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———" Quæ non fecimus ipsi
Vix ea nostra voco."
Ovid.

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REVIEW.

Geometry without Axioms. By T. Perronet Thompson, Queen's College, Cambridge. Fourth Edition, 1835.

The study of Geometry, rising, like most other sciences, out of utility, was at a very early period fixed upon for its value as an exercise of the reasoning powers: nor has all the brilliancy of modern analysis ever displaced the earlier science from its claim to pre-eminence in this respect. Not as though its reasonings were more rigorous than those of Algebraic Mathematics, nor as though the latter did not vastly surpass the former in the efforts of inventive genius and subtle discussion: but because the subject on which Geometry treats is far more popular in its character, than any of the higher branches of Analysis can be made. But few can afford the time and application needed to master these latter, nay, to gain an apprehension even of the definitions, so far as to conceive the spirit of the method: while all have a more or less accurate notion of spheres and cubes, angles and distances. The subject matter is familiar, (if it be but treated as such) and, like the first principles of Algebraic equations, affords a method of calling the attention to accurate processes of reasoning; without so filling the young student's time, as though all were to be professed mathematicians.

When such is the object for which Geometry is pursued,—not for the sake of its results, but for the sake of its processes,—we cannot easily be hypercritical upon the soundness of the reasoning. That which would be mere cavilling, in review of a practical treatise, may constitute valid objection against a treatise which is proposed as a model of perfect reasoning. In such case, if the link of the argument be broken, the error or defect *should be avowed*. To hush up the matter, and pawn off imperfectly connected propositions as demonstratively proved, is like training a student in the habit of smothering his doubts, and taking proof for granted. Perhaps in this matter geometers have been far less honest than they should have been. It is certainly strange, that when *geometrical*, as opposed to *analytical*, is popularly apprehended to mean *rigorous* as opposed to *lax*; possibly no algebraic treatises have flaws so fundamental as those which exist in the common elements of Geometry. And what is worst, geometers

West of England Journal of Science and Literature 1.1 (1835): 68-82.

are prone to disguise their incapacity to prove what they ought to prove, under the specious name of axioms, which they allege to be self-evident truths.

A very slight reflection must show every man who opens a geometrical treatise, how many propositions are demonstrated by geometers, which are yet to him quite as self-evident as those which they style axioms: therefore he will at least expect some reason assigned, why some more than others should be thus designated. But it needs no great insight into the matter, to perceive further, that the setting up of axioms is *a mere afterthought*: that is, no geometer calls a proposition self-evident, because he intuitively discerned it; but because, after trying, he failed to prove it. Further: it is easy to see, that of half a dozen propositions equally self-evident, the geometer selects arbitrarily which shall be his axiom: one will do as well as another; but one or other he must have. This appears exceedingly like a *petitio principii.* If done openly, with an avowal that it should not be so, the student has fair warning: but when all is smuggled under fine words, axioms, postulates, self-evident truths, &c. a mist is thrown over the principles of abstract science, and false philosophy is so far inculcated.

The only circumstance under which there appears the least pretence for axioms, is, when a definition is notoriously in defect. We all know that there must be some stop to defining. If we be asked to define the terms of our definitions, the matter may be pushed back and back, till we are in inextricable confusion. We are then to expect terms which involve ideas so easy and so primary, as that he who alleges them to be incapable of definition, will say nothing unplausible. We would give in illustration the words Quantity and Equal. If a mathematician finds it necessary to reject all the definitions that are offered of these words, on the ground that the words themselves recur in the very definitions alleged; he may think himself forced into another track, viz. to lay down a set of propositions concerning the words in question, [as, quantities which are equal to the same are equal to one another, &c.] which propositions, taken together, shall adequately restrict and settle the meaning of the words. In this view, a set of axioms are substituted for a definition, precisely as a set of functional equations for the explicit declaration of a function. Thus, if the defining of Napier's logarithms were attended with any difficulty, the two following *axioms* would amount to a definition.

$$\log x + \log y = \log (xy) \ldots \text{for all values of } x \;\&\; y \ldots \text{and}$$
$$\log. 2{\cdot}7182818 = 1.$$

But of course we are here beset by the difficulty of determining, à priori, whether we have given too much data for the result; or in algebraic language, whether our equations have a possible solution. And this objection renders it quite unprofitable to adopt axioms concerning any thing that is shortly to be assumed as in substantive existence: for we have afterwards a new process to secure ourselves from fallacy. Thus, though we may, if we please, use axioms to fix and ascertain the idea of *straightness*, it will remain to be proved that there is between every two points one line and one only conceivable, to have the property of straightness.

But when a geometer professes to define all his terms, he has not the shadow of a pretence to put forth axioms. If the definitions are sound, they are infallibly adequate to prove all truths essentially connected with them. If the definitions be inadequate,—descriptions rather than definitions,—it should be avowed. Indeed the words Equal and Quantity are not ordinarily defined by geometers; hence the axioms which relate to these terms are bearable enough: (though they belong to the earlier theory of Quantity and Number, on which all Mathematics is based; and not specially to Geometry:) but those axioms which involve the terms *line*, *angle*, *straight*, &c. are nothing but unwarrantable assumptions, as all these have been (ill or well) defined.

It is easy to see the analogy between axioms in pure science and laws in mixed science. The only reason why a treatise on Hydrostatics needs to be based on the experimental laws of fluids, is because the writer cannot or will not give a definition of *fluidity*. If he does give a definition, he turns his treatise into pure science; but it is possible then that he is amusing himself with writing on that which does not exist. A metaphysician might yet arise among us, who should not unplausibly maintain, that Geometry, though it treat not of substantial bodies, yet as it treats of space, which is a something with which we have acquaintance through the senses, ought philosophically to be regarded as one of the *mixed* sciences. Such a one would not be inconsistent in making simple experiments the basis of his reasoning: and his justification would be in this, that the words space or solidity are undefined or illdefined: a circumstance which seems most remarkably to escape notice.

We are led to the following observations by the perusal of the little book which is named at the head of this article. The number of past fruitless attempts to get rid of the twelfth axiom of Euclid, ought not to make us look with contempt on new efforts, until some one has shown that it *cannot* be proved. But our present author, in his fourth edition, which we have before us, has not confined himself to the much vexed question of parallelism: he has spent much labour on the definitions of the straight line and plane. And what gives new interest to his treatise is, that he has set out on an entirely original method, making the discussion of certain properties of the sphere precede the definition of the straight line and plane. This makes the book peculiarly worthy of notice, whatever judgment be formed of it: and we propose to set before our readers some account of his object and method, before making our own remarks on the execution.

The object proposed is to prefix or insert propositions to the first book of Euclid, in such wise as to make the argument continuously logical, without employing axioms. The defects are manifest. A straight line is defined indeed by Euclid, but so defined as to be nowise clearer than before; and Euclid himself never appeals to the definition, but to his axioms. This is a less guilty fallacy than that involved in his definition of the plane, in which an assumption is smuggled; since he proposes more than enough data for the generation of the plane surface. To remedy this, Mr. Thompson has an "Intercalary Book,' or more properly, an "Introductory Book," ending in the establishment of that property of the plane, which brings us from our airy flights on to the terra firma of plane Geometry. He proceeds to dovetail the rotten parts of Euclid's first book, but of course with the twelfth axiom chiefly in view. After various minor flourishes, he concludes in an appendix by summing up and refuting all other proofs that have been offered of the main principle of parallels.

His *mode* of proceeding is as follows.—Equality of distance is determined by means of supraposition of points even before the straight line is defined: it is then easy to define a sphere. He proceeds to prove his first important proposition, that *spheres touching externally touch only in a single point.* It readily follows that this point *lies evenly* between the centres: that is, turns about itself without change of place, if the two spheres being united as one mass revolve together about the two centres. Mr. T. does not use the word *evenly*; but it is very appropriate; and, so explained, makes Euclid's definition of a straight line adequate. (Such is nearly Mr. Leslie's view.) It follows, that by altering the size of the spheres in contact, the point of contact is made to generate a path, connecting the centres, every point of which lies evenly betwixt the centres; and this is called a straight line. Out of this definition instantly flow all the primary properties of the straight line. Mr. T. labours unnecessarily at them.

The second proposition of difficulty is, to show that *intersecting spherical surfaces coincide only in a circle;* which is virtually equivalent to Eucl. I. 8. that the angles of a triangle are determined when the lengths of the sides are given.

His third difficult proposition is to generate a plane, and to show that his plane has the property which Euclid makes its definition. And so ends the Intercalary Book.

There is moreover introduced into Euclid's First Book, to prove the twelfth axiom, fifteen pages of close print, in this fourth edition. Diagrams occupy a portion of the pages, and our author's style is that of over full reasoning; leaving not even the very obvious steps and reasons to be supplied. The quantity of matter is not therefore so formidable as might appear: yet we must call it very hard and unreadable. The proposition however which he is aiming to establish, is this: that "if the angles at the base of a tessera be acute, the angles opposite to the base are not right." We should add, that by a *tessera* he understands, a trapezium which has a diameter perpendicular to one of its sides.

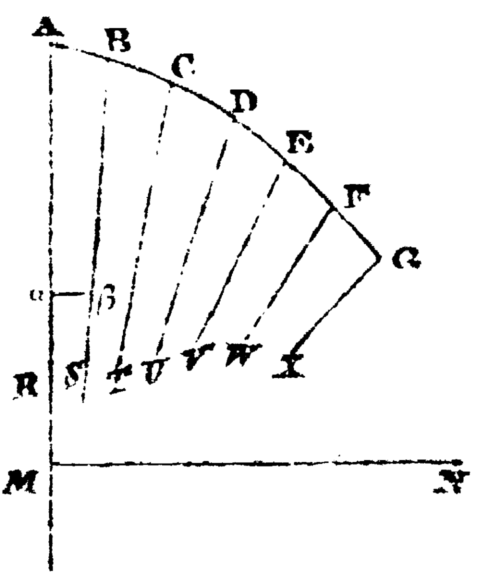
We have now to remark on the character of his reasoning, in these his four main propositions. The *first* of them has four cases; that spheres cannot touch externally, neither in a surface, nor in a self-rejoining line, nor in an open line, nor in isolated points. The proof of the first case is virtually grounded on the principle, (we fear we ought to say *axiom*!) that if any portion, however small, of a body's *surface* be immoveable,—as if for instance it be glued down to another surface that is immoveable,—no oscillating motion, however small, is conceivable in the former. We suspect Mr. T. would have bantered Le Gendre very cleverly for any such assumption, telling him that for aught he knew, an axis of rotation might *have breadth*. Did it ever occur to Mr. T. to inquire whether he could *prove* that it cannot? We do not say he cannot: but if he can, it should be prominent: while at present he labours hard in giving reasons where we want none, and here he does not at all clearly tell his reader what he is assuming, or why. We had written various objections to the proof of the three remaining cases: wherein we exceedingly disapprove of the vague and even unintelligible language of *above* and *below*, before we have defined *straight line* or *plane*, and when they cannot be changed into "this and that side of such and such a surface:" and equally do we disapprove of his arguing that "the line C D cannot turn *its face* (!) to all sides in succession without change of place;" more especially when he has not yet proved that C D may not be such a line as we afterwards call *straight*. His last case is proved only when there are *two* isolated points, and fails when there are *three*. We had accumulated yet more objections, when we met with the Quarterly Journal of Education, No. XIII., in which it is remarked that the proof applies quite as well to intersecting spheres, as to spheres in contact. This remark is obviously and instantly fatal to the last three cases: but we think slight verbal changes would enable the first to evade the charge.

We are sorry to think the failure of this proof is so decisive: for the proposition is to us the most interesting in the book. It seems an original thought to prove directly from first principles, that the external contact of spheres is but in one point; and we are not at all in despair that it may be done: but if done, it must be done by perspicuous and easy proof, or we shall not value it. Out of this proposition instantly flows the inference, (which we are surprised at Mr. T.'s not drawing) that a straight line is the shortest between two points, and is therefore *the measure of distance*. For it is manifest that no line connecting the centres can be the shortest, (or as short as any) unless it pierce both surfaces in the point of contact. But while we think that Mr. T. has decidedly failed of proving his proposition, he has usefully set forth what is meant by three or more points *lying evenly*, and called us to consider whether the early introduction of the sphere would not tend to perfect the doctrine of the straight line.

On his second main proposition, (his eleventh) we have to remark similar defects as in the former. It has two cases, of which the former is doubtfully proved: the latter we think is certainly not proved: for he does not show that the points M, N, O, P may not be in one straight line; and this is essential to the proof, if we rightly understand it. Yet we confess it is very hard; so hard as to make us doubt every thing; where in deed all the hypotheses are so monstrous, that the mind is bewildered in the midst of so many false lights. When it is hard to say what is more manifestly absurd than what, the *reductio ad absurdum* is a most dangerous proof; needing a perpetual effort of most painful vigilance, even from practised mathematicians.

His third main proposition is satisfactory, though excessively tedious. But in fact, there is no difficulty at all in the doctrine of the plane, so soon as Euclid I. 8, has been established; which is virtually Mr. T.'s eleventh proposition, just noticed. For if a right angle be first defined, (which is not difficult) we may generate a plane by supposing one leg of the angle fixed while the other revolves round it; then the general property of the plane is readily established by a method analogous to Eucl. XI. 4.

We now feel ourselves bound to attempt to convey to our readers some notion of his method of treating parallels, the more especially as other reviewers decline the task, which is not an easy one. Anxious to do him justice, we have diligently studied what he rightly, but funnily calls, "the pinch and nip" of the argument, and we would fain put to him some *viva voce* questions, where the slippery materials appear to elude the grasp of our forceps. We shall, without apology, throw his matter into the form that strikes us as most intelligible to our readers.



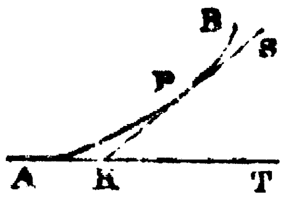
Conceive the equal angles R A B, A B S in a given plane, which *may* be taken so small as that A R, B S meet towards R and S: while if they be right angles, we know that A R, B S will meet neither way. The question to be decided is, whether they will certainly meet, if the angles be *ever so little less* than right angles.

If possible, let it be otherwise, (for precision, we might suppose the angle the least possible, consistently with the condition of the lines not meeting.) Suppose the system to turn about B S, till R A B imprints its counterpart T C B on the plane, at the opposite side of B S. Similarly we may produce the systems T C D U, U D E V, V E F W, W F G X.. &c., *ad infin.*—all counterparts of the first. It follows also that C T never meets B S, nor does D U meet C T, nor E V meet D U, &c... so that *à fortiori*, no line in the series as F W or G X can meet A R. This puts the absurdity of the original supposition in a striking light before the eyes: but this is not to give reasons for the thing. Let now M N be perpendicular to A M. It is then easy to take the distance A M so short, that M N may meet the crooked path; but if A M be great, it is not so obvious that this will happen. In reality we know it will not; for we know that that path will bend round and cross A R—but this is excluded by hypothesis—so that the argument is a dilemma; *either* the path shall cross A R, and then all we want to prove is conceded; *else*, however distant M may be, I SAY, M N shall meet the path. Here is the pinch and nip! "For else, let A M be the least distance such that M N does not meet it. Then A G, M N are infinite lines asymptotic to each other; of which the latter is straight and the former is *convex* towards the latter, which is absurd." Thus we have ventured to supply our author's reasoning. It seems to us, that the effect of his labours is to reduce the twelfth axiom of Euclid to another much more obvious, and which perhaps may be proved, but which nevertheless he has not proved; that "a crooked line cannot turn its *convexity* towards a straight line which is *asymptotic* to it." If this be granted him, he proves that any tessera A B β α [cut off from A R and B S, A α = B β, and join α β] which has the angles at A and B acute, cannot have the angles at α and β right; which leads by easy steps to Euclid's twelfth axiom. We conceive he is still engaged with the second part of the dilemma. *Either* the crooked path meets A R, and so the twelfth axiom is granted outright: *else* he proves this and that about the tessera, whence ultimately the twelfth axiom is still made to follow. If we be right, Mr. T. ought not to have isolated his propositions as he has done.

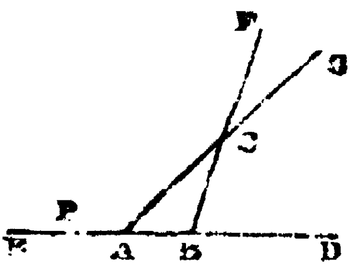
And this strikes us as a sufficient reply to the startling objection of an ingenious reviewer,* that for any disproof which Mr. T. has offered, the point D might coincide with A, E with B, F with C, &c. . . . for *all conceivable values* of the angle and length. We are not surprised that Mr. T. did not foresee so extraordinary a thought, and we give credit to our contemporary for the inventiveness of it. Doubtless it would vitiate all Mr. T.'s after-reasoning: but it would amount to a direct concession of all that he is seeking to prove; viz. that the path must meet A R a second time. The objection however was just, as levelled against a proposition, which ought not to stand (we conceive) as a positive truth, but as on a hypothesis still kept up till the chain of argument is complete. The reviewer, though acute and clear, had evidently a strong prepossession against the possibility of Mr. T.'s success; and was deterred from reading further, when he met

* Quarterly Journal of Education, No. XIII.

such a fallacy. We believe the tedium of Mr. T.'s proofs was the true dissuasive with him, and we acknowledge the power of such a sedative.

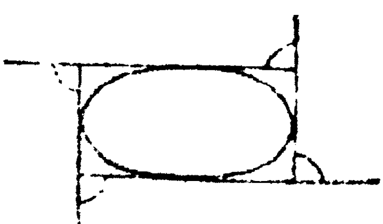


The attempt to prove that a line cannot turn a convex front towards its asymptote, leads directly to the consideration of deflections and deviations. Some of our readers may exercise their wits in confirming or disproving the following:—If a point P describes a line A P B which perpetually deflects from the tangent towards the same side, and A R T, R P S are tangents at A and P, it is ordinary to assume the $<$ S R T as the measure of the total deflection performed through the arc A P: and this, by reasoning from first principles, without any reference to the doctrine of parallel lines. Is this warrantable or unwarrantable?



Now consider a similar case. Let A B C be any triangle: prolong A B to D: I say, the $<$ C B D $= <$ A $+ <$ C. For let B A, B C, A C be prolonged to E, F, G: and let any point P describe the crooked path E A C F. It makes at A the deviation G A D, and at C the deviation F C G; and in the rest of its motion does not deviate at all. But in describing C F, it is manifest that it has deviated from the straight line E A D by the angle F B D; therefore,* *since the total deviation is equal to the sum of the partial deviations*, the $<$ F B D $=$ $<$ G A B $+$ $<$ F C G; or $<$ C B D $=$ $<$ C A B $+$ $<$ A C B: which was to be proved.

We are sure Mr. T. would desire no *mercy* shown to any of his proofs, but only fair play. Indeed he handles his antagonists very roughly; (for all his predecessors in the attempt to solve the problem of parallels appear as his antagonists): and to say the truth, we do not admire the decision with which he puts his extinguisher (in his Appendix) on some attempts which have much interest for us, as hopeful and admitting improvement. We do not assent to his condemnation of M. Bertrand's proof. But we especially have in view Legendre's analytical proof,—not that we approve the introduction of algebraic considerations:—we speak of the fundamental principle, which, to say the truth, Mr. T. seems to us not at all to understand. Indeed he himself complains, that when they insist on the "angle being a portion of a finite whole; and the straight line, of an infinite whole;—there is no reasonable or demonstrated connexion between the



* Playfair's proof assumes more than this; viz. that the total deviation in a self-rejoining line is four right angles; which seems equivalent to assuming outright what he is proving, that the four exterior angles of any figure together equal four right angles. Yet it may furnish suggestions.

facts alleged, and the consequences assigned to them." Would Mr. T. excuse our attempt to clear the reasoning? as we think the metaphysical question worthy of attention, and moreover since it is very easy to deduce from it all that we desire, *without* appealing to algebraic notions. We conceive the argument to stand thus:—"Lines cannot be calculated from angles alone: FOR when angles are given, no linear unit is hereby given. It is OBJECTED that this proves too much; for when lines alone are given, no angular unit is hereby assigned; yet angles may be hence calculated. We REPLY, Not so; of angles there is an extreme value, viz. the sum of two right angles; which, for any thing which *à priori* we know to the contrary, might virtually furnish an angular unit; and this *à posteriori* we positively know to be the case: but contrariwise, a straight line has no maximum or minimum value. The cases then are not parallel, and the objection falls to the ground." Whether the original argument be valid, is to be considered again; but assuredly the reply which Mr. T. thinks so little to the purpose, is directed very accurately against his insuperable objection. But he, as Mr. Leslie, seems to have much spite against this principle. That lines *cannot* be calculated from angles, is a proposition notoriously true; and the truth of which we readily learn, without wading through the properties of triangles. Why should he be incredulous as to the possibility of giving a direct demonstration of it from first principles?

But he meets us on another ground: "The *substantial inference*," says he, "is, that they have confounded the quantities which Euclid in his book of Data would call *given*, with the quantities which must be employed as elements in actual calculation." And here Mr. T. himself makes a great blunder: and we are disposed to think he has more acuteness in detail, than sound philosophical views of the science. His very example might have confuted him. If a, b, c, are the sides of a triangle, and A, B, C the opposite angles: we know c is determined when a, b, C are given. "Yet," argues he, "c must be collected from the formula—

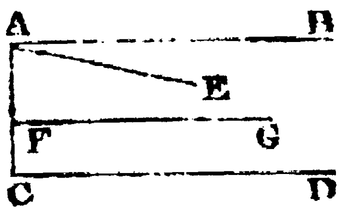
$$\tan\frac{A-B}{2}=\left(\frac{a-b}{a+b}\right)\tan\left(90^\circ-\frac{C}{2}\right)$$

Here start up among the *practical* elements of the calculation *two straight lines* in the shape of the tangents of two arcs," &c. He would seem to forget that c is at once determined by the formula—

$$c=\sqrt{a^2+b^2-2ab\left(1-\frac{C^2}{1.2}+\frac{C^4}{1.2.3.4}-\&c.\right)}$$

but to found an objection to Legendre's reasoning, on the idea that the tangents and the cosines in the tables are *straight lines*, is truly extraordinary—as though any linear unit were determined by them. With equal want of consideration he compares a hyperbolic arc with a straight line, as though "new elements, such as *major and minor axes* might start up in the case of the straight line." He forgets that we *already know* a straight line of unlimited length to be determined by *any two points* in it. Give five points in a hyperbola, which is only just enough, and we shall find no "new elements start up." But in fact any distinction between the *data* which *determine* the value of quantities and the *elements* whence those values are calculated, is utterly untenable and unphilosophical. Suppose the central angle of an elliptical sector is given, and the axes of the ellipse: if these are enough *data to fix* the length of the arc, then they are *sufficient elements for its calculation*, however many be the intermediate shifts by which the ultimate result is obtained. If Mr. T. can disprove this, he will give a fatal blow to the whole of the modern analysis. This is in fact its *fundamental principle*, wherever we reason concerning functions whose form is as yet unknown.

Before quitting this head, we will show how concisely the doctrine of Parallels may be proved from the principle that "lines cannot be calculated (nor therefore be determined) by angles alone." Let A B, C D be each perpendicular to A C; and let A E divide the angle C A B. If then A E, however far prolonged, does *not* meet C D, shorten the distance A C, and it is manifest that there is some least distance A F, in which C D, coming into the position F G, (still perpendicular to A C) is a true asymptote to A E. Then the distance A F is determinate: yet there are no data to fix it but the angles at A, which is absurd.



To remark generally on Mr. T.'s philosophy; while he aims at exceeding precision, we often find him lax, or as we judge, erroneous. He has retained the old definition of a solid, "that which has *length*, *breadth*, and *thickness*"—containing three words positively unintelligible till we conceive of three rectangular axes. His definition of equality is exceedingly questionable, as it involves us in the well-known paradox, that as a circular area, however cut up, would never fit into a square, therefore no square can be equal to a circle. Yet it is on this definition that he rests for abolishing all the axioms that are not specially geometrical. He indeed *assumes* that "all surfaces are of one kind," as regards magnitude; without commenting on the difficulty. We have already complained of the laxity of *above* and *below*, and of the *faces of a line* that may be straight, or may have double curvature, for aught we know. We disapprove also of his defining an angle to be a plane surface, when he reprobates all inferences drawn from it. "All references," says he, "to the equality of magnitude of infinite areas, are intrinsically paralogisms." We are not at all convinced of this. Yet if an angle be a surface, it is an infinite surface: and when one angle is said to be double of another, it must mean that one infinite surface is double of another infinite surface. It is surprising that he should think any thing gained by the change. In his Appendix, however, we were chiefly startled by the inconsistency of a passage which seems to show that he regards Geometry, as a science whose *elements* may possibly depend on the planetary system. He asks how we could tell *à priori* that "Nature," instead of making the angles of a triangle equal to two right angles, had not made their sum depend on the relation of one of their sides to *some grand modulus existing in nature, suppose, the radius of the earth's orbit.* If this be for one moment even conceivable, all Geometry must be based on experimental laws, and nothing then can be so unreasonable as Mr. T.'s aversion to appeal to the senses. We trace however the same indistinctness of apprehension, when he complains of Euclid's definition of a straight line, as being "an identical proposition." Had he said, "the definition itself needs an explanation," we should assent. But his tone seems to show, that he considers identical propositions as nugatory. *All* propositions in *pure* science are as truly identical as the binomial theorem and the multiplication table: yet they are not therefore nugatory. Either geometrical propositions are such; or Geometry is a *mixed* science, and must be based ultimately on sense. We can find no intermediate view.

We further feel that Mr. T. inadequately appreciates the force of the objection; that "a proof is too difficult;" and again, that "the parts of a subject are straggling." He says: "It may be a great irregularity, that *nature* should not have framed the elements of Geometry, so as to present a concinnous whole;" but "we ought not to quarrel with *the dispensation.*" If indeed our object were to ascertain the certainty of a practical truth, of course we should be thankful to have *any* demonstration: and where the truth is such as no man is aware of till it is formally proved, we do not complain if the proof be difficult. But when the mind has a direct and distinct perception of a truth without proof at all, it cannot be the natural method to lead us through many tedious and intricate ways to arrive at it. Let an intelligent person unversed in Mathematics be asked "whether spheres, if very large, might touch in more points than one: and probably his first conception will be that they may; because he unawares appeals to experience, by trying to conceive how it would be to his eye. But when reminded that it is owing to the roughness of the surface and the compressibility of the material, that they come into contact more exactly: he soon convinces himself that *perfect* spheres would touch but in one point. It is then clear, first, that the truth is not intuitive; next, that it is not learned by experience; thirdly, that it is not gained by wading through a whole book about the triangle and circle, nor yet through Mr. T.'s long proof. The mind does certainly find some short cut, connecting the definition of perfect sphericity with the property alleged: and by analysis of the vulgar reasoning, the philosophic geometer should endeavour to arrive at the natural method. By this not only would the study be made more pleasant and satisfactory, but more light would be thrown on the processes followed by the mind. With such views, we much deprecate the artificial system and unbending style, pursued in most geometrical treatises. Indeed we suspect that Mr. T practically mistakes formality for rigour; and thinks that his proofs are more cogent, because he puts in premises which in ordinary reasoning we suppress. If a man reason thus: "Good statesmen are valuable to a country: Lord Liverpool is a good statesman; therefore he is valuable to the country:" do we think his argument the sounder for the affected formality?* It may be requisite now and then; but is disgusting in the extreme, and painfully perplexing, if much carried out. The moment we quit Geometry, we give up the innumerable formalities of etiquette therein insisted on, (and to which Mr. T. seems much attached,) nor is it conceived that we lose in rigour. In fact† all illustration, so that it be marked off *as* illustration, tends to rigour, by giving more vivid apprehensions: while it relieves the tedium of dry reasoning. But as regards the *order* of a geometrical treatise, nothing, we apprehend, need be said of this *à priori*. Let only the natural methods of proof be well analyzed, and some luminous order will assuredly result; though it may be of a kind that we did not anticipate. But we do think that the appearance of complete disorder, is a most serious presumption against a geometrical compendium: the more especially since the value of mathematical study is *quite as much* in training the mind to a habit of clearness, as in the bare apprehension of what is good and what is bad logic;—nay, we should say, *much more*. It is a valuable habit, to discriminate quickly what is easier, what is more difficult; what is best proved from what; in what order truths must be set before the mind:—but a geometrical work which is *just not* fallacious, may train us to the very reverse of this.

And this leads us to express our conviction, that no patching of Euclid will make his Elements a work worthy of the eulogiums that have been lavished on it. We do not complain that it is ill adapted as an introduction to modern Mathematics, (though that be quite true); nor that he has various petty flaws; nay, nor that he has fundamental defects; (for we are supposing them all mended up:) but the treatise will still remain disordered, crude, and formal. We will take this opportunity of throwing together reasons, why all pretence of making his work the text book should be abandoned.

The natural connexion of subjects is broken, first, by the strange introduction of ratios in the fifth book; secondly, by interweaving with his theorems problems of mere construction. We shall speak of each sepa-

* Our illustration is perhaps too strong. We only mean to say, that his proofs would gain clearness and pleasantness, and lose nothing in rigour, by being less diffuse.

† Mr. T.'s Scholia are very good; and his explanations of many terms would be serviceable to a beginner, *though not to a person capable of reading his book*.

rately. No good reason appears for treating on ratios in a geometrical treatise at all; but if any where, it should be in the introduction; because at present he is forced to delay to his sixth book what should have been partly in the first and partly in the third. It is objected that the fifth book is too hard for a beginner. But that is the author's own fault, for taking so extraordinary a method. Oh! but, say the advocates of this extolled book, it is so geometrical! and so philosophical!—We apprehend that if it be really "geometrical," this is a fault; for a treatise on ratios ought no more to be geometrical, than astronomical or optical. But if it be meant, that it is adapted to those who understand nothing of Arithmetic, then we reply that such will remain incurably ignorant of ratios, though they read Euclid fifty times. And this brings us to the boasted philosophy of the method, which we venture to describe thus. Having given a useless definition of ratio, (which R. Simson rejects,) he then gives his serviceable definitions of the phrases, *same ratio*, *greater ratio*, *less ratio*, *compound ratio*; in such wise, that the words *greater*, *less*, &c. have new senses,* or rather no sense at all as isolated words, any more than *cir* and *cle* of the word *circle*. Accordingly it becomes necessary to prove that "Ratios which are the same with the same ratio, are the same with one another." Thus the upshot of this treatise on ratios, is, that we do not and may not know what "ratio" means. For if we do know, then since greater and less and same (or equal) are well known terms, it is unlawful to lay down new definitions of *same ratio*, *greater ratio*, &c., and every proposition in the book that concerns proportion is vitiated.†

Again, Euclid has chosen to limit his proofs to figures which he can construct with rule and compass, for which two instruments he makes humble request in three postulates. The compass we willingly give him; concerning the ruler we demur, until he has shown how the straight line is generated. But all are aware that he might as reasonably ask for elliptic trammels or any other instrument, as for the ruler; in short, in constructing his figures, it is enough to show that his description of the figure contains nothing self-contradictory. His squeamishness here has entailed on him much disorder, besides the frequent introduction of petty problems,

* (Note by R. Simson on Euclid, V. 10.)—"It was necessary to give another demonstration of this proposition, because that which is in the Greek and Latin and other editions, is not legitimate: for the words *greater*, *the same*, *or equal*, *lesser*, have a quite different meaning when applied to magnitudes and ratios, as is plain from the fifth and seventh definitions of Book V." As usual, this editor makes sure that the error *could not* have been Euclid's own! *Theon* is generally the name on which he heaps the odium.

† R. Simson's additions at the end of this book are quite a literary curiosity. For ourselves, we cannot read the statement of Prop. K. with gravity. It contains the word *ratio* twenty-three times, as in school-boy days we remember to have counted. He says "they are frequently made use of by both ancient and modern geometers:" which we believe on his word; but what he called *modern* is not now modern.

most of which we could advantageously part with: some only should be turned into theorems, as in the fourth book. Further, in the employment of supraposition, he virtually uses another postulate, and when he approaches curvilinear areas, his system breaks down entirely. Thus, on Euclid xii. 2. R. Simson remarks, that to complete the proof we must insert: "For there is *some* square equal to the circle:" a thing true enough; but which rule and compass will never construct.

To do him justice, we must remember that he himself must have thought his fifth book unsatisfactory; for he treats ratios over again, and that numerically, in one of the books which we omit; and in another, he collects into a better c. all the scattered properties of figures.

But further; in Euclid's day no doubt the second book was necessary to Geometry. At present it is highly ungeometrical and useless; because we at once discern, that out of the infinite number of algebraic developments, it selects a few to demonstrate geometrically. A sufficient substitute for these, is, to show (what at any rate needs to be shown) how the areas of rectangles are numerically represented, after making the square of the linear unit our unit of surface. The twelfth and thirteenth propositions of this book belong rather to Trigonometry or to Algebraic Geometry, than to this department; and the fourteenth is a problem misplaced.

What then is needed to make Euclid's Elements a logical, well-ordered, and perspicuous whole, confined to its proper limits, yet adapted to the existing state of mathematical science? Strange liberties have already been taken with it. The seventh, eighth, ninth, and tenth books, are sunk in the darkness of allusion: the lengths and breadths and depths and heights in the eleventh and twelfth are seen in shadowy distance; but the student is out of breath and comes to a halt, before he reaches the "method of exhaustion:" the fifth book is very frequently given him in grace, and an explanation of the fifth definition is thought enough. The second book will vanish of itself by a different arrangement, viz. by splitting the sixth in twain, and kneading it up with the first and third. The fourth book consists entirely of problems, most of which are to be wrenched out from all the books; twisting some into theorems, ramming down others into an Appendix, or book of exercises. A better distribution of various subjects is much needed. *Similarity of shape* should form a separate section, and a single definition be given of the word similar, applicable to figures plane or solid, curved or rectilinear. *Areas* equally need to be systematized. *Contact* and *curvature* in the circle should be so treated as to prepare the student for following out the subject with other curves; in short, according to the enlargement of view which the moderns have thereby gained. *Circular arcs* and *circular areas* need to have something said about them, and what is said, should be connected and compact. We have not noted how large changes in the first book may be required for proving the twelfth axiom: but, if attainable, we desire more method in exhibiting the properties of triangles of and parallels. After all, it must be remembered that something introductory is needed, (if not Mr. Thompson's thirty-nine pages,) before we can get our passport to land on Plane Geometry at all. And after the most necessary of these changes, how much of Euclid's twelve books will be left? We are therefore sorry to see men of original mind, like Mr. T. undertake the ungracious task of mending up an old and incommodious structure. It reminds us of the knife which had had three new blades, and two new handles: indeed his Intercalary Book is so startling a contrast to Euclid, that the old Grecian would justly complain of modern innovations, to say nothing of Prop. XXVIII. A, B, C, D, whose weight strains the threadbare substance around them, threatening to make the rent worse.

F. W. Newman

Nautical and Hydraulic Experiments, &c. By Col. Mark Beaufoy, F.R.S. 4to. 1834. pp. 688. Vol. I.

This volume is a record of a great number of experiments made by the late Col. Beaufoy, upon "the comparative resistances which different solids, constructed upon the same base and perpendicular, meet with in moving through a fluid." The results of the experiments are given in a convenient tabular form, the tables being arranged according to the face of the solid presented to the resistance. The fifth table is one of comparison with the former, the solids experimented with remaining of the same breadth, but of greater length.

The work is magnificently got up, and contains a large number of plates and diagrams. We have thought it right to mention its appearance now; but on account of the great practical importance of the experiments, we shall defer a regular analysis of its contents, until the appearance of the two remaining volumes shall enable us to include the whole in one view.

Manual of Mineralogy. By Robert Allan, Esq., F.R.S.E., &c. 8vo. pp. 351.

Mr. Allan is known to the scientific world as the possessor of a mineralogical collection, second only in importance to that of the British Museum, and collected during a long period by his late father, under whose superintendence the materials for the present volume were compiled.

The Introduction, which occupies about seventy pages of the work, commences with very brief observations on the crystalline forms of Minerals in general, considered according to the system of Mohs. The various physical properties of Minerals are then treated of in the following order; Specific Gravity, Hardness, Colour, Transparency, Lustre, Streak, Cleavage and Fracture, Double Refraction and Effect on Polarized Light, and the Parasitic (Pseudomorphous) formation of Minerals. The Introduction closes with the method of employing chemical tests and the blowpipe, in the analysis of mineral substances.