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XXIV. Geometry and geometers

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diffusion product. These last experiments were made at an early period with another object in view, namely, to ascertain whether in closely related salts, such as the present sulphates of magnesia and zinc, the two salts might be elastic to each other, like the particles of one and the same salt, so that one salt might possibly suppress the diffusion of the other, and diffuse alone for both. The experiments lend no support to such an idea.

It appears from all the preceding experiments, that the inequality of diffusion which existed, is not diminished but exaggerated in mixtures, a curious circumstance, which has also been observed of mixed gases.

[To be continued.]

XXIV. *Geometry and Geometers.*
Collected by T. S. Davies, *Esq., F.R.S. and F.S.A.**
No. VI.

AS I have had frequent occasion to speak of Dr. Matthew Stewart in the earlier portion of these papers, it will be proper here to make one statement more.

From the character of his General Theorems, I have long entertained the opinion that Dr. Stewart had discovered a considerable number of Porisms besides those printed in his book, and the two he gave to Dr. Simson. The description of his MSS. given by Playfair, in his memoir of that illustrious geometer, led me to believe that it would be possible to make out not only the propositions but likewise their demonstrations, somewhat in the manner that I did with the Porismatic part of his General Theorems in the Edinburgh Transactions a few years ago. I consequently applied to an eminent mathematical archæologist to obtain for me information as to what had become of those papers, and whether they were accessible for such a purpose. In a short time he sent me the copy of a letter from the proper custodian of the papers, decisive on this head. *They are all destroyed—deliberately burnt*; and not only his, *but likewise all the MSS. of his son Dugald Stewart.* Into the motives for this act, which its perpetrator offers, I will not enter; and shall only state that he was a descendant of those two men whose writings he has thus irretrievably destroyed. I have thought it desirable to put upon record this fact: for though public indignation will not restore the lost treasures, it may prevent others from imitating the incendiary.

A great portion of the correspondence with Nourse and

* Communicated by the Author.

his successor Wingrave is of the most ordinary business character. This might be expected. A few passages even in the dreary list of £ *s. d.* are not without interest.

For instance, *the price of authorship*:—Mr. John Landen thus writes (Aug. 28, 1758) to Nourse, after describing his "Residual Analysis:"—

"I would have it very elegantly printed in quarto, (with Wood or Tin Cuts) upon such paper and with such letter as my Lucubrations. If you chuse to purchase the copy you shall have it for less than I would take of any other person.

. . . . . .

"The subject is very interesting, and I p ... ne [illegible—presume?] a considerable number will be speedily sold; therefore ex pect you will not give me less than two Guineas per sheet. However I shall leave it to you to pay me according as it shall sell."

It proved here, as it often does, that an author is himself the very worst judge of what will "sell." Nobody but systematic collectors of classes of books, knows anything of Landen's Discourse on the Residual Analysis, except accidentally by mere name. The philosophy of Landen "never took;" and the truths delivered, or professedly deduced by means of it, were neither new nor in any way remarkable. Still the book was not without merit; nor the author destitute of very high mathematical powers. He was not, however, deficient in the *amour propre*; but, on the contrary, was remarkable for carrying out the principle to an extreme degree.

Dr. Gregory (who from being "bred and born" in the neighbourhood of Peterborough was likely to be well acquainted with the gossip of the place) has informed me that Landen was a man of "imposing presence and imperious manners." He was steward to Earl Fitzwilliam, for that nobleman's Northamptonshire estates. The then countess, who appears to have much disliked the bearing of the steward, described an interview between him and her liege lord, as that "between Lord Landen and his steward Mr. Fitzwilliam."

Landen was perhaps the only non-academic mathematician F.R.S., who did not join with Horsley, Hutton, and the other seceders from the Royal Society on the accession of Sir Joseph Banks to the chair. Philosophy in the Society had then degenerated into Faction: a sort of scientific imitation of Whig-and-Toryism. The contest was one of partisanship, and it was conducted in the true spirit of political party. The naturalists have censured the mathematicians, and the mathematicians the naturalists, for their conduct in that discreditable dispute. The question has descended even to our

own day; and it has very recently been mooted in the Philosophical Magazine. It was a scene which showed undeniably the decadence of the *philosophic spirit* in the Society, rather than the overriding of one branch of science by another; although it has been almost uniformly represented that the "mathematical sciences were ousted from the Society by the overwhelming influence of the naturalists." The terms themselves were *mere symbols of party*: but we are not to assume that mathematical science was excluded from the Society because Dr. Horsley was foiled in his aspirations for the Chair, and Dr. Hutton divested of the Foreign Secretaryship. The "little band" overrated their influence in the Society; and the time is come when some definite idea of their mathematical powers and pretensions can be formed, quite independently of factious prejudices. It would be well, therefore, to judge the question apart from all party considerations.

That Horsley had as good a claim to the Chair as Banks, there is no doubt: but he attempted to carry his purpose on fictitious grounds—as the representative of the mathematical section of the Society. That his claims, however, were not overwhelming, but only *comparative with those of his competitor*, no mathematician will now venture to assert. Every work he published is "completely shelved," and no one, I believe, reached a second edition. His *name* indeed is only remembered in scientific circles by his connexion with these unhappy disputes. That his supporters and fellow-seceders were so many Newtons and Halleys, who will assert, even though the names of Maskelyne, Maseres and Hutton were on that list? Waring, Milner, Landen and others, kept aloof from all share in such a partisan-system of enforcing the superiority of the mathematical sciences over those of observation.

So much has been said on the other side that it does not become me to speak upon it. I am no judge of the scientific merits of the actors in it. Of the long period of "misrule" which followed, I have only to say that something of the kind might have been expected: the reign of "naturalism" was the reign of actual conquest—the conquest of a faction bearing one symbol over another faction bearing another symbol. Perhaps the political condition of Ireland at this moment is only the same history on a larger scale.

I have been tempted into this long digression, from the consideration that it is high time that disputes of so long standing should be looked at apart from the symbols of the respective parties—symbols to which the actors on either side had little claim. It is certainly absurd enough that because

two sets of aspiring parties quarreled "over a bone" some eighty years ago, that men who cultivate two very difficult and ever-expanding sciences, should *now* look upon each other with jealousy, because those two factions assumed the names of these two sciences as the symbols of their factions. Even as regards the suppression or publication of papers in the Philosophical Transactions, it will be more frequently found that any impropriety has arisen from the influence of persons pursuing the same science than the opposite one—at any rate the alleged impropriety.

Another writer proposes to Nourse to take a work on his own hands, under the title of *Syntagma Analyseos: or a New Introduction to the Mathematics.* After a laudatory account of himself, he gives the contents, and concludes with the following:—

"As I often write to the Diaries, Magazines &c. under various fictitious appellations, I may thereby forward its sale by recommending or quoting it."

The method of indirect puffing was not unknown even then! Till I met with this letter, however, I had taken the name of the writer (Malachy Hitchins, Exeter College, Oxford) to be itself fictitious. His contributions (at least under his own name) are respectable for the time, though none of them bespeak powers far above mediocrity. I do not think that Nourse was taken with the bait of his "recommending" his own book; and indeed I have no knowledge of its having been published at all.

The following passage is only curious as showing a mathematician's notions of *amusement.* It is from a printed proposal to publish by subscription a "Complete Course of Mathematics" in 96 sixpenny numbers; the scheme of which was abandoned, and the MS. offered to Nourse, who also appears to have declined it.

"Even to those who peruse books for amusement, the author ventures to recommend this work, presuming that more real entertainment, much more genuine satisfaction must flow from it, than can arise from an insignificant romance or fictitious tale, which serve chiefly to vitiate the taste and corrupt the morals."

This is dated Newcastle 1770, and the author is John Davidson—a mathematician of great local note, and of some general reputation in those days.

A letter from Mr. Andrew Marshall, dated Dover, Oct. 9, 1773, contains a proposal to Nourse to take a translation of the first three books of *Simsoni Sect. Con.*, or a complete translation of the whole work, if Nourse should prefer it. The letter is valuable in one respect, as giving a reason for this proposal.

"It was at the request of Dr. Matthew Stewart and Dr. Williamson professor of Mathematics in the University of Glasgow, that I undertook that translation: and the reason why they interested themselves in it was, that they thought their colleges were not so well attended, while the students were obliged to read a latin book on so abstruse a subject, as they would be, was the text book in English and at a lower price."

The translation—at least so I judge, but at any rate a translation—was published in Edinburgh by Charles Elliott, in 1775. Nourse's name does not appear among the London publishers on the title-page. It had been well for English science if all the books that Simson and Stewart ever wrote had been printed in our own language.

Trinity College, Dublin, has been, perhaps, the most tenacious adherent to the use of Latin in its lectures, exercises and responsions. Amongst the works published for the use of that college was Dr. Hugh Hamilton's *Sectiones Conicæ*, 4to, 1758, in Latin. This work, however, though compulsorily read in college, was otherwise slow in its sale—much slower indeed than its great merit would have led us to expect. In a letter to Nourse in 1768, he says, "there were 600 copies printed, and about 100 of them remained" then in Dublin; whilst he speaks of 230 copies in the hands of Johnston, the London publisher. These last, with the copper-plates and copyright, he offers to Nourse, and ultimately appears to have sold them to him, with all else relating to the work, for two shillings per volume, though he says the print alone cost him more than three shillings per copy. He adds:—

"You were certainly right in supposing that the treatise would have sold much better had it been written in English, for Johnston told me two years after it was published that he was sure he might in that time have sold almost the whole impression had it not been in latin. This makes me imagine that when you get the Property of Work (and the copperplates which cost me 20£) it may be worth your while to publish a Translation sometime hence, a Person of tolerable skill would translate such a Book as easily as he could transcribe it."

He then goes on to mention the additions he would make

to it, in the form of an appendix, when translated. This, however, in a subsequent letter he proposes to replace by a few occasional scholia. Nourse appears to have sent him the translation in MS.; which, though so easy to make, he considered to be very faulty, and an amended translation in its turn was only somewhat less faulty. It ended in two Trinity men, who were reading for fellowships, recommended by Dr. Hamilton, being employed. Their remuneration was *ten guineas*!

The following is one of the most singular facts, perhaps, in Irish Church history:—

"Let him [his friend Matthew Raper] know I would write to him had I any thing worth communicating, further than one thing (which I know his friendship to me will make him pleased with) that I have *very fortunately and without any solicitation of mine got the Deanery of Armagh, the best preferment in our Church under a Bishoprick and equal in value to some of them.*" (Letter, Ap. 30, 1768.)

The worthy Dean views his good fortune under a sufficiently worldly aspect: and a fair share of worldly tact is shown in the following extract from another of his letters (Nov. 23, 1772):—

"It is proper to observe to you that the Euclid to which my citations refer is Whiston's latin edition of Tacquet's Euclid. And I think it will be proper to apprize the reader of this at the end of the Translator's Preface, for I am of your opinion that such a preface will be absolutely necessary, since you cannot avoid saying in the Titlepage, *Translated from the Latin.* I think you or any one that is acquainted with the work might very easily write such a preface; as nothing more would be necessary than to say that the following Treatise written in Latin was published at such a time and has since been *so well received by the learned that the Professors in the several universities in England and Ireland* have used and recommended it in preference to all others on the same Subject, and therefore the *Translator* thought that an English Edition would be an acceptable Present to the Public in a Country where so many had distinguished themselves by their great proficiency in mathematical studies tho they had not much cultivated the learned languages. He might then (*to show he had read other works of this kind*) mention some of the Particulars in which he thought *this work had the advantage of others.* He might say some thing in general of the Method or plan upon which the Author proceeds, and his manner of executing it, and then for a further account of the method refer the Reader to the Author's Preface which follows.

"I have only mentioned these hints to show what a Preface of this sort usually contains and that there can be no great difficulty in drawing it up. And I dare say if you were to write to Mr. Williamson or any other Teacher who has read the Book and communi-

cate these hints *as your own thoughts*, and request his assistance he would very soon draw up a Preface to your purpose, and you might have it in your power to *oblige him again*."

"Secrets in all trades *but mine!*" The learned author and the liberal bookseller combine to represent a mere speculation of their own as the act and judgement of the "translator,"—that invisible, if not imaginary, personage of all ages as well as our own.

It does not appear who did draw up the "translator's preface:" but it is executed pretty closely in accordance with the above prescription; and some changes are introduced into the text as suggested by Dr. Hamilton, which form real improvements upon an already valuable treatise.

It seems somewhat strange that Dr. Abram Robertson should print a ponderous 4to in seven books, only a few years after (1792), *also in Latin*, and very much on the same plan as those of Simson and Hamilton, though more closely imitating the latter. The necessity that was felt, even by authors and publishers, for putting the scientific works they issued in a language which could be read by *all*, had produced no influence at Oxford; but even *there* it was ultimately felt to be absolutely necessary to give an English edition of at least a part of the work, which was accordingly done in 8vo a few years afterwards.

Even Euclid was recently read in Latin in the Dublin College; but as it has since been translated, I presume it is now* read there in English. Dr. Elrington's edition, which is there used, differs a good deal from Simson's, but most of all in the treatment of proportion: but I only refer to it here as another instance of the paramount necessity for writing all books on science in our vernacular tongue.

By some letters from the Rev. Francis Holliday, Rector of West Marsham, Notts, it appears that the price paid for the copyright of that gentleman's Fluxions was twenty-three guineas; that is, *one guinea per sheet.* (Letter to Nourse, con-

* Since this was written, a Dublin friend whom I had asked about the *motive* for the retention of Latin as the medium of so much of the Trinity College exercises, "wonders where I could have met with so antiquated a thing as a Latin Euclid used in the College." My own copy is marked "editio quarta, 1813." The Latin appears to be still the medium of examination for the junior fellowships; though (judging from the Dublin University Calendar) dispensed with in the degree examinations. This traditionary practice is hence gradually "wearing out" even in its last stronghold; and certainly in the case of fellowship examinations, less exception can be taken to the practice, than in the case of degrees, whether in honours or not.

taining a statement of accounts, Nov. 22, 1777.) He asked thirty guineas, and estimated it at seventeen or eighteen sheets; see letter, Sept. 26, 1776. Fluxions were cheap then, but they command no price at all now.

It may be worth noticing, that Dealtry's is the last book published with the name and notation of Fluxions in this country; but Jephson's the last with the name only, having the notation of Leibnitz instead of Newton's; the former in two successive editions, and the latter in two volumes issued at different times. The eleventh edition of Hutton's Course is, however, the last English book in which either the name or notation appeared (1835, 1837); and though I strongly urged upon Dr. Gregory, who acted as principal editor of that edition, the necessity for a change, I failed to convince him. This was the more inexplicable to me, as he had for many years admitted the language and notation of the Differential Calculus (though, perhaps, not its metaphysics) into the Ladies' Diary, of which he was the editor. It first appears in 1824 in the Diary*. His argument for retaining it in Hutton's Course was, that Dr. Hutton himself would have insisted on its retention; and he felt himself bound in honour to make no further changes in that work than the author himself would have made under the same circumstances, and with a full knowledge of the state of mathematical science in 1836. As I am personally interested in this question, I may be allowed to state that I consider that view to have been a mistaken one; and that a resolute adherence to what it was supposed Dr. Hutton would have done, has driven the work "out of the market." The ultimate changes made in it came too late; and yet no man was more eminently qualified to make them than Dr. Gregory.

It appears from letters written in 1811 to Wingrave (successor to Nourse) by Miss Maskelyne, that *the Greenwich observations were the private property of Dr. Maskelyne.* They are claimed as such; and Sir Joseph Banks's authority is quoted in support of it. Yet the Board of Admiralty paid

* See two interesting papers on the Introduction of the Notation of the Differential Calculus into this Country by an eminent *anonymé*, and by Mr. Wilkinson, in the Mechanics' Magazine for 1849. It may, however, be remarked, that Dr. Gregory introduced the differential notation into his Trigonometry (using the δ instead of the d) as far back as 1816; and that to the edition of Hutton's Course of (vol. ii.) 1837, he gave a translation, literally, of a portion of Lubbe's treatise. I am not able at this moment to give the dates of either Dealtry's or Jephson's works; but the former ranged somewhere from 1812 to 1814, and the latter a little before and after 1820.

for paper, print, instruments, and a salary for making those observations! Many strange affairs, however, have occurred with respect to books printed by the Admiralty; to some of which I may hereafter direct more particular attention—relating, of course, to "by-gone days."

It seems that even 80 or 100 years ago the booksellers published a good deal "on commission" for the authors: but they do not seem to have carried this kind of business to the extent that we see it in our day, nor to have charged quite so heavily for their services as we now find to be the case. Nor do they appear to have been so eager for that kind of business as their successors have become.

A Mr. John Wright of Edinburgh, a friend of Marshall's, sent 100 copies of a work on Trigonometry (intended as a supplement to Simson's Euclid) to Nourse in 1772 for sale. In 1783 it appears that copies to the amount of six guineas had been sold; and that the "charges" against this were three pounds seventeen shillings!

Many large works that have appeared were first published by public subscription. Usher's Astronomy, Vince's Astronomy, Horsley's Newton*, Taylor's Tables, and some others, are matters relative to which there is more or less correspondence in this mass of papers. A memorandum by Wingrave respecting one work, the name of which I cannot decipher, is "not enough sold to pay the advertisements."

Several papers of Emerson's occur amongst Mr. Maynard's collection, but none of them of much scientific importance; and, indeed, all that is of any value was subsequently incorporated with his published works. His books are even now ubiquitous, and his name is familiar to every tongue; and hence quotation would be idly superfluous. In fact, but for the purpose of correcting a very general popular error with respect to him, his name might have been altogether omitted from these papers.

Emerson wrote nearly twenty works in all, and upon all subjects, into the service of which such mathematics as he possessed could by any contrivance be pressed—from arithmetic to increments, fluxions, mechanics, architecture, music

* It appears from a printed list that Horsley had obtained 369 subscribers: crowned heads, nobility, personages in high civil and diplomatic offices, university and college libraries—in short, the *élite* of Europe. A less pompous and less pretending editor (though as competent to the undertaking, as Horsley was confessedly incompetent) must have sought his patrons elsewhere, and have been satisfied with a smaller number of them.

and chronology. A few of his works (and few only) display a certain amount of rough invention: but he was singularly confused in his development of a process, and one of the most uncouth of all the mathematical writers of this country. It is very probable that, abating the Method of Increments (which owed its value to its being then the only one in our language, Dr. Brook Taylor's being in Latin and untranslated), his most useful works have been the Elements of Geometry and the Conic Sections. The former contains a few theorems which are, as far as I know, original*; and the latter is va-

* It is often extremely difficult to decide respecting originality in elementary investigations; for no one can undertake to look carefully through every elementary book that has been published, to ascertain whether some particular and simple proposition might not possibly be contained in it. Nevertheless some general criteria might be laid down, which would contribute towards probability on one side or another in most cases; and this presumed probability would often limit the trouble of the search to very narrow bounds. Almost every proposition naturally refers itself to a class; and if once observed, others of that class must soon follow. If, then, upon our observing such a proposition we find it isolated, it is highly probable that it originated with the author who there gave it, or at least not long before. In the few cases which I have had occasion to examine minutely and carefully, I have rarely found this rule to fail—indeed, in no one to signally fail. This, too, is precisely the same in respect to analytical devices—and not widely different is the testimony of the history of experimental science.

This remark is made in consequence of a property of the triangle, now universally known, which appears to have been first given in an *elementary treatise* by Emerson (*Geom.*, b. ii. pr. 32, 1763). "The perpendiculars from the angular points of a triangle to the opposite sides, pass through the same point." The property was, however, enunciated by Mr. Thomas Moss, an exciseman and able geometer, in 1751; and two neat demonstrations given to it shortly after by a writer who signs ΣΟΦΟΣ (probably Simpson), and by Edward Rollinson, in Turner's Mathematical Exercises. A property still more general had been given four or five years previously in the Mathematician, edited by Rollinson. The less general property was not, however, perceived to be deducible from the more general one, and both passed without further remark than the mere solutions till attention was called to the system of connected inquiries in the Mathematical Repository (vol. vi.), under one aspect; and under another in the Phil. Mag., vol. ii. p. 26, 2nd Ser., and the appendix to the Ladies' Diary, 1835.

It may seem strange that so simple a property, and so many others similar or related to it, should have been unobserved by the antients, and by their earlier followers after the revival of letters. It must be recollected, however, that the Greek geometers only valued a theorem (or even a Porism) except so far as it contributed to the solution of a problem. There are no traces, nor even intimations, of their having regularly attempted to form classed collections of theorems, or to arrange systematically the many beautiful series of properties of figures that could not fail to have presented themselves during the solution of problems. Such properties were only selected as would be actually required in demonstrating the constructions of the cases of a problem. Of these the seventh book of Pappus is a collection of instances. The properties of the ἄρβηλον given by him, form

luable for the great number of properties which the author has brought together, of which a fair portion were original. *As geometry*, however, both works are extremely impure, every geometrical difficulty being unceremoniously got over by an algebraical equation—a practice only too common amongst the so-called geometrical writers of our own time. This, however, is not the geometry bequeathed us by the Greeks, and exemplified by the Andersons, the Gregories, the Halleys, the Simsons, and the Stewarts of these isles.

The popular error to which I referred is, that Emerson was the Coryphæus of the non-academic class of geometers. He has never been recognized by themselves as the head, the founder, or the leader of their class; but I suppose the frequency of his books on the stalls has led men little acquainted with the history of English geometry to infer that they either are, or have been, in great demand, and consulted as oracles. This never-ending reappearance is more due to the almost indestructible paper on which they were printed, and the firm bindings in which they were issued, than to any other cause; as very few of them ever reached a second edition, and a great number lay on the bookseller's hands at the time of Emerson's death. They were pushed into notice by the per-

almost the only marked exception—or perhaps also Euclid's Porisms. We find at all events, extremely little (if anything, properly speaking) concerning lines meeting in a point, or points ranging in a straight line. The modern French geometers were the first to enter upon this class of researches with any degree of system; and the results have justified their expectations, however sanguine. We need not then, after all, feel much surprise at finding the proposition in question claiming so recent a place in geometry; and the same may be said of a great number of now-familiar truths.

Postscript, August 24.—Whilst reading the proof sheets, the Mechanics' Magazine of this date reached me, containing one of Mr. Wilkinson's able and elaborate analyses of our English mathematical periodicals, viz. of the *Miscellanea Curiosa Mathematica*, 1745–53, edited by Holliday, whose name has been already mentioned. As there is one passage which renders a slight modification of the preceding paragraph necessary, I quote it as it stands—from its offering less trouble, both to myself and the printer at the last moment, than recomposition and resetting would do.

Speaking of art. xxxix., "A new Proposition in Geometry demonstrated, by Mr. William Chapple," he says:—

"This proposition is the now well-known property, that 'the three perpendiculars of any triangle intersect in the same point,' and although taken for granted in the solutions of Quest. 45 *Gentleman's Diary* for 1743–44; Quest. 260 *Ladies' Diary*, 1745–46, the honour of a *formal* enunciation and demonstration appears to be due to Mr. Chapple. The property is stated both for the acute and obtuse-angled triangle, 'the same demonstration serving for both, which however is not conducted in so purely geometrical a manner as one could wish.'" He then mentions some more recent researches, which, however, need not be introduced here.

tinacity of Nourse (who formed a higher opinion of him than was at all just), rather than by any intrinsic merits of their own.

Of Emerson's personal character this is not the place to say much; and indeed it would be unnecessary to do more than refer to Hutton's Dictionary (*in loco*) for a description of his eccentricities, were it not for a *non-sequitur* that has been drawn from his and some similar cases. It surely does not follow that because Emerson and some others habitually indulged in a rough discourtesy of bearing towards others, that it arose from the nature of their studies, or inevitably followed from the tone of feeling generated by mathematics. Why not, then, charge medical studies with the same tendency on the ground of an Abernethy, or the legal on the ground of a Thurlow, belonging to those professions? The inference is indeed absurd enough; but many a time in my life have I heard it made.

However, I have to beg, once for all, that if the non-academic body of mathematicians are decreed to have a head of their school, they may at least be allowed their own choice. That election would fall, without a dissentient voice, on Thomas Simpson. Amongst themselves, he, and not Emerson, virtually fills the post; and they cannot but feel aggrieved by hearing a man whose character they do not respect and whose works they seldom open, thus held up as the prototype of themselves.

This series of notices of the Nourse papers must necessarily be incomplete without some account of the man who was the real focus of the mathematical literature of his time—now verging upon a century ago. I regret that few materials of a positive kind have fallen in my way from which I can satisfy so reasonable a desire on the part of my readers. Most of the letters are written in more or less of that dry, business-style, that only brings in an interesting incident now and then, and always casually. One series of letters, however, of a more familiar and intimate character, from John Robertson of Portsmouth (author of the Treatise on Navigation, and other works, and subsequently "Clerk" of the Royal Society), throw some light on Nourse's personal character. A few scraps, too, of Nourse's own geometrical speculations betoken a mind of no ordinary powers, and a taste in science such as even few professional mathematicians have evinced.

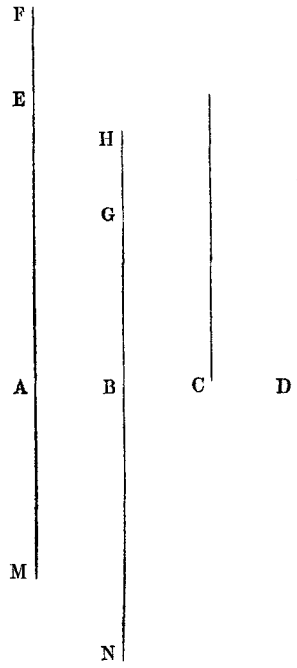
From the frequent jocular allusions of Robertson, it would appear that Nourse was a grave self-possessed person, who nevertheless enjoyed a pun or a good joke as well as his friend did. A quiet pipe with its adjuncts in the shop-parlour seems to have been his sum total of indulgence. He rarely quitted his

business; and when he did, it was only to go to Oxford where he had relatives—a brother, Sir Charles Nourse, and a sister, the wife of Dr. Hornsby, the Radcliffe astronomer. One of the letters seems to imply that he was a short and somewhat corpulent man, who always fancied himself to be "wasting away." He was always alive to his business, and the number of works of which he was the publisher was unprecedented in his day; his undertakings appear to have been generally successful, and his dealings scrupulously honourable. He does not appear from any allusions in these letters to have been married: and he amassed considerable property, which was bequeathed to his brother and sister.

The mathematical character of Nourse is best shown by a specimen of his geometry. I therefore annex two: one a demonstration of the converse of Euc. v. 25, which he required as lemma for some emendations of a proposition on the conic sections in the works of Mylne and Simson; and the other a remarkably simple and elegant problem in *Angelis de inf. Parebolis.* These are given precisely as I find them in the MS., taking in the textual corrections made by himself.

"Lemma (Euclid v. 25 convers).

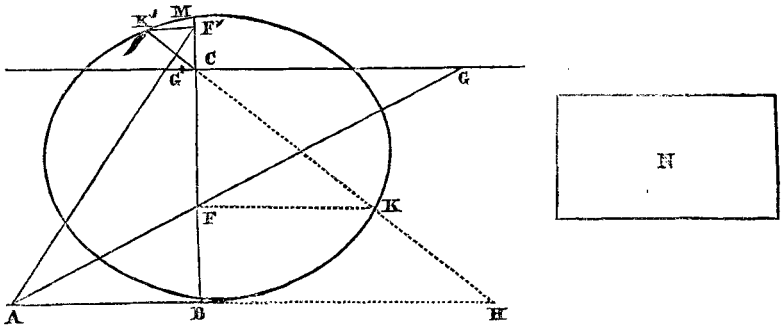
"Si quatuor magnitudines fuerint proportionales, et prima cum quartâ major fuerit secundâ cum tertiâ; erunt prima et quarta maxima et minima quatuor proportionalium.



"Sint enim proportionales AF. BH. C & D. Et quoniam prima cum quartâ major est secundâ cum tertiâ. non erit igitur prima AF tertia C. æqualis, sed vel major vel minor eà. Sit primò major. fiatque ipsi C. æqualis AE. et ipsi D æqualis BG. et quoniam tota AF. est ad totam BH. ut ablata AE. ad ablatam BG ergo reliqua EF est ad reliquam GH. ut tota ad totam. Jam ipsi D. æqualis fiat AM. et ipsi C. æqualis BN. et erit MF. æqualis primæ cum quartâ. et NH æqualis secundæ cum tertiâ. Est itaque MF (ex hypothesi) major quam NH. Sed ME æqualis est NG. etenim ipsarum utraque equalis est C & D simul. Ergo, si ab inæqualibus MF & NH. quarum MF major est, auferentur æquales ME &NG. residuæ erunt etiam inequales, nempe EF major erit quàm GH. Sed supra ostensum est EF. esse ut GH ut AF. ad BH. Ergo AF major

est quàm BH et C major quàm D. Sed ex hypothesi AF quàm C major, ergo BH quàm D & AF quàm D major. Ergo AF & D sunt maxima et minima quatuor proportionalium. Similiter ostendemas, si ponatur prima minor quàm tertiâ. Nam invertendo ordinem proportionalium ut quarta vocetur prima & tertia vocetur 2^{da}. et sic deinceps. eadem erit demonstratio. (nullo verbo mutato).

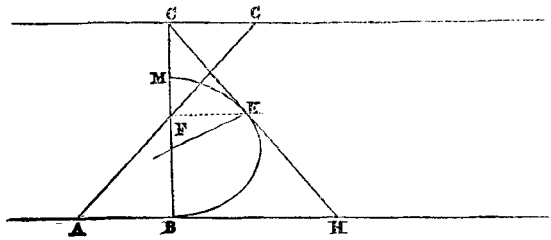
" Prop 29. ad 36. Angelis De infinit. Parebolis p. 25 &c. ad 108.



" Datis parallelis AB . CG, quas secit BC ad rectos angulos. data etiam puncto A : Ducere AG ita ut aggregatum triangulorum AFB. CFG equali sit spatio dato N.

" Fiat AB in BM equali duplo spatio N. Quoniam igitur duplum N (vel AB in BM) equatur rectangulis AB×BF et FC×CG simul; erit AB in FM equali rectangulo FC × CG. unde erit AB ad CG, hoc est, BF ad FG, ut FC ad FM. Patet igitur solutio, ut sequitur.

" Fiat enim BH equalis BC. et ducta circa diametrum BM. circulo MKB, qui secet CH in K. et ductâ KF parallelâ ipsi BA, quæ secet BC in F. Ducatur AG per F. dico factum. Patet ex analysi.



" Insuper in hac analysi patet, aggregatum triangulorum AFB . CFG tum fore minimum, quando linea BM minima est omnium quæ conditionibus in analysi positis satisfaciant. Hoc autem evenit in illo casu ubi BM adeo parva vit, ut circulus hac diametro descriptus minimo secet lineam CH, sed tantum in uno puncto tangat. Fiat igitur HK (vel fiat BF=$\frac{1}{2}$CH) equalis HB et ducatur KF parallela AB. et ducta AF. triangula AFB . CFG simul sumpta minima facient spatium quod abscindi possit à quavis lineâ per punctum A ducta.

" Quod ci spatium ad construendum propositum minus sit rect-

angulo ex AB in lineam datum CK (vel $\frac{1}{2}$BM) problema construi nequit."

Though the determinations are very neatly given here, the circumstance of the double solution has escaped his notice, viz. the point of intersection of CH with the remaining semicircle, as represented by the accented letters, which I have put in for the purpose of showing it. It will be worth the while of the younger geometrical reader to examine this case, and discover whether this second solution be that of the proposed problem or of a collateral one. It involves no material difficulty.

Shooter's Hill, Aug. 15, 1850.

XXV. *An Instantaneous Demonstration of* Pascal's *Theorem by the method of Indeterminate Coordinates.* *By* J. J. Sylvester, *M.A., F.R.S.**

THE new analytical geometry consists essentially of two parts—the one determinate, the other indeterminate.

The determinate analysis comprehends that class of questions in which it is necessary to assume *independent* linear coordinates, or else to take cognizance of the equations by which they are connected if they are not independent. The indeterminate analysis assumes at will any number of coordinates, and leaves the relations which connect them more or less indefinite, and reasons chiefly through the medium of the general properties of algebraic forms, and their correspondencies with the objects of geometrical speculation. Pascal's theorem of the mystic hexagon, and the annexed demonstration of its fundamental property, belong to this branch of the subject, and afford an instructive and striking example of the application of the pure method of indeterminate coordinates.

Let x, y, z, t, u, v be the sides of a hexagon inscribed in the conic U. Let the hexagon be divided by a new line ϕ in any manner into two quadrilaterals, say $xyz\phi$, $tuv\phi$.

Then
$$ay\phi + bxz = \mathrm{U} = \alpha u\phi + \beta tv;$$
$$\therefore\ (ay - \alpha u)\phi = \beta tv - bxz;$$

$\therefore$ $ay - \alpha u$ and ϕ are the diagonals of the quadrilateral $txvz$.

By construction, ϕ is the diagonal joining x, v (*i. e.* the intersection of x and v) with z, t; and thus we see that $ay - \alpha u$ is the line joining t, x with v, z; but this line passes through y, u. Therefore x, t; y, u; z, v lie in one and the same right line. Q. E. D.

26 Lincoln's-Inn-Fields,
August 1850.

* Communicated by the Author.