Early Science and the Printed Book: The Spread of Science Beyond the Universities

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The history of science as a separate branch of cultural and intellectual history is not very old; not much older than the concept of the Renaissance itself. That concept, though it has been subjected to attack in recent years, is particularly applicable to the study of history of science, at least in its strictest and most literal sense of a rebirth. In my opinion this is best seen in relation to the advent of the printed book, and the picture of this relation that I wish to put before you can best be introduced by a brief statement about the case of physical science, to which my remarks will in any case be essentially limited.

Physical science has a special relation to mathematics. Now, there is a sense in which mathematics is the only subject in which antiquity possessed exact knowledge of permanent value that had become lost to the official scholarship of the Latin West long before the invention of printing. Obviously I do not mean to say that all knowledge of ancient mathematics had been lost. Clearly it had not; the case of Euclid makes that clear. But there were important segments of ancient mathematics that were capable of altering the whole face of physical science that had long dropped from sight. Moreover, the standard medieval Latin translation of Euclid and the commentary that accompanied it had failed to give a correct account of ancient refinements in at least one fundamental branch of mathematics, the theory of proportion. A study of medieval mathematics has convinced me that certain ingenious substitutes for the classic theory of proportion had made it most unlikely that that would be again hit upon in any way other than by the restoration and study of old texts, a specific activity associated with the concept of the Renaissance.

As to debates in our time over the validity of the Renaissance concept, these appear to me to have been conducted in a particularly unfruitful manner. It is obviously useful for historians to have a descriptive name for a period that was characteristically neither medieval nor modern. The fact that no precise date can be set for the beginning and end of such a period in general does not destroy the utility of the concept. In any branch of intellectual and cultural history, specialists can define a Renaissance period according to criteria appropriate in that field, and those criteria will then serve also to clarify the meanings of 'medieval' and 'modern' with respect to its development. An overlapping of periods defined in that way, by a variety of specialists in many fields, enables us to say something about the general state of culture within any particular range of dates. This seems to me a perfectly defensible way of going about our business as historians.

Generally, when the Renaissance concept is attacked, that is done not on the basis of utility but on grounds of propriety. Should we use one word, 'Renaissance,' to denote a wide variety of periods? Confronted with this question, excessively scrupulous historians tend to disparage the word as a name for something that had no clear existence, and to disown the concept as a capricious nineteenth-century creation corresponding to no demonstrated historical facts. Hardier souls have in effect adopted in its defence the position of Humpty Dumpty; at any rate, they seem to me to have said little more than 'when I use a word, it means whatever I want it to mean, nothing more and nothing less.'

The proper procedure, it seems to me, would be not to engage in ethical disputes over the right to coin words, or in ontological disputes over the existence or non-existence of historical patterns, nor yet to support uncritically the right of everyone to use any word, especially a term of art, in any manner he pleases. Rather, we might better make clear the criteria by which words like 'medieval,' 'Renaissance' and 'modern' are to be applied in a designated area, and then go boldly ahead and apply them, to see whether the patterns of historical fact that thus emerge are interesting, or instructive, or of heuristic value. If in some fields they are not, or if it is impossible to make the criteria clear, then not only the word 'Renaissance' but also the word 'medieval' should probably be abandoned in those fields, but without prejudice to the terms themselves; though in such cases there is not much point in retaining the word 'modern,' either, unless that is to be contrasted with 'ancient.' For however hard it may be to define a period between medieval and modern ideas and activities in a given cultural field, it must be still more difficult to fix a date in that field on which medieval ideas and pursuits went out of date and were replaced by modern ones. Hence it seems to me that wherever we can agree to retain the word 'medieval,' we are likely to find the Renaissance concept also useful.

Now, the history of science is very definitely one field in which both terms are useful. As I have said, the word Renaissance is particularly apt, in its most literal sense, when applied to one of the main developments in sixteenth-century physical science. It was then that ancient Greek mathematical and scientific works were rediscovered that had been unknown to, or utterly neglected by, medieval writers. Correct texts were established, proper translations were made, and appropriate commentaries were written also for other ancient works that had previously been known only in part or only in corrupt versions. Now, the rectification of mathematical texts has an essentially different role from that of the restoration of authentic literary or philosophical works, the interest of the latter being mainly, if not entirely, antiquarian in character. But the works of Euclid, Archimedes, Apollonius of Perga and Pappus of Alexandria were replete not with ancient opinions, but with extremely valuable knowledge, most of it as valid in the sixteenth century, and today, as when it was written. It is at least open to question whether the retrieval of the authentic texts of Plato and Aristotle represented anything like the same acquisition of permanently valuable knowledge, putting antiquarian interest aside, as those of Archimedes. Indeed, a defective philosophical text could easily be more inspiring than a correct one, through what Aldous Huxley called 'lapses into lucidity.'

In any case, the restoration of classical mathematics in the sixteenth century did make possible some immediate scientific advances that would have required a great deal of time to achieve in any other way. Given unprecedentedly wide circulation in printed books, it had a direct and far-reaching effect on both the form and content of physical science. This was a true rebirth of ancient scientific knowledge, most aptly described by the word 'Renaissance.' If there had been no Renaissance, historians of science would have had to invent it.

Immediately preceding this, and therefore independently of it, a reawakening of interest had begun in direct inquiries into particular phenomena, as against the speculative assignment of causes to things that characterized medieval science. The most prominent representatives of this aspect of Renaissance science are well known – Leonardo, Copernicus, Paracelsus and Vesalius. Indeed, an exaggerated attention paid to a few such men has rendered the phrases 'Renaissance mechanics,' 'Renaissance astronomy,' 'Renaissance chemistry' and 'Renaissance anatomy' significant of little more to most people than the work of those four men respectively. Even among historians of science generally, the adjective 'Renaissance' seldom carries with it the kind of thematic characterization that accompanies the words 'ancient,' 'medieval,' and 'modern,' when applied to specific scientific disciplines. Yet Renaissance physics, at least, does have a character of its own – or rather a split personality – that has escaped notice for rather interesting reasons.

The first historians of science naturally relied almost exclusively on printed books as their sources of information. Now, very few medieval works of recognizably scientific interest ever passed into book form, whereas most of the ancient scientific works known in the sixteenth century, as I have already mentioned, were widely circulated then for the first time. Hence medieval science tended to be overlooked by early historians. Until quite recent times, the period from the close of the Alexandrian era to the opening of the sixteenth century was dismissed as of no historical importance for the exact sciences. Thus the question of a special character for Renaissance science did not immediately arise; the 16th century was simply regarded as the initial stage of modern science, inspired by ancient models.

That view, associated with the name of William Whewell and the 19th century, could no longer be maintained after Charles Thurot and Pierre Duhem turned at the end of that century to the serious examination of medieval manuscripts. In particular, Duhem brought to light a conscious program of applying mathematics to physics in writings of the 13th and 14th centuries. Since Galileo and Descartes in the 17th century mathematicized physics, Duhem assumed a line of transmission from the Middle Ages through the Renaissance to the pioneers of modern science. The assumption is so plausible that it pervades history of science today, though still undocumented through printed books. Thus again the question of a special character for Renaissance science was slighted off, and the sixteenth century was regarded as merely a period of transmission of much older works, medieval as well as ancient.

From this it is evident that the study of printed books as against manuscript sources has always been fundamental to the historiographical question of Renaissance science. That being the case, one might expect the differences as well as the resemblances between books and their predecessor manuscripts to have been brought to the fore and carefully analyzed. But the advent of the printed book seems instead to have been treated by historians of science, and perhaps by other intellectual historians, not as a fundamentally important event, but as a mere replacement of the scriptoria by printing-houses. This seems to me as absurd as if social historians in the future were to regard the replacement of the horse by the automobile as having introduced no fundamental change. Transportation was made faster and more generally used, but that is not all. The printed book made it easier for a scholar to acquaint himself with the work of other scholars, and if that had been all it did, it might reasonably be regarded as relatively unimportant. The nature of scholarship would not have been affected, but only its rate of progress. But in science, at least, printed books *did* change the nature of scholarship. When we recognize that fact, we also see that the older historical view of Renaissance science was not entirely wrong, nor is the view that has replaced it entirely right.

The printed book changed the nature of scholarship by effecting a fundamental alteration in the composition of the world of learning. Perhaps this is more evident, as it is certainly more significant, in mathematics and physical science than anywhere else. The point may be illustrated by naming half a dozen medieval writers whose works are now recognized as of paramount importance in the history of physical science, and seeking their counterparts in the sixteenth century. The medieval writers that occur to me are Jordanus Nemorarius, Robert Grosseteste, Thomas Bradwardine, Jean Buridan, Albert of Saxony and Nicole Oresme. Of these men, all but Jordanus were certainly university teachers, and even Jordanus, a shadowy figure, is known to have composed at least one work specifically for university instruction. Physical science through the sixteenth century may be represented by Leonardo, Niccolò Tartaglia, Girolamo Cardano, Giovanni Battista Benedetti, Guidobaldo del Monte and Simon Stevin. Of these men, only Cardano taught in a university, and his contributions to physics are the least important of any man in the list. It is safe to say that during the Middle Ages, physical science was a university monopoly, whereas after the printed book it was far from being so.

Obviously other lists could be drawn up, and perhaps my last statement could be shown to be an exaggeration. But the basic idea contained in it would not be changed. It may be debated whether purely technical treatises on the one hand, and purely philosophical discussions of physics on the other hand, should or should not be included in the lists. I believe the result will be pretty much the same, so long as the criterion is the same for both periods. If spokesmen for medieval continuity in science should produce a list to show that medieval science was not a university monopoly, I think they would find that in that list the unity of medieval science – that special character by which they recognize it in later periods – will have slipped from their grasp. That may be precisely what some of them desire, but in that case they are merely asking us to abandon a valuable working concept in intellectual history when they question the idea of a Renaissance.

The change in composition of scientific scholarship that followed the advent of the printed book consisted essentially in the addition of self-educated men and talented amateurs of liberal education to the ranks of those who made substantial contributions to science. Their actual contributions differed in an essential way from those that continued to be made by official scholarship; that is, university professors. I shall illustrate the difference presently, but first I wish to speak of the half-century lag between the invention of printing and the effect on physical science that I attribute to the printed book. Obviously the invention itself could not produce the effect immediately. First it was necessary that books having some relevance to science appear, which brings us to about 1480. Mathematics was especially relevant to the effect in physics, and so far as geometry is concerned the crucial date is 1482; for algebra, it is 1494. By 1500, wide and relatively inexpensive production of books had become a reality throughout Europe, which means that the process of self-education and the discovery by amateurs of their own particular talents in fields previously unfamiliar to them had begun. The number of such persons in the general population was certainly very large as compared with the university population; but not everyone, even in the universities, was destined to make a lasting contribution to his field. The half-century lag between the invention of printing and the appearance of significant scientific books written by men outside the universities is therefore no reason for denying an intimate connection between the two events.

A rapid multiplication of treatises on scientific topics took place after 1500 that contrasts with the relatively even production of manuscripts on such subject in preceding centuries. This phenomenon probably owes a good deal to the difference between the printer and the professional scribe whose work he forced into obsolescence. The scribe generally copied only those works he was commissioned to copy, and his methods of work were unrelated to the contents of his manuscripts. The situation of the early printer was different. Although some books were printed to order or by commission, for the most part they were commercial ventures attended with risk and involving considerable capital. That printers took risks in such untested markets as physical science suggests a special interest on their part. Printers were concerned with the lever and the screw as essential devices of their art. They were also concerned with alloys of metals and methods of working them with tools, with the composition of inks and the properties of paper, and with other technical problems in a way in which the scribe had not been concerned at all. It is therefore not surprising if some printers took both a practical and a theoretical interest in mechanical principles and devices, as well as in commercial arithmetic and the geometry of design, so that books dealing with mechanics, mathematics, chemistry or metallurgy appeared to them likely to find a market among men who had similar interests.

In some instances, scientific pioneers were themselves printers. Rudolf Hirsch remarked, in a recent book on printing and reading, that printers generally relied on advisers in selecting titles, and went on to remark that: "The earliest event in 'new science' was an exception: Regiomontanus printed in Nurnberg between 1472 and 1476 writings of his teacher, Georg Peurbach, and his own, which according to well-informed critics heralded the beginning of modern astronomy and mathematics. Printers and publishers especially of the 16th century seem to have been quite willing to produce contemporary works which contained novel theories or presented the results of investigations in a new way; they would not have done so unless they could sell these products successfully." Professor Hirsch's remark about novelties here has particular significance with respect to the role of university science vis-a-vis the new science typical of the sixteenth century, of which I shall speak presently. In the 16th century, universities were conservative institutions, whether or not they still are today. Their job was to preserve learning, examine it critically, and impart it; not to seek or create new knowledge, or at least not that primarily. Printers of books also served that purpose, but did not confine themselves to it. Their first productions were heavily weighted in the direction of classical texts and learned commentaries, for most of their assured markets lay in the universities and among the clergy. But in seeking *new* markets, in which the competition would be less direct, they did welcome writers for whose works they could claim novelty, and this was of no small assistance to the spread of unconventional science.

In saying that the age-old university monopoly on science was broken in the sixteenth century, as a direct result of printed books, I do not mean that science passed out of the universities and into the hands of other men. Rather, there were now two streams of scientific thought where before there had been but one. Professors of philosophy continued to write on physics throughout the sixteenth century, and well into the seventeenth, in the same style and with the same objectives as their medieval and early Renaissance predecessors. Fashions changed in the particular topics on which they wrote, but physics remained in their domain so far as official science was concerned. Indeed, they were the men who first attacked the new physics of Galileo in the seventeenth century, long before the Church became concerned about his views. He himself remarked in his *Dialogue,* written after a lifetime of experience, that philosophers would never be swayed by 'one or two of us who bluster a bit.'

Thus one phase of Renaissance science was quite literally a continuation of medieval science. Centered in the universities, this phase remained Aristotelian to the core, and defined science as the knowledge of things through their causes. It was also influential outside the universities; even Descartes was to criticize Galileo's science on the basis that by trying to investigate particular motions without first determining the cause of motion, he had built without foundation. But the continued existence of official science, and even its spread beyond the universities through the printed book, does not give Renaissance science the unity that had existed in medieval science. There is no getting around the independent origin of a totally different set of inquiries outside the universities. It is this non-U science, to use Miss Mitford's apt term, that is really a distinctive Renaissance entity, and in my opinion it could not have come into and remained in existence before the printed book. I can think of nothing that could have kept such inquiries alive and separate from the centers of learning except its inexpensive and widespread circulation in durable form.

The medieval university community was clearly relatively small and homogeneous as compared with the rest of society in the Middle Ages. A large number of copies of any given work was never necessary within that community, whether we think of this as a single university, of several universities in a geographical area, or even as all the universities in Europe. It was unlikely that a person living in the university community would long remain unaware of the existence of a significant work related to his particular interests. If a copy was not immediately at hand, it was probable that the scholar who told him of the existence of the work would also be able to outline the nature of its contents. But outside the university community, this situation was reversed. It was there highly unlikely that a person would know of all the works in manuscript pertaining to his particular interests. If told of the existence of such works, it was improbable that his informant could also tell him the nature of their contents. For these and similar reasons, the invention of printing was potentially much more important to society outside the universities than to men within them. In a large and heterogeneous community there is never any lack of special talents or of varied interests, but there is always the problem of keeping alive from one generation to the next the fruits of any application of a particular talent to the appropriate interest. Before printing, this problem found no practicable solution.

These and similar considerations of statistical probability seem to me sufficient to account for the relative absence of non-U science up to the closing years of the fifteenth century, and for its swift emergence thereafter. In every period, excellent contributions to knowledge may have been made by men outside the official world of scholarship. Before printing, however, their contributions would be expected to languish unless noticed by a member of the learned community. Those so noticed would be brought into the official stream of science. A manuscript not thus brought into the main stream, either for lack of merit or lack of notice, would be most unlikely to fall under the eyes of another non-U person (outside the scholarly world) who would be interested in it and capable of appreciating it. The probability was always that a work would be either lost, or incorporated into the main body of doctrine. It is in these terms that I should account for both the unity of medieval science and the university monopoly on science before the printed book.

The conservative character of the university in the sixteenth century is further attested to by a curious fact that seems not to have been mentioned by others, if it is indeed a fact. Not only does it appear that meritorious scientific ideas propounded in sixteenth-century books by authors outside the universities aroused no interest or attention inside their walls, but even the new editions and translations of classic Greek mathematics were not incorporated in university studies, so far as I have been able to discern. It is well known that the astronomy of Copernicus was not taken up by the universities generally. The one exception known to me is Tübingen, where Kepler's teacher, Michael Maestlin, was favourable to Copernicus; but even in this instance he seems not to have gone further than to teach it together with the Ptolemaic and Tychonic systems. The same thing seems to be true of new ideas in mechanics published by Tartaglia, Guidobaldo and Benedetti. Their books appeared in more than one edition, and some of them in more than one language during the sixteenth century, but I have not yet found their ideas taken up in any book published by a professor of the period. On the other hand, each of these non-university men mentions the work of the others who had written before him, either critically or approvingly. Multiple editions and cross-referrals show that a viable body of mechanical science existed that simply did not impress the university community. Two or three professors of mathematics at the University of Padua in the sixteenth century did lecture on mechanics, but only as a commentary on the pseudo-Aristotelian work on that subject that had come to light for the first time late in the fifteenth century. So a neglect in university circles of contemporary writers on mechanics cannot be excused on the grounds that this was not a university subject. Rather, it supports the view that the doors of sixteenth-century universities were closed to new scientific ideas from the outside.

It has been mentioned that the theme of official science was the knowledge of things through their causes. In general, that is a different theme from the mathematical description of physical events. Mathematics could not be admitted as a cause of anything under the Aristotelian view, and descriptions failed to disclose causes in the Aristotelian sense. Nevertheless, as Duhem long ago pointed out, medieval philosophers of the 14th century had formulated some ingenious and elaborate methods of introducing mathematics into Aristotelian physics. Some of the best of those medieval works had reappeared in printed books from about 1480 to 1530. Many historians today believe that the same doctrines continued to be discussed in the universities throughout the 16th century, and that Galileo was inspired by them to undertake his own program of mathematicizing physics. The evidence of printers' production seems to me to be against this. The number of such books was greater before 1500 than after, and the medieval treatises in question were not reprinted after 1530. About that time, the universities (except Paris) seem to have taken a greater interest in going back to pre-medieval commentaries on Aristotle, of which a great number were printed and reprinted from about 1525 on into the seventeenth century. More important from the standpoint of science is the popularity of debates on method in sixteenth-century universities, particularly in Italy. The certainty of mathematical knowledge was a topic explored by several prominent philosophers inside and outside the universities. These debates on method and on mathematical certainty are just now coming to be seen as important to the history of science, in view of the later development of physics. But since the philosophers who wrote on these subjects did not also contribute to scientific knowledge, it is small wonder that they were not recognized by the earliest historians of science.

Critiques of method were also written by anti-Aristotelian philosophers of the late Renaissance; among these we may count Bernardino Telesio, Pierre de la Ramée, and Francis Bacon. It seems to me likely that these critiques would have been made in any case, whether or not a new astronomy and a new mechanics had been proposed by sixteenth-century scientists, because neither the debates on method and mathematical certainty within the universities nor the anti-Aristotelian programs of other philosophers were based on the idea that new discoveries had to be accommodated, or that new sciences ought to be admitted. Indeed, Bacon was outspoken against the Copernican astronomy, which at that time had made more headway in England than anywhere else, and as I have previously mentioned, Descartes opposed the new sciences of Galileo. The ferment of ideas on method and of opposition to the authority of Aristotle seems to me to be a product of the general broadening of discussion that followed the printed book, rather than a by-product of the new ideas in science itself. The two are linked together by later developments in science, but perhaps not by common interests of the authors who put them forward.

It is time now to turn our attention away from official science, which in effect continued in the universities after the invention of printing much as it had before, but which spread out from them into a more general literature, and to consider the two movements in science that arose outside the universities and that give to Renaissance science its own characteristic content, neither medieval nor modern.

The first of these is the editing, translation, and writing of commentaries on classic Greek mathematical and scientific works. Euclid was of course first in the field, and least removed from the medieval tradition. In fact the first Euclid printed was in the medieval translation (Latin from Arabic) by Campanus of Novara. This was followed in 1505 by the Renaissance translation of Zamberti directly from the Greek. Both were reprinted many times in the sixteenth century, chiefly in France and Italy. The first vernacular translation was into Italian and appeared in 1543, the same year as the great works of Copernicus and Vesalius. An English translation with much improved commentaries was published in 1570; partial French and German translations also belong to the sixteenth century. Vernacular translations put rigorous mathematics into the hands of a much larger population for the first time, while the first really competent mathematical and textual commentary, written by Federico Commandino, set new technical standards. The first mathematical works of Archimedes to be printed came in the first decade of the sixteenth century, followed in 1543 by his two works dealing with physics, and the following year by the Greek text of most of his works and a Renaissance Latin translation. Ptolemy's astronomy was printed in 1515 from an Arabic text; better translations followed, and the Greek text, by mid-century. The mechanical and mathematical works of Hero and Pappus of Alexandria appeared in Latin, and some of them also in Italian. None of these Renaissance translations, so far as I know, was the work of a university professor, though most of them were by men who had been educated in universities. Others were the work of mathematicians and engineers whose efforts seem to have no explanation other than sheer admiration for the mathematical excellence of the works, and a desire to make them accessible to a wide public.

The original work of Renaissance scientists outside the universities cannot be better illustrated than by the case of Niccolò Tartaglia, who was born at Brescia in 1500. His family was very poor, and the death of his father when he was still a young child meant that he had almost no schooling. According to his own account, he had learned only half the alphabet from a writing master when funds for the tutor ran out, and he completed his education alone. Even allowing for some exaggeration in this story, it is evident that Tartaglia was largely self-educated, for his Italian style was anything but cultured, and occasioned derision from some of his better-educated contemporaries. His only publications in Latin were editions of medieval translations in which he did not venture to make textual corrections. But Tartaglia had a great talent for mathematics, particularly algebra, and when he reached maturity he began to offer private instruction in that field. Instances of self-education to such an extreme degree are not common even in the Renaissance, and it is hard to see how they would even be possible before the existence of printed books.

In 1531, when Tartaglia was tutoring private pupils at Verona, he was asked by an artilleryman how to point a cannon in such a way as to attain the maximum distance for a shot with a given charge. This problem he solved correctly, and then became interested in the possibility of working out a general mathematical treatment of projectile paths. Having done this to his own satisfaction, he says that he decided not to publish it because it would be sinful to teach Christians how better to slaughter one another. However, when the Turks threatened Venice in 1537, he overcame his scruples and published it in a book called *Nova Scientia,* written in Italian, dedicated to a military commander, and intended for the practical use of soldiers.

This is the first book I know of that proclaims novelty of a science as a recommendation. Such titles were not uncommon thereafter, as in his rival Girolamo Cardano's *Opus novum de proportionibus* and Galileo's *Due nuove scienze.* Tartaglia's new science of ballistics was presented in mathematical form, with definitions, axioms, postulates and theorems. So far as I can tell, it is devoid of influence by medieval physics; its principles are drawn from Aristotle and Euclid and its form is neither syllogistic nor argumentative, but that of mathematical deduction. Perhaps the work of Archimedes on floating bodies suggested the pattern to him. Several traits of this first significant attempt at a new physics strike me as characteristic of the age: use of the vernacular, stress on practical utility in the dedication, pride in novelty, and the stress on mathematical rigour in scientific argument.

This first book of Tartaglia's went through half a dozen Italian editions during the sixteenth century and was translated into German and English. Its results were adopted by many writers on military matters, who continued to cite it in the 17th century even after Galileo's correct theoretical analysis of projectile motions, which was not of practicable application to gunnery. But despite this widespread acceptance, I have never run across any reference to Tartaglia's theory that would connect it with any university course, or any citation of it by a professor writing on motion or on physics.

Tartaglia published the first vernacular translation of Euclid in 1543, working from the Latin of Campanus and Zamberti, whose commentaries he also translated, adding further commentaries of his own which contain some interesting textual conjectures. He probably had very little knowledge of Greek, and seems not to have consulted manuscript sources. In the same year he published some medieval Latin translations of works by Archimedes, including the two which dealt with mathematical physics. All these works are of interest in their departure from ordinary scholarly practices. No professor would have bothered to make a vernacular translation, and still less to publish uncritically a Latin translation from the Greek. Tartaglia's motivation can hardly have been anything other than zeal to provide for others an easier road to self-education than that which he himself had followed.

In 1546 he published his second original work, a collection of scientific questions with which he had dealt in his tutoring. In his book he made what was perhaps the first open challenge to the scientific authority of Aristotle, directed against the treatment of certain problems related to the balance. As a remedy to this he published in Italian an improved version of the medieval science of statics due to Jordanus Nemorarius. In 1551 he followed this with a treatise on the raising of sunken ships, accompanying it with an Italian translation of and explanatory commentary on the work of Archimedes on floating bodies. With this publication, Tartaglia had set forth in Italian virtually everything of importance that the past had to offer on theoretical mechanics, whether ancient or medieval, and had contributed a good bit of his own. His final work was a vast treatise on mathematics, most of which was published posthumously about the time Galileo was born.

Tartaglia's publications, and a certain notoriety he had obtained as a mathematician by winning challenge contests and by a celebrated dispute with Cardano, failed to win for him any university chair in mathematics, though he made at least one serious attempt to obtain one. He was certainly a much better mathematician than the incumbent at Padua, Catena, who is remembered mostly for his mystical interpretations of mathematics. It is clear, however, that Tartaglia's work had a wider effect, and did more good for science, than it was likely to have done from within a university. He enormously widened the access of Italian readers in every walk of life to mathematics and its application to practical problems. One of his private pupils was Benedetti, who was in many ways Galileo's most important Italian predecessor, and another of his pupils is supposed to have been Ostilio Ricci, Galileo's own teacher of mathematics and also unconnected with a university.

As I remarked, Tartaglia affords a striking illustration of the things that distinguish Renaissance from medieval science, but he is not thereby typical of it. Many important contributors to Renaissance science through original works, translations of classic mathematics, or scientific commentaries were university-educated men who did not remain to teach. Such men were Commandino, Guidobaldo, and Bernardino Baldi. In England they had counterparts in Robert Recorde, Thomas Digges, Henry Billingsley and Thomas Harriot; in Switzerland, Walter Ryff and Michael Varro; in Holland, Simon Stevin. In France, no second stream of science outside the universities seems to have flourished, perhaps because the University of Paris continued much longer than other universities to pursue the medieval tradition that linked mathematics to physics. Possibly there is also a connection with the fact that printing in Paris was much more closely linked to the University than in any other city.

If in conclusion I were to try to characterize Renaissance physical science, I should say that it was the period during which the unity of science was lost by its passage outside the universities, a period that opened with the inexpensive printed book and that closed with the work of Galileo, which re-established a unity of science that depended not on its institutionalization, as it had in the Middle Ages, but on its removal from the tyranny of any authority other than those of mathematics and of nature herself.