, established the truth of the Newtonian doctrine upon different principles*

* Those who wish to examine the arguments drawn from the vibrations of a pendulum, may consult Dr. Hutton's Mathematical and Phylosophical Dictionary, Murdoch's Mercator's Sailing, or Maclaurin's Fluxions, Vol. 2.

We shall therefore take a slight view of some other absurd notions which our author has adopted, and which naturally follow from his theory.

In what he denominates objections to providence, [Study III.] he has introduced many of the visionary ideas of *M. De Buffon*, who has surmised, that the earth was struck off from the sun by the collision of a comet, and that before it took its present form, it was in a state of fusion. To these absurd notions, which could not have originated in an imagination less fertile than his own, *M. De Saint Pierre* subjoins a variety of conclusions equally visionary, merely for the purpose of contradicting them. He states as the opinion of some, and he seems to insinuate that it is the general opinion of the Newtonians, that the mountains of the torrid zone, and particularly near the equator, were formed by the centrifugal force occasioned by the diurnal rotation of the earth. For [Page 124, Vol. 1.] he declares, " If a centrifugal force had swelled the mountains of the globe while it was in a state of fusion there must have been mountains much more elevated"

In opposition to this opinion, which was ne-

ver entertained by any one who understood the Newtonian theory, he denies the existence of a centrifugal force; but he does not seem to understand either the nature or the cause of it. For speaking of the earth as a body detached from the sun [Page 221, Vol. 1.] he says, "The sun, it is said, has a "centrifugal force. The globe of the earth there- "fore must be retiring from it. No, it is alleged, "because the earth has a constant tendency toward "that luminary. It must accordingly, have lost "the centrifugal force, which should adhere to its "very nature, as being a portion of the sun." From this exhibition of his sentiments it is evident, that he considers a centrifugal force, not as it is in fact, a mere effect of motion, but as a property of matter. And because he cannot reconcile the effects of central forces with his own conceptions he denies their existence. "The centrifugal and centripetal forces" says he [Page 123, Vol. 1.] "seem to me no more to exist in the hea- "vens, than the two circles denominated the equator and the zodiac." having disposed of those forces, he introduces his own theory, respecting the figure of the earth, as amply sufficient to supply the defect; and in illustration of the supposed consequences of his hypothesis, he remarks [Page 17. Vol. III.] "All truths run into one another

"like the links of a chain. We acquire a know-
"ledge of them only by comparing them with each
"other. Had our Academicians made a proper
"use of this principle, they must have discovered
"that the flattening of the poles was an error. They
"had only to apply the consequences of this doc-
"trine to the distribution of the seas. If the poles
"are flattened, their radii being the shortest on the
"globe all the seas must press thitherward, as be-
"ing the most depressed place of the earth; on the
"other hand if the equator were the most elevated,
"all the seas must retire from it, and the torrid zone
"would present, through its whole circumference.
"a zone of dry land of six leagues and a half of
"elevation at its centre; as the radius of the globe
"at the equator, exceeds by that quantity the ra-
"dius of the poles, according to the Academi-
"cians."

If the earth were at rest, the phenomena above described would undoubtedly be produced; for as there would be nothing to oppose the gravitation, the waters of the ocean would descend towards the poles, and in order to obtain an equilibrium, would take a spherical form; so that while they overflowed the polar regions, they would sink far below the shores of the torrid zone. But, by the

diurnal motion of the earth, a force is communicated to those waters, in proportion to their distance from the earth's axis, by which they are carried towards the equator, where they settle into a spheroidal form like that of the earth itself.

The force thus communicated, by a rotatory motion, is called a centrifugal force; and is so natural a consequence of all motion round a centre, that it is impossible to give a circular rotation to a body of any kind without producing it.

To explain the nature of this force, to those who have not duly considered it, suppose a solid body, of any kind, or form, to be laid upon the upper surface of a mill-stone; then as soon as the mill-stone has acquired a degree of velocity sufficient to overcome the friction, the body will recede from the centre to the circumference, and there, if it be not obstructed, it will quit the surface of the stone, and fly off. The force thus communicated to the body will be in proportion to its distance from the centre of the mill-stone, because the velocity is in that proportion,

Just in this way are the waters on the surface of the earth carried, by its rotation, towards the circumference of its greatest circle, and were

they not retained there by the power of gravitation, they would be detached from the earth, and fly entirely off. But when two forces act in opposition to each other, the effect produced will be proportional to the difference, only, of those forces. Thus the centrifugal force at the equator, substracts from the gravitation, a force equal to its own, and consequently the water rises there until the quantity accumulated balances the defect, and an equilibrium takes place over the whole surface of the earth.

It may perhaps be asked, " if the gravitation " at the equator prevents the water from flying off, " or receding farther from the center, why do not " the same power of gravitation prevent its receding so far, when, at a distance from the equator, " the force of gravitation is the same, and the centrifugal force less ?

In answer to this, let it be observed, that the gravitating force acts in a direction tending towards the center of the earth, whereas the centrifugal force acts, every where, in a direction perpendicular to the earth's axis; therefore the gravitation does not act in direct opposition to the centrifugal force, except at the equator. In all other places the directions of the two forces

intersect each other in an angle which varies as the distance from the equator is greater or less. And in high latitudes, they are almost at right angles to each other, consequently the gravitation there can have very little effect upon the centrifugal force.

We shall here just notice another circumtance which our author brings as a proof of the elongation of the earth at the poles. This he deduces from some barometrical observations. "It is well known" says he [Page 19, 20. Vol. III.] "that the height of the atmosphere diminishes in "proportion as we ascend upon a mountain. Now "this height diminishes likewise as we advance "towards the pole. The mercury in the borometer, at Paris, sinks one line at the heigth of elever fathom; it sinks likewise one line in Sweden, "on an elevation of ten fathom, one foot, six "inches, and four lines. The atmosphere of Swe"den therefore is lower, or what amounts to the "very same thing, its continent is more elevated "than the land at Paris. The earth therefore "lengthens out as you proceed northward." He draws a similar conclusion from some observations, said to be made by Captain Cook, in the southern hemisphere.

Without questioning the accuracy of these experiments, we shall only observe, that because the atmosphere is lower in Sweden, it does not follow that the land must be higher there than at Paris; for the same causes which operate in giving a spheroidal form to the waters of the ocean, must produce a similar effect upon the atmosphere. This argument therefore, instead of supporting our author's system, militates directly against it.

Here then let us pause, and reflect a little. Had *M. De Saint Pierre* duly considered the facts and contemplated nature as it really is, he might have discovered beauties transcending by far, all the visionary systems of the most exalted genius; for, even in the figure of the earth, we may trace the footsteps of infinite power, wisdom, and goodness! We here see, that the protuberance of the equatorial regions, was not formed *by*, but *for* a centrifugal force. It is as evident that the earth was created in a spheriodal form, in order to accommodate it to a diurnal rotation, as that the eye was made for seeing, or the ear for hearing. Had the torrid zone been created a little higher, or a little lower, it would in the latter case have been overflowed,

and in the former deserted, by the ocean. Or, had the velocity of the earth's diurnal motion, or the time of its rotation, been either greater, or less, similar effects would have been produced. Who then that consider these things, can ascribe this configuration of the earth to a fortuitous concourse of discordant elements? Who can be so blind as not to discover in it the effects of consummate design? Or who can view the harmony that exists between the various parts of this vast machine, and not be constrained to exclaim, " O Lord, how manifold are thy works, in wisdom hast thou made them all!"

The diurnal motion of the earth has been adduced as a proof of its spheroidal figure; but we do not know that the converse of this argument has been applied to prove its motion. It may, however be so applied, and it will furnish us with a demonstration of the diurnal motion.

We know, from experiment, that in consequence of the power of gravitation, all fluids on the surface of the earth, tend to preserve an equilibrium between their several parts; from whence it follows, that if the earth were perfectly at rest, the surface of the ocean would set-

tle into the form of a perfect sphere; nor do we know of any power that could possibly alter this spherical form, except a diminution of the gravitation in some parts of the earth, or an augmentation of it in others.

We know also, from actual mensuration, that the earth is not a sphere, but that the parts about the equator are much higher than the poles; from whence likewise it follows, that unless a diminution of gravity took place at, and near the equator, the ocean must recede from those parts, leaving the shores considerably elevated above its surface.

Moreover it is evident, from experiment, that a circular rotation round a center, diminishes any tendency towards that center; consequently a rotation of the earth must diminish the gravity near the equator; and we know of no other cause adequate to that effect.

Therefore, as it is certain that the ocean does not recede from the equator, we may fairly conclude, that the earth revolves on its axis.

But to return to *M. De Saint Pierre.* He has

said that " All truths run into one another like the links of a chain," and he might have added, that error very frequently does the same. Of this he affords us a variety of examples.

He is not only unwilling to admit the cause, but seems to deny, or not to understand, the effect of the earth's annual motion; and, in order to support his own hypothesis, he attributes the different seasons of the year, not to the constant position of the earth's axis, * and its obliquity to the ecliptic, but to a daily variation in the inclination of the axis, occasioned by a supposed gravitation of the poles towards the sun. Speaking of the projectile and gravitating forces, and their operation upon the earth in its orbit, he says [Page 56. Vol. II.], " On the supposition that " these two contrary forces were combined happily " enough in favour of the globe, to fix it, with its " vortex in a corner of the firmament, where these " forces should act without destroying themselves,

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* The small variation that takes place in the inclination of the earth's axis, producing what is called the precession of of the equinoxes, we have possed over, as being insensible to common observation.

"it would present its equator to the sun with as "much regularity as it describes its annual course "round him. From these two constant motions "never could be produced that other motion so "varied, by which it daily inclines one of its poles "toward the sun, till its axis has formed, on the "plane of its annual circle, an angle of twenty-"three degrees and an half; then that other retro-"grade motion, by which it presents to him with "equal regularity, the opposite pole." His own hypothesis, respecting the same phenomena, he gives us [Page 165. Vol. I.] in the following words. "As the ice of this pole which its gravity "inclines towards the sun melts in proportion to "its vertical approximation to the source of heat, "and as on the contrary, the ice of the opposite "pole increases in proportion to its removal, the "necessary consequence must be, that the first "pole becoming lighter, and the second heavier, "the centre of gravity passes alternately from the "one to the other, and from this reciprocal pre-"ponderancy must ensue that motion of the globe "in the ecliptic which produces our summer and "winter." If this passage have any meaning, it would from hence appear, that he considers the obliquity of the ecliptic, and the motion of the earth in it, as insufficient to account for the dif-

ference of the seasons; and to supply the imaginary defect, he supposes the axis of the earth to librate upon its centre, making, every year, an angle of forty seven degrees. A supposition as repugnant to reason, as it is contrary to experience and fact. Upon this hypothesis the axis of the earth would not be directed towards the polar star, except at the equinoxes; for at the solstices it would tend to a point twenty three degrees and a half from it. The declination of the fixed stars also, must vary the same as that of the sun, and their meridian altitude in the summer would differ from that in the winter by forty seven degrees. But the consequences of this supposition are too absurd to merit a serious refutation.

Although our author seems to admit the motion of the earth in its orbit, he denies the action of the projectile and gravitating forces in producing, and regulating that motion. This he has done in the passage before quoted, and he also says [Page 123, Vol. I.] "It appears to me, no more natural to compose the uniform motion of the earth through the heavens, of the two motions of projection and attraction, than to attribute to similar causes that of a man walking on the

earth." Here he seems to have made a correct observation without knowing it; the comparison which he brings being evidently intended to ridicule both the suppositions. In this, however, he is mistaken; for to a combination of the two forces of projection and gravitation, or to one of them, must be ascribed, not only the motion of a man walking, but all local motion produced on the earth, by any means whatsoever.

The application of these forces to the motion of the earth, and of the planets, has been so clearly explained, by various writers, that any additional illustration of the subject would be superfluous; but their application to those motions which fall under our daily observation, and particularly to that of any person walking, may perhaps require some elucidation.

In the act of walking, the person, by the exertion of his muscular strength, presses his foot against the ground, by the reaction of which his body is projected forward, in a direction oblique to the horizon; so that the centre of gravity of his body ascends, not only so long as his muscular power continues to act, but until the power of gravitation counteracts that of projection. Having

obtained its greatest height, it then descends by the power of gravitation, not in a perpendicular to the horizon, which it must do if the effect of the projection had ceased, but in a curve compounded of the two forces. Thus in the act of walking, the center of gravity of the person's body describes an undulatory path, moving in a curve somewhat akin to the cycloid.

The application of these forces will be more conspicuous in running, for then the hindermost foot is taken off the ground before the foremost meets it, and, during the interval, the body is carried forward entirely by the projectile force, the effect of which must consequently continue after the muscular exertion which produced it has ceased to act.

The undulatory path, described by a person in walking, is not generally observed, but if a cord were stretched horizontally at a small distance above the person's head, that path would be very apparent to a bystander. This undulation may also be distinctly seen in a camera obscura.

That both these forces operate in the act of walking is evident, for neither of them singly

could produce the effect. If the gravitation did not act, a person, having raised his body from the earth, could never return to it again, but must continue to fly off in the direction, and with the velocity which, by his muscular exertion, he had given to it. On the other hand, if the effect of the projectile force did not continue after the exertion which caused it ceased; then, in running, when the person has taken his hindermost foot off the ground, his body must instantly descend in a perpendicular to the horizon, and not be carried forward, as we see in fact it is.

This effect of projection, however, could not be produced by any person in walking, or running, nor indeed could he walk at all, if the reaction of the earth were not equal to the action of his foot upon it. This principle *M. De Saint Pierre* denies, although it is self evident, and is one of the primary laws of nature. We know the impossibility of demonstrating an axiom, and therefore shall not attempt it; but if any person doubt the existence of this law, let him be suspended in the air, and he will find that, without something to react against it, the greatest exertion of his muscular power will never remove the center of gravity of his body, out of the perpendi-

tular line passing through the point of suspension. If the reaction be imperfect, the effect of the action will be diminished in proportion. To be convinced of this, let a person walk up a chalky hill after a shower of rain, and he will find that much of his strength is exerted to no purpose.

In the same way may the forces of projection and gravitation be applied to all local motion, produced by other means ; and we have been the more particular in our illustration, because the application of them, in common cases, has not been much attended to ; and because the consideration of such application may lead to a more easy comprehension of their effects in producing the planetary motions.

Of all the hypotheses which our author has adopted, perhaps the most extravagant is his theory of the tides. This vision he endeavours to erect upon the "baseless fabric" of the oblong figure of the earth; therefore, as the foundation is unsupported, the superstructure must fall with it. It would be wasting time to attempt formally to refute a system so absurd. And to give an illustration of the true theory of the tides is unnecessary, as that has been done by various

writers, much more competent to the task than we dare pretend to be.* In our remarks, therefore, upon this hypothesis of *M. De Saint Pierre*, we shall only take a view of its leading features, in order, chiefly, to shew what lengths in absurdity a person may be carried by a fond pursuit of visionary theories. Here we may see that errors "run one into another like the links of a chain." From hence also we may learn the danger of admitting any doctrine without a strict and careful examination; for that the beginning of error, like "the beginning of strife, is as when one letteth out water."

The ground of this theory, as before observed, is the prolate figure of the earth. The elongated parts about the poles being, according to his idea, composed of huge masses of ice, which, melting as the sun approaches each pole, descend towards the equator, and form general currents in the Atlantic, and Pacific oceans; while the inequalities in the fusion, occasioned by the daily approach of

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* See Dr. Hutton's Mathematical and Philosophical Dictionary; or Maclaurin's Account of Sir Isaac Newton's Discoveries.

the sun towards the meridian, produce a diurnal swell in the ocean that causes the tides. But let him tell his own tale.

In the explanation of the plates [Page xxxviii. Vol. I.] he says, "Let us now consider the course "of the polar effusions produced by the action "of the sun on the ices of the poles. There issues "every year a general current from that which "is heated by the sun: and as that great lumi- "nary visits them alternately, it follows that there "must be two general opposite currents which "communicate to the seas their movement of cir- "culation, and which are known in *India* by the "name of the easterly and westerly monsoons, or "winter and summer.

"This being laid down, let us examine the ef- "fusions of the south pole, which is there repre- "sented in its summer. The general current which "issues from it divides into two branches, the one "of which sets in toward the Alantic ocean, and "penetrates even to its northern extremity. When "this branch comes to force its way between the "prominent part of *Africa* and *America*, finding "itself straightened, on passing from a wider to a "narrower space, it forms on the coast two coun-

"ter currents, or *vortices*, which proceed in con-
"trary directions." Having described the course, and cause of these supposed currents, he goes on to inform us [Page xxxix.] that "in the mid-
"dle of the Altantic ocean, and beyond the strait
"of the two continents, it pushes on to the north
"in full force, and advances to the very northern
"extremities of *Europe*, and of *America*, bringing
"us twice every day along our coasts the tides of
"the south, which are the half-daily effusions of
"the two sides of the south pole."

He proceeds to describe "the other branch
"which issues from the south pole," which, he says, "takes a direction to the westward of *Cape*
"*Horn*, rushes into the South sea, and produces
"in the *Indian* ocean the eastern monsoon, which
"takes place in India during our winter."

Having given us an account of what takes place in the southern hemisphere, he goes on and tells us, that "six months after, that is in our
"summer, commencing towards the end of March,
"when the sun at the line begins to forsake the
"south pole, and proceeds to warm the north, the
"effusions of the south pole are stayed; those of
"our pole begin to flow, and the currents of the

"ocean change in all latitudes. The general cur-
"rent of the seas then takes its departure from our
"pole, and divides, like that of the south, into two
"branches."

One of these branches, he says, "descends
"through the Atlantic ocean, passes the line, and
"finding itself confined at the same strait of *Guinea*
"and *Brazil*, it forms on its sides two lateral coun-
"ter currents, which set in northward, as those
"formed six months before by the current of the
"south pole, set in southward. These counter
"currents produce, on the coast of *Europe*, the
"tides which always appear to come directly from
"the south, though they actually come at that
"season from the north."

"The other branch of the current, which," he tells us, "flows from our pole on the opposite
"side of our hemisphere, issues through the pas-
"sage called the North-strait, situated between the
"most easterly extremity of Asia, and the most
"westerly of America."

Having at a considerable length endeavoured to explain and illustrate his system, and quoting extracts, in support of it, from various navigators,

our author enters into a recapitulation of the principal points; in which he sets out with stating [Page ix. Vol. I.] that "The tides are the half "daily effusions of the ices of one of the poles, "just as the general currents of the ocean are its "half yearly effusions."

This sentence contains an epitome of our author's hypothesis; it is doubtful, however, whether he clearly comprehends his own theory. But, be that as it may, it is evident that he does not understand that of Sir Isaac Newton; for he says, [Page xlviii. Vol. I.] "that the Atlantic "current comes from the north, and sets in south-"ward, in our hemisphere,—but only during sum-"mer. Of consequence it then proceeds directly "from the effusions of the ices of the north pole, "which in our summer flow toward the south; and "it evidently destroys, by this direction toward the "equator, the pretended action of the moon be-"tween the tropics, which according to our astro-"nomers, impresses on the ocean a motion toward "both poles." By this declaration our author betrays his ignorance of the Newtonian theory; for the action of the moon upon the ocean is diametrically opposite to that which is here stated.

Thus have we given an account of *M. De Saint Pierre's* theory of the tides, in his own words, and at a length sufficient to be understood; so far, at least, as his system is intelligible. This theory being built upon the supposition that the earth is lengthened out at the poles, must needs be as fallacious as the hypothesis upon which it is founded; it therefore, as we have already observed, requires no formal refutation. We may, however, remark, that many of the arguments adduced in its support, and which are drawn from the observations of different navigators, are misapplied; while the observations themselves are in some cases doubtful, and in others depend upon particular and local circumstances; they are consequently incompetent to prove the truth of a general theory. We shall just notice a few of these, merely to shew the miserable subterfuges to which persons may be driven, in order to support a favourite tenet.

The observations made in the southern hemisphere, to which our author refers, are perfectly consistent with the Newtonian theory; and therefore can furnish no argument in favour of his own. But in order to prove that a general current, as well as the tides, in the northern hemisphere,

proceed from the polar regions, and actually set in from the north, he produces some extracts, from several voyages, that seem more consistent with his hypothesis.

He declares [Page 144. Vol. I.] "It is im-
"possible to deny, in the first place, that the cur-
"rents and the tides come from the pole, in the
"vicinity of the polar circle."

"*Frederic Martens*, who, in his voyage to
"Spitzbergen, 1671, advanced as far as to the
"eighty-first degree of northern latitude, positively
"asserts, that the currents, amidst the ices, set in
"toward the south. He adds farther, that he can
"affirm nothing with certainty respecting the flux
"and reflux of the tides."

"Henry Ellis observed with astonishment, in
"his voyage to Hudson's Bay, 1746, and 1747,
"that the tides there came from the north, and that
"they were accelerated, instead of being retarded,
"in proportion as the latitude increased. He
"assures us that these effects, so contrary to their
"effects on our coasts, where they come from the
"south, demonstrate that the tides, in those high
"latitudes, do not come from the line, nor from
"the Atlantic ocean."

After some remarks upon this observation of Ellis, he goes on, and says [Page 145, Vol. I.] "Ellis farther observed, that the course of these "northern tides of America, was so violent at "*Wager's* strait, which is about 85° 37′ north la- "titude, that it run at the rate of from eight to ten "leagues an hour. He compares it to the sluice "of a mill. He remarked that the surface of the "water there was very fresh, which puzzled him "exceedingly, by damping his hope of a com- "munication between this bay and the South Seas," To this *M. De Saint Pierre* subjoins the follow- ing curious remark. "He remained," says he, "nevertheless, convinced of the existence of "such a passage; *such is the pertinacity of "man in favour of pre-conceived opinions, in the "very face of evidence.*"

In addition to these, he produces several other authorities, tending to prove that, near the polar circle, the tides, as well as the general currents, set in from north to south. But, without questi- oning, the veracity of these navigators, or the accuracy of their observations, we shall oppose to them some observations of equal authority, and of a more recent date.

In the year 1773, an expedition was fitted out by government, for the purpose of making discoveries towards the north pole. This was carried into execution the same year, under the command of the Honourable *Constantine John Phipps,** on board the Racehorse, accompanied by the Carcass bomb. From the journal of that voyage the following extracts are selected.

"June 29th,—"hoisted out the boat and tried "the stream; found it both by the common, and "Bouguer's log, (which agreed exactly,) to run "half a knot north, Lat. 77° 59′ 11″. Lon. "9° 31′ E.

"July 4th,—"Lat. by observation 79° 31′ "Magdalena Hook bore N. 39° E. distant about "4 miles. About three in the afternoon, when the "boat wason shore, it appeared to be high water, "and ebbed about three feet. This makes high "water, full and change, at half past one, or with a "S S W moon; which agrees exactly with Baffin's "observation in 1613. The flood comes from the "southward."

"July 13th,—"Calm till noon, the ship driving "to the westward with the current, which we ob-

* Now Lord Mulgrave.

"served to be very irregular, the *Carcass* being "driven at the same time to the eastward. Near "the main body of the ice, the detached pieces "probably affect the currents and occasion the "great irregularity which we remarked."

"July 17th.—"We found the latitude of the "island on which the observations were made, to "be 79° 50′ Longitude 10° 2′ 30″ E. The tide "rose about four feet, and flowed at half an hour "after one, full and change. The tide set irre-"gularly, from the number of islands between "which it passed; but the flood appeared to "come from the southward."

"July 25th—[At Moffen island] "It was low "water at eleven o'clock when the boat land-"ed, and the tide appeared to flow eight or nine "feet: at that time we found a current carrying "the ship to the N W. from the island, which be-"fore carried us (at the rate of a mile an hour) "towards it."—It appears extraordinary that none "of the old Navigators, who are so accurate and "minute in their descriptions of the coast, have "taken ~~no~~ notice of this island, so remarkable "and different from every thing we had seen on "the western coast; unless we should suppose

" that it did not then exist, and that the stream " from the great ocean up the west side of " Spitzbergen, and through the Waygat's straits, " meeting here, have raised this bank and occa- " sioned the quantity of ice that generally blocks " up the coast hereabouts." —" At four in the af- " ter noon, hoisted out the boat, and tried the " current which set N E by E. at the rate of " three quarters of a mile an hour."

" August 3d, " We found the ice very deep, " having sawed sometimes through pieces twelve " feet thick. This labour was continued the " whole day, but without any success; our ut- " most efforts not having moved the ships above " three hundred yards to the westward through " the ice, at the same time that they had been " driven (together with the ice, to which they " were fast) far to the N E. and east by the cur- " rent, which had also forced the loose ice from " the westward between the islands, where it be- " came packed, and firm as the main body."

From the aggregate of these observations, it is evident that the tides near the polar circle, as well as in many other places, are affected by local circumstances; and therefore no conclusion

can be drawn from them in support of a general theory.

Had the observations made on board the Racehorse fallen into the hands of *M. De Saint Pierre*, it is probable that he would have explained them so as to favour his own hypothesis, although they evidently militate against it. The difficulties that arise from causes of this kind, he obviates with a wonderful degree of facility, of which we shall produce an instance.

Having mentioned [Page 35, Vol. III.] some observations made on the western coast of America, he proceeds, particularly, to state some remarks made by Captain Cook, in an inlet upon the same coast, about the fifty-eighth degree of north latitude. On sailing up this inlet, to a place where it was only four leagues broad, that Navigator observes; "Until we got thus far, the "water had retained the same degree of saltness "at low as at high water; and at both periods was "as salt as that in the ocean. But now the marks "of a river displayed themselves. The water ta-"ken up this ebb, when at the lowest, was found "to be very considerably fresher than any we "had hitherto tasted; insomuch that I was con-

" vinced we were in a large river, and not in a " strait communicating with the northern seas."

Upon these remarks of Captain *Cook*, our author makes the following comment. [Page 36, Vol. III.] " What *Cook* calls the inlet, to which the name of " *Cook's* great river has since been given, is, " from its course, and its brackish waters, neither a " strait nor a river, but a real northern sluice, " through which the effusions of the polar ices are " discharged into the ocean. We find others of " the same kind at the bottom of Hudson's Bay. " *Ellis* was mistaken in these, in taking them for " straits which had a communication from the " Northern ocean to the South sea."

Another observation made by Captain *Cook*, in what our author calls a " continuation of the " discovery of the interior of the inlet, or *Cook's* " great river," he gives us as follows, [Page 36, Vol. III.] " After we had entered the bay, the " flood set strongly into the river *Turnagain*; " the ebb came out with still greater force; the " ~~and~~ water falling while we lay at anchor, twenty feet perpendicular."

Our author's comment upon this remark, is

more extraordinary than the preceding. "That," says he, "which *Cook* calls the ebb, or the reflux, "appears to me to be the flood, or the flux itself, "for it was more tumultuous, and more rapid than "what he calls the flux; for the reaction can "never be more powerful than the action. The "falling tide, even in our rivers, is never so strong "as the rising tide. This last generally produces a "bar at the mouth of the stream, which the other "does not."

Strange indeed must it be, if an experienced Navigator, at anchor in a river not more than four leagues wide, and where the water falls twenty feet perpendicular, should not be able to distinguish the flood from the ebb! But nothing surprizes our author, nor indeed, can any thing much surprize his readers, after perusing the preceding remarks, unless it be what follows.

Speaking of the discoveries made in Hudson's Bay, [Pages 37, 38, Vol. III.] he introduces the following quotation from Captain *Cook's* third voyage. "Middleton, who commanded the ex- "pedition in 1741 and 1742, into Hudson's "Bay, had proceeded farther north than any of "his predecessors in that navigation. He had,

" between the latitude of 65° and 66°, found a " very considerable inlet running westward, into " which he entered his ships; and after repeated " trials of the tides, and endeavours to discover " the nature and course of the opening, for three " weeks successively, *he found the flood constant-* " *ly to come from the eastward,* and that it was a " large river he had got into, to which he gave " the name of Wager's River."

Upon this quotation our author, in the very same page, makes the following observation: " Wager's river accordingly *produces a real tide* " *from the west,* because it is one of the sluices " which open from the north into the Atlantic " ocean; it is evident therefore that *Cook's* great " river produces, on its side, a real tide from the " east, because it is likewise one of the sluices of " the north into the South Sea."

It appears impossible for the imagination to form a system more completely absurd and preposterous than this; and yet it may admit of a question, whether the system itself, or our author's method of supporting it, be the most ridiculous. We shall, however, quit this subject lest, by pursuing a phantom, we expose ourselves

to an equal degree of ridicule. Nor should we have carried our remarks thus far, but merely to shew the "*pertinacity of man in favor of pre-*"*conceived opinions, in the very face of evidence.*"*

Our author's theory of the tides has led him into an error of another kind. To account, in some measure, for the spring tides, he ascribes to the moon the power of exciting a sensible degree of heat, by reflecting the rays of the sun. Speaking of the moon [Page 6. Vol. I.] he says, "I make her to act on the frozen seas of the poles "by the reflected heat of the sun." And he farther observes [Page 488. Vol. I.] "The moon when "full has an effective and evaporating warmth "she must act, therefore, on the polar ices, espe-"cially when at the full. The academy of sciences "formerly maintained that her light did not warm, "after experiments made on her rays, and on the "ball of a thermometer, with a burning mirror.— "This error," he adds, "has been completely re-"futed, first at *Rome*, and afterwards at *Paris*, by "a very simple experiment. Some one," he tells us, "took a fancy to expose a vessel full of wa-"ter to the light of the moon, and to place one si-

* See Postcrip.

"milar to it in the shade. The water in the first "vessel was evaporated much sooner than that in "the second."—This experiment, if it do not convince our judgment, can scarcely fail to excite our risibility.—But experiments made with the most powerful burning mirror, by collecting the rays of the moon, could never produce any sensible heat. And it has been computed, by *M. Bouguer*, and others, that the light reflected by the moon at the full, is to that of the sun, only as 1 to 300,000. Now it is evident, that their effects in exciting heat must be nearly in the same pr portion; therefore that of the moon must be altogether insensible.

In estimating the effects of the sun's direct rays in the production of heat, *M. De Saint Pierre* seems to have been equally extravagant. He concludes, that if the apparent path of the sun were confined to the plane of the equator, the surface of the earth, within the tropics, would be actually set on fire. He admits, notwithstanding, that the degrees of heat, in different climates, do not entirely depend upon the direction of the sun's rays. He knows that there are mountains in the torrid zone, whose summits are perpetually covered with snow. And [Page 177. Vol. I.] he says, " I know by experience that the summer's

"heat is as powerful at *Petersburg* as under the "line." If therefore instead of indulging himself in visionary speculations, he had followed the simple dictates of nature, he might have avoided that inconsistency into which he has now fallen; and must have discovered more substantial proofs of the beneficence of providence, than any that can be derived from his improbable surmises. If the sun were to move in the plane of the equator, the effect would be precisely the same as that which takes place at the time of the equinoxes; therefore no such consequences, as he supposes, could follow from its continuance there. The geography of the torrid zone would have furnished him with abundant matter of contemplation. The effects of the sun in those regions are not only mitigated, in many places, by lofty mountains and perpetual snows, and the heat of the day alleviated by the length of the night; but the phenomena of the equatorial seasons evidently shew that the summers there are less intense than in many places near the tropics.

At the equator there are two summers, and two winters, if they may be so called; and, as the sun never declines from the line more than twenty-three degrees and a half, when he arrives at the twelfth degree of declination, the winter

may be said to commence ; therefore by comparing the time when this takes place we shall find that the length of their southern winter is a hundred and twelve days, and that of their northern winter a hundred and nineteen days, while their summers consist of only sixty seven days each. Hence it is evident, that the time of the sun's continuance near the zenith, is less at the equator than near the tropics ; and it is very probable that the obliquity of the ecliptic upon which these phenomena depend, is such as to produce to the inhabitants of the earth in general a maximum of advantage.

Our author has also fallen into a strange mistake, respecting the effect of the rays of the sun upon different coloured bodies. He says [Page 286, Vol. I.] " White reflects the rays of the sun, " and black absorbs them. The first, accordingly, " redoubles the heat, and the second weakens it. " Experience demonstrates this in a thousand dif- " ferent ways." Here, from just premises, he has drawn a false conclusion. It is certain that white bodies reflect the rays of the sun more copiously than black ones, and for that very reason the latter imbibe a greater degree of heat, in a given time, than the former. This difference will become very sensible by applying the hand succes-

sively to two bodies, the one white and the other black, which have been equally exposed to the rays of the sun.* Black bodies receive a given degree of heat, and actually take fire, much sooner than white ones; and, indeed, all bodies become black before they take fire. From the contrary conclusion, however, *M. De Saint Pierre* infers [Page 10, Vol. II. Note.] that "the black "colour of the skin is a blessing from heaven to "the nations of the south, because it absorbs the "reflexes of the burning sun under which they "live." Whether the black colour of their skin be a blessing, or a curse, to the Africans, we will not dispute; we will, however, venture to assert, that, if it be the former, it is not for the reason assigned by our author; and if it be the latter, it is not the greatest curse experienced by multitudes of those miserable people. May the time speedily arrive, when their intercourse with the

* We know not whether the different degrees of heat absorbed, and retained by bodies of different colours, have been accurately ascertained; but the experiment may be easily made, by covering the balls of two thermometers with substances of different colours, and exposing them equally to the rays of the sun. Experiments of this kind, it is presumed, would not be altogether useless.

European nations will be converted into a blessing.

Having followed our author thus far, we shall accompany him, for a few steps, into the vegetable kingdom; where, after attending to a description of various productions, we find him drawing this conclusion: "I should never have done," says he, [Page 240, Vol. I.] "were I to run "over ever so hastily, the different sports of ve- "getables; what I have said is evidence sufficient, "that there is not a single one whose direction is "determined by the vertical column of air. This "error has gained currency from its being taken "for granted, that plants affected the greatest vo- "lume of air; and this error in physics has produc- "ed another in geometry; for, on this supposition, "they must all precipitate themselves to the ho- "rizon, because there the column of air is more "considerable than in the zenith. We must in "like manner reject the consequences which have "been deduced from it, and laid down as a prin- "ciple of jurisprudence for the division of lands, "in our boasted mathematical treatises; such as "the following, *That no more wood, or corn, or* "*grass, can grow on the declivities of a moun-* "*tain, than what would grow on the area of its*

"*basis.* There is not a wood-cutter, nor hay-
"maker in the world, who could not demonstrate
"the contrary from his experience."

Positive as our author seems in this declaration, it is not only unsupported by fact, but contradicted by daily experience. Every one knows, that all vegetables, capable of supporting their own weight, shoot up in a direction perpendicular to the horizon, unless they are turned out of that direction by some extraneous obstacle. When two trees grow very near to each other, their collateral branches shoot in opposite directions; and if one of them be taken down, the other will appear as if its branches, on that side, had been cut off. This is a fact which "every wood-cut-
"ter can demonstrate," and which no one, who is the least acquainted with the progress of vegetation will attempt to contradict.

That no more wood, or corn, can grow on the declivities of a mountain, than what can grow on the area of its basis, is a proposition which, were it necessary, we could easily demonstrate; and indeed the contrary opinion cannot, for a moment, be supported, unless it could be proved that vegetables shoot in a direction perpendicular

to the plane upon which they grow. Upon this subject, however, we shall only observe, that the advantages accruing to mankind, from the irregularity of the surface of the earth, do not depend upon the quantity of surface exposed to the atmosphere, but upon various other circumstances which are obvious to every one, and which it is needless to enumerate.

There has never existed a more fruitful source of imaginary speculation than the universal deluge. Persons in every age, for centuries past, have been very solicitous to account for the causes which produced that catastrophe; and theories, as various as the visions of their inventors, have been formed, to solve the difficulties arising from that direful event.

Our author, as might be expected, has joined this visionary corps, and, not content with the systems of his predecessors, has advanced one of his own. The illustration of this theory occupies a considerable portion of his work; but the following outline may be sufficient to shew its leading principle: "My supposition then is," says he, [Page 170, Vol. I.] "that, at the epo-"cha of this tremendous catastrophe, the sun,

"deviating from the ecliptic, advanced from south "to north, and pursued the direction of one of "the meridians which passes through the middle "of the Atlantic ocean and of the South sea."

This hypothesis is founded upon that of the prolate figure of the earth, and appears, to us, neither more nor less visionary than many others which have preceded it. We shall not, however, follow our author into the mazy paths of conjecture, nor attempt to disprove what never has been, nor ever can be proved. The various strata of shells, and other marine productions, which are found in the bowels of the earth, at very great distances from the present site of the ocean, are, to us, a convincing proof that such an event did actually take place; and with this we shall rest satisfied, without endeavouring to explore the causes of it.

We now take our leave of *M. De St. Pierre*, with every sentiment of respect that is due to a person who means well. His errors, if not detected, may, perhaps, have a pernicious tendency; but his intention is commendable. Many of his observations will furnish us with useful les-

sons of instruction, while, from his mistakes we may learn to correct our own.

Here, in the first place, we may see the folly of those who misapply their talents in the invention of imaginary theories. Speculations of this kind naturally tend to mislead and bewilder the understanding, and frequently become the sources of error; but they seldom, or never, promote the cause of truth, or assist in the attainment of useful knowledge. A partiality to visionary theories has attached to some of the greatest geniuses of the last age; striking instances of which we have in *Kepler*, *Des Cartes*, and some others: while the speculations of *M. De Saint Pierre* evidently shew that men, in the present day, are liable to be misled by the same passionate attachment. Experiment and demonstration, founded upon fact, are the only means of acquiring physical knowledge; by the help of these, effects may be traced to their causes, and the mysteries of nature unfolded; while those who pursue the contrary course, assuming, as causes, systems of their own creation, or admitting principles purely metaphysical, whereby to explain the various effects which we observe in nature, will undoubtedly fail

in their attempt, and involve themselves in the labyrinths of perplexity and error.

In the study of nature, it is not only necessary to pursue the right path, but we must proceed by gradual and cautious steps. Perhaps there is nothing that tends more to check the actual progress of knowledge than precipitation in the pursuit of it. Some there are, who, taking for granted every thing that has been demonstrated, or even barely asserted by others, set out from an improper point, and hastily attempt to gain the summit of knowledge, while they remain ignorant of its rudiments. Those who begin their studies of nature in this way, seldom attain the real object of their pursuit; but, on the contrary, frequently fall into error, and become the dupes of some imaginary theory: nor is this to be wondered at, for with equal propriety might a geometrician commence his studies with the fifth book of Euclid. Those who wish to improve in natural knowledge, must set out with first principles, and carefully examine every step they take. For want of this, many who assume the character of philosophers, seem not to understand the first laws of nature. Thus, for instance, some persons misunderstand the inertia, or passive nature

of matter. They will, perhaps, admit that a body at rest can never put itself in motion; but they will not allow, that, for the same reason, a body in motion must continue in that state of motion until it meets with some obstruction. In order, therefore, to supply the supposed defect, they substitute some imaginary agent, the ideal operations of which being carried through all their speculations, naturally lead to visionary theories, and tend to establish them in error. Thus is the simplicity of nature obscured, and the progress of real knowledge impeded.

The studies of nature may also be drawn into an improper channel, by pursuing our speculations too far. Where demonstration fails, our *real* knowledge ends:—all beyond is mere hypothesis. We would not insinuate, that, at this point, our speculations should terminate; but the mischief is occasioned by confounding demonstrable truth with hypothetical assumption. Between these a clear distinction should always be made; for when an hypothesis appears to be unfounded, all those truths which are blended with it will also appear doubtful, to some persons at least, even when such truths are supported by actual demonstration.

The study of nature is sometimes perverted by a false application. By some, it is set in opposition to religion; and by others, it is made to give way to their particular religious sentiments. Both these extremes should be avoided, and the truth steadily pursued, while our judgment remains free, and unbiassed by extraneous considerations. To this purpose we have an excellent observation made by one of our best mathematicians:* "While," says he, "we guard against "atheism, and opinions that approach towards it, "we ought likewise to beware of listening to su- "perstition; which discourages enquiries into na- "ture, lest by having our views enlarged, we "should escape from her bonds, and our discove- "ries should weaken some darling tenets. If those "tenets are true, they will rather be confirmed by "our enquiries; and if they are false, surely it is "better they should be detected. We may pur- "sue truth steadily, secure that it will always be "found consistent with itself, and stands in no "need of the jealousies and dark suspicions of the "superstitious to support it; in whose hands truth

* Maclaurin:—Account of Sir Isaac Newton's Discoveries, Page 5.

"itself is apt to suffer, by the base alloy they mix "with it, and by the detested means they have too "often employed to maintain so incongruous an "union. The philosophers who have been de- "voted to so mean views, have never failed to "expose themselves to just ridicule, without do- "ing service to the cause which they espoused." He goes on to observe, that some pretended philosophers have descended "so low as to adopt the "folly, or rather impiety, of astrologers, in de- "riving the good or evil that happens to man "from the propitious or malignant influences of "the planets. True religion requires no such sa- "crifices; nor are its interests advanced by feign- "ing philosophical systems purposely to favour "it: for, when we afterwards find these to be ill- "grounded, we may be in danger of falling into "scepticism."

We shall only observe farther, that the study of nature, if properly regulated, will lead us to its Divine Author; it will excite in us a desire to advance his glory, and to promote the real happiness of mankind. To these objects, which will ever be inseparable, the views of the philosopher are, or ought to be, constantly directed, and in the pursuit of these he will always find ample

scope for the exercise of his abilities. The man who sees the importance of these objects, will not waste his time in frivolous speculations, but will embrace every opportunity of employing his talents in such useful inventions as may be conducive to the welfare of his fellow-men. He also who, in the study of nature, contemplates the perfection of the Deity, and at the same time takes a comparative view of the present state and condition of mankind, may discover that, in the attainment and completion of human happiness, something is wanting which nature cannot supply. He may see that a revelation of the will of God is necessary, and will be led to examine the authenticity of that revelation which he has given us. He will duly appreciate the evidence upon which christianity is founded; while his views of nature will preserve him from atheism on the one hand, and from superstition on the other: and, under the influence of these considerations, he will regulate his conduct by the precepts, while he endeavours to promote the interests, of real religion.

POSTSCRIPT.

SINCE the foregoing pages were written, it has been intimated, by some who have seen them, that our author's theory of the tides is not clearly refuted. To us, that theory appears to need no refutation; because the arguments, if arguments they may be called, which he adduces in its support, flatly contradict each other. Those who have any doubts upon this subject, if they will strictly examine it, will find the hypothesis of *M. De Saint Pierre* insufficient to account for the most ordinary phenomena.

The approach of the sun to the meridian cannot produce the daily tide; for, in the polar regions, the power of the sun, to excite heat, is nearly the same at all times, because the variation

in altitude is almost insensible. But, even if it could produce the tides of the day, those of the night cannot be ascribed to the same cause ; neither can those which happen at six o'clock in the morning and evening, when the sun is ninety degrees from the meridian.

It is equally impossible, from this theory, to account for the spring and neap tides, or for the regular daily variation in the time of high water. Our author, indeed, has erroneously supposed that the rays reflected from the moon produce a sensible degree of heat, and increase the "fusion of the polar ices;" and this, he says, causes the spring tide which happens at the full moon. But the cause of that which happens at the change he has not told us, when none of the rays, reflected from the moon, fall upon the earth.

At the time of the equinoxes, also, the action of the sun upon the poles is nearly equal, and therefore, according to our author's theory, the tides at that time ought to be the least; but it is well known that the tides at that season are much greater than in the height of summer, or the depth of winter.

From these considerations, the fallacy of *M. De Saint Pierre's* hypothesis must evidently appear. Those who, after comparing his system with the phenomena, can still adhere to a theory so unfounded, will not be convinced by the most direct and positive demonstration.

THE END.

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