

ON THE DIFFICULTY OF COR-
RECT DESCRIPTION OF BOOKS.

BY AUGUSTUS DE MORGAN. WITH
INTRODUCTION BY HENRY GUPPY

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ON THE DIFFICULTY OF CORRECT DESCRIPTION OF BOOKS.

BY AUGUSTUS DE MORGAN, WITH AN INTRODUCTION BY
THE EDITOR.

TO reprint a paper upon such a topic as the correct description of books, written as long ago as the year 1853, and which in the estimation of some of our readers may be thought to have already done its work, would seem to call for some sort of justification.

That justification is not far to seek, for we need only turn back the pages of the RECORD to find in the opening paragraphs of the paper, "Suggestions for the Description of Books printed between 1501 and 1640," read before the Bibliographical Society of Lancashire, and printed in our issue for March, 1901, that Mr. J. P. Edmond has furnished us with the very arguments we need.

We cannot do better than reproduce Mr. Edmond's own words:—

"It must be the desire of all who have taken an interest in the founding of the Bibliographical Society of Lancashire, that the publications to be issued in the future should contain descriptions of books that are free from ambiguity and at the same time scientifically accurate. It seemed to me that one of the most important points to aim at in the earlier life of the society is the laying down good, sound rules. These models, if not to be slavishly followed in all instances, yet may be a guide to those who, in the interests of bibliography at large, and of this society in particular, will work on some one or other of the virgin fields which

await the zealous and earnest student. To frame such rules is no easy matter, and I trust that you will not for one moment think that what I advance is done in a dogmatic spirit. They are, as I have entitled them, merely suggestions, and I hope that they may lead to a discussion that will be helpful to us all.

“It has been said wisely that if any man prides himself on his accuracy, the best cure is to print a catalogue of books. I should advise him to print the description of a score of books of any date before 1640. I think the chances are that after the criticisms of a few competent scholars he will feel a humbler and I hope a wiser man than he was before. In this connection I would recommend to your perusal a paper, “On the Difficulty of Correct Description of Books,” by Augustus de Morgan, which appeared in the *Companion to the Almanac* for 1853. I am not aware that it has ever been reprinted, but it, as well as the other bibliographical papers by the same writer that appeared in that work over a long series of years are full of most valuable information set forth with the lucidity for which De Morgan was unrivalled.”

In the discussion which followed the reading of Mr. Edmond's paper, the present writer stated that for some time he had had the intention of reprinting De Morgan's paper in the pages of the RECORD, but that other and more pressing claims upon his space had hitherto rendered it impracticable.

The tribute which Mr. Edmond paid to the value of De Morgan's work was a most welcome confirmation of the estimate which we ourselves had formed of that work, and led us to express the determination to reprint in our pages the paper to which reference had been made, as early as circumstances would permit.

In redeeming that promise we have ventured to prefix to De Morgan's paper a few biographical notes respecting the author, drawn from the various available sources, in the hope that by so doing we shall be rendering a modest service to bibliography, by quickening interest in De Morgan and his invaluable work.

Should our action in printing the present paper meet with the approval of our readers, which we are presumptuous enough to think it deserves, we shall from time to time reprint other papers, not only by De Morgan but by other bibliographers of the past, whose work does not receive the recognition that it merits from present-day workers in the same field.

Augustus de Morgan was born in the year 1806 at Madura in the Madras Presidency, and when only seven months old was brought to England.

He received his early education in several private schools, and before the age of fourteen was a good Greek and Latin scholar, whilst his classical and general reading was as wide as it was varied. It is told of him by one of his schoolfellows that he read algebra like a novel, and used to prick out equations on the school pew instead of listening to the sermon.

In 1823, at the age of sixteen, he entered Trinity College, Cambridge. Two years later he gained a Trinity scholarship, and in 1827, before he had completed his twenty-first year, took fourth place in the mathematical tripos, though far superior in mathematical training, it is said, to any man in his year. He was disappointed by the result, which was due to his discursive reading, and retained through life a strong dislike to competitive examinations as tending to give the advantages to docile over original students, and to encourage "cram".

He was prevented from taking his master of arts degree, or from obtaining a fellowship by his conscientious objection to signing the theological tests, then required from masters of arts and fellows at Cambridge. A strong repugnance to any sectarian restraint upon the freedom of opinion was one of De Morgan's most marked characteristics throughout life. We can admire him as a young man who cultivated the habit of thinking for himself, and who had the courage of his convictions in refusing to fetter his conscience by yielding up the right to follow her dictates in matters of religious belief.

His own university being practically closed against him

in consequence of his fearless honesty, he resolved to go to the bar and entered at Lincoln's Inn, but finding the law unpalatable he applied for the chair of mathematics in the University of London which was then forming, and was (in 1828) unanimously appointed first professor of mathematics in what is now known as University College, London. In this capacity he served the College with the utmost zeal and success for considerably over a quarter of a century.

As a teacher of mathematics De Morgan was unrivalled, and although the best hours of the day were given to arduous college work, his public labours in other directions were very extensive. His mathematical writings contributed powerfully towards the progress of that science. Two of his most elaborate treatises are to be found in the *Encyclopædia Metropolitana*, namely, the articles on "The Calculus of Functions," and "The Theory of Probabilities," of which the latter is still the most complete mathematical treatise on the subject in the English language.

To give some idea of the number and importance of De Morgan's mathematical writings, it needs only to be pointed out that in the catalogue of the Royal Society alone forty-two entries will be found under his name.

De Morgan was also responsible for a number of most elaborate and valuable papers and memoirs on logic, but by far the largest part of his writings—in volume at least—consists of detached articles contributed to various periodicals and composite works. For example: his articles in the *Penny Cyclopædia* are said to be no less than 850 in number, and they have been estimated to constitute a sixth part, and probably the most valuable portion, of the whole *Cyclopædia*.

Some of De Morgan's most interesting and useful writings, from our point of view, are to be found in the *Companion to the British Almanac*, to which he regularly contributed one article each year from 1831 up to 1857 inclusive. In these carefully written papers he treats a great variety of topics relating to astronomy, chronology, decimal coinage, the history of science, bibliography, etc., most of which, as in the case of the paper here reprinted, are as valuable now as when written.

It is impossible in a brief sketch such as the present even to touch upon the many sides of De Morgan's character and work. There is, however, one other characteristic we must not allow to pass unnoticed, we refer to his intense and yet reasonable love of books. He was a true bibliophile, and longed to surround himself, as far as his means allowed, with curious and rare books. He revelled in the mysteries of water-marks, title-pages, colophons, catch-words, signatures and so forth, and gave to bibliography the position of an important and exact science. As he himself wrote: "The most worthless book of a bygone day is a record worthy of preservation; like a telescopic star, its obscurity may render it unavailable for most purposes; but it serves, in hands which know how to use it, to determine the places of more important bodies". His evidence before the Royal Commission on the British Museum in 1850 should be studied by all who would understand the principles of bibliography or the art of constructing a catalogue.

De Morgan's nice sense of bibliographical accuracy is strikingly illustrated in the following incident: By some accident the work on "Probability" written by Lubbock and Drinkwater Bethune was attributed to De Morgan, an error which so seriously annoyed him, that for fifteen years he did all in his power to correct the mistake, finally writing to *The Times* to disclaim the authorship.

A sample of De Morgan's bibliographical learning is to be found in his account of *Arithmetical Books from the Invention of Printing*, and also in his *Budget of Paradoxes*, a work as amusing as it is learned, consisting of articles, most of which were originally published in the *Athenæum*, describing the various attempts which have been made to discover perpetual motion, to square the circle, or to trisect the angle; but De Morgan took the opportunity to include many curious bits of lore gathered from his extensive reading.

His library consisted at the end of his life of about three thousand volumes. He was a genuine book-hunter, though his means compelled him to limit himself to occasional purchases from the bookstalls. He made many quaint

marginal and learned annotations, and turned his bibliographical researches to good account in his writings. His library was purchased after his death, which took place in 1871, by Lord Overstone for presentation to the University of London.

The *Memoir of Augustus de Morgan* . . ., published after his death, in 1872, by his widow, makes delightful reading. Mrs. de Morgan has included a list of her husband's writings, which, excluding his contributions to the *Athenæum* and *Notes and Queries*, extends to fifteen pages, indicating that the extent of our author's literary and scientific labours was altogether extraordinary. And yet, quality was not sacrificed to quantity, for no less an authority than Stanley Jevons says of De Morgan: "Every publication was finished with extreme care and accuracy, and no writer can be more safely trusted in everything which he wrote".

De Morgan's life was not without its clouds and disappointments, but under them and in spite of them he maintained a genial and affectionate disposition, a vivacity of character which was entirely free from all sordid self-interest. There is no better tribute to the unselfishness of his character than that contributed in 1872 to the monthly notices of the Royal Astronomical Society by Mr. Raynard, who speaks of his master and friend in the following terms: "He was the kindest, as well as the most learned of men—benignant to every one who approached him, never forgetting the claims which weakness has on strength".

On the Difficulty of Correct Description of Books.

WE have often had occasion, in articles contributed to this work, to notice error and difficulty arising out of incorrect or insufficient description of books. The study of *bibliography*, that is, of books as books, in all matters which are requisite to avoid the errors and difficulties just alluded to, has been left to librarians and to *bibliomaniacs*, as they have been called. Recent events, however, have brought bibliography into collision with the want of it, in a remarkable way.

The year 1850 turned the attention of literary men to the

subject, both in England and France: but in very different ways in the two countries. In England the report of the Royal Commission appointed to examine the state of the British Museum became public, and with it the evidence on which it was founded. This report and evidence contained the details of a severe contest between bibliographers on the one hand, and literary men opposed to bibliography on the other hand, as to the mode in which book catalogues should be made. The report of the Commission, the comments of the leading reviews, and the subsequent silence of the journals which had for years attacked the librarians of the Museum, gave the victory to the advocates of detail sufficient for accuracy, as one side called it, or of unnecessary minuteness leading to confusion, as the other side called it. And the great extent to which both the antagonist philosophies taught by examples, makes this report, with its evidence, an excellent collection of exercises, and a manual, so far as that term can be applied to a blue-book, of practice for the young bibliographer.

The corresponding display made in France was not altogether so creditable to the literary aspirations of the nation. In the year 1850 appeared the *act of accusation* against M. Libri,¹ an eminent mathematician and bibliographer, and a member of the Institute, charged with robbing the public libraries to the value of many thousands of pounds; on which, by default of appearance, he was condemned. The amount of incapacity which either belonged to the framers of this indictment, or was presumed by them to belong to the courts and the literary public before which it was to come, far exceeds all that was exhibited by the

¹ The reader will find some account of the details of this persecution in *Bentley's Miscellany* for July, 1852, and in the *Athenæum* for 27th May, 1848, and 12th May, 1849. How completely the charges are to be attributed, in the first instance, to political and private malice, is now sufficiently known. "He was condemned," says *The Times*, "for stealing books, many of which are now to be found in the very places from which he was said to have taken them; he was condemned for stealing books which he was proved to have bought of Messrs. Payne & Foss in London; he was condemned for stealing books which it was beyond the power of the French courts to identify, or even to describe correctly." All this we know to be true, with the exception of what is implied in the word *even*: correct description is no such everyday matter.

ignorers of bibliography in England. None of these last ever thought, or wished to make others think, that the stamp¹ of a convent *library*, imprinted on the front of an old book, is evidence of an intention, on the part of the stamper, to pass the book off as *printed* in the town where the convent is.

Except, however, to express our belief that these recent events in France and England will be of some effect in widening the circle within which bibliography is studied, we have nothing to do with them here, though we may cite them as among our encouragements for presenting an article on the subject. Our intention is to show, by instances, to how great an extent inaccurate bibliography prevails, both in the descriptions which are given of books, and in those which they give of themselves. We began, in pursuance of this intention, and that we might produce a new case² or two, by taking the first four old books that we happened to lay our hands on, the selection being dictated by the mere accidental proximity of the volumes on our

¹ The examiners of M. Libri's books found the Aldine Catullus of 1515, Venice, with what they read as "Bibliothecæ S. 10 in Casalibus Placentiæ" either stamped in old type, or in manuscript (they could not tell which!) on the front leaf. The "S. 10," had they known how to read, would have been "S. Jo.," and the whole would have shown that the book once belonged to the Library of the Convent of St. John of the Canals at Piacenza. They impute to M. Libri that *he* stamped these letters, first, to hide the marks of another stamp which they assert to have been erased, next, to pass off the work as *printed* at Piacenza. The terms in which they crow over their unanswerable proof, as they take it to be, that the book had been stolen, will perhaps be cited in bibliographical treatises for centuries to come: ". . . le titre annonçait une édition de Plaisance, et la bibliothèque [de Montpellier] avait perdu une édition de Venise. . . . Pour dissimuler les traces du grattage dont il a été parlé, on avait mis à la place de l'estampille ces mots, . . . BIBLIOTHECÆ S. 10. IN CASALIBUS PLACENTIÆ. Manuscrits ou appliqués avec de l'ancienne fonte, ces caractères jouent l'impression. Mais la fraude ne pense pas à tout: tandis que le titre falsifié annonçait une édition de Plaisance, la dernière page révélait une édition de Venise. . . . De tels faits ne se discutent pas, ils s'exposent." The supposition that a practised bibliographer, desiring to falsify the place of printing, would forget that it is *almost always* at the end in very old books, is more amusing to those who are looking at the last pages of such books every day, than those who do not look into them can easily imagine. Their proverb ought to have been, *la fraude ne pense à rien*.

² We are not indebted, throughout this paper, to any one instance which was introduced in evidence before the Commissioners, either by ourselves or others.

shelves. If no one of these four volumes had given us either error produced, or difficulty likely to produce it in time to come, our associations would have been rudely invaded; for we have been accustomed to consider it almost impossible to take two old books at hazard without encounter one or the other. It happened that *all* the four gave us what we wanted to illustrate.

The first book was a collection of four geometrical and two astronomical treatises by John Werner, of Nuremberg, quarto, 1522, beginning "In hoc opere hæc continentur. Libellus Joannis Veneri Nurembergen. Super vigintiduo elementis conicis. . . ." It is said that this book was so rare in the time of Tycho Brahé that he could not find it in all Germany, though he secured a copy at last in Italy. The two last treatises being astronomical, we turn to Lalande's *Bibliographie Astronomique*, and we find at the right year, 1522, that this book consists of the two astronomical treatises, followed by an epistle of Regiomontanus to Cardinal Bessarion on the meteoroscope [instead of preceded by four geometrical treatises of Werner himself]. The authority is Weidler, who, says Lalande, adds two other tracts as contained in this work, of which Scheibel observes that they have never been printed at all. Here is a heap of confusion, in which three noted writers of mathematical history are concerned. Looking at Weidler (at the page cited), we find reason to think the case stands as follows. Weidler, after hinting that Werner printed the works of others as well as his own, gives a list as extant, in which he takes no care to distinguish between what Werner only printed, and what he both wrote and printed. In the middle of this list comes the letter to the cardinal. The last five of the list are five of the treatises which really are in the work before us, the sixth being omitted. Then, says Weidler, these last five works appeared at Nuremberg in 1522. From this it would appear as if Lalande had selected two astronomical works of Werner, the letter of Regiomontanus, and two others which he does not name because Scheibel said they were never printed.

We had turned to Weidler's *History*, because Lalande

cites it (p. 334). We then turned to Weidler's *Bibliography*, and here we really find that the Nuremberg quarto of 1522 is said to contain five treatises, the three given by Lalande, with two others by Werner, not any of those yet named. And Weidler refers to p. 334 of his own book, in which, as already seen, he gives a very different and more correct account. So that the confusion is as follows. Weidler describes the book in his *History* with nothing but an omission. In his *Bibliography* he gives a totally wrong description, for which he refers to his own more correct *History*. Lalande adopts the account given in the *Bibliography*, and joins to it the reference to the *History*, without stating that his reference to the *History* is only a copy from the *Bibliography*. No one, without the book before him, could have unravelled this skein of mistakes. We took the work of Lalande because it is decidedly the best piece of scientific bibliography which, at its appearance, had ever been in existence, and therefore gave the best chance of a correct description. But, like other descriptive works which make a commencement of correctness upon books which the authors had examined for themselves, it relies in a great degree upon works prior to the introduction of any effort at minute description.

In the last instance, it happens that the mistake can be traced to its source in a manner which leaves no doubt that it is a mistake. But the unpractised reader must not come to such a conclusion too rapidly. If Lalande had not named his authority, as often happens with him, we should have had three alternatives to consider. (1) A mere mistake. (2) The circumstance of his having happened to fall in with a book in which some one had bound together some astronomical tracts of Werner with a copy of Regiomontanus's epistle. (3) The possibility that Werner made two distinct publications at Nuremberg in 1522, one containing his own *six* tracts, the other joining the last *two*, which are astronomical, with the astronomical epistle in question. Either of the first two hypotheses is credible enough. The third looks very unlikely. But it must be remembered that it is utterly impossible to enumerate the number of odd things which

occurred in the first century of printing, before authors and publishers had fallen into a common understanding upon their modes of proceeding. Anything imaginable may have taken place in one or more instances; and it happens sometimes that the unlikely thing, stated by a writer who is frequently inaccurate, turns out to be the truth, in spite of the more probable account of a generally more accurate writer. And a strange assertion, which appears to be an obvious distortion of one which is known to be true, may nevertheless be one separate truth, with or without some admixture of the matter of the other. For instance, a poor authority on books, Granger, says that Roger Palmer, afterwards the notorious Earl of Castlemaine, husband to one mistress of Charles II., and ambassador to the Pope of James II., invented and wrote on a "horizontal globe". Now since *John Palmer*, in 1658, did certainly write on the "Catholique Planisphær," and since the phrase *horizontal globe* looks very much like an awkward rendering of the word *planisphere*, we at one time took the liberty of thinking that Granger or another had confused the two Palmers; and we were not without our suspicion that the *Catholic* planisphere had perhaps assisted in the transfer of the book to a *Catholic* author. Nevertheless, we afterwards found¹ that Lord Castlemaine published in 1679 a work on what he called the "English Globe". Again, the rule of three, in middle Latin, is *regula detri*, so that, seeing *Detri* mentioned among arithmetical authors, we took it to be pretty certain that, as has sometimes happened, the name of the subject of a book had been substituted for that of the author. Nevertheless, we have since seen in a careful sale catalogue, the description of the work of N. Detri: in which we believe, in spite of the existence of another work by N. Petri.

The second of our four instances is the *Cosmographia* of the celebrated Maurolycus, 4to, Venice, 1543, the year of publication of the great work of Copernicus. At the end it is stated that this work was finished in 1535, and the preface

¹If Granger had only looked into the *Catalogue of Royal and Noble Authors* by Horace Walpole, to whom his own work is dedicated, he would have seen an accurate title-page of this work.

is dated 1540. It is in dialogue; and the teacher says (p. 12) that nothing more need be said about the earth, unless diversity of opinion and human fickleness should so far increase, that there should be ground to suspect some one of believing and maintaining that the earth turns on its axis. I should hardly think, says the pupil, that such a strange opinion would enter the head of any one. Why not, rejoins the teacher, many teach themselves greater absurdities; but be this as it may, to remove all possibility of such opposition, I will demonstrate that the earth cannot move. If all this were first published in 1543, in spite of the date of the preface, we should reasonably presume that the intention of Copernicus had reached the ear of Maurolycus, and had given rise to the introduction, at the last moment, perhaps, of what precedes. For in 1540, Rheticus¹ published at Dantzic his account of the forthcoming work of Copernicus. In much less than two years this might be circulated over Europe, for everything found² its way easily to and from Rome, and opinions travelled by epistolary description much more than now. But, if what precedes *were* written before 1540, it shows that, anterior to the publication of Rheticus, there was a feeling that discussion on the earth's motion was at hand. This would be worthy of note, for no one has hitherto shown that, in the case of the earth's motion, there was that previous expectation of change which has marked the approach of most other new doctrines. Had

¹ The best chance any reader will have of seeing this remarkable precursor of Copernicus will be by looking for the second edition (Basle, 1566, folio) of Copernicus himself, to which it is attached. We have never seen either of the two separate and previous editions of the tract of Rheticus; but a letter from Gassarus of Lindau, prefixed to that of 1566, mentions the receipt of the first edition from Dantzic, and is dated 1540. So that neither Lalande nor Weidler is wrong on this point.

² There is reason to suppose that foreign books of second-rate name travelled from one country to another during the earlier ages of printing, in larger numbers than now; that is, immediately after publication. At the present time, in the case of a book of no great note, published in France or Germany, hardly more than two or three straggling copies will forthwith find their way to England. But in 1670-80, the bookseller always imported immediately; and the mathematical bookseller complained that he could not sell more than *twenty or thirty*, until the book had gained reputation, in a manner which implied that even this state of things was a falling off.

there been, no doubt the work of Copernicus would have given the signal for that sort of opposition which was reserved for Galileo. All this would lead us to suppose that the remarks of Maurolycus were suggested by the special publication of Rheticus, and not by any knowledge, on the part of Maurolycus, of a diffused disposition to think about the actual question¹ of the earth's motion.

But now comes a difficulty. A preface, dated in February, 1540, of a work published in 1543, gives some presumption, not a very great one, of a previous edition in 1540 or 1541; rather too much² to neglect, though far from enough to pronounce upon. Lalande, relying again upon Weidler, affirms that this work of Maurolycus *was* first printed in 1540; and Weidler makes the statement both in his *History* and in his *Bibliography*. And, what is more, Riccioli (in 1651) makes the same assertion. It matters little or nothing that the work of 1543 is not called a second edition, for it not unfrequently happens that a reprint shows no sign of that character. And though neither the Abbé Scina, in the life of Maurolycus, nor the compiler of the list of works presently mentioned, notes any edition earlier than 1543, yet neither seems to have made much search, and both, to judge by their modes of description, would rest content with the earliest edition they happened to have seen. Thus,

¹ The reader should be aware that both Rheticus and Copernicus propounded the theory of the earth's motion only as an hypothesis, to *save appearances*: using this phrase in the old sense, though most historians suppose that they also intended the thing signified by its more modern meaning. The phrase to *save appearances* is a cast-off phrase of physics; we now say to *explain phenomena*. Thus the supposition that the earth turns on its axis preserves the diurnal appearances of the heavens, and makes them follow: and the old explanation does the same. Copernicus contends for the supposition of the earth's motion as the most simple mode of deducing and calculating the celestial phenomena: leaving the question of its actual truth or falsehood open. The utmost extent to which he commits himself on this point is (lib. 1., cap. 8) the affirmation, that on the balance of *a priori* reasons, the motion of the earth, especially the diurnal motion, is more probable than its stability.

² Castiglione, who published Newton's *Opuscula*, knew that the *Optics* were published in 1704, and had a copy of 1706. He took for granted (pref. p. vii.) that there could not be two editions so near in time, and therefore announced that by the printer's negligence the edition of 1704 had 1706 on the title-page. The fact is that there was an English edition in 1704, and a Latin one in 1706.

though inclined¹ to believe that the edition of 1543 is really the first, and therefore that the remarks we have quoted are specially directed against Rheticus, we should not be at all surprised if an edition of 1540 or 1541 were to turn up.

It is known that there were among the ancients some who maintained the diurnal motion of the earth, and some who maintained the annual, at least as possible; Ptolemy alludes to them, and gives his reasons against them. Down to the time of Copernicus, we are not told of any (except Cardinal Cusa,² who is not worth alluding to on this point) who really thought anew on the subject, so as to produce fresh arguments either for or against. Nevertheless, it appears, though we cannot find it mentioned by any historian, that Regiomontanus had seriously considered the subject. One of the greatest preservers of his writings was John Schoner, of Carlstadt (1477-1547). In the collection of Schoner's works, first³ published in 1551, Nuremberg, folio, is an *Opusculum Geographicum*, the first chapter of which is a *disputatio* of Regiomontanus on the subject of the earth's rest or motion. In this short discussion, while deciding the question against the earth's motion, on grounds resembling those of Ptolemy, he cites, as from the ancients, the comparison of the earth to meat roasting on a spit, and of the sun to the fire which cooks it; as also the argument

¹ Maurolycus, in 1553, received a pension expressly to enable him to publish his works: which makes it likely that some of those previously published had been delayed, and the more so as there was remarkable delay even after the receipt of the pension.

² The Cardinal's argument was founded on the non-existence of a centre, deduced from the non-existence of a circumference, to the universe. A book might be written on the manner in which purely subjective notions of the centre and its necessary properties influenced the arguments on this subject, from those of Cusa to those of the Sieur de Beaulieu (1676), who says that the presumption of Copernicus led him to "advance in geometry a proposition as absurd as it is against faith and reason, by making the circumference of a circle fixed and immovable, and the centre movable, on which geometrical principle he maintained the stability of the sun, and the motion of the earth".

³ Weidler, in the *History*, gives a correct account of this work: in the *Bibliography*, which refers to the *History* (p. 337), he makes the mass of its contents to belong to the subsequent edition of 1561, and retains only the three last treatises in that of 1551. Lalande copies him, together with the reference "p. 337," and thus again seems to misstate the matter of his own reference.

that it is the business of the mutton, which wants heat, to turn round before the fire, and not of the fire to turn round the mutton. To what old writer he refers, we cannot tell, as we cannot find this simile in any of the passages which have been quoted from classic authors. We mention the discussion in which it occurs to point out that it would not be a very easy matter to ascertain whether Copernicus (who died in 1543) could or could not have seen it. According to the preface, the date of composition of this *Opusc. Geogr.* is 1533; from which Lalande says it was printed in 1533; but we can find no notice of any impression previous to that in the collected works of 1551. Weidler says this collection contains some things which had not been previously published: but this can only mean that *he* had not found them.

The third of the books in our list is a quarto printed at Leyden in 1649, the title of which tells us that it is the Geometry of Descartes, first printed in French in 1637, and now rendered into Latin with notes, etc., by Francis Schooten. This is then certainly a second edition, at least. Now we learn in many places that in the second edition of Descartes's Geometry, by Schooten, the additions contain papers by Van Heuraet, Hudde, etc., of the greatest note in the early history of the differential calculus. Not the smallest trace of these things appears in the book before us. Some persons must have been puzzled by this: the truth is, that instead of naming the second edition of Descartes, by Schooten, writers should have named the *second of Schooten's*¹ *editions* of Descartes, Amsterdam, 1659, which has on the fly-title, "Renati Descartes Geometria, Editio Secunda"—a wrong description. Thus it appears that the titles of the books themselves may contain the very errors which it is the tendency of bad catalogues to create when they do not exist,

¹ A difficulty of this kind is far from uncommon. An editor leaves us in doubt as to whether the numbering of the edition refers to impressions, or to the impressions which that particular editor has superintended. It would be well if the word *impression* were used in the general sense and *edition* in the particular. Thus if A publish four editions of his own work, and if the commentator B then publish three more, there will be seven impressions in editions of four and three; and the sixth impression of A's work will be B's second edition.

and which it is very difficult to avoid, or to correct, even in good ones. This instance is particularly appropriate, for it is of the simplest kind. "It may help to enforce a truism which seems of late years to have been almost entirely lost sight of"—[a gentle mode of expression for vigorously denied and opposed]—"that the making of catalogues correctly, like the making of dictionaries, requires in the 'harmless drudge' who practises it an amount of qualifications which those who despise him are often far from possessing." This quotation comes out of the review of an attempt to catalogue the library of the linguist Mezzofanti, made by a Roman bookseller,¹ who entered a Cingalese grammar, printed at Colombo, under the United States of America, and a Gaelic translation of Thomas à Kempis as a work of Chr[istopher?] Leanmhuinn, the words *Leanmhuinn Chriosd* being Gaelic for *De Imitatione Christi*. It is not necessary to choose collections of so recondite a character before the opinion we have quoted can be given: if it were, it should then be added that a great public library like that of the Museum is not only a larger collection of languages than that of Mezzofanti, but of all other special pursuits as well. And the instance of which we have spoken is a better illustration than any blunder which detects itself by its own absurdity. The English or the Gaelic scholar will not be deceived by the cases we have quoted: the worst that can happen is, that the inquirer who looks under Ceylon for Cingalese misses a book, and it is as if Mezzofanti had not possessed that book. But if, as might possibly happen, Schooten's Descartes of 1649 were entered as what it really is, the second edition; and if that of 1659 made part of a set of Descartes's works,

¹ An English auctioneer was brought forward to give evidence upon the catalogue of the British Museum, who declared that cataloguing was not only easy, but very simple indeed, with the assistance of the librarians of the Museum, or of his own clerks. This gentleman was no way to blame, but those who imagined that a sale catalogue would serve the purposes of literature: if the Museum library were to be sold off, his evidence would be valuable; but the librarians of the Museum must not be employed as he proposed. For these gentlemen have no idea, with a volume of six tracts before them, of entering the title of the first, followed by "and five others": moreover, they waste time in writing down names of authors in their nominative cases, when the books before them give genitives; and in other ways.

as it often does ; and if, as is very common, the *Opera omnia* were insufficiently detailed in the catalogue ; and if, as generally has happened, a mathematical historian were somewhat easy on the point of bibliography—the works of Hudde, etc., might disappear from history, as other works have done in a similar way, without disappearing from libraries.

The fourth book in question is another work of Maurolycus, the *Opuscula Mathematica*, Venice, 1575, 4to. On the title-page it appears that this collection was then published for the first time. It consists of a collection of tracts, and of a work on arithmetic with a second title-page of the same place and date as the first. Here, as often happens, is a source of confusion ; in binding, these works are separated, each title-page being made the beginning of a separate work. Two things are very common at the date now before us, the binding up of different publications in one, and the distribution of one publication under different title-pages, often without any mark by which to know that all the titles belong to one work. Hence catalogues sometimes represent different publications as one, and sometimes represent one publication under several heads ; the binder being the authority in both cases. On looking¹ more narrowly, to see whether the work itself gives any information on this point, we find, on the verso² of the first title-page, a table of contents, at the end of which is “*Quibus, omnibus arithmeti corum libri duo demum accesserunt*”. This is conclusive as to one difficulty, but it

¹ The *Anti-bibliographers* contended that any one could make a catalogue who could write a title-page : they did not appear to be aware of the necessity of examining the book. In one book we have before us three treatises of Ozanam, on conic sections, loci, and equations, all Paris, 1687, 4to, all from one publisher, whose residence is described in one way in the first and second, and in another way in the third. Unless they are three separate works, or all one work, either of which is very possible, the presumption furnished by the title-pages is that 1 and 2 were published together, and 3 separately. But an examination of the prefaces shows that 1 was published separately, and afterwards 2 and 3 together.

² The *recto* and *verso* of a leaf are the two pages in the order in which they come. We must use the technical term here, because, if we had only said that on turning over the title the table of contents was seen, it might have been on the *recto* of the next leaf, and no reprint of the title could have been inferred.

introduces another. The word *demum* usually indicates that the edition in question is not the first: *at last*, we are told, the two books of arithmetic are added. Either then there are previous editions without the arithmetic, or at last the arithmetic is added, and a new title-page, probably of later date, printed before all. Nevertheless, we can find no indication of any earlier edition or earlier title-page. None is mentioned: the arithmetic now under consideration has in it a list of works, distinguishing and dating those which were printed, but not containing anything to our present purpose. The maker of a catalogue would be compelled to raise the doubt of a second edition, or of a title-page with a new date, unless he happened to know that Maurolycus died in 1575. We now learn the meaning of *demum*: the work had been waiting for the arithmetic, which the author could not or would not finish, and his death *at last* enabled the publisher to obtain the manuscript, and complete the undertaking. This view of the case is enforced by our finding the arithmetic wholly destitute of preface or introduction, and with some gaps in the manuscript.

These circumstances, apparently so unimportant, help to decide an historical question which is not without interest. We have seen that the *Cosmographia* has a passage which indicates a lurking fear that the doctrine of the earth's motion was likely to be maintained. Though, by 1543, there was plenty of time to become aware of Rheticus's announcement of Copernicus, yet Maurolycus tell us, with the utmost definiteness, at the end, that the work was "finished at Messina, in the straits of Sicily, on Thursday, October 21, indiction ix, in the year of grace 1535, being the day on which the Cæsar, Charles V., returned to Messina from his African expedition". May we, in the face of such an announcement, suppose that this work afterwards underwent augmentation? If not, we have such presumption of the doctrine in question being in agitation, as it might be difficult to find elsewhere. But this presumption is destroyed by the work on arithmetic, which, though certainly unfinished, is terminated by the announcement, that it was finished "at the eighteenth hour of the Sabbath, July 24, when the viceroy

Jo. Cerda was expected at Messina, *cum multo pontis et arcus apparatu*, indiction xv, 1557".

These four books, taken down for a first chance, merely to make an opening, have caused great inroad on our space. We shall take a few other illustrations. *Publication* is now commonly confounded with *printing*, though history swarms with instances in which the first was long prior to the second. There are those who would contend for the equivalence of the two words; but perhaps there is no instance more to the point, in proof of the general aptitude to distinguish between the two, than the case of the Academy of Sciences. This body did not begin to print its periodical volumes of transactions, in the manner done by the Royal Society from 1665, until after the *renouvellement* in 1699. It was not until 1729 to 1733 that the Academy published the collection in eleven volumes (fourteen parts) containing the memoirs from 1666 to 1699, which is now considered as a commencing part of the series. Nevertheless, no one ever referred to the memoirs therein contained, as published at any other date than that at which the subsequent printed volumes showed them to have been communicated to the Academy. The real earlier publications made at the instance of the Academy are the "Mémoires de Mathématique et de Physique," in two parts, Paris, 1692, 1693, folio; the "Divers Ouvrages de Mathématique et de Physique," Paris, 1693, folio; and the "Regiæ Scientiarum Academiæ Historia," by J. B. du Hamel, of which the second¹ and enlarged edition is Paris, 1701, quarto.

The time at which the confusion between publication and printing is most injurious is that at which the printed book was not the exclusive medium, and the manuscript had not altogether disappeared; a period which includes at least a century after the invention of printing. For so long, at least, did writers who had no particular pretension to be antiquaries, cite manuscripts and printed books indiscriminately; and very often without distinction of character: so

¹ This work must be considered as the accredited contemporary corporate early history of the Academy of Sciences; and Brunet (in his earliest edition at least) makes it head an article on this Academy.

that subsequent writers, who thought only of printed books, have taken as printed all they could find cited as published. In this way we have the two unprinted works of Werner, as already mentioned, incorporated by Weidler with the printed ones.

We have noticed, as an introduction, and by way of amusement, the manner in which the French *experts*, as they are called, made the bibliographer forge a title at the *beginning* of an old book, by way of altering the edition, forgetting the description at the end. Those who have experience in books, even of a very moderate extent, know that they must always look at the end; because publishers of a former day did sometimes change the venue: not indeed by stamping in the names of convent libraries, but by printing special title-pages. We have before us a folio which, according to the title-page, is Candalla's Euclid, Paris, 1602. Though a tolerably good copy, and in old morocco, with gilt leaves, it was picked up on a mean stall in the open air, at a very low price. The fact is, that in its descent, it did not meet with any real *expert*, who looked at the end, where it appears that it is the Lyons edition of 1578, and that the Parisian title-page is a substitute. It is the only edition of Candalla that contains all the three books which he added. We have not called such a proceeding a trick and a forgery, because it was often something else. A long time elapsed before the characteristics of a book became matter of settled convention. At first there were *no* title-pages; all the description came at the end; and a word or two after the publisher's preface, if any, such as, "Joannis de sacrobusto anglici viri clarissimi Spera mundi feliciter incipit," was the reader's introduction to his subject. Afterwards, very short fly-titles or half-titles, as they are now called, were introduced in a blank leaf. Thus in one book we have, "*Ad inveniendum novam lunam et festa mobilia. Liber perutilis*"; in another we have, "*Quæsto e ellibro che tracta di mercatantie et usanze de paesi*". As regular title-pages were introduced, the full descriptions at the end being still generally retained, the publishers seem to have frequently made use of them as a kind of advertisement *prefixed* to the

book, of which they were hardly yet considered as a *part*: just as, in our time, we do not consider the lettering at the back as part of the book. Hence, when a stock of any book came into the hands of a bookseller who was not the original publisher, he frequently printed a new title-page to attract attention to the place of deposit, the original place, date, etc., being still to be read at the end. But the same practice continued when the *colophon*, or final description, fell into disuse, and the practice then ceased to have any justification, since the title-page had become the principal direct means of identifying the book. And thus it happens that, in all time, difficulties occur with titles. Nor do we see any hope of their final disappearance, as to books yet to be published; unless indeed an increased taste for bibliography should direct opinion against the following practices.

First, new titles are frequently printed, with new dates, sometimes with, and sometimes without, the words *second edition*. Sometimes the words *revised and augmented* are added, without any change whatever in the book. An author may thus lose his priority of discovery, of adaptation, or of introduction. A printer may thus lose his character as an artist; he may be judged in 1852 by his type of 1842; and similarly, the skill and knowledge of the author in 1852 may be set down as being what they were in 1842. It often happens that the author has no knowledge of what has been done: the edition may pass from the hands of the original publisher into those of another, with whom the author has nothing to do. Sometimes the alteration is made by the original publisher.¹

Secondly, a title sometimes undergoes alteration which, whether by intention or carelessness, gives an account very different from the truth. We have before us a book in which the genuine title describes it as containing matter from 1700 to 1846, mostly German; the substituted title describes it

¹ We have heard of a case in which a publisher contracted to pay a certain sum to an author on the appearance of a second edition. Forgetting this contract, and finding the book sell but slowly, he tried to help it forward by the bait of a new title-page, with the words *second edition*. The author immediately claimed his due, which the publisher was obliged to pay. *O si sic omnia!*

as containing all matter up to 1846, in Germany and the adjacent countries.

Thirdly, even in the original title, it is not uncommon to make the date a year later than the actual date of publication. When the book is published in the last months of the year, so that the right date will soon make it appear a year old, the next date is frequently used. Authors should look to this practice, by which their priority may be seriously compromised. Fifty years hence, a discovery, or other matter of merit, under the date 1851, will certainly be held to have preceded the same under the date 1852. But if a publication made in September, 1851, be dated 1852, there is time enough for another to republish it under the date 1851, and thus, with or without intention, to secure it in future history. From the preface of the Latin edition of Wallis's Algebra, it appears that this practice of advancing title-pages was common in the year 1685. The truth is, that the year alone is not now definite enough: every title-page should bear the *month* of publication, as well as the *year*. It would also be of much advantage if there were an understood place, as at the end of the preface, where the author should mark the last date at which any matter was added to the work, not including the verbal alterations which take place in correcting the press.

Fourthly, it is becoming common to publish books without a date whenever they are of a species which rapidly grows old, as in the case of atlases and of popular astronomical books.

All these things are objectionable, and will certainly cause confusion. The accurate date of any book, no matter how obscure in its own day and in that which follows, may become of importance at a still later epoch.

But though title-pages have frequently made erroneous announcements, still more frequently have their contents been misrepresented; in no particular more frequently than as to the name of the author. There is a loose system of description under which any prominent proper name is taken for that of the author. If the modesty of a commentator should lead him to print his own name in smaller capitals

than that of his original, it is very possible that his comment may be entered as the original work. If a friend or patron should contribute a preface, he will perhaps get credit for the whole; thus Billingsley's English Euclid has been entered under the works of John Dee, who wrote the introduction. The inventor of logarithms has before now figured as the author of the *Bloody Almanac*, which to an unattentive title reader is "by the noble Napier". The reason is that John Booker, the real author, announces his work to contain an "Abstract of the prophecies . . . by the noble Napier". The Latin forms of names do their part: we remember to have noted some confusion between the contemporaries, Francis Patrizi and Francis Barozzi, arising out of their descriptions as *Franciscus Patricius* and *Franciscus Barocius Patricius Venetus*. Must a librarian set down J. Ralphson, F.R.S., the author of a mathematical dictionary in 1702, as a different person from J. Raphson, F.R.S., who wrote at least four other works of a contemporary date? He will be wrong if he do; but nothing except an examination of the lists of the Royal Society will enable him to be certain. Out of such trifles as these spring many mistakes, such as can hardly be avoided, except by knowledge beyond what the books themselves can give. And as to the books themselves, nothing short of a studied examination will show the difference between a perfect and imperfect volume. A folio collection of astrologers (1533) which has at the end of the contents "*Postremo Othonis Brunfelsii . . .*" has the work of this Otho first instead of last. We have seen many volumes which were really perfect marked *imperfect*, on the assumption that the contents and their tables of contents must tally in order, as in a modern work. But there is a source of confusion about very old works which has not been much noted hitherto, and which promises to give rise to much inquiry. The copies of the *same edition* of the *same work* do not agree with one another. Sometimes there is a discrepancy of this kind. The impression seems to have been printed without any of the large and ornamented capital letters: these were stamped into *a part* of the impression afterwards, leaving the remaining copies with empty spaces for those who preferred

to have these letters wholly the work of the illuminator. Sometimes different headings were put in to suit different tastes. For example, we have before us a copy of the first printed edition of the Alphonsine Tables, Venice, 1483. The heading or title is in red ink, as follows: "Alfontii regis castelle illustrissimi celestium motuum tabule: . . ." Hain's description (*Repert. Bibliogr.*) shows that he had inspected a counterpart of this. But Captain Smyth (*Cycle*, etc., vol. ii., p. 215) has given a facsimile from another copy of this same edition, by which it appears that the heading is in black ink, having a picture of some astronomers looking at an armillary globe, imbedded in the following inscription: "Tabule Tabularum Celestium Motuum Divi Alfonsi Regis Romanorum et Castelle illustrissim".

Again, the *Summa de Arithmetica*, etc., of Lucas Pacioli, 1494, has the first pages¹ differently printed in different copies, ending with different words. This one book begins in three different ways, certainly; perhaps in more.

The reader who wishes to find more extensive accounts of bibliographical difficulties is referred to the report of the Commissioners mentioned at the beginning of this article, and² to its appendix. He may also consult the *Quarterly Review*, No. 143, May, 1843, or the *Dublin Review*, No. 41, September, 1846. We need not add more examples; we content ourselves with the production of enough to show those who have only seen the popular view of the controversy that there is a case on the other side which it is easy to support by instances. And this case might be much strengthened by having recourse to examples from literature instead of science: the former subject presents a wider field, more causes of accidental confusion, and more cases of intentional obscurity.

Much of the misapprehension which has prevailed on

¹ The author of this article showed, in his *Arithmetical Books*, that there are two commencements of this edition. Prince Boncompagni, to whose researches the early scientific bibliography of Italy is much indebted, and will be more, has since found a third.

² Particularly (No. 12, p. 378) a letter addressed by Mr. Panizzi to Lord Ellesmere, the chairman, at the commencement of the proceedings: this letter ought to be republished in a separate form.

the question of library catalogues in this country has probably arisen from the anomalous position in which the Museum library has been placed. On the one hand, it is the resort, daily or occasional, not only of those who know what accurate research is, but also of those who are learning it, who arrive thither to make some investigation, and are led on, by the *genius loci* and the temptation of ready means at hand, until they attain a depth far beyond their first intention. It is difficult to overrate what this national library has done, and is doing, for the cause of accuracy. And though a certain writer who describes himself, by implication, as of *delicate intellect*, sneers at the *manufacture of the stuff called useful knowledge*, which is carried on at the Museum, yet all whose understandings deserve a sounder title will see how much better that indispensable manufacture must go on, with such a library at command of the workman. This workman, fifty years ago, could obtain nothing but what his publisher could lend him in nine cases out of ten. To all of whom we have hitherto been speaking, a correct description of books is most essential; and by half of them, at least, old books, such as we have been examining, are frequently consulted. On the other hand, the library is frequented by many who only require the books of most easy description, and by many who come but for books of amusement. These classes might be suited by a very easy catalogue, as to most of the books which they want; probably such entries as *Encyclopædia Britannica* and *Guy Mannering* would serve their usual purposes. But these classes have not been useless. It may be suspected that the respect with which the House of Commons has treated the Museum library is due to the system under which most voters may obtain admission; and also that, if a library of research had been set apart for men of research, its interests would have been joked, yawned, or sneered out of the House by the unlearned majority. Nevertheless, so soon as literature can run alone, there are many and obvious reasons why a separation should take place between the libraries of the *reader* and of the *investigator*.

The mistakes into which professed bibliographers once

fell have been illustrated in this article, but not their application; for which, at length we had not room. We had, however, no doubt that, before our conclusion arrived, we should casually meet with something new and striking on this point, which might serve as an instance; and we were not disappointed. The *Bibliotheca Philosophica Struviana* . . . Gottingen, 1740, 2 vols., 8vo, by L. M. Kahle, is a professedly bibliographical work, and dates from about the time when Newton's system began to find general favour on the continent. After describing Motte's English version (1730) of the *Principia*, Kahle adds that one instance will be quite enough to show the bad faith of the version. He then quotes the celebrated scholium in which Newton admits the claim of Leibnitz, and quotes Motte's translation, which is of course of a very different purport; adding that the English translator, in order to deprive Leibnitz of honour, has been impudent enough (*eo usque procedit impudentiæ*) to alter Newton's words. Had the bibliographer remembered, or taken care to ascertain, that Newton himself published three editions, he would have found that Motte was correctly translating from the third of them, and that the substitution was made by Newton himself. At the same time, Kahle's blunder may serve to warn translators that they ought to be very precise in stating the editions on which their versions are made, and the most important, at least, of the variations: together with a sufficient description of the previous editions. And further, foreigners should take notice that English writers are well able to pay in kind any confusion made among the writings of Newton. In proof of this, we have, since the preceding sentences were written, fallen in with a recent work in which Kahle is placed under suspicion of having, under the name of *Kayle*, answered Voltaire by plagiarizing an answer written by *Kahle* seventeen years before Voltaire wrote.

If we ourselves should have fallen into any mistakes, they will serve our purpose, as helping to prove the truth of our title. They will do us a service of the same kind which a lapse of memory of Mr. Macaulay's does for him. In his review (which, like the work itself, is much too short)

of the *Pilgrim's Progress*, speaking of the tediousness of the Fairy Queen, he observes that "very few and very weary are those who are in at the death of the Blatant Beast". The reviewer himself, no doubt one of the few, was also one of the weary; for the blatant beast is *not* killed, and the very last verse extant of the poem shows us that Spenser kept him alive for good reasons of his own.

A. DE MORGAN.

University College, London,
October 4, 1852.