The Historical Metamorphosis of Science, or: What Enabled Modern Science To Conquer The World?

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In previous contributions to GEHN conferences and its predecessor at Cumberland Lodge I have sought from a variety of viewpoints to impress upon participants one major point. This is the indispensability of the 17th century Scientific Revolution, which is the uniquely European event out of which modern science grew, for the early-19th century emergence of the modern world in which we live. Below I shall point-wise sum up my core message, but for the rest I spend no more words on it. As we are now meeting for the final GEHN conference, I wish in my present contribution to look at the phenomenon of science at a somewhat higher level of abstraction. I am not going to offer you some personal, neatly rounded-off philosophy of science. I do not have one, in good part because philosophers are inclined to think of science as a timeless entity not subject in its basic structure to change of any kind. Instead, I aim to give some answers to the following question: What properties that modern science possesses as its various predecessors in the acquisition of knowledge about nature did not, enabled it in the end to become a major (albeit far from the only) causal factor in the making of the modern world? How could what was once a component of culture with minimal economic impact turn into a major production factor?

Modern science and the modern world: A recapitulation

- Certain pieces of machinery key to the Industrial Revolution were not just highly ingenious inventions but differ fundamentally from customarily trial-and-error craft procedures in that they contained inside themselves certain basic components of modern science. This is true notably of the steam engine. The very machine that enabled Europe to escape from the perennial energy bottleneck could not have been constructed without prior knowledge of, and prior experience with, the void and air and steam pressure.
- Nor are the insights and experiences that came into play here isolated ones that might have turned up anytime anywhere. Rather, they form integral parts of one big, largely coherent historical event that comes down to the emergence of recognizably modern science and is still best labelled 'The Scientific Revolution of the 17th century'.
- This was neither a chance event, nor one foreordained. For a variety of reasons it could not have happened in China. It might have happened in Islam civilization, but it did happen in another civilization to adopt the Greek corpus in nature-knowledge, not in medieval Europe though, but (after a second adoption following upon the fall of Byzantium) in Renaissance Europe.
- The 17th century emergence of recognizably modern science, while responsible in good part for the coming-into-being of our modern world, was not *solely* responsible for it. Pioneers like Galileo and Bacon recognized right from the start the potential of the advent of science of a new kind for changing rule-of-thumb craftsmanship into science-based technology. All over the 17th century were scientists of the new type busily exploring how such a fundamental change could be brought about, almost

entirely as yet in vain. In 18th century Britain engineers of a new, scientifically informed type began to practice the change with increasing measures of success. Also in 18th century Britain did networks come up which linked scientists of the new type and engineers of the new type with entrepreneurs who thus came to be alerted to investment chances of a new type. How the emergence of such networks *coincided* with market conditions of a kind as to make such new investments profitable is one question that remains, to my mind at least, among the key riddles of the coming-into-being of our modern world.

The contrast, in brief

So far my recapitulation of points made at earlier occasions. From here on I shall in a mood of irresponsible generalization contrast recognizably modern science such as it came out of the Scientific Revolution with a variety of approaches to nature of pre-modern type. A variety indeed: In presently drawing some broad contrasts I do not wish to suggest that prior to the Scientific Revolution there was only one way to seek to attain a systematic understanding of natural phenomena. All over the Old World there were many such ways, and they differed among themselves in numerous respects. So as to avoid calling the systematic investigation of natural phenomena from before the watershed of the Scientific Revolution by our modern name 'science', I have coined the ugly expression 'modes of nature-knowledge'. This expression stands for consistent ranges of approaches to natural phenomena, subject to transformation of a more or less drastic kind and different among themselves in several important ways.

Modes of nature-knowledge differed in scope. Some, like ancient mathematical science such as pioneered in Alexandria and later recovered and also extended somewhat in Islam civilization and then in Renaissance Europe, addressed issues in deliberately piecemeal fashion. To empiricist and experimental approaches the same applies. Others, notably natural philosophy in the Athenian tradition, took a comprehensive view of the world meant to explain the whole of it.

They differed in the ways in which knowledge was attained. Some, notably those two Greek modes of nature-knowledge, were predominantly intellectualist (i.e., highly deductive mental constructions). In contrast, China's nature-knowledge was predominantly empiricist (based on the sheer observation of phenomena), as also a new current that arose in Renaissance Europe beside the revival of the two Greek ones.

They differed in the practices that might go with them, which could be just observational but might turn experimental, with or without the aid of scientific instruments.

They differed in their ultimate objectives. Knowledge could be sought as an end in itself. It could also be sought with a view to improving certain practices, as was the case to some extent in China but more outspokenly and insistently so in Europe's succession of empiricist modes.

Of particular concern in distinguishing a variety of modes of nature-knowledge is what I call their 'knowledge-structure'. This concerns issues like the following:

How was knowledge organized? For instance, the acquisition of knowledge could proceed wholesale, as one bold grab for the whole, or it could proceed by handling one issue at a time.

How did the search for nature-knowledge stand oriented in time? Did practitioners conceive of themselves as reconstructing past perfection? This was true notably of all those who, be it in Islam civilization or in medieval Europe or in Renaissance Europe, were engaged in recapturing and then enriching the Greek corpus of nature-knowledge, be it in its Alexandrian or in its Athenian variety. Or did they rather conceive of themselves as working toward an open future? This is what Europe's empiricists did, and what in the 17th century Galileans and Baconians began to do in more thoroughgoing fashion.

How were empirical facts being handled? These could be treated in their own right, as (by definition) in empiricist approaches. They could also be made to serve some *a priori* schema. If the latter, this could either be by way of illustrative confirmation, as in natural philosophy, or for purposes of *a posteriori* checking, as notably in Galileo-style, mathematical-experimental science.

In going ahead now to contrast recognizably modern science with pre-modern modes of nature-knowledge I look at them from a dual viewpoint familiar to GEHNers: reliability and usefulness. I leave to my oral presentation a few sweeping remarks about certain other, less tangible aspects that may come in for instructive contrast: rationality, relation to world-views, autonomy, and visibility. To sum up my conclusions in advance: very broadly speaking, the uses to which pre-modern modes of nature-knowledge were put were incidental, on a down-to-earth level, and more culturally than economically significant; they further were not very reliable; rational in ways that only the historian may stand ready to recognize as such; shot through with world-views; dependent for support on quite unstable institutional arrangements, and nearly invisible for all but an intellectual elite. Instead, the science that by the late 18th century was well on its way to alter the face of the world was reliable, or at least up for orderly rebuttal; further, with increasing frequency useful economically; rational in senses that are with us still; not for its conclusions dependent on any world-view; socially autonomous to a relatively high degree, and highly visible throughout society.

In elaborating these contrasts I deliberately refrain from drawing them in too sharp a fashion. One widely- spread misconception about the viability of the very idea of the Scientific Revolution stems from the concept being taken in an absolute sense, as if everything ought then to have changed at one stroke. The Scientific Revolution certainly marks a watershed in how over the ages humanity has conceived of the external world, enabling humans in the end drastically to alter it. Yet the contrast, albeit decisive, is not absolute but relative. The pioneers, in the very act of instigating the overhaul of numerous traditional ideas and practices, inadvertently stuck to many others and also went about exploring new venues which later generations were in a better position than the pioneers themselves to gain full clarity about. The implications of the advent of recognizably modern science were so vast as on the one hand to make the rise of our modern world possible, but on the other hand still to defy full awareness and full realization of those implications for generations to follow upon the pioneers.

Reliability

For the two varieties of nature-knowledge in the Greek tradition, nothing less than certainty was as a rule being claimed. In each case the knowledge attained was held to be reliable in an absolute sense. In mathematical science, such as practiced with Alexandria for intellectual centre, certainty was claimed for mathematical proof, at the cost however of the very abstract nature of the derivations made and the conclusions reached — the knowledge attained could be relied upon but its real-world relevance (let alone impact) was minimal. In natural philosophy, such as established in four rival schools in Athens, adherents to each specific school claimed certainty for the first-principles on which their particular conception of the world depended. That is, the first-principles posited in each case were themselves the warrants of their indubitable certainty. The world could not possibly be otherwise than the first-principles dictated, and everything else followed from them. Therefore in natural philosophy empirical phenomena served no other role than as illustrations of verities established beforehand. If you and I see a branch fall to the ground, then a faithful Aristotelian watches not just that, but really an event that he understands perfectly and in all its universe-wide ramifications. The falling branch instantiates just one more case of a heavy body moving toward its natural place, which for objects made of earth and water is in a right line toward the centre of the universe. And why this is so, follows in its turn from Aristotle's core conception of change as realization of an end that has from the beginning resided in the object changing. If we consider this sort of argument today, the problem is not so much that most conclusions are just wrong. The historically relevant point is rather that the knowledge-structure from which they stem has become so utterly foreign to us.

It has become so due to the rise of recognizably modern science. Right from the start pioneers like Galileo, while convinced that the natural philosophers (mostly Aristotelians) had just been on the wrong track, realized that they were in need of new ways to safeguard the certainty of their conclusions. Galileo and his disciples for the first time made mathematical science deal with the real world, using experiment as the stairway between an upper level of abstract idealization and the bottom level of everyday reality. They immediately faced the problem of how to decide whether and, if so, to what extent experimentation sufficed to make their mathematical rules match the real world. In similar fashion were those who, under Bacon's aegis, began around 1600 to use experimentation in a fact-finding manner, bound to grapple with the problem of whether artificial equipment and their own observational bias did not destroy or at least obscure beforehand the very phenomena that untainted nature presents to us. Claims that 17th century pioneers made *in public* varied from the indubitable certainty many stuck to in order not to look less worthy than the natural philosophers, to claiming no more than some degree of probability. More interesting and innovative than such public claims, however, was their actual practice — what sort of things did they do to satisfy themselves that their conclusions matched natural realities? More and more did both mathematical-experimental and fact-finding experimental scientists come to regard natural philosophy in the Athenian tradition as shot through with fanciful assertions and as innately incapable of putting a stop to its fancying. How, then, to check the clearly present risks of unbridled fancy in their own work?

The Scientific Revolution may be regarded as a veritable epistemological laboratory — the possibilities and the limits of scientific validation were being explored from scratch. The general tenor of the search has nicely been captured by Pascal in one of those immortal one-liners of his:

> "We have an incapacity for proof which no amount of dogmatism can overcome. We have an idea of truth which no amount of skepticism can overcome."

I shall now consider in some detail the search for checks upon unbridled fancy in mathematical-experimental science first, then in fact-finding experimental science. I put an illustrative example at the head of both.

In his 1644 treatise ‘On the motion of water’ Galileo’s pupil Evangelista Torricelli derived a theorem (known since as ‘Torricelli’s law’) on the speed with which water flows out of a hole drilled in a vessel at some point below the water surface. He took the case as analogous with Galileo’s treatment of falling bodies, and thus assumed

> “that those water jets which flow out with violence, possess at their point of outflow the same impetus which any heavy body, or one drop of that very water, would possess if it were to fall naturally from the upper surface of that water down to the orifice out of which it flows.”

From this principle of equal ‘impetus’ (in the Galilean context to be understood as a capacity, acquired in falling down, to return to its previous height) Torricelli quite rightly inferred that outflow speed is proportional to the square root of the distance between that hole and that surface. He sought to shore up the principle by some theoretical arguments drawn from what would happen in communicating vessels, but then went on to grant that he had trouble confirming his principle experimentally. His manner to derive the speed of horizontal outflow implies that a jet not allowed to flow out but at once redirected upward should reach the level of the upper surface. This, however, it failed to do to such an extent as to cause Torricelli to complain in some exasperation that “the experiment itself seems in a certain sense to prove the principle, even though in a certain sense it also seems to destroy it.” Whence this lack of conclusiveness? Torricelli’s test runs revealed as optimal, though still not sufficient, conditions for the experiment that the hole be small, and the vessel both large and always full of water. (The latter requirement may have meant either that one may neglect the level drop as too small to count or that the water must actually be replenished over the duration of the experiment). Conceivably, then, the cause of apparent non-confirmation might have rested in his experiments failing to meet these two conditions.

The issue was taken up again in the late 1660s by Huygens and two colleagues in the Paris Académie. Their search was after second-order variables. In the end Huygens decided these to reside in air resistance; in the fall of the water back onto itself; in the ‘adhesion’ of the water to the vessel walls, and in its mode of outflow. These four variables ought jointly to account for the optimal conditions that Torricelli had specified on basis of his test runs alone, so they now appeared no longer as arbitrary. But then Huygens went on to voice a worry of another kind. The principle from which Torricelli had derived his theorem had none but an experimental foundation (somewhat shaky to boot), rather than being “demonstrated by reason”. That is, Huygens no longer accepted as sufficiently persuasive the analogy with freely falling bodies originally invoked by Torricelli, and required in addition better theoretical proof for it.

In the 1690s another Paris Academician, Pierre Varignon, went a step further. No such better proof would ever be forthcoming, he argued, since the analogy itself ought to be rejected. However plausible at first sight, the resemblance with freely falling, hence, uniformly accelerated bodies is misleading — “since the water is contiguous over its entire length, the water above descends with the same speed as the water below; consequently, there is no acceleration in the vessel at all.” Varignon then managed by means of Leibniz’ recently invented calculus to derive Torricelli’s law from another principle. This principle did not only obey Varignon’s own point of departure in uniform rather than uniformly accelerated motion. It also met Huygens’ requirement that it be established ‘rationally’, i.e., mathematically, rather than just by means of what Varignon evidently took to be the more fallible source of demonstrative knowledge — experiment.

So what we have here is an ongoing balancing act between efforts at mathematical derivation and at experimental confirmation. Galileo himself had been profoundly ambiguous over the role of experimental tests (which he himself pioneered) in the mathematical treatment of natural phenomena. At one occasion he stated that “the knowledge of one single effect acquired through its causes opens the mind to the understanding and certainty of other effects without need of recourse to experiments”; at another, that “we must find and demonstrate conclusions abstracted from the impediments, in order to make use of them in practice under those limitations that experience will teach us.” No doubt he meant both pronouncements sincerely when he made them. In his actual practice, his root conviction of the ultimately mathematical structure of reality drew him much closer to the former position of experiments being really superfluous for other purposes than persuasion, than to the latter, humbler stance of readiness to accept experimental outcomes as guides toward how far one may actually go in abstracting away empirical impediments standing in the way of the mathematical-ideal phenomenon. But more important even than how he himself chose to proceed in face of this central field of tension in the working life of the mathematical scientist was the clarity with which he thus laid it out right from the start. Experimental outcomes may lead one astray insofar as they may represent no more than, indeed, irrelevant ‘impediments’ obscuring some underlying mathematical pattern; mathematical deduction may lead one astray in view of all that is unpredictably messy about the world of natural phenomena. There are no hard and fast rules here, but only room for confidence that interaction of some kind between mathematical abstraction and its experimental testing is to get one farther, with the balance between them to be struck anew in every next case at hand.

But the crux of the matter is that with the onset of ongoing interplay between mathematical theory and experimental practice, a very much novel, truly crucial element had now been introduced into the pursuit of nature-knowledge as such — *the opportunity thus gained to check one's conceptions against natural reality, and this not incidentally but in a way built into one's general procedures*.

Mathematical science in its Alexandrian guise had not required much of a check against reality because it scarcely addressed reality (but only used it for starters). Empiricist modes of nature-knowledge had remained confined as a rule to accurate observations, with possible empirical correction remaining confined to the degree of accuracy. Natural philosophers alone had ventured to address empirical reality in ways transcending that reality, yet outcomes, for all the indubitable certainty claimed for these in view of the first-principles from which they flew, had always remained plausible at best. That is, one could always make a persuasive-sounding case for an assertion and then cling to it regardless. Ways and means to rein in the perennial temptation of the human mind and its wonderful powers of imaginative discovery to move unnoticed from the imaginative to the fanciful did not present themselves. Craft practice as a rule offers feedback of a very basic kind — a bridge holds, or it comes tumbling down; a mixture stop sounds brilliant, or it jangles. But the pursuit of nature-knowledge does not spontaneously offer to reality opportunities for demanding correction in a similarly hard-hitting vein. Systematic experimentation in the framework of some theoretical structure, however, comes near it, and mathematical-experimental science in Galileo's vein is where this was discovered and worked out first. It appeared soon enough that the constraints set by nature are never such as to leave no room for decisions on how in everyday practice to act upon the opportunity for correction which feedback may thus provide. One may (dogmatically) decide not to seek it, or (sometimes foolishly, sometimes prudently so) not for the time being to heed it, or (in a sceptical-positivist vein) to settle for none but the observable in despair of any human ability to go beyond what our senses tell us. Such, and many more, ways out are always open to us. Here, too, there are no hard and fast rules; ‘falsification’ is not, and never can be, a self-regulating process left untouched by the endlessly varied ways of the human mind. But the point is that, with the onset of realist-mathematical science, nature-knowledge did acquire quickly expanding features of *falsifiability*, and has kept refining those features ever since.

With the other strand of recognizably modern science arising, fact-finding experimentalism in roughly Bacon’s vein, things stood mostly different. Here practitioners were perennially on the lookout for what unexpected phenomena nature may prove to have on offer, and that is how nature’s *whimsy* came in prominently to upset experimental research. Take 17^{th} century experiments undertaken in connection with ideas about electrical phenomena. Not only did these lead as a rule to highly ambiguous or even mutually inconsistent outcomes. At times they also gave rise to the puzzling appearance of phenomena unknown and unsuspected (in retrospect, their most productive service). John Heilbron has nicely captured what this might mean:

> The malevolence of inanimate objects is nowhere better instanced than in the phenomena of frictional electricity. Their apparent caprice consistently frustrated the efforts of early theoreticians trying to reduce them to rule. Consider the effect of moisture on the surfaces of insulators and in the air surrounding them. The early electricians realized that contact with water enervated an otherwise vigorous electric, like amber, but they did

> not fully recognize the effect of humidity. On a sultry summer day, or in the presence of a sizable perspiring audience, experiments that had often succeeded might suddenly and inexplicably fail; while the operator himself, sweating at his task, helped to dissipate the charges he intended to collect.

What to do in such cases? Throw up one's hands in utter resignation? That was not the mood that kept a Scientific Revolution going. As a rule, phenomenal whimsy was met by some *ad hoc* theorizing. But how, in the face of damning critiques of dogmatic philosophers who already had an answer ready for every riddle that nature might present, how to ensure a modicum of validity for such theorizing?

Burdened with the task of facing nature's whimsy as best they could, practitioners began in their public utterances to boast of their ability to clear their own minds of bias of various kinds. But in experimental reality we find men like Boyle and Hooke busily exploring in three particular directions checks upon outcomes reached. Aware that results might well turn out to be spurious, they made great efforts at purification of substances and at compensation for apparently consistent measurement errors. They also went out of their way to ensure the presence of witnesses, where possible actual witnesses of proper social standing, and where impossible through something that Shapin & Schaffer have nicely dubbed 'virtual' witnessing. By this they mean the circumstantial reporting of every experimental detail observed that came to fill so much space in the world's first scientific journal, the *Philosophical Transactions* published on behalf of the Royal Society from the early 1660s onward. Finally, fact-finding experimentalists found a certain measure of confirmation of their results in a rather tenuous blending of background worldview, *ad hoc* hypothesis, and experimental outcome. That is, while reticent as a rule about the causes of phenomena (which after all natural philosophers of first-principle reasoning had abused with their limitless fancying), they could rarely resist throwing out some hypothesis or other on how the phenomena observed might come about. These hypotheses originated most often in some background world-view, most often a corpuscularian one (i.e., a conception of the world as made up of particles in incessant motion). If the experimentally observed phenomena, the hypothesis, and the background world-view seemed to match well among themselves, this served as some sort of confirmation that the phenomena were at least not spurious.

All these efforts at holding something fixed amidst manifestations of nature's whimsy may seem to be a far cry from such falsifying instances as their fellow mathematical-experimental scientists were after, as also from modern science in its present-day guise. What all these efforts at checking one's results against natural reality have in common, however, and what has scarcely a precedent in any previous mode of nature-knowledge, is this perennial balancing act between theories, hypotheses, and models on the one hand, and the messiness of the world on the other. Behind the variety of 17th century checks lurks the core issue of how best to balance the neatness of the (most often mathematical, but in any case simplifying) model with the (where possible, experimentally reduced) messiness of the full world meant to be pictured in the model and subsequently needed to check it. As noted, there are no hard and fast rules for such balancing acts, so as to have the feedback gained from the messy world be taken into account *automatically*. But taken into account it can be. How to do that in the best possible manner, is one core issue first explored in course of the Scientific Revolution. When by the late 17th century that Revolution had come to a provisional end, the realm of the mathematical model, on the one hand, and the realm of the phenomenal world and its messiness and experimentally apparent whimsy, on the other, were still largely separate. Barring only Newton's unique case, it was not until the early 19th century that the two realms were made to fuse to such an extent as to become regularly susceptible to overall somewhat smoother and more routinely applicable ways to do the balancing. Still, insofar as scientific method comes down to just the regular application of such balancing acts, prior to the Scientific Revolution one finds none of it, whereas by the 18th century it had already come pretty near standard practice.

Usefulness

Take Ibn Qutaiba, a late 8th century critic of such Greek learning as had early in the century begun with great zest to be translated into Arabic under the auspices of the early Abbasid caliphs. Ibn Qutaiba voiced his complaint in part on behalf of the faith, which he felt to make such foreign learning superfluous. His judgment of it being superfluous rested also upon his conviction that its most prominent components appeared *useless* for everyday life. In his time already, Greek learning had struck so deep roots in Baghdad's intellectual elite that, to Ibn Qutaiba's regret, even lowly placed government clerks found it incumbent upon themselves to master it. At the time his critique went nowhere; not until by mid-10th century the waves of barbarian invasions began, did objections along such lines get a chance to stifle ongoing pursuit of nature-knowledge. But for us the significance of his objections rests in the question of how realistic his assessment of the Greek corpus of nature-knowledge was — was it indeed as devoid of any use for everyday life as Ibn Qutaiba claimed?

This depends a good deal on what 'use' may be taken to mean. From the perspective of caliph al-Mansur, who set up the translation movement in earnest, there was a clear-cut use. For a variety of mostly quite specific reasons, foreign learning could be of great help in legitimizing the regime of the descendants of Mohammed's uncle Abbas, who had come out victorious in a fierce civil war that ousted the vested dynasty of the Ummayads.

Further use resided in the Quran enjoining believers to face Mecca when at prayer elsewhere. To lay out the building of mosques at places far away in strict obedience to this requirement constituted an advanced problem in spherical trigonometry. It took successive generations of mathematicians in Islam civilization some two centuries to solve it. Similarly, early Quranic tradition prescribes five prayers a day, the timing of which has to do with dawn and dusk and shadow length. For exact determination much astronomical expertise is required, which Muslim mathematical scientists gave to it as they did with the *qibla*. Similarly, the Quran has strict rules for the division of legacies, and quite some effort in arithmetic was spent on working these out in practical detail. Finally, a good deal of alchemical research was going on, which, so most people thought, might in due time yield a suitable recipe for quickly converting lead into gold and also perhaps for attaining the elixir of life, that is, immortality.

As this final case already suggests, the use actually made of nature-knowledge at the time fell short of promises made for it. Reasons for this varied from the inherent impossibility of fulfilling the alchemical dream to a conscious refusal by the common run of ulama's to employ those very triumphs of religiously-inspired mathematical science — they went on un-disturbedly to declare sunset when they saw it and to lay out their mosques in the general direction of where they thought Mecca must be roughly located.

We now leave behind this one example of how nature-knowledge of Greek descent fared, so as to treat the issue in more general terms. We consider the two distinct modes of Greek nature-knowledge apart.

Mathematical science of Alexandrian descent proved as a rule of use in two respects only. Its flourishing depended until far into the 17th century upon patronage. If no patron came forward, or if his patronage was withheld due to change of whim or deposition or death, this most often brought ruin to the mathematical scientists involved. Princes have seldom if ever been outspoken about their motives for sustaining the pursuit of nature-knowledge. Among their unspoken reasons, two are likely to have stood out. One was to add lustre to their courts in addition to what other cultural lights, in poetry or sculpture or music, could add to it. The other concerned a feat of which only mathematical scientists were capable — to provide accurate, up-to-date horoscopes (incidentally, Nathan Sivin has illuminatingly observed that the many shrewd political calculators to be found among princes must have seen through the essential emptiness of astrological prediction, and to have used their court astrologers as informal political advisers to circumvent the formal recommendations of their bureaucracies).

Natural philosophy had many more uses in store than mathematical science. This was so in the first place because natural philosophy invariably came in one package with large chunks of ethics and political science — pre-modern philosophy was not only concerned with the external world but also, most often primarily so, with worldly wisdom about how to attain stability in the state and happiness in the individual. Instruction in philosophy was therefore felt to be of use among cultural elites almost anywhere anytime. Still, the role of nature-knowledge in instruction was quite limited — ongoing sustenance in society enabled philosophers to devote part of their time to it, but it was not as such in large demand. Moreover, at times natural philosophy could be felt to trespass boundaries set by the faith, particularly albeit not at all necessarily so when the faith was of the monotheist variety.

So much for the two modes of nature-knowledge of Greek origin and their profoundly intellectualist approaches. The issue of use stands differently for such more empiricist modes of nature-knowledge as held sway of old in China and also came up in Renaissance Europe to accompany the next recovery of the Greek corpus. Here uses were less incidental. This was so in part because certain subjects were broached for the very reason that certain practical problems gave rise to it. This is true, for instance, of earthquakes in China, which provided a fitting occasion for much empirical investigation into conditions for their occurrence with a view, of course, to stand prepared for them in the time of need. It is similarly true of, e.g., Emperor Frederick II von Hohenstaufen's treatise on falconry. But use was also more frequent than with the Greek varieties of nature-knowledge because of a desire, less or more deliberate, less or more outspoken, to make any investigation of natural phenomena, even if undertaken out of sheer curiosity, serve in addition practical ends. This desire was particularly outspoken in Renaissance Europe. Whether Vesalius described with utmost accuracy the human body, or Paracelsus derived mineral cures from his peculiar conception of the constitution of matter, almost always a practical objective was meant to be realized thereby. Again, such objectives could vary all over the spectrum of human activity, be it cultural or political or economic. What they had in common, was their being directed at a relatively low level of abstraction, fairly close to the level of phenomena that was the strong point of those empiricist approaches anyway.

It is in these latter domains that nature-knowledge came closest to contemporary craftsmanship. Or, to be more precise, Renaissance Europe is where this veritable *ideology* of useful nature-knowledge furthered the emergence of several pieces of craftsmanship that, but for an infusion with pieces of nature-knowledge of a non-trivial kind, could not have come into being at all. For 15^{th} and 16^{th} century Europe this concerns three specific subjects: in painting, linear perspective; in warfare, the building of fortresses to keep defendants safe from cannon fire; in the determination of place on Earth, map-making and star-shooting. The products of these successful attempts at creating large-scale interfaces between nature-knowledge and the crafts were substantial, and the rewards were of considerable significance for Europe's welfare and well-being. Yet the content of such knowledge as went into these craft products was not particularly elevated. It was just common geometry that found itself applied in each case. And that is how the subsequent emergence of mathematical science of a revolutionary novel, realist and experimental kind, such as practiced for the first time by Galileo, suggested to him and his disciples at once the possibility of overturning customarily rule-of-thumb craftsmanship from top to bottom, and of replacing it by something that is best called by its modern name 'science-based technology'. Here are two examples of how things fared from there, one concerning gunnery and the other the scientific handling of water streams.

In 1625 one of Galileo's pupils, Benedetto Castelli, was invited to give his learned opinion on how to redirect toward the river Reno in the Po delta a side-stream that had been diverted away from it long before with unfortunate consequences. What Father Castelli did in response, was to apply his master's geometric approach to the problem. This led to the publication, in 1628, of a brief tract containing as its principal fruit what has since become known as 'the law of continuity'. It states that "the cross-sections of the same River discharge equal quantities of water in equal times, even though the cross-sections themselves are unequal". Castelli went on to prove this somewhat counterintuitive theorem by means of an equally geometric argument. Such a way to analyze the issue by means of geometric abstraction was accorded a rather hostile reception by fellow-consultants working in the customary, trial-and-error style, on grounds of practical irrelevance. They had two main points to make. Castelli's model, even if valid in the abstract, ignores numerous, no less influential determinants of flood behaviour. For instance, flow speed appears to vary with height as also with slope. Secondly, such an approach in Galileo-like fashion (soon extended by the master himself when consulted in his turn about a Tuscan river, the Bisenzio) stands too far removed from reality to be of use anyway. Indeed, a good deal of practical expertise had over previous centuries been built up by Renaissance engineers (among their sometimes splendid, intuitive insights those noted down in private by Leonardo stand out in retrospect). And the men who by the early 17th century had taken up their heritage, who were mostly Jesuits accustomed to adorning their Aristotelian philosophy of nature with some quantitative *finesse*, were loath to give up that accumulated expertise.

In following decades controversies along these very lines flared up time and again. Still, behind the scenes an intricate process of mutual *rapprochement* can be identified. Practitioners of realist-mathematical science sought to take more and more determinants of proven practical value up in their mathematical models. For instance, by the 1680s academic experts like Montanari and Guglielmini began to take effects of fluid pressure into account. The other side, while insisting all along that the proper way to proceed rested in a cautious, preferably yet not necessarily quantitative estimation of pertinent elements in carefully observed, everyday reality, nonetheless began to acknowledge that, in principle, processes of mathematical idealization might contribute to a fuller picture of the vagaries of streaming water. Even so there were limits set to all such groping toward compromise. Consider deliberations in a papal committee charged in 1692/3 with adjudicating between the conflicting preferences of the cities of Bologna and Ferrara over (once again) the Reno. The man to represent the Galilean approach, the former superintendent of Bolognese water management and now the city's professor of mathematics, Domenico Guglielmini, had known how to free his model from many a Galilean prejudice. Even so, for all the enhanced sophistication meanwhile on display on the side of mathematical science, the committee's decision making was much more marked by partisan infighting than determined by the still hard-to-assess, respective merits of the two distinct approaches put before it.

So once again we watch here in operation a balancing act between mathematical modelling on the one hand, and low-level manifestations of the messiness of the world, on the other. What approach was to prove more useful to follow, was as yet undecided; once again my point is no more but also no less than that the Scientific Revolution marks the very advent of this particular balancing act. The balancing does not automatically yield a science-based technology, such as this began to come into its own by the early 18th century. But without the basic components required to do the balancing, no such science-based technology would at all have been possible.

Summary and epilogue

So far I have been arguing that the Scientific Revolution was indispensable for turning customary modes of nature-knowledge of overall little reliability and little albeit broad use into science of such a kind as to yield the sort of balancing act from which greatly enhanced, truly modern reliability *and* usefulness could spring.

I have further been arguing that the turnabout was as revolutionary as it was halting. Nothing changed just at one stroke; rather, the Scientific Revolution of the 17th century became a try-out site for reliability and usefulness of an utterly new kind. By its end, the new science had not yet been turned into the major production factor it was to become about a century later; but one decisive turn toward this long drawn-out process had meanwhile been made.

There was one further precondition for all this to become possible — the new science was badly in need of being accepted as an activity worth pursuing. Certainly in the absence yet of clear-cut evidence, protagonists' claims for its being so un-precedentedly reliable and useful were not enough. Needed for sustained acceptance was some measure of apparent conformity with the society's deepest values. This is where such other aspects of the new science come in as rationality of a new kind, relation to world-views of a new kind, and an unprecedented degree of autonomy and visibility. About these, not so directly tangible aspects I shall say a few words in my oral presentation.