CAMBRIDGE LIBRARY COLLECTION

Books of enduring scholarly value

Mathematics

From its pre-historic roots in simple counting to the algorithms powering modern desktop computers, from the genius of Archimedes to the genius of Einstein, advances in mathematical understanding and numerical techniques have been directly responsible for creating the modern world as we know it. This series will provide a library of the most influential publications and writers on mathematics in its broadest sense. As such, it will show not only the deep roots from which modern science and technology have grown, but also the astonishing breadth of application of mathematical techniques in the humanities and social sciences, and in everyday life.

Arithmetical Books from the Invention of Printing to the Present Time

In the preface to this work, mathematician Augustus De Morgan (1806–71) claims that 'The most worthless book of a bygone day is a record worthy of preservation.' His purpose in writing this catalogue, published in 1847, was to provide an accurate record of the early history of publishing on arithmetic, but describing only those books which he had examined himself. He surveyed the library of the Royal Society, works in the British Museum, the wares of specialist booksellers, and the private collections of himself and his friends to compile a chronological list of books from 1491 to 1846 (the final book being a work of his own), giving bibliographical details, a description of the contents, and sometimes comments on the mathematics on display. De Morgan's *Formal Logic* and a *Memoir of Augustus De Morgan* by his widow are also reissued in the Cambridge Library Collection.

Arithmetical Books
from the Invention of Printing to the Present Time

Being Brief Notices of a Large Number of Works Drawn Up from Actual Inspection

Augustus De Morgan

CAMBRIDGE UNIVERSITY PRESS

CAMBRIDGE
UNIVERSITY PRESS

University Printing House, Cambridge, CB2 8BS, United Kingdom

Cambridge University Press is part of the University of Cambridge.
It furthers the University's mission by disseminating knowledge in the pursuit of education, learning and research at the highest international levels of excellence.

www.cambridge.org
Information on this title: www.cambridge.org/9781108070959

This edition first published 1847
This digitally printed version 2014

ISBN 978-1-108-07095-9 Paperback

ARITHMETICAL BOOKS.

LONDON:
PRINTED BY ROBSON, LEVEY, AND FRANKLYN,
Great New Street, Fetter Lane.

ARITHMETICAL BOOKS

FROM

THE INVENTION OF PRINTING TO THE PRESENT TIME

BEING

BRIEF NOTICES OF A LARGE NUMBER OF WORKS

DRAWN UP FROM ACTUAL INSPECTION

BY

AUGUSTUS DE MORGAN

OF TRINITY COLLEGE, CAMBRIDGE
SECRETARY OF THE ROYAL ASTRONOMICAL SOCIETY: FELLOW OF THE CAMBRIDGE PHILOSOPHICAL SOCIETY
AND PROFESSOR OF MATHEMATICS IN UNIVERSITY COLLEGE, LONDON.

"Much surprised, no doubt, would the worthy man have been, had any one told him that two hundred years after his death, when no man alive would think his ideas on the nature of mathematics worth a look, the absence of better materials would make his list of" arithmeticians "not only valuable, but absolutely the only authority on several points."—DUBLIN REVIEW, No. XLI.

LONDON
TAYLOR AND WALTON
BOOKSELLERS AND PUBLISHERS TO UNIVERSITY COLLEGE
28 UPPER GOWER STREET

1847

TO THE

VERY REVEREND GEORGE PEACOCK, D.D.

DEAN OF ELY, LOWNDEAN PROFESSOR,

&c. &c. &c.

MY DEAR SIR,

It never entered into my head till now to adorn the front of any book of mine with an eminent name: and the reason I take to be, that I have hitherto never chanced to write a separate work* upon any subject with which the name of one individual was especially associated in the minds of those who study it. But you are the only Englishman now living who is known, by the proof of publication, to have investigated both the scientific and bibliographical history of Arithmetic: and this compliment, be the same worth more or less, is your due, and would have been, though my knowledge of you had been confined to your writings. And it is the more cordially paid from the remembrance of nearly a quarter of a century of personal acquaintance, and of many acts of friendship on your part.

I have rather grown than made this catalogue. It never occurred to me to publish on the subject, till I found, on a casual review of what I had collected, that I could furnish from my own books a more extensive list than Murhard, Scheibel, Heilbronner, or any mathematical bibliographer of my acquaintance, has described *from his own inspection*. Knowing, from sufficient experience, the general inaccuracy and incompleteness of scientific lists, I therefore determined to do what I could towards the correction of both, by describing as many works as I could manage to see. From

* Had the regulations of the work in which it appeared permitted, it would have been most peculiarly appropriate to have inscribed my treatise on the *Calculus of Functions* to my friend Mr. Babbage.

the Royal Society's library, the stock of Mr. Maynard the mathematical bookseller, and my own collections, with a few from the British Museum and the libraries of private friends, including three or four of great rarity from yourself, I have accordingly compiled the present catalogue. I have also given in the Index, in addition to the names of the authors whom I have examined, those of all whom I could find recorded as having written on the subject of Arithmetic, whether as teachers, historians, or compilers of special tables for aid in the main operations, independently of logarithms and trigonometry.

A great number of persons are employed in teaching arithmetic in the United Kingdom. In publishing this work, I have the hope of placing before many of them more materials for the prevention of inaccurate knowledge of the literature of their science than they have hitherto been able to command, without both expense and research. Your History, unfortunately for them, is locked up in the valuable, but bulky and costly, Encyclopædia for which it was written. I may have the gratification of knowing that some, at least, of the class to which I belong, have been led by my catalogue to make that comparison of the minds of different ages which is one of the most valuable of disciplines, and without which the man of *science* is not the man of *knowledge*.

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.

The effect of this work which would please me best, would be, that the professed bibliographer should find it too arithmetical, and that the student of the history of the science should find it too bibliographical. I might certainly have entered more into the methods of the several works, with advantage to the reader. But I could not attempt to write the complete annals of arithmetic; this would require still more books: neither could I, without losing sight of my plan altogether, combine the information here presented with that derived from other sources. That plan has been, to attempt some rectification of the numerous inaccuracies of existing catalogues, by recording only what I have seen myself. And it is a sufficient justification of the course I have taken, that I have produced in this way a *larger* catalogue than yet exists among those devoted, in whole or in part, to this particular subject.

None but those who have confronted the existing lists with the works they profess to describe, know how inaccurate the former are; and none but those who have tried to make a catalogue know how difficult it is to attain common correctness. There is now a prospect of this country possessing in time *such a record of books as can be safely consulted in aid of the history of literature*,—I refer to the intended catalogue of the library in the British Museum. I, for one, can only hope that the chance will not be lost by any attempt to expedite its formation, in deference to the opinion of those who either are not aware how bad existing lists are, or are willing to take more than a chance of having nothing better. If, through negligence or fear on the part of those who have really compared book-lists and books, the expression of public feeling which any *primâ facie* case against public officers so easily obtains, should succeed in hurrying the execution of this national undertaking, the result will be one more of those magazines from which non-existing books take their origin, and existing ones are consigned to oblivion by incorrect description. However extensive the demand for spoiled paper may be, it should be remembered that the supply is immense, and that there is no need to insist on Parliament, at least, furnishing a larger contingent than it is obliged to do already.

I make no apology for troubling you to read this diatribe: it is your affair, and mine, and that of every one who believes accuracy to be an essential characteristic of useful knowledge.

I remain,
My dear Sir,
Yours sincerely,

A. DE MORGAN.

UNIVERSITY COLLEGE, LONDON,
April 29, 1847.

*** In consequence of a little change in the plan of this work, made after the printing was commenced, there may be one or two places in which the reader must consult the *Introduction*, where he is told to consult the *Index*; and the *Additions*, where he is told to consult the *Introduction*.

INTRODUCTION.

At the end of this work will be found an Index containing, besides the names of the authors mentioned in the Catalogue, who may be known by the paginal figures opposite, every name which I have found *any where* as belonging to a writer on arithmetic before 1800. Since that period I have not swelled the list from mere *catalogues*, but have contented myself with what I had either seen, or learnt from some other source. For the existence of such writers, or of their works, of course I do not vouch, except in those cases in which the work is found in the body of the Catalogue. Of those who stand unpaged in the index, all I can say is, that they are taken from many sources, and that there is in each case plenty of reason to make inquiry for their writings, and strong presumption that their works are still to be found.

A word with an asterisk before it is the name of a writer who is referred to by Dr. Peacock. The great apparent preponderance of German names arises partly from the number of works written on the subject in Germany being very considerable, partly from the bibliographical catalogues of that country being more full than those of any other, and partly from the names which seem to be German, really including Danes, Dutch, Belgians, and some Swiss. I am far from thinking that this list contains even the third part of the names of those who have really written on the subject. I have not been particular in searching for any thing after 1750, though I have not refused what came in my way. I might have very much increased the list of recent Germans, from Rogg's *Bibliotheca Selecta* (Tubingen, 1830). Looking at the various countries which enjoyed the art of printing from 1500 downwards, I have an impression, from all that I have gathered, which would lead me to suppose that the number of works on arithmetic published in Latin, French, German, Dutch, Italian,

Spanish, and English, up to the middle of last century, cannot be less than three thousand, which gives to each language less than an average of one a year. Few of these would seem to fall within the province of the historian. Dr. Peacock refers to about a hundred and fifty. Unfortunately, history must of necessity be written mostly upon those works which, by being in advance of their age, have therefore become well known. It ought to be otherwise, but it cannot be, without better preservation and classification of the minor works which people actually use, and from which the great mass of those who study take their habits and opinions. The *Principia* of Newton is, if we believe the title-page, a work of the seventeenth century: but the account of the effect which it produced on science belongs to the eighteenth. It was not till many years after the publication of the *Principia*, that its predecessor in doctrine, the great work of Copernicus, produced its full effect upon general thought and habit. Nor have we any reason to suppose that it could have been otherwise. The great exceptions will always bear, perhaps, as large a ratio to the average power as ever they did: it is as likely as not, that if the intelligence of the sixteenth century had been sufficient to verify and receive the opinion of Copernicus at once, some predecessor of his might have been Copernicus, and he or another of his day might have been Newton.

It is then essential to true history, that the minor and secondary phenomena of the progress of mind should be more carefully examined than they have been. We must distinguish between the progress of possibilities and that of actual occurrences. Our written annals shew us too much what might have been, and too little what was: they give some words to the slow reception of an improvement, and more sentences to the account of the one man who was able to make it before the world at large could appreciate it.

The public is beginning to demand that civil history shall contain something more than an account of how great generals fought, great orators spoke, and great kings rewarded both for serving their turn. The progress of nations might well be described, for the most part, with much less mention of any of the three: but the parallel does not hold of knowledge. Copernicus and Newton would fill a large space, though the history were written down to that of every individual who ever opened a book: but it seems to me that they, and their peers, are made to fill *all the space*. Nor will it be otherwise until the historian has at his command a readier access to

second and third rate works in large numbers; so that he may write upon effects as well as causes.

This list contains upwards of fifteen hundred names, of which a few may be duplicates of others, arising from the wrong spelling of my authorities.* But these must be much more than counterbalanced by the number of names which belong to two or more authors, but which only appear once in my list. Thus there are two Digges's, three Riese's, two named Le Gendre, Walker, Newton, Wallis, &c., and several Taylors, Butlers, &c., though each name is only mentioned once. There are several cases in which I have not ventured to strike out one of two names, though there is every reason to suppose one is a mistake for the other; as Caraldus and Cataldus, Cappaus and Cuppaus, Döhren and Dühren. But I have often been deceived in this way; and have more than once been obliged to re-insert names which I had struck out, supposing them to be only different spellings of names already in the list. There are several whom I have seen in more places than one, who are clearly Germans with names metamorphosed by wrong reading of the black letter, or use of the genitive for a nominative, or both. Thus Petzoldt has become Pekoldts, Seckgerwitz has been made Sectgerwik, Schultze has been made Schulken, and so on. These obvious mistakes of course have not been admitted. Moreover, several persons are, I suppose, down in the list under two names by which they are known. Thus, Fortius may be Ringelbergius (whose real name was Sterk), Blasius may be Pelacanus. But as I cannot undertake to assert that there is no Fortius except Ringelbergius, &c., I have let both names stand in the list. Again, next to Buckley comes Budæus. Many foreign writers, Heilbronner among the rest, have turned *Buclæus* into *Budæus*, so that in all probability the second of these names is a mistake for the first. And yet there may be also some Budæus who has written on arithmetic; though, having excluded writers on weights and measures only, I have not put down the author of the work *De Asse*.

I have had, in one or two instances, to throw away German *authors*, for a very obvious reason. The reader will not find the works of *Anleitung*, or *Grundriss*, or *Rechenbuch* in my list, which is more than can be said of every one which has preceded it.

I have not attempted to translate the names of those who

* It is necessary, for instance, to keep close watch upon a writer who introduces among his English authors *Gul. de Cavendy dux de Xeucathle*.

wrote in Latin, at a time when that language was the universal medium of communication. In every such case I consider that the Latin name is that which the author has left to posterity; and that the practice of retaining it is convenient, as marking, to a certain extent, the epoch of his writings, and as being the appellation by which his contemporaries and successors cite him. It is well to know that Copernicus, Dasypodius, Xylander, Regiomontanus, and Clavius were Zepernik, Rauchfuss, Holtzmann, Muller, and Schlüssel. But as the butchers' bills of these eminent men are all lost, and their writings only remain, it is best to designate them by the name which they bear on the latter, rather than the former. In some cases, as in that of Regiomontanus, both names are frequently used: in others, where the Latin consists in a termination only, as Tonstallus for Tonstall, or Paciolus for Pacioli, it matters nothing which is used. It may happen that errors are introduced by returning to the vernacular in a wrong way. I should like to know how it is shewn that Orontius Fineus was Oronce Finée: or in what respect this reading is more finically correct than Horonce Phine, which has great antiquity in its favour? In the case of Vieta, Viète is certainly wrong: he was called by his contemporaries Viet (which I suspect to be derived from the Latin form) and de Viette, never Viète.

It may also be asked, why the unlatinizing process should, for confusion's sake, be practised by the learned only; when it is pretty certain that the world at large will never reconvert Melancthon into Schwartzerd, or Confucius into Kaen-foo-tzee. Neither will they restore to the Popes and other priests of the Roman Catholic Church the names under which they were born and educated.

In many cases it would be impossible to recover these last. For myself, I am well content with the name under which an author was known in literature to his contemporaries, and has been handed down to us, his successors. I know of no canon under which it is imperative to speak of a writer rather as his personal acquaintance than as his reader: and, so far as feeling of congruity is concerned, I think *Alexander ab Alexandro* looks better at the head of a Latin preface than *Saunders Saunderson*. Those who really wish to catch the tone of the middle ages, and shew themselves quite at home, should dwell on the *Christian* name, and make the surname a secondary distinction; should learn to think of *Nicolas* (who happened to be called Copernicus, or Zepernik, it should matter little which) and *Christopher*, to whom the calendar was entrusted.

That this list must be very imperfect I am well aware, for I have been able to add many names to it which I never found in any catalogue. But it will not be useless. It may furnish a reason for preserving any work whatsoever which comes in the way of the reader of this book. If the name be in the list, a book should not be destroyed which has been somewhere catalogued and recognised as a portion of the existing materials for the history of science. But if the name be not in the list, it is obvious that there is some curiosity about a writer whose name is not in the most numerous catalogue of arithmetical authors that has ever been collected in any one place. And it is undeniable that every name must be in one or the other predicament.

I now come to the Catalogue which forms the body of this work. The defects, which every one who has examined the lists that are extant, knows to prevail, arise, in great measure, from the titles and descriptions of books being copied by those who had never seen the books themselves. This is not the worst; for a true copy of a true copy is a true copy: many of these accounts have finally become formal representations of informal titles, which, though not originally intended for more than sufficient indication of the book mentioned, have been used as if they had been full and accurate descriptions. There is a large number of works, not much distinguished in the history of science, each of which has, nevertheless, done its part in its day. The minor points of the same history depend much upon these books, which, being neither of typographical curiosity, nor of literary fame, are gradually finding their way either to the waste-paper warehouse, or the public library. These two depositories are almost equally unfavourable to works of no note assuming their place in the annals of the knowledge to the progress of which they have contributed. Take the library of the British Museum, for instance, valuable and useful and accessible as it is: what chance has a work of being known to be there, merely because it is there? If it be wanted, it can be asked for; but to be wanted, it must be known. Nobody can *rummage* the library, except those officially employed there, who will only now and then have leisure to turn their opportunities to account in any independent literary undertaking. And it would perhaps be difficult to make any regulation, under which persons not belonging to the institution might have access to see what is there. Nor will the publication of the catalogue do much towards supplying the de-

fect. Titles of books are but vague indicators of their contents; and a catalogue* of half a million of entries, even if its contents could be guessed at by the titles of the books, is not made to be read through.

It would be something towards a complete collection of mathematical bibliography, if those who have occasion to examine old works, and take pleasure in doing it, would add each his quotum, in the shape of description of such works as he has actually seen, without any attempt to appear more learned than his opportunities have made him. This is what I have done in the descriptive catalogue of works on arithmetic, without selection, or other arrangement than order of date. The only reason for a work being in this list, is that it has come in my way: the only reason for one being out of it is, that it has not come in my way, at least at the time of compilation. Whenever any statement is made which is taken from any other writer, it will be put in brackets [ ], except when the statement itself cites an authority: though I have sometimes put the brackets even in the latter case. The only other mode of proceeding would be, to collect lists from authority, naming the sources of information. But, having found so many errors in these sources, when my opportunities have enabled me to bring them to the test, I did not feel inclined to be the tenth transmitter of inaccurate copies. My mistakes shall be of my own making, and it would not be easy to invent one which should want high precedent for its species.

The description of the books in the Catalogue is uniformly as follows. There are given:

1. The place at which the work was printed, in Italics, and generally in English.

2. The date of the title-page, or colophon, or, when both are wanting, of the preface, in words; but not quite at length. Thus instead of fifteen-hundred-and-eighty-eight will be found fifteen-eighty-eight. I am sure that dates will never be given correctly until this plan is universally adopted: for two reasons. First, the chance of error in printing is very much diminished: particularly the risk of a transposition of figures *at the press*. Secondly, the writer, who is, on the whole, and

* When the great catalogue of the Museum is published, those who can give house-room to forty or fifty volumes, and time enough to their examination, may have some of the advantage which they would derive from actual access to the books themselves. And those known to be engaged in research will derive a still larger portion of the same advantage from the readiness with which the officers of the Museum will go out of the usual routine of duty to help them.

one date with another, more to be feared than the printer, has time to be accurate. A glance at four numerals and four strokes with the pen, is too rapid a process for certainty: and those who think they can rely upon themselves to stop *every time*, and look at what they have done, will frequently find reason to wish they had been less confident. But *Incidit in Scyllam*, &c.: I had very nearly announced an edition of Cocker as being unmistakeably the seventeen-hundred-and-twentieth (see page 56).

3. The author's name. When an initial only is given, it is because the author has left no more, either in title or preface.

4. As much of the title as will certainly identify the book. In spelling, initial capitals, &c., I have generally followed the author closely. When there is any defect in this respect, I suppose it will be in those works which I had not any opportunity of re-examining while the sheets were in proof. Imitation of type I have not attempted. To have given the full titles would have swelled my book too much: at certain periods the authors of elementary works were much given to write out descriptive chapters in their title-pages; scores of them would each have filled two or three pages of even the smaller type used in the Catalogue.

5. The form in which the work is printed; a matter which will require some explanation.

A folio, quarto, octavo, duodecimo, or smaller work, is now generally known by its size, though not always. In the folio the sheet of paper makes two leaves or four pages, in the quarto four leaves, in the octavo eight, in the duodecimo twelve, and so on. But even a publisher thinks more of size than of the folding of the sheet when he talks about octavo or quarto; and accordingly, when he folds a sheet of paper into *six* leaves, making what ought to be a *sexto* book, he calls it a duodecimo printed in half sheets, because such printing is always done with half-sized paper, or with half-sheets, so as to give a duodecimo size. From a very early period it has been universal to distinguish the sheets by different letters called *signatures*. In the book now before the reader, which is a half-duodecimo (or what I call a *duodecimo in threes*), the first sheet which follows the prefatory matter, B, has B on the first leaf, and B 2 on the third; which is enough for the folder's purpose. But in former times the signatures were generally carried on through half the sheet, and sometimes through the whole. Again, in modern times, no sheet ever goes into and forms part of another; that is, no leaf of any one sheet ever

lies between two leaves of another. But in the sixteenth century, and even later in Italy, it was common enough to print in *quire-fashion*. Imagine a common copybook, written through straightforward, and the string then cut: and suppose it then separates into four double leaves besides the cover. It would then have sixteen pages, the separate double leaves containing severally pages 1, 2, 15, 16; 3, 4, 13, 14; 5, 6, 11, 12; 7, 8, 9, 10. If a book were printed in this way, it would certainly be a folio, if the four double leaves of any one quire or gathering were each a separate sheet: and if the sheet were the usual size, it would give the common folio size. But if each gathering had the same letter on all its sheets, if the above for instance were marked A_1 on page 1, A_2 on page 3, A_3 on page 5, and A_4 on page 7; the book, when made up, would have all the appearance of a more recent octavo in its signatures. In order to give the size of a book, and at the same time to give the means of identifying the edition by its signatures, I have adopted the plan which gives the following rules:

(a) The words *folio, quarto, octavo, duodecimo, decimo-octavo*, refer entirely to size, as completely as in a modern sale-catalogue, the maker of which never looks at the inside of a book to tell its form. All the very modern distinctions of *imperial, royal, crown, atlas, demy*, &c. &c. &c. I have relinquished to paper-makers and publishers, who alone are able to understand them. But in old books, the reader must expect to see the several sizes, each one of them, smaller than in the modern books. When the work is decidedly small of its name, I have noted it by the word 'small.'

(b) When the single word occurs, without any thing more, the signatures are as in the genuine meaning of the word. Thus, as to signatures, *folio* has two leaves to one letter of signature, *quarto* four leaves, *octavo* eight, *duodecimo* twelve; and of course double the number of pages.

(c) When the modern word occurs with the addition of *in twos*, or *in threes*, &c. the addition expresses the number of *double leaves* which belong to one letter of signature: and which I believe would be found, if the books were taken to pieces, to be in each quire or gathering. Thus, *folio in ones*, or *quarto in twos*, or *octavo in fours*, or *duodecimo in sixes*, would in each case be unnecessary repetition; for the first word, when alone, is intended to express the gathering in the third. But *folio in twos* would mean the folio size with two double leaves in one quire, *folio in fours* with four double leaves. Thus, a book of the octavo size, with the quarto sig-

natures, is *octavo in twos:* had it been larger, I should have called it *quarto*.

By this means there is something as to size, and something as to signatures, in every description. But whether any book which I call *octavo in twos*, for instance, really was printed on whole sheets, or on half sheets, that is, really was a small quarto, or a divided octavo, is more than I can in any case undertake to say. All I know is, that with these rules, the reader has two indications in every case, to guide him in determining whether the book he has in his hand be the one I describe or not.

This is no unnecessary excess of description. For so frequently, in the sixteenth and seventeenth centuries, were there issues of the same impression under different titles, that all I have done will in many cases only give a presumption as to whether a book in hand is or is not the edition I have described. Were I to begin this work again, I would in every instance make a reference to some battered letter, or defect of lineation, or something which would be pretty certain not to recur in any real *reprint*. Ordinary errata would not be conclusive: for these might be reprinted for want of perceiving the error.

Rules are given for determining the form of printing by the waterlines of the paper, and by the catchwords. It is supposed that the latter are always at the end of the sheet, and also that the waterlines are perpendicular in folio, octavo, and decimo-octavo books, and horizontal in quarto and duodecimo. But in the first place, a great many old books have catchwords at the bottom of every page, many have none at all; and as to the rule of waterlines, I have found exceptions to every case of it. Pacioli's Euclid, *Venice*, fifteen-nine, *folio in fours*, has horizontal waterlines: the Hypomnemata of Stevinus, an undoubted folio, has thick waterlines *both ways*.

6. In the smaller type are entered such remarks as suggested themselves on the manner or matter of the work, or on any point arising out of it. In some cases these have extended themselves to short dissertations, such as, on the geometrical foot, page 5,—on Sacrobosco's knowledge of the Arabic numerals, page 14,—on the invention of + and —, page 19,—on the age of Diophantus, page 47,—on the genuineness of Cocker's Arithmetic, page 56. But for the most part I have contrived to keep within a very moderate compass as to what is said under each work.

The principal point on which I have distinguished one work

from another, is as to the use of the *old* or *new* method of performing division, which, more than any other single point, decides the character of a work. The letters O, N, or ON, will tell whether the work uses the old, the new, or both. Not that the verbal distinction is here very correct; for neither method is older than the other; and both appear in Pacioli. A description of the now disused method is given by Dr. Peacock, p. 433.

With regard to the works themselves, I have made no selection, as before noticed. No book that I *have* seen during the compilation has been held too bad to appear; no book that I *have not* seen too good to be left out. I have had but one discretion to exercise, namely, to determine the extent to which algebra should be considered as arithmetic. In the earlier day, the distinction was slight: I doubt whether I have not overrated it; but it is not an easy line to draw.

The history of Arithmetic, as the simple art of computation, has found little notice from the historians of mathematics in general. They shew themselves deficient in the knowledge of its progress, and of the connexion of that progress with the rest of their subject. The writers whom I can name as having attempted—some more and some less—to supply this defect, are Wallis, Dechâles, Heilbronner, Scheibel, Kästner, Leslie, Delambre, Peacock, and Libri. I speak of the progress of arithmetical writings as works on science, independently of bibliography properly so called, and biography.

Wallis (p. 44 and Additions) was one of those writers whose works remain the standard of the erudition of their day. His *algebra*, so called, is rather the history, theory, and practice of both arithmetic and algebra. Its miscellaneous and badly indexed form prevents any from knowing what is in it, except those who make a study of it, which none of our day will do, unless they intend to go rather deeply into the history of the exact sciences. But many and many a page by which the writer intended to gain the credit of research, will be found to be a transcript from Wallis. As a connected history, however, it is nothing; and as a bibliography, less. For example, that Regiomontanus used decimal *fractions*—a very common story—is the consequence of Wallis's confused method of stating that he introduced the *decimal* instead of the *sexagesimal* radius into trigonometry; the confusion arising from his not having a clear knowledge of what had been published of Regiomontanus. And again, we find him, though the editor of an edition of Oughtred, balancing as to which was written first,

Oughtred's *Clavis*, or Harriot's *Praxis* (which were published in the same year), apparently ignorant that the great method as to which the two authors were chiefly to be compared, did not appear in Oughtred's first edition at all. But for all this, if Wallis be cautiously watched as to books and dates, his works are most valuable magazines of historical suggestion. From them might be collected a much better *scientific* history of arithmetic than existed in his time, or, indeed, in any time preceding the publication of Dr. Peacock's article on the subject.

Claude Francis Milliet Dechâles was a Jesuit, who published, in four very large folios, a complete course of mathematics, including architecture, carpentery, fireworks, and all that was then held to belong to the exact sciences (see page 53). The first volume opens with about a hundred pages (large folio, double column) *de progressu matheseos*, consisting entirely of description of books, in order of date. The part relating to arithmetic fills nine of these pages. The whole is done with much care, and is, for the mode of describing books current at the time, very accurate; and the opinions given on the books shew that Dechâles had read them. But he is strongly addicted to the very common mistake of judging the books according to what ought to have been said of them, if they had been published in his own time. For example, he finds that there is not sufficient demonstration in Tonstall; which is true, absolutely speaking; but Tonstall is a very Euclid by the side of his contemporaries.

Heilbronner's Historia, &c. (page 69), though it professes only to give writers up to the beginning of the sixteenth century, makes a particular exception in favour of arithmetic. Up to the year 1740, about 170 authors are recorded, a great many of whom he had not seen. There is also historical dissertation on points of arithmetic. This is a work of great value to the inquirer: he must not rest upon its statements; but he will find more than usual materials for further research.

Scheibel (Additions) may be considered as partly repetition, partly extension, of Heilbronner. He is one of those bibliographers who collect from various sources the names and dates of more editions than those who know catalogues will readily believe in.

Kästner (Additions) falls under the following censure from Dr. Peacock: "The meagre sketch which Kästner has given of some insulated works on the subject, generally contrives to omit almost every particular which is essentially connected

with the history of the progress of the science." This is well merited, inasmuch as the author chose to call his work a history, instead of a *bibliography;* and as the former, nothing can be more incomplete. I have almost as good a right to call my work a history, as Kästner his. But, as a bibliography, it may be urged in defence, that he gave fuller descriptions of books than his predecessors. Scheibel, Heilbronner, and Dechâles sink into title-writers (and bad ones) before Kästner.

The late Professor *Leslie* (page 89) was one of those men, the strength and asperity of whose opinions would make it fair to deal with them as they dealt with others. In his *Philosophy of Arithmetic* he has entered incidentally into much of its history. He was, by taste, a searcher of old books; and various dates, &c. occur, which shew that he had more knowledge of books than can be got from catalogues. A few words will sometimes shew this to a person who has compared the books with the accounts of them. But, writing in a popular manner, he does not give references to his authorities, which is a serious diminution of the value of his work. Of Leslie, as an historian on controverted points, one principal thing to be cautious of is, his almost monomaniac antipathy to every thing Hindoo—a most unfortunate turn for an arithmetical investigator. Those who inquire into this subject will see what he is in Colebrooke's hands; those who do not, may compare his description of the *Lilivati,* "a very poor performance, containing merely a few scanty precepts," with the summary of the contents of that work in the *Penny Cyclopædia,* article *Viga Ganita.* Leslie also generalises most fearfully every now and then. He informs us that it was the *practice throughout Europe* to reduce the rules of arithmetic to memorial verses, and that Buckley's *Arithmetica Memorativa* appears at one period to have *gained possession of the schools and colleges of England.* Now the truth is, that the *verses* attributed to Sacrobosco had never even been printed when Leslie wrote; and Buckley, so far as is known, was printed only once alone, and two or three times as an appendix to a work on logic. Dr. Peacock expresses the truth in saying that, *before the invention of printing,* the practice of *writing* memorial verses was common, as appears by manuscript libraries. It is needless to say that, had the practice of *using them* been common, the presses of the fifteenth and sixteenth centuries would have given them forth in great numbers. But I cannot learn that any metrical work was printed in the fifteenth century, except the *Compotus* of Anianus, and that only once.

Delambre (page 84) wrote on the history of Greek Arithmetic in such a manner as to set that part of the subject fairly going. I doubt if any one since the time of Wallis, at least of their order of note, has exhibited so much of the union of the *scholar* and the *computer*. Of the obligations under which the history of astronomy lies to him, it is unnecessary to speak here, or any where: no one man was ever so closely connected with that science, with its past, its present, and its future, by history, observations, and tables.

Of my opinion of Dr. *Peacock's* work (page 91) I need hardly say any more, after the pains which I have taken to give reasons on every point in which I differ from its author, and to correct every little error which I have found. This I judge to be the most undoubted compliment which can be paid to any work. Having looked carefully over it, with a great many of the works mentioned in it in my hands, it would be a strong evidence in its favour, were it needed, that I have found no more to set right than is there noted, in matters of dates and circumstances. It is much to be wished that this treatise should be published separately:* those who can obtain it will find that it gives life and spirit to the catalogue of books which forms the main part of this work. Up to this moment it is the only work which can be called a history of arithmetic.

M. *Libri's* history of science in Italy (page 95) is the work of a man who, to the character of a mathematician, adds that of a man who is well versed in literature, and a successful collector of the rarest works in that of his own country. There is much which is interesting in its early history of Italian Arithmetic. Unfortunately, it is not yet finished; and, I am told, is not likely to be speedily resumed.

The history of most of the sciences resembles a river which sinks underground at a certain part of its course, and emerges again at a distant spot, swelled by certain tributaries, which have joined it in the tunnel. Of arithmetic in parti-

* It was once intended to publish these treatises separately. Nine years ago, the proprietors of the *Encyclopædia Metropolitana* so fully intended to publish separately, that they considered themselves aggrieved because I, who had written the *mathematical* article on probabilities, wrote a *popular* work with that word in the title-page, which they alleged, through their agent, was in effect a republication of the former work, &c. Not being able to get them either to litigation or arbitration, I was obliged to write a pamphlet to prove that the charge was frivolous. The pamphlet is unanswered, and all the treatises unpublished (separately) to this day. The latter I regret, for the sake of science. It is a great pity that Sir John Herschel's treatises on light and sound, Dr. Peacock's arithmetic, Mr. Airy's tides, &c. are thus locked up.

cular, we note the disappearance in the seventh century of all that was good in the Greek system; and we see the rise, at the invention of printing, of the most trivial part of it, combined with the additions which are now well known to have been received from Eastern sources.

The manuscript literature of the middle ages will prove no very productive source of information, to judge by all that has been made of it hitherto. But then it is to be remembered how seldom, if ever, it has happened, that the investigator of it has united the character of a sufficient mathematician with that of an industrious and well-trained palæographist. Neither the one nor the other can proceed alone: the former has not only to learn how to read, but what to read; for the actual habitat of the manuscripts, and how to get preliminary knowledge of their existence, is a study of itself. The latter is apt to know no difference between what is sound, and what is worthless.

When Euclid, Ptolemy, &c. are first seen to reappear, they come in as Arabic writers. They may be called Greek, but the first translations are from the Arabic; and their effect upon the literature of Europe is, in the first instance, just what it might have been if the authors had been Persians or Saracens. How the communication took place has been considered as a point of small curiosity compared with the importation into Europe of the Arabic numerals.

This subject, both generally, and with reference to the several countries, has been long on the anvil: not the authorship of the letters of Junius has tasked research and ingenuity more than the introduction of the nine digits and the cipher. I suppose nobody would listen to the hypothesis that the former wrote themselves: but I am much inclined to suspect that the latter introduced themselves. The *endosmose* constantly going on between nations connected by war and commerce would not merely explain so easy a matter, but would render it very difficult to explain how it did not happen, if it had not happened. That the Venetian merchants should not know the system of accounts of those with whom they traded, is incredible: that, at the beginning of the thirteenth century, many priests and some soldiers, returned from their crusades, should not bring back with them the account of so very elementary a difference between themselves and those with whom they had treated, and whom they had in many instances served as slaves taken in war, is unlikely. That Leonard of Pisa, as he is called, was the first who wrote on or in the new system,

is pretty generally affirmed by the Italians of the fifteenth and sixteenth centuries: and there can be no doubt of it. But because we trace the first formal user or expounder to Italy, it by no means follows that the other parts of Europe owe the system to that country in the first instance, however certain it may be that they owe much assistance in the course of the general establishment. It was so common in England in the thirteenth century, that Roger Bacon recommends it (page 14) as a study, of course as a practicable study, for the clergy. Had it not been commonly known, at least as to what it *was*, he would have done more than give the name of the science, and the names of its rules; he would have added some description. Those who have searched into the matter, merely with a view to Arabic *numerals*, and without any collateral thoughts about *arithmetic* in their heads, may have passed over much valuable evidence. They did not know, perhaps, that the organised rules of computation always went with the Arabic system, and never with the Roman or Boethian: that if ever they came to the connected mention of addition, subtraction, multiplication, and division, it ought to have been a sign that they were reading on *algorism* as distinguished from *arithmetic*. This passage of Roger Bacon has been neglected; though the mere occurrence of the word *algorism*, in a most interpolable clause of one sentence, occurring in Matthew Paris, has received notice and discussion from Mr. Hallam, from a learned writer in the Archæologia, and probably from others.

The rejection of the work attributed to Sacrobosco may be accounted for from the general ignorance of its having been printed under his name at an early period. Though this does not prove the genuineness of the work, it very much adds to the evidence for it. As early as 1523, the learned world was invited to dissent from the assertion that the treatise in question was written by Sacrobosco, and did not do it. It was often cited afterwards, and never, as far as I have seen, with any doubt. But the only writer of this or the last century that I can find, who describes the *printed* edition, is the Italian editor of Fabricius, *Bibliotheca Latina*.

Come how they might, however, we find ourselves, at the invention of printing, in possession of the knowledge that two distinct systems of arithmetic were current. The streams which had united in one bed had not mingled their waters; and for nearly a century two distinct classes of writings present themselves. Their only common point is the use of the Arabic or Indian numerals and method of notation.

The first, or *algoristic* system, as we may call it, proceeded by systematic rules to the performance of questions of computation. It embraced mercantile arithmetic, and what was then called algebra: and laborious and prolix efforts were made to combine the two; that is, to represent in such a form as we should now call algebraic, as distinguished from arithmetical, the solutions of questions of commerce, or of what might become such, if the connexion were diligently fostered. I cannot suppose that the multifarious problems of exchange of different kinds which abound in Pacioli and his immediate followers, were actually useful to the merchant: but there is an obvious leaning to the idea that they ought to be so. Geometry was also an application of this arithmetic; and in a manner which strongly marks its eastern character, as will appear to those who compare the work of Pacioli with the Indian books.

The second, or *Boethian* system, as I shall call it, because the work of Boethius was its great text-book, did not give any rules of calculation, nor apply itself to any application. The study was the properties of numbers, and particularly of their ratios. There was no art about it; and we have no means of telling whether the philosophers of this school reckoned on their fingers, or used an abacus, or put pen to paper for the performance of some organised method of computation. To judge by the smallness of the numbers used in the instances adduced, we must suppose that the writers left it to their readers to do as they liked best.

For some specimens of the laborious manner by which the Pythagorean Greeks, in the first instance, and afterwards Boethius in Latin, had endeavoured to systematise the expression of numerical ratios, I may refer the reader to the article "Numbers, old appellations of," in the Supplement to the *Penny Cyclopædia*. If I were to give any account of the whole system, on a scale commensurate with the magnitude of the works written on it, the reader's patience would not be *subquatuordecupla subsuperbipartiens septimas* — or, as we should now say, *seven per cent*—of what he would find wanted for the occasion. Not that the books I am speaking of get quite so far as this. I am hardly prepared to say exactly what number under fifty ought to be named as the terminus of the Boethian store of numbers; but certainly we rarely find them choose their instances from numbers above it. The system broke down under its own phraseology, as did that of the Yancos mentioned by Dr. Peacock, who could not get further

than *three*, because they could not express this idea by any thing more simple than *Poettarrarorincoaroac*.

The Italian school of *algorists*, with Pacioli at their head, found followers in Germany, England, France, and Spain; and in all but England, the Boethian school also. I cannot discover a single English work which pays any detailed attention to the latter class of arithmeticians. Tonstall, and still more Recorde, for the former appears to have been little known, were the preservers; and before the end of the sixteenth century the ordinary style of commercial arithmetic, which has prevailed among us ever since, was in course of establishment.

This gradual formation of the English school of commercial works will be apparent enough in my list. From the time of Recorde, we always were conspicuous in numerical skill as applied to money. The questions of the English books are harder, involve more figures in the data, and are more skilfully solved. It is possible that I might, if my French, German, and Italian list were more complete, produce exceptions to this rule. But nothing, I think, could arise to alter my conviction that the efforts which were made in this country towards the completion of the logarithmic tables in the seventeenth century, and the instantaneous appreciation of the value of the discovery of logarithms, were the result of that superiority in calculation which I assert to have been formed in the sixteenth. And yet this last-named century produced one man in France, one in Germany, and one in Italy, with either of whom no one English calculator could compare in extent of operations: I allude to Vieta, Rheticus, and Cataldi. There was no opportunity to compete with these men; for the subjects on which they worked were not introduced here. It was only towards the *end* of the sixteenth century that what were then the higher parts of the mathematical sciences began to be disseminated with effect in Britain.

To the commercial school of arithmeticians above noted we owe the destruction of demonstrative arithmetic in this country, or rather the prevention of its growth. It never was much the habit of arithmeticians to prove their rules: and the very word *proof*, in that science, never came to mean more* than a test of the correctness of a particular operation, by reversing the process, casting out the nines, or the like. As

* At first I had written "degenerated into nothing more;" but this is incorrect. The original meaning of the word *proof*, in our language, is testing by trial.

soon as attention was fairly diverted to arithmetic for commercial purposes alone, such rational explanation as had been handed down from the writers of the sixteenth century began to disappear, and was finally extinct in the work of Cocker, or Hawkins, as I think I have shewn reason for supposing it should be called. From this time began the finished school of teachers, whose pupils ask, when a question is given, what rule it is in, and run away, when they grow up, from any numerical statement, with the declaration that any thing may be proved by figures—as it may, to them. Any thing may be unanswerably propounded, by means of figures, to those who cannot think upon number. Towards the end of the last century, we see a succession of works, arising one after the other, all complaining of the state into which arithmetic had fallen, all professing to give rational explanation, and hardly one making a single step in advance of its predecessors.

It may very well be doubted whether the earlier arithmeticians could have given general demonstrations of their processes. It is an unquestionable fact of observation, that the application of elementary principles to their apparently most natural deductions, without drawing upon subsequent, or what ought to be subsequent, combinations, seldom takes place at the commencement of any branch of science. It is the work of advanced thought. But the earlier arithmeticians and algebraists had another difficulty to contend with: their fear of their own half-understood conclusions, and the caution with which it obliged them to proceed in extending their half-formed language. It was not merely by oversight, I suspect, that Oughtred so often calculates $ab+ac$ by two multiplications, instead of using $a(b+c)$: but rather from that same general fear of abbreviation, and suspicion that error may lurk in it, which possesses men of business who dare not multiply by 10 by the annexation of a cipher, but proceed with each figure, and carriage, as they would do if the multiplier were 8 or 7. I have seen this often enough; and things nearly as strange times without number. But what shall we say to the following; a most sufficient recommendation of the study of old works to the teacher, as shewing that the difficulties which it is now (I speak to the *teacher* not the *rule-driller*) his business to make smooth to the youngest learners, are precisely those which formerly stood in the way of the greatest minds, and sometimes effectually stopped their progress. Perhaps no man of his day had so much power over mathematical language as Wallis. But the following extract of a letter from