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The complex historical process that resulted in the creation of Viète's and Descartes' "new algebra"

Oblique light on the Zilsel thesis

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Prolegomenon I: The Zilsel thesis

Whoever began their work on the history of science with Joseph Needham's writings (I did) will know about the Zilsel thesis – if not in detail then by name.

But let us start with the details.

Edgar Zilsel was an Austrian sociologist belonging to the circle of logical empiricists.

Together with Otto Neurath and Jørgen Jørgensen he was one of those who believed in the possibility of achieving reliable knowledge about the external world

- together with Neurath and Jørgensen also closer to Marxism than most members of the movement).
- When Rudolf Carnap gave up genuine empiricism in 1932 with his introduction of the concept of "protocol sentences" (whose relation to some real world was considered outside the philosopher's field), Zilsel was the first to attack him.

In Zilsel's case, with his background in sociology, the method supposed to lead to the goal was sociological and historical comparison, not Neurath's "physicalism".

Zilsel was marginal in the Vienna environment.

He remained marginal after his post-*Anschluss* emigration to the U.S., where he was associated to the International Institute of Social Research, the emigrated version of the Frankfurt Institut für Sozialforschung.

After his suicide in 1944, he was at first almost forgotten (the history of science constituting a partial exception) – in particular he disappeared from historical accounts of logical empiricism.

That situation only started to change after 2000, when logical empiricism itself was no longer neither an inspiration nor an object of philophical attack.

During his stay in the U.S., Zilsel worked (until 1941 on a Rockefeller grant, then in the scarce time left over from earning a living) on a project on the social origins of Modern science.

The articles communicating partial and preliminary results from this project have secured him some fame among historians of science – not least thanks to Joseph Needham.

The first to appear in print was “The Sociological Roots of Science”. Its abstract runs as follows:

> In the period from 1300 to 1600 three strata of intellectual activity must be distinguished: university scholars, humanists, and artisans. Both university scholars and humanists were rationally trained.
>
> Their methods, however, were determined by their professional conditions and differed substantially from the methods of science.
>
> Both professors and humanistic literati distinguished liberal from mechanical arts and despised manual labor, experimentation, and dissection.
>
> Craftsmen were the pioneers of causal thinking in this period. Certain groups of superior manual laborers (artist-engineers, surgeons, the makers of nautical and musical instruments, surveyors, navigators, gunners) experimented, dissected, and used quantitative methods.

The measuring instruments of the navigators, surveyors, and gunners were the forerunners of the later physical instruments. The craftsmen, however, lacked methodical intellectual training.

Thus the two components of the scientific method were separated by a social barrier: logical training was reserved for upper-class scholars; experimentation, causal interest, and quantitative method were left to more or less plebeian artisans.

Science was born when, with the progress of technology, the experimental method eventually overcame the social prejudice against manual labor and was adopted by rationally trained scholars.

This was accomplished about 1600 (Gilbert, Galileo, Bacon). At the same time the scholastic method of disputation and the humanistic ideal of individual glory were superseded by the ideals of control of nature and advancement of learning through scientific co-operation.

In a somewhat different way, sociologically, modern astronomy developed.

The whole process was imbedded in the advance of early capitalistic society, which weakened collective-mindedness, magical thinking, and belief in authority and which furthered worldly, causal, rational, and quantitative thinking.

Summing up the summary, neither the university tradition nor Renaissance Humanism nor technicians created the scientific revolution on its own – what was decisive was the *interaction* between and the mutual fecundation of the three groups.

Zilsel's ideas – together with Boris Hessen's and Robert Merton's work on 17th-century England – have inspired other workers to agreement or debate.

My intention here is to see how far the idea can be applied to a parallel field which neither Zilsel nor the discussions after his time have taken up:

the emergence of "Modern algebra" (that of the outgoing 16th and earlier 17th century, to be distinguished from the *Moderne Algebra* created by Emmy Noether and Emil Artin and made famous by Bartel L. van der Waerden).

Prolegomenon II: Internalism versus externalism

Also when I started work on the history of science, a hot debate regarded "externalism" versus "internalism". It was inspired in part by readings of Zilsel, but more strongly by readings of Hessen and Merton.

Terms have changed since then, but hardly the substance; if anything, pseudo-philosophical jargon has muddled up the issue.

I shall therefore stick to the old words.

An “internal” history of science is a history of scientific doctrines and results (good or bad results etc., that is not at issue);

an “external” history is a history of scientific institutions, of the uses of science, and of the sociocultural setting for scientific activity (etc.) – on the whole, the *conditions for scientific practice*. Both are valid enterprises.

“Internal*ist*” and “external*ist*” historiography, on the other hand, are claims about the validity of explanatory models.

“Internalist” historiography claims that what scientists do (and formerly did) should be explained as a continuation of what science has or had achieved so far,

or as a response to new problems that have or had come to the fore because of these achievements;

“externalist” historiography claims that scientific practice is a consequence of its institutional or sociocultural settings or a response to social needs (etc.).

The debate is trivially absurd, and always was. (As I claimed that in the late 1970s, colleagues saw this claim as somewhat scandalous).

Scientists; indeed, can only respond *as scientists* to social needs because they build on existing scientific results and techniques, so "externalism" presupposes "internalism";

but whether some people engage themselves in scientific activity depends crucially upon the existence of institutions that allow them to learn about this possibility and about what science has done so far,

- and no less crucially on the existence of a general sociocultural climate which induces some people to find it a worthwhile choice, and of economic structures which allow them to dedicate much of their time to it.

Already at this basic level, "internalism" thus also presupposes "externalism".

This observation does not exclude the possibility to take either science as it exists at a particular moment for granted and discuss how scientists react to “external” influences *on these given conditions*,

or, reciprocally, to presuppose the institutional and sociocultural framework and look at how they react to the problems created by a new scientific insight.

Both questions are fully legitimate, and it is rarely possible to discuss more than a few aspects of a complex network of influences at a time.

One thing, however, is a principle expressed in such general terms; another thing the actual implementation.

Here, I shall therefore apply the principle to François Viète's and René Descartes' creation of two variants of a new algebra around 1600 – that is, to my oblique test of the Zilsel thesis.

I shall trace the process from the beginnings of abbacus algebra.

This algebra was created as a superstructure on the teaching in the abbacus school

- as I said in my third lecture, a north- to mid-Italian school for artisans' and merchants' sons, where they were taught basic commercial arithmetic for 1½ to 2 years around the age of 11 or 12.

Algebra was not taught here; it served the masters for display of ability in competitions for municipal appointments and for students. Some also took pride or pleasure in this ability.

It was inspired by Arabic algebra, but was not taken directly from books we know – neither al-Khwārizmī's nor Abū Kāmil's algebras, nor from any of al-Karajī's works.

Even surviving works from 12th–13th century Maghreb can be left out of the picture.

It seems to have been borrowed from a now lost vernacular algebra tradition thriving somewhere in the Ibero-Provençal area, and going back (via al-Andalus) to a "diluted al-Karajī".

Many features distinguish the “new algebra” from these beginnings – irrespectively of whether we take al-Khwārizmī or early abbacus algebra to represent these.

Not least:

The use of symbols – that is, glyphs (letters or otherwise) that are operated upon directly and do not serve as abbreviations for words serving within the syntax of normal language.

At first, such symbols were used fairly regularly in abbacus algebra for root-taking (℞), + (for instance, *p*) and – (for instance, *m*).

At first the unknown (the *cosa*, “thing”) and its second power (the *censo*) were represented by words – neither by glyphs meant to be read as words serving within normal syntax nor *a fortiori* by symbols.

Already before the abbacus adoption, higher powers had also come in occasional use.

Powers beyond the cube were composed by multiplication – the “square of the cube” or “cube of the square” (always known to be the same) were thus meant to be the fifth, not the sixth power.

Some algebra writers would do the same with roots, where “multiplication” is even more glaringly absurd.

The cube root of 512 is 8, and the cube root of the cube root of 512 thus 2, the 9th, not the 6th root of 512.

Accordingly, the multiplicative composition of roots disappeared rather soon, while the multiplicative composition of powers gave way only slowly and unsystematically to composition by embedding:

> That is, to an understanding of (for instance) “cube of the square” in agreement with our $(x^2)^3 = x^6$

At the end of the 15th century, however, embedding had taken over completely. That created the need for new terms for the 5th, 7th and higher prime powers.

Already around 1335, some abbacus writers used abbreviations or other glyphs for the *cosa* and the *censo* – most only as abbreviations, but some used them as genuine symbols within specific calculations.

One such type was the multiplication of polynomials within schemes emulating the one used for multiplication of Hindu-Arabic numbers, as here:

```
 6 chose e 8 e ℞9
 6 chose e 8 e ℞9
__________________
censi  ρ    n   ℞
      36    9
36    96    48
            64
__________________
36c  132ρ  121n
```

Another was the "formal fraction", that is, a division of a polynomial by another polynomial written as a fraction and dealt with in agreement with the arithmetic of fractions.

Such fractions, however, did not necessarily make use of abbreviations, they might also write in full words.

$$\frac{100}{\textit{per una cosa}} \quad \frac{100}{\textit{per una cosa e piu } 5}$$

This leads to an important observation: Symbolic syntax has to be understood as separate from symbolic lexicon.

The link between the two is opportunistic: complex formulas are not easily written without the symbolic lexicon, and they easily become opaque.

Neither glyphs serving as mere abbreviations nor glyphs serving as symbols were used systematically even in the late Italian 15th century.

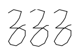
Moreover, there was no agreement about what these glyphs should be. As Luca Pacioli observed in 1494, as his reason to describe several systems,

tante terre, tante usanze “as many regions, so many usages”, and *tot capita: tot sensus*, “as many heads, so many opinions”.

That was to change when algebra matured in early 16th-century German lands as *Rechenmeister* algebra.

Here, a single system was established (Heinrich Schreyber tried a notation of his own using simply the exponents in 1521, but nobody followed him).

It encompassed higher powers, and it was used systematically by Christoph Rudolff, Michael Stifel, Johann Scheubel and others.

ϕ	*dragma* or *numerus* (degree 0)
	*radix* (degree 1)
	*zensus* (degree 2)
	*cubus* (degree 3)
	*zensdezens* (degree 4)
ß	*sursolidum* (degree 5)
	*zensicubus* (degree 6)
bß	*bissursolidum* (degree 7)
	*zensdezensdezens* (degree 8)
	*cubus de cubo* (degree 9)

Only the new algebras were to start using symbols also for the *coefficients* of equations – in the terminology I shall use, to operate with *abstract coefficients*.

Several unknowns

Early algebra dealt with a single unknown, the *thing* and its second power.

Abū Kāmil and al-Karajī (as well as other Arabic algebraic writers) kept the *thing* as a principal unknown but sometimes used one or more extra unknowns. Fibonacci did so too.

Abbacus algebra began doing the same in the outgoing 14th century, Antonio de Mazzinghi certainly independently, others perhaps inspired by direct or indirect Arabic contacts (not by Fibonacci, who is different).

Antonio did so in second-degree algebra, the others only in linear problems.

In Antonio's *Fioretti* (*ca* 1385?) we can follow over several steps how he creates the technique and brings it to perfection.

The first approach is in problem #9 about two numbers (A and B), fulfilling the conditions that

$$AB = 8 \ , \ \ A^2+B^2 = 27 \ .$$

At first Antonio solves the problem by means of *Elements* II.4.

Next he offers an alternative:

> we can also make it by the equations of algebra; and that is that we posit that the first quantity is a *thing* less the root of some quantity, and the other is a *thing* plus the root of some quantity.

♠Antonio is quite aware that the two unspecified “quantities” are identical, as can be seen from his ensuing calculations. But because of his failure to make it explicit, the formulation of these is quite a piece of acrobatics.

What he does can be expressed

$$a = t+\sqrt{?}\ ,\quad b = t-\sqrt{?}$$
$$a^2+b^2 = 2C+??\ ,$$

and the fact that “??” equals two times “?” stays in his mind.

In the next problems he gets closer, without yet being quite articulate.

However, after different intervening problem types, #18 unfolds the idea:

> Find two numbers which, one multiplied with the other, make as much as the difference squared, and then, when one is divided by the other and the other by the one and these are joined together make as much as these numbers joined together.
>
> Posit the first number to be a *quantity* less a *thing*, and posit that the second be the same *quantity* plus a *thing*. Now it is up to us to find what this *quantity* may be, which we will do in this way.
>
> We say that one part in the other make as much as to multiply the difference there is from one part to the other in itself.

> And to multiply the difference there is from one part to the other in itself makes 4 *censi* because the difference there is from a *quantity* plus a *thing* to a *quantity* less a *thing* is 2 *things*, and 2 *things* multiplied in itself make 4 *censi*.
>
> Now if you multiply a *quantity* less a *thing* by a *quantity* plus a *thing* they make the square of this *quantity* less a *censo;* so the square of this *quantity* is 5 *censi*. [...]

This probably goes beyond what Antonio was able to do by mental implicit use of a second unknown, or at least beyond what he found it possible to convey to a reader in this way.

This is the likely reason that he now makes the use of two unknowns explicit, and also chooses a more stringent language, pointing out that the *same* quantity is meant in the two positions.

Awareness that something new and unfamiliar is presented to the reader is reflected in the explanation that now "it is up to us to find what this *quantity* may be";

it is never stated that the *thing* has to be found, neither here nor elsewhere in problems with a single algebraic unknown – that goes by itself.

The procedure can be translated into familiar symbols as follows:

$$AB = (A-B)^2 \;,\;\; {}^{A}/_{B}+{}^{B}/_{A} = A+B$$

with the algebraic positions

$$A = q-t \;,\;\; B = q+t \;.$$

Then

$$(A-B)^2 = 4C \;,\;\; \text{while} \;\; AB = q^2-C \;,$$

whence

$$q^2 = 5C \;,$$

that is,

$$q = \sqrt{5C} \ .$$

In consequence we have the preliminary result

$$A = \sqrt{5C} - t \ , \ \ B = \sqrt{5C} + t \ .$$

Inserting this in the other condition we get

$$\frac{A}{B} + \frac{B}{A} = \frac{\sqrt{(5C)} - t}{\sqrt{(5C)} + t} + \frac{\sqrt{(5C)} + t}{\sqrt{(5C)} - t}$$

which, after cross-multiplication, becomes

$$\frac{A}{B} + \frac{B}{A} = \frac{(\sqrt{(5C)} - t)^2 + (\sqrt{(5C)} + t)^2}{5C - C} = \frac{6C + 6C}{4C} = \frac{12C}{4C} = 3 \ .$$

Therefore, since

$$A + B = 2q = 2\sqrt{5C}$$

we have

$$2\sqrt{5C} = \sqrt{20\,C} = 3 \ ,$$

whence

$$20C = 9 \ .$$

Tacitly interchanging “first” and “second” number, Antonio thereby obtains that

$$B = 1\,{}^{1}\!/_{2}+\sqrt{{}^{9}\!/_{20}}\ ,\quad A = 1\,{}^{1}\!/_{2}-\sqrt{{}^{9}\!/_{20}}\ .$$

This would probably have been very difficult even for a mathematician of Antonio’s calibre without the explicit use of two unknowns.

In a huge manuscript written in 1463, Benedetto da Firenze also develops the use of several unknowns, in problems of the first degree only but making use of genuine symbolic writing.

> We possess his autograph, so we may be sure that we really follow his development of the idea.

His starting point is this intricate variant of a familiar recreational problem:

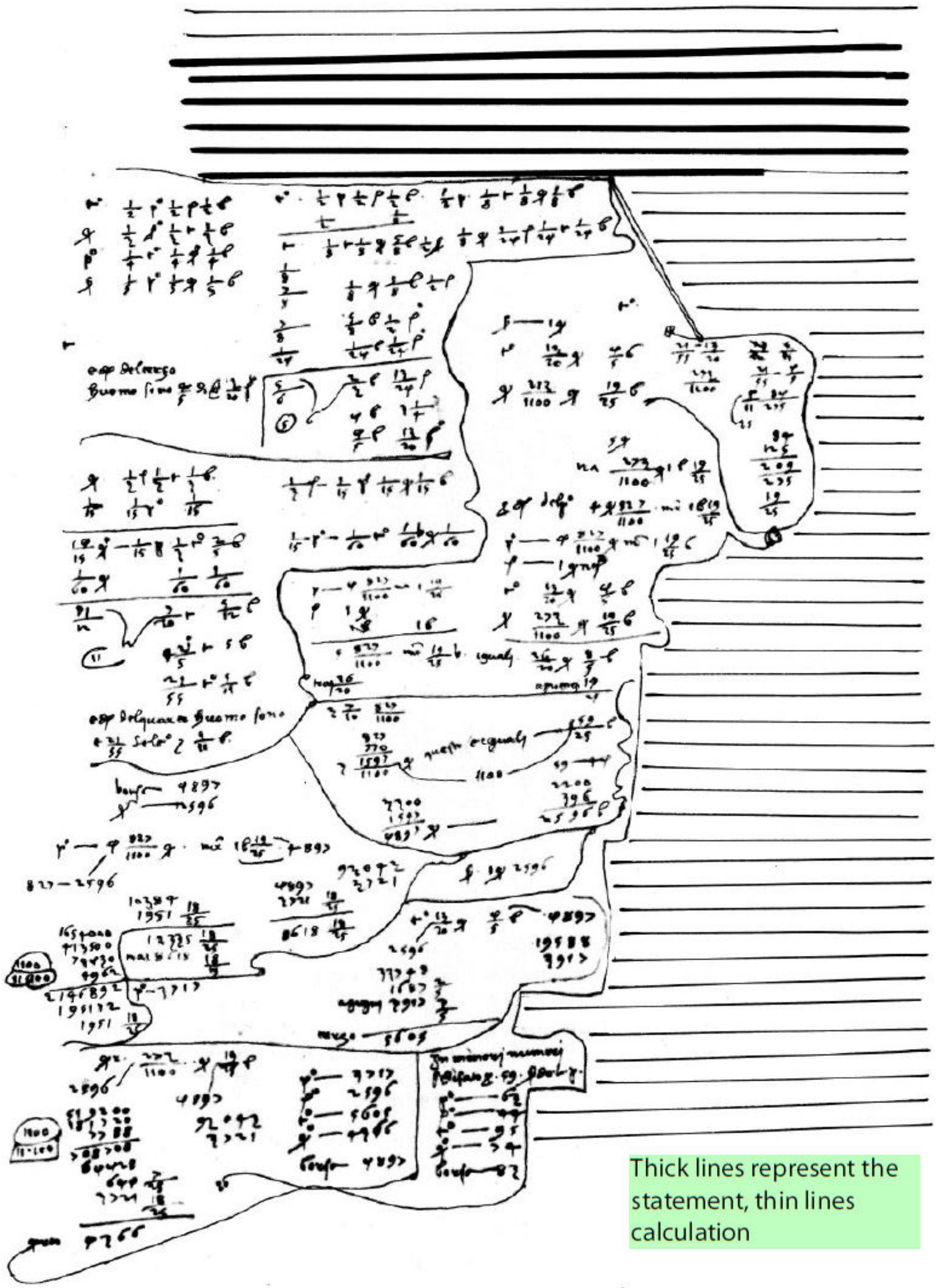
> Four have *denari*, and walking on a road they found a purse with *denari*.
>
> The first and the second say to the third, if you give us the purse we shall have 2 times as much as you.
>
> The second and the third man say to the fourth, if we had the *denari* of the purse we should have 3 times as much as you.
>
> The third and the fourth say to the first, if we had the *denari* of the purse we should have 4 times as much as you.
>
> The fourth and the first say to the second, if you give us the *denari* of the purse we shall have 5 times as much as you. It is asked how much each had, and how many *denari* there were in the purse.

At this point (that can be seen from the organization of the page), Benedetto starts making symbolic algebraic operations in the "margin".

Using already familiar standard abbreviations for "the first", "the second", "the third" and "the fourth" (which I shall represent by α, β, γ and δ) and *b* for the purse (*borsa*) he first writes the equations

(juxtaposition means addition, enlarged distance equality)

$$\gamma \qquad {}^1/_2\alpha \;\; {}^1/_2\beta \;\; {}^1/_2 b$$
$$\delta \qquad {}^1/_3\beta \;\; {}^1/_3\gamma \;\; {}^1/_3 b$$
$$\alpha \qquad {}^1/_4\gamma \;\; {}^1/_4\delta \;\; {}^1/_4 b$$
$$\beta \qquad {}^1/_5\alpha \;\; {}^1/_5\delta \;\; {}^1/_5 b$$



and then he starts operating algebraically on these.

Afterwards, but only afterwards, he describes the calculations in words in whatever space is left over by the "marginal" calculations – confirming by a number of mistakes that this description copies the (correct) symbolic calculations.

That is, Benedetto undertakes an algebraic symbolic calculation with five unknowns, apparently without thinking that this is something particular.

The organization of his calculations is improvised and not too clear, but the calculations themselves are correct.

The solution to this somewhat later problem is similar but somewhat more orderly:

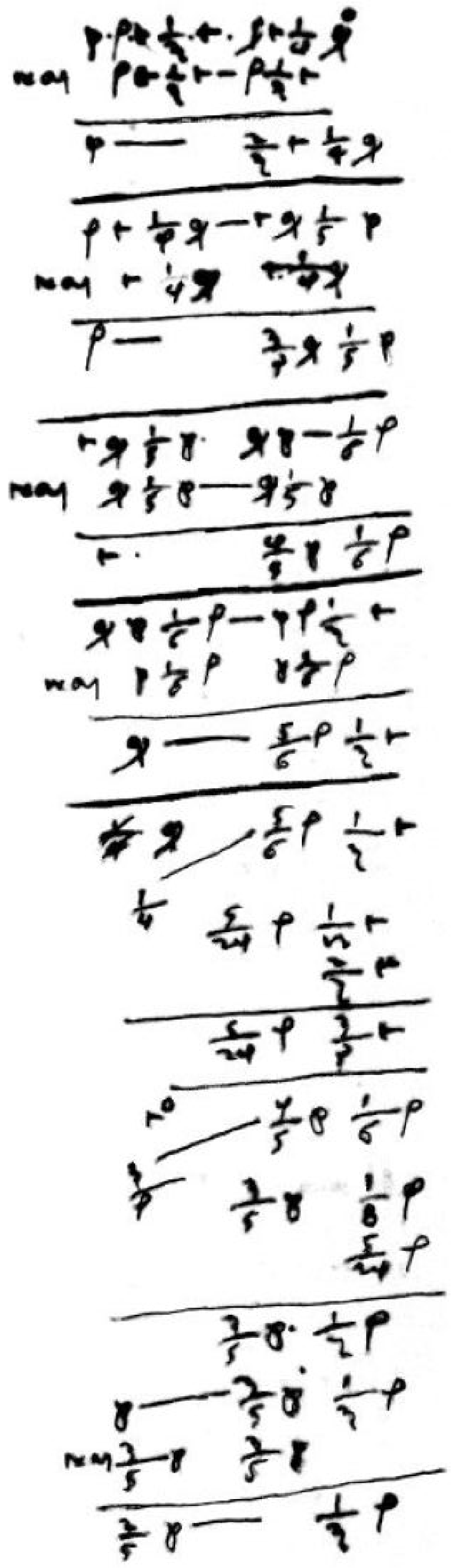
> Four men have *denari* and want to buy a horse, and no one has so many *denari* that he can buy it. The first says to the second and the third, if you give me $^1/_2$ of your *denari*, with mine I shall buy the horse.
>
> The second says to the third and fourth man, [...].It is asked, how many *denari* each one had, and what the horse was worth.
>
> Even though there are many ways to solve such cases I shall take the most convenient, or let us say the least tedious. [...]

At that point, Benedetto starts making marginal symbolic calculations.

As we see, he is aware of using a particular, “less tedious”, method.

With another “purchase of a horse” problem, that method gets a name: “by equation”:

> Four have *denari* for which they want to buy a horse, and none of them has so many *denari* that he can buy it. The first and the second say to the third man, if you give us the $^1/_3$ of your *denari*, we shall buy the horse. [...].
>
> It is asked, how much each one had, and what the horse was worth. We shall do it by equation.



Now, the technique is ready, it is named, and the calculations are extremely orderly.

It seems that Benedetto has now started making his calculations on a separate support, then copying them into the manuscript.

Benedetto formulates yet another problem that has to be solved “by equation”, and leaves a frame for the calculations. But he does not fill it out, nor does he go through the solution in words.

And this is the end of it.

We have two partial copies of the treatise, but both omit this difficult part. Benedetto’s idea seems to have been noticed by nobody.

In Nicolas Chuquet's *Triparty* from 1484 and Luca Pacioli's *Summa* from 1494 there is another approach to several unknowns.

Both operate with a second unknown, a *quantity*, but recycle it so as to work effectively with several unknowns.

None of them appear to think much of the technique. According to Pacioli, this use of a "second thing" is well known from "ancient practical books" – he does not say which, and they are unknown to us.

Girolamo Cardano follows Pacioli in his *Practica arithmetice, et mensurandi singularis* from 1539, but even he does not see the technique as something important.

Quite differently, Christoph Rudolff sees this *regula quantitatis*, as he calls it, as "a completion of the *coss*, indeed in truth a completion without which it would not be worth much more than a *pfifferling*" ["a trifle"].

Rudolff shares the name *regula quantitatis* with Étienne de la Roche, who had used it 1520; other more striking coincidences suggest that Rudolff was inspired by this otherwise not very influential book.

In 1544, further unfolded in 1553, Michael Stifel introduced an alphabetic notation for indefinitely many supplementary unknowns. We shall come back to that.

Internal development under which external conditions?

These developments can be described and more or less “explained” by internal dynamics, *given the institutional settings* within which algebra developed.

Abbacus algebra thrived, as indicated by the name, within the abbacus school environment.

Abbacus algebra had no practical uses. It was a prestige topic, and therefore also served in competitions

- either arranged officially by authorities for the recruitment of teachers to the municipal abbacus schools;
- or, more informally, taking place by means of challenges meant to impress the fathers of prospective students and show which master had the best school.

That provided a drive to solve more complicated problems than those traditionally known – not least problems of the third and fourth degree.

Already in 1328, a problem collection gives false rules for irreducible cubics, which (soon supplemented by others) survived in Italy, where they were still repeated uncritically by Piero della Francesca

(being even repeated in an arithmetic printed in Portugal in 1550).

A modicum of algebraic insight might have exposed the fraud, but that was not to be expected from the members of city councils or fathers of prospective students (nor in most cases from competitors).

However, as we know, such situations engender genuine mathematical interest in some.

Beyond mathematically competent abbacus masters like Antonio and Benedetto we may mention Giovanni di Bicci de' Medici, initiator of the Medici rise to power in Florence and at the time fully occupied by the expansion of the Medici bank.

In 1397 he engaged in a discussion about the solvability of certain irreducible equations.

None the less, the concentration on the solution of complicated problems did not push toward development of *coherent* new insights

– except a negative empirical insight formulated by Pacioli: *So far* no general way is found by which to solve three-member equations where the powers are not "equidistant".

Striking is the case of Benedetto da Firenze. We have seen how he created a technique for solving complicated linear problems by means of symbolic algebra with many unknowns.

When that was done he left things there, not even pointing out to the high-status dedicatee of his huge treatise that he was offering something going far beyond what had been done before.

As we have also seen, the section in question is only found in Benedetto's autograph but left out in the other copies we possess of the *Trattato*.

Benedetto's innovation remained unknown

(at least until, three ago, I took the trouble to work through the 1012 folio pages).

10 minutes' break

The setting of *Rechenmeister* algebra was different from that of the abbacus masters.

Firstly, the *Rechenmeister* participated in the new print culture. False solutions would soon have found a competent reader and thus have been unmasked.

Competition, moreover, was not located in challenges judged by more or less competent or incompetent mathematical laymen.

The medium for the *Rechenmeisters'* competition was the book market, for which reason the hundreds of *Rechenbücher* published between 1500 and 1650 almost invariably claim to be totally new

– which is just as invariably fully false or almost false: they are highly repetitive.
And thus, it must be said, with few exceptions rather boring

Beyond this general setting we should observe that those who *brought algebra* to German lands and language were generally trained in the university tradition.

Since some of them are anonymous we cannot claim that they *all* were, but we have no counterevidence.

The core of abbacus algebra, as said, was the solution of intricate problems.

More precisely, given the basis in the abbacus school, problems that looked as if they dealt with questions pertinent to commerce.

For instance: Barter problems where the price of merchandise was raised over the cash price not by a fraction but by its square root.

Or, at most, recreational classics like men buying a horse or finding a purse.

The *universitarian tradition* was different.

At the early (say, “high-school”) level we find *Boethian arithmetic* – taught as some kind of theory (without proofs).

It was trained by means of a board game – not really problems but at least asking for individual dexterity in calculation.

At the university level, many encountered *algorism*, the computation with Hindu-Arabic numerals meant to serve in astronomical calculation.

We must presume that its algorithms were trained, but even that does not allow us to speak of a problem culture .

And then, as mathematical high point, there were lectures: for many towards the end of the Arts course *Elements* I or the first book of Witelo's optics.

For a few (we may guess, we have no numbers) more advanced matters.

Lectures taught theory, and the disputations linked to them mostly served metamathematical discussions. That can be seen from collections of *quaestiones*, emulations of disputations.

> If a mathematical theorem is well proved, the only thing left to discuss is indeed the foundations.

There was no opportunity to develop a problem culture.

Jordanus of Nemore's *De numeris datis* illustrates this orientation well: It can be seen as a stand-in for Arabic algebra emulating Euclid's *Data*.

But instead of solving problems (which could have been done if the problems of the *Elements* had been taken as model) it is formulated as a collection of theorems, that is, as *theory:*

> If certain arithmetical combinations of some numbers are given
> (for instance, for two numbers their sum and product)
> then the numbers themselves are given.

This was the mathematical upbringing of Johannes Widmann, Andreas Alexander, Heinrich Schreyber and Christoph Rudolff.

When they taught algebra, they still taught it as a tool for solving problems;

but we may presume that their background was what caused them to use for instance their notations and organize their material much more systematically than the Italians had done.

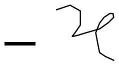
Stifel, on his part, did not *teach algebra* in his *Arithmetica integra* from 1544 – at least not as the others had done.

He *presented the theoretical basis for algebra* as just one aspect of "the whole of arithmetic" – namely in book III. There are illustrating problems, but they are as secondary as the problems of a modern mathematical textbook teaching theory.

But within this context he offers a decisive innovation which, when at all mentioned in descriptions of the book, gets no more than a few lines:

namely the alphabetic notation for indefinitely many supplementary unknowns which I have already referred to.

This was not yet the new algebra, but as we shall see an *indispensable first step*.

Stifel keeps the *thing* (in the German tradition the *coß* – ) with its powers (, *zensus,* and , *cubus*) as primary unknown – henceforth I shall transcribe *r* (for *res*), *z* (for *zensus*) and *k* (for *cubus*).

But then he adds unknowns 1*A*, 1*B*, 1*C*, etc.

More precisely, these are the first powers of the supplementary unknowns, and explained initially to stand for 1*Ar*, 1*Br*, 1*Cr*, to be understood as "the *A*-kind of *r*", "the *B*-kind of *r*", etc.

The second powers are 1*Az*, standing for "the *A*-kind of *z*", etc.

The product of *r* and *A* is written *rA*, the product of *A* and *B* is *AB*.

If only we remember that order carries meaning (thus that Ar does not mean the same as rA) the system is without contradictions though cumbersome and dangerously close to being misleading.

But only without contradictions until we need to write the product of Az and B – how do we know that AzB means $(A^2)B$ and not $A(zB)$?

Stifel could certainly have found a way out, but he had no need to do so in the present context.

Almost all of his examples are indeed simple – Stifel's aim is not to display his ability to solve problems which others, not possessing his technique, would not be able to solve.

Instead, Stifel is *teaching a technique*. Thereby he confirms that the book is primarily rooted in the university tradition, only expanding its reach so as to encompass also *Elements* X and the new field of algebra.

The first two examples of how to use the notations are borrowed from Rudolff; the third is a familiar problem type which since centuries had been solved without algebra:

> 7 persons owe me money; the sum of the debts less, the first, the second, the third, etc. are given. The sum of all these given numbers is 7 times their total less 1 time the total, that is, 6 times the total. Etc.

Stifel gives the names *r*, *A*, ... *F* to the debts, and then eliminates these unknowns one by one

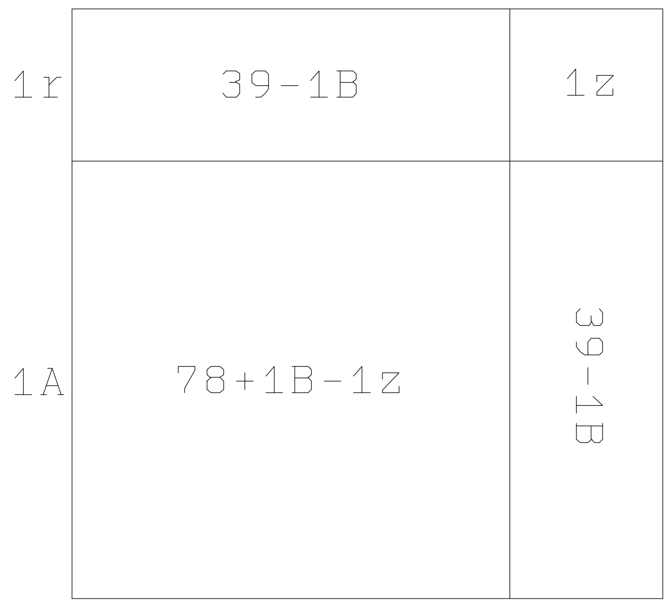
– that is, even if Stifel had wanted to use algebra, Rudolff's method would have been fully adequate, a single name *quantity* would have sufficed.

The next problem is a reducible quartic. It asks for two numbers (say, P and Q) fulfilling the condition

$$P^2+Q^2-(P+Q) = 78\ ,\ \ PQ+(P+Q) = 39\ .$$

Stifel posits the first number to be r and the second to be A, and for convenience represents their sum by B.

He proceeds in a way that has more to do with *Elements* II or with square-grid geometry than with algebra, using a diagram:



From the second condition he gets that $rA = 39–1B$. That allows him to complete the square, etc.

Two things are to be observed here.

Firstly, that Stifel avoids using his new formalism in non-linear *algebra*. The difficulties inherent in his notation therefore do not materialize.

Secondly, that the geometric embedding allows Stifel to take over from geometry the habit of naming more than a minimal set of unknowns by letters.

In a lettered geometric diagram all occurring entities may indeed be treated on an equal footing.

That will be important in what I shall have to say later.

Problems with several unknowns return later in Stifel's book.

Only two are non-linear. We need not discuss them.

More important for the present discussion is an "improved and much augmented" edition of Rudolff's *Coss* which Stifel prepared in 1553.

Here he replaces Rudolff's notation for the second unknown by his own, and also uses it in a number of problems that Rudolff had solved without using a second unknown.

Of much greater interest is an *Anhang*, "Appendix", containing 24 new problems. 12 of them (all of higher degree) make use of the new technique.

In this context, the mathematical type changes (returning to that of Rudolff): instead of presenting theory and techniques, Stifel demonstrates his ability to solve complicated problems.

In order to do that conveniently, however, Stifel has to improve his notation.

His unknowns are now r, A, B, C, etc. The second power of A is now AA, etc.

In 1565, Mattheus Nefe inserts a single example illustrating the technique in a *Rechenbuch* that otherwise does not take up algebra – a mathematical analogue of Stifel's debt problem, and in so far not remarkable.

But Nefe has seen that there is no reason to distinguish between primary unknown and secondary unknowns, his unknowns are A, B, C and D.

Stifel's technique is also presented (without this simplification) by Clavius in his *Algebra* from 1608.

That was Descartes' schoolbook in algebra, but Descartes seems not to have read this part of it.

Much more important was probably Valentin Mennher's *Seconde arithmetique* from 1556.

Mennher had come to Antwerpen as an accountant in Fugger service and settled as a *Rechenmeister*.

His competence went far beyond what would be expected from a *Rechenmeister* – he also published a book on spherical trigonometry (Antwerpen was a sea-faring city) based on Regiomontanus's *De triangulis* but performing the calculations.

His presentation of several unknowns, on the other hand, does not go beyond what had been done by Stifel in 1553 – but Mennher's presentation is very full, does not copy, and shows he understands to perfection.

Descartes' friend and mentor Isaac Beeckman possessed a copy.

As late as 1666, moreover, John Collins also recommended Mennher's book as a good introduction to algebra.

Mennher was thus at hand and well known in the 17th century, and Stifel's 1553-techniques thus available also for those who read French but not German.

That, however, still did not create the new algebra. For that to happen, another transformation of mathematical culture was needed.

Evidently not global, but a transformation of a particular strand.

There was, it is true, interest in algebra in 16th-century France.

That is illustrated by the two printings of Johannes Scheubel's introduction to algebra in Paris in 1551 and 1552:

– a first edition may be a printer's misjudgment of the market;
– a second edition so soon proves it was not.

There was even a kind of tradition to write about algebra in France. We may mention the names:

Étienne de la Roche (1520) – Jacques Peletier (1554, Latin 1560) – Jean Borrel/Buteo (1559) – Pierre de la Ramée (1560) – Guillaume Gosselin (1577)

But these writers did not produce anything that can be considered *a tradition*, there is neither accumulation nor continuity.

Peletier, eager to list both those algebraic precursors he had read and those he had only heard about did not know de la Roche in 1554.

The new algebras did not unfold from them

– Viète, hiding his traces, may have borrowed some inspiration, but hardly much;
  - all we know for sure is that he took over the names for powers
    - from Xylander's translation of Diophantos;
  - he is likely, on the other hand, to have read at least Mennher's
    - spherical trigonometry, perhaps therefore also his arithmetic;

– Descartes took nothing at all (nor from Viète, by the way).

The new mathematical culture was a *jeu de prestige*, a “prestige game” – at least as agonistic as the culture of the abbacus masters, probably more.

But the competition had a new mathematical basis, which we may call “Humanist mathematics”.

Early Humanism – from Petrarca until Lorenzo Valla – had not been interested in mathematics – neither in practical mathematics nor in Greek theory.

They respected Archimedes because of what was said about him by Plutarch and Cicero, but they knew him only an engineer and as a servant to King and Country.

His mathematics was unknown. If seen as a *mathematicus*, then in the sense of “astrologer”.

Then, around the mid-15th century, some high-level mathematical practitioners with Humanist affinities started to change that – most famous Leon Battista Alberti.

However, what they could know as mathematics was a combination of the medieval-universitarian and the various “practical” traditions.

The first Humanist translations of Greek mathematics were published only from around 1500 onward:

Giorgio Valla, 1501 (posthumous): Euclidean and Archimedean/Eutocian fragments;

Bartolomeo Zamberti, 1505: problematic translations of Euclid;

Memmo, 1537: translations of Apollonios's *Conics* I–IV;

Commandino, 1566: another translation of Apollonios;

Xylander, 1575: Diophantos

Various printings of Archimedean works in Moerbeke's 13th-century translation were also published by Luca Gaurico in 1503 and Tartaglia in 1543.

And then the editions of Greek texts:

The Grynaeus edition of Euclid with Proclos's commentary in 1533;

Pappos's *Collection* in 1538;

Archimedes in 1544.

These translations and (for those who read Greek) editions made a new and much more challenging kind of mathematics available.

However, as everybody knows who comes out of a supermarket alive, not everything that is available has to be put into the basket.

Why would others than those who made the translations or the editions be interested in what they could find on the shelves?

Here, a detour over general Humanist culture helps.

Humanism had always been interested in *utility*.

Not vulgar utility, of course. Humanists despised those crafts which produced their food and clothing just as much as any German 19th-century humanist scholar would look down on such *Banausen*.

They did not need to assert that *mathematicus non est collega* (“the mathematician is no colleague”), as their 19th-century successors – that was so obvious to Renaissance Humanists that the mere statement would have been an obscenity.

> When Widmann was made the first specialist mathematics lecturers at the already Humanistically tainted Leipzig university it was no promotion; it served to make sure he would never proceed to higher, better paid matters.

The Humanists meant *civic utility*: what was useful for the embellishment of courtly and patrician life, and eventually for military safety.

During the 15th century, that implied that even architecture though a practical art could be appreciated (as it was already appreciated because it was dealt with in Latin by Vitruvius);

but also such kin as painting and sculpture became acceptable.

Around 1400, the Duke of Milan "was often heard to say that he was not damaged as much by a thousand mounted Florentine warriors as by Coluccio Salutati's style".

Salutati, we should know, was the Humanist Chancellor of Florence from 1375 to his death in 1406.

After the *grand tour d'Italie* of the French artillery in the 1490s, it became clear that Latin letters might (perhaps) outperform mounted warriors, but certainly did not suffice against modern gunnery.

Around the same time, it turned out that the Portuguese systematic promotion of navigational mathematics brought unexpected fruit to the Portuguese crown.

So, certain mathematical *technologies* had to be included in the panoply of what was civically useful.

Hans Holbein
the Younger
The Ambassadors
(1533)

That mathematics, not least its prestigious though hardly useful higher level, had become a state affair, as once Latin style, is evident from a famous episode from the history of mathematics (I quote H. L. L. Busard's biography of Viète):

> Viète's mathematical reputation was already considerable when the ambassador from the Netherlands remarked to Henry IV that France did not possess any geometricians capable of solving a problem propounded in 1593 by Adrian van Roomen to all mathematicians and that required the solution of a forty-fifth-degree equation.
>
> The king thereupon summoned Viète and informed him of the challenge. Viète saw that the equation was satisfied by the chord of a circle (of unit radius) that subtends an angle $2\pi/45$ at the center. In a few minutes he gave the king one solution of the problem written in pencil and, the next day, twenty-two more.

The story also illustrates that Viète (and his likes) were known to the court, and ideologically as well as practically linked to it.

But which were Viète's mathematical interests?

They can be identified from his *Book 8 of various responses about mathematical matters* from 1593:

- two intermediate proportionals;
- squaring and rectification of the circle and of circular segments, using Archimedean spirals and the quadratrix;
- construction of a regular heptagon;
- lunules; etc.

In the end Viète deals with spherical trigonometry, a topic that had his special interest;

this is the only topic that points to broader practices (astronomy and navigation).

Soon, Pierre de Fermat, Gilles de Roberval – and of course Descartes – were to widen the horizon, taking up not only further areas of Greek mathematics but also, for instance, the geometry of kinematics.

Geometry, however, was the core and also made up most of the periphery. More precisely, geometrical *problems*.

And, even more precisely, *problems rooted in ancient Greek geometry* – perhaps problems known from Pappos, perhaps problems going beyond but of the kind.

No longer, as in Benedetto's case, abstruse versions of recreational classics about four men buying a horse or finding a purse.

Now, Viète as well as Descartes knew algebra as a tool for solving problems.

Viète, as already said, is as taciturn as Euclid when it comes to explanations of "why" and "from where".

Descartes, fortunately, is not.

So, what do we find in Descartes' *Geometrie* ? And in previous notes and letters preparing the project?

In a letter to Isaac Beeckman from 1619, Descartes expresses the ambition to solve all problems "dealing with any kind of quantities, discrete as well as continuous", by means of curves corresponding to higher-degree equations.

By then – and even in 1628 – he was still using Clavius's notation for a single unknown (identical with that of Rudolff).

> That notation, by the way, is the one of which he would speak in 1637 in *Discours de la méthode* as "a confused and obscure art that puts the mind in difficulty instead of a science that cultivates it".

In a note from 1628, Beeckman states that Descartes when visiting him had told him to have invented a general algebra where all the *notae cossicae*, the “cossic characters”, are represented by lines (as they were to be in the *Geometrie*).

These *notae* are still those of Clavius and Rudolff. That will have been a perfect occasion for Beeckmann to show Descartes what Mennher had done.

Also possible though less likely is that Descartes discovered Mennher in Beeckman’s library on his own.

Then, in the *Geometrie* (whose manuscript was finished in 1632), Descartes has this to say:

> When wishing to solve some problem, one should first look at it as already solved, and give a name to all the lines that seem to be needed in order to construct it, those that are unknown as well as the others.
>
> Then, without making any difference between these known and unknown lines, one should run through the difficulty according to the order it shows, the most natural of all, in which way they depend mutually on each other, until the point where one has found a way to express one and the same quantity in two ways:
>
> which is called an equation.

That, of course, is only possible if equations can be formulated in terms of a plurality of unknowns – “give a name to all the lines that seem to be needed”.

Moreover, as we see, letter names are also given to the known quantities. These then become coefficients and constant terms.

AND HEREBY WE HAVE THE NEW ALGEBRA!

That is, *Descartes'* “new algebra”.

What about that of Viète?

Viète presents his new algebra independently of the problems to which he intends to apply it.

But as expressed in the famous closing phrase of the *Isagoge* from 1591, the aim is *nullum non problema solvere*, “to leave no problem unsolved”.

In chapter V of the same treatise, Viète describes the procedure to be used when an algebraic solution is aimed at:

> Magnitudes, those which are known as well as those which are asked for, should be combined and compared, adding, subtracting, multiplying and dividing, always observing the law of homogeneity.

So, Viète's road toward the abstract coefficients appears to have been very similar to that of Descartes.

Neither can really be said to have "invented" them.

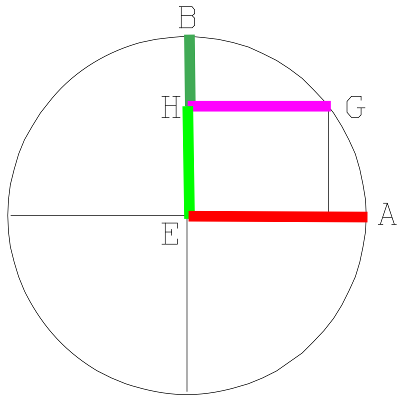
Both received them as a gift not asked for but coming by necessity out of the application of algebra with an unlimited number of unknowns to intricate geometric problems.

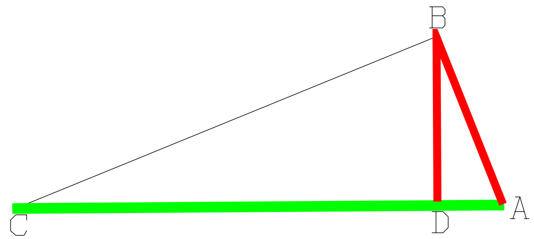
A parallel illustrates the essential role played by the availability of a *plurality* of unknowns.

ʿUmar al-Khayyāmī, as a mathematician, was certainly at the same level as Viète and Descartes.

Arabic algebra had operated occasionally with several unknowns for long when al-Khayyāmī wrote; however, al-Khayyāmīs's algebra was the classical type with a single unknown.

This algebra he applied when attacking a difficult geometric problem about a particular partition of a circular arc.





The arc *AB* is to be divided at G in such a way that *AE* : *GH* = *EH* : *HB*. A long analysis reduces this to the finding of a right-angled triangle *ABC*, with height *BD*, in which *AB*+*BD* = *AC*.

In order to apply *his* algebra with only one unknown, al-Khayyāmī needs to posit that *AD* = 10; that leads him to an equation whose coefficients are numerically fixed.

Descartes and Viète would have posited *AD* to be, for instance, *b*, which would automatically (though obviously after as much calculation as made by al-Khayyāmī) have produced an equation with abstract coefficients.

So, the immediate push to invent the new algebra with its many unknowns and abstract coefficients came from the possibilities that were on offer in already existing mathematics:

> Both the kind of problems that provided the occasion
>
> and the tool of many unknowns.

Both, though belonging to different categories, would traditionally be characterized as “internal factors”.

But why was mathematics seen as a worthwhile challenge in the environment to which Viète and Descartes belonged?

And why would these problems at all be at hand?

Answers to these questions, as we have seen, can only be given if we take into account what would traditionally be seen as “external factors”.

To which is to be added that the very metaphor of “factors” is questionable:

It intimates a dubious separability.

In a way, it is thereby a counterpart of the algebraic analysis created by Viète and Descartes.

But that analysis, within a generation, gave rise to another analysis, Wallis’s study of infinites, with all its infinite series.

There is no reason to assume that historical analysis should be simpler.

However, like Wallis we have to be satisfied with a hint:

“&c.”

Back to Zilsel

We might stop here.

This ending fits what I have often said to my students:

“things are always more complex” – namely, more complex than can be expressed in a finite text. But the exposition has to stop at some point, “footnotes to footnotes are not allowed by typesetters”.

However, I started with the Zilsel thesis, and should include it when wrapping up.

The thesis spoke about three groups: Renaissance Humanists, university scholars, and higher artisans.

In our story, Renaissance Humanists can be taken over directly.

At the global level, university scholars also recur.

However, what is interesting for us are not the natural philosophers of Merton College and their kin but the readers of Euclid and of other ancient mathematicians, and those who taught such matters

- in particular taught the appurtenant norms for what constitutes mathematical truth at the university.

Artisans, finally, are not to be understood as gunners, surgeons and master builders but as the abbacus masters who taught in the Italian abbacus schools and (sometimes) developed new knowledge far beyond what they taught.

The first really fruitful interaction was that between the latter two currents in the creation of *Rechenmeister* algebra in the 16th century.

That created a systematic approach to algebra, with consistent though restricted use of symbols;

and, with Stifel, the important tool of an unrestricted range of unknowns.

Next came the interaction of the Humanist current, forced by new military and socio-economic conditions, with theoretical, Greek-inspired geometry

- not directly with the broad universitarian mathematical tradition but presupposing it as its basis.

The final leap was made when this new "Humanist" mathematics took over the algebraic tool as shaped by Stifel, Mennher and Clavius, and transformed it in agreement with its own needs.

So, even though mathematics is next to invisible in Zilsel's original sketch, his basic idea applies well to the first step in the creation of the algebraic 17th-century starting point for the new mathematics.

The next step – infinitesimal analysis – would ask for a different analysis, and would have to tell a different, probably more intricate story.