

'NO END OF FANSYING'

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Let us talk about the truth value of science. There are, perhaps, four ways to do so. One may affirm the claim to universal validity often made on behalf of science; one may relativize it; one may ignore it, or one may historicize it. Affirmation of the claim that science possesses a unique capacity to inform us about what the natural world really is like used to be the standard view, almost self-evidently adopted by scientists and historians of science alike, until the 1980s. Then a wave of wholesale relativization set in, leading to the so-called science wars. With those exchanges of blasts and counterblasts mercifully behind us, historians of science have tended since to bracket the issue rather than face it, let alone seek to resolve it. It has become customary to situate past events in their local and temporal context while leaving aside any question of how to assess or address the truth value of views discussed and discoveries investigated.

This widely adopted posture has had its consequences. Notably, the idea of a 17<sup>th</sup> century event due to which a variety of previous efforts at coming to grips with natural phenomena began to be turned into modern science has just evaporated. Thus, on the second page of his book *The Scientific Revolution* Steven Shapin dissolved the subject of his book into "a diverse array of cultural practices". On the final page of the same book he called science "certainly the most reliable body of natural knowledge we have got". In between these two pages (that is, in the entire book), there is just nothing that has prepared the reader for understanding how science may have acquired that unique reliability. Here relativization and bracketing go hand in hand. All this means that an issue central to the history of science, one major reason for people outside the profession to take an interest in it, is left wide open in our everyday investigations. Whence the unique reliability of modern science that Shapin rightly speaks of? Where did it come from? Perhaps from some event once upon a time called the Scientific Revolution without more ado?

To be sure, I am far from advocating a return to meanwhile deservedly outdated concepts of the Scientific Revolution. In a lengthy, meanwhile completed book under the working title 'How Modern Science Came Into the World. A Comparative History' I have abandoned the master narrative, and sought to refresh the concept of the Scientific Revolution from the bottom up. A shorter pop version that I wrote in my native language, Dutch, has come out in October last year and has even managed to my utter surprise to sell over 10,000 copies so far. The full, scholarly, English-language parent volume is likely to appear next year.

In regard of the truth value of science I likewise do not advocate a return to the earlier stage of unquestioning affirmation. What interests me instead is to historicize the issue. Rather than just, Shapin-like, positing the unique reliability of modern science as an unexplained and unhistorical given, I want to find out where it came from. How did 17<sup>th</sup> century pioneers themselves seek to clinch their various claims about phenomena in the natural world? In what particular manner or rather manners did Galileo or Descartes or Beeckman or Torricelli or Cassini seek to sail between the Scylla of wholesale skeptical denial of all possible knowledge and the Charybdis of sheer dogmatic assertion? This is a major theme in my book. I have not followed earlier historians and philosophers of science in examining the methodological writings of the

pioneers; instead, I have sought to infer from their actual everyday practice how the pioneers themselves sought to raise their conclusions beyond the arbitrary or the just plausible.

Take, for instance, the partly similar, partly different ways in which Descartes and his elder friend and inspirator Isaac Beeckman attempted to come to grips with phenomena of musical sound. The way they both went about the topic was informed in all respects but one by the ancient natural philosophy of atomism. That is, they set out to explain the world and all phenomena in it the speculative way, and they derived their explanations from certain first principles. The one big difference with ancient atomism was of course that their first principles were not confined to the supposition of subvisible particles of various shapes and sizes, but that they focused on the various motions of these particles under various circumstances. So Beeckman and Descartes were agreed that musical sound is produced, propagated, and also received, by the motions of certain particles. But here already their ways parted. Descartes assumed musical sound to come about by the regular trembling of particles of air brought about by a vibrating string or a pipe, whereas Beeckman assumed a string or a pipe to cut the ambient air into globules and send them away. From these different points of departure both men went on in different ways to explain numerous phenomena connected with musical sound, such as pitch, or how our hearing comes about, but also musical consonance, sympathetic resonance, or beats. For instance, Descartes accounted for our hearing by assuming the tremblings on arrival at the eardrum to pull down from the brain a fine nerve filament, thus releasing certain particles in the brain that actually bring about our sensation of hearing. Beeckman rather accounted for this by assuming certain material spirits, activated by the air globules that ruffle on our eardrum, to run up our hollow auditory nerve.

How did these two pioneers of the speculative natural philosophy of moving particles satisfy themselves and others about their explanations? What criteria for the validity of their explanations can they be seen to adopt?

Obviously, no direct empirical check could possibly be made, even though in the 1650s and 1660s early microscopists like Henry Power entertained hopes for a while that the microscope might reveal to our very eyesight the actual presence of all those subvisible mechanisms only assumed hitherto. In the absence of direct empirical criteria, checks of four kinds appear to be explicitly or implicitly acknowledged. These are (1) foundational certainty; (2) consistency; (3) analogies with the macro-world, and (4) a human faculty with due caution best to be called by its modern name 'physical intuition'.

*Foundational certainty* could take a more or a less elaborate guise. It could take the elementary guise of Beeckman's *a priori* criterion for an acceptable explanation of natural phenomena, to wit, that it be *picturable*, in the sense of it being to Beeckman inconceivable "how something immaterial can move something material." Foundational certainty could in addition take the stricter guise of those securely grounded first-principles *cum* secure deduction that marked from the outset the Cartesian variety of the natural philosophy of particles in motion.

*Consistency* did work as a possible check on the foundational level, but not beyond. At the level of phenomena one might in consistency with one's first-principles change one's mind over explanatory details, for instance in the case of Beeckman he at times correlated pitch with the sizes of the emitted air globule, yet at other times with their speeds.

The demand of picturability, joined to the subvisibility of corpuscles and their movements, left *analogies* with the macro-world as the only empirical check upon any given explanation. For example, Beeckman sustained his globule account of musical sound by means of an analogy taken from the sound one may produce by gently rubbing the rim of a water-filled glass with a moist finger, which goes accompanied at times by regular jumps of water droplets. A certain preference for analogies drawn from the domain of the arts and crafts is further noticeable, especially where the operations of the human body are concerned. A case in point is how Beeckman compared the way the air globules strike our hearing with the ruffles of a military drum, or the way spirits stream up our auditory nerve with how water streams through or alongside water conduits.

One final check concerns '*physical intuition*'. It is more intricate than the others, and its historically responsible handling requires considerable circumspection. In a positive sense it was at work in Beeckman's perceptive account of beating, which betrays an unmistakable sensitivity to vibrational cycles and how they may lag or overlap. Descartes applied it negatively in his curt dismissal of Beeckman's globule conception of sound as just '*ridiculous*'. In fully modern times, '*physical intuition*' is attributed as a rule to seasoned practitioners with a special, piercing gift for grasping how in some given case things actually do, or conversely could not possibly, work. We may not without more ado transplant the notion to earlier times, certainly not to the first half of the 17<sup>th</sup> century. Still, Beeckman's and Descartes' defining move of focusing on the particles' motions made it possible for explanatory mechanisms to get so intricate and so detailed as to bring this human faculty of '*physical intuition*' into play for the first time on a more than incidental scale. We must take care to differentiate here, to be sure. On the one hand, there is our modern appreciation, due to which it is hard to avoid being impressed with Beeckman's account of beating or with Descartes' account of wave formation through successive rarefactions and condensations. On the other hand, there is the kind of implicit intuition that at the time found expression, more often than not, in such curt qualifications of somebody else's favorite mechanisms of particles in motion as '*ridiculous*', or '*just a fantasy*', out of an apparent awareness that certain otherwise well-conceivable mechanisms of particles in motion just could not possibly work. Take some phenomenon for which two rival explanations were put forward, each well in line with the three other checks. That is, each was then dutifully conceived in terms of movements of particles alone; also internally consistent at least in outline, and, finally, well-provided with fitting analogies drawn from the macro-world. In such a case (as with Beeckman's and Descartes' rival explanations of musical sound) '*physical intuition*' could and did act as a fourth check, independent of the others. On making one's first acquaintance with all those incessant particle-in-motion explanations sprinkled over those bulky treatises that kept being produced all over the 17<sup>th</sup> century, one may well get the impression that there was no check on possible absurdity at all; yet between them practitioners exercised some.

It may be the vast contrast between the allegedly undubitable certainty of speculative particle thinking and the radical arbitrariness that its explanations so often displayed in practice, that caused Isaac Newton, in a 1679 letter to Boyle, to speak of "*natural philosophy, where there is no end of fansying*". Where there any remedies? Of course, additional criteria might be adopted from elsewhere, that is, from outside the realm of sheer speculative natural philosophy. One might turn to mathematical science as practiced by Galileo and his pupils and disciples. Here practitioners were engaged in an ongoing balancing act between mathematical

rule and hoped-for experimental confirmation, in a field of tension that Galileo himself defined. At one place in the *Dialogo* 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 place in the same book he rather argued 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.

Others made other decisions. Here is an instructive case in point, for which I have found the data in work by Michel Blay. It concerns a theorem that Torricelli derived 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. He 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.” This principle of equal ‘impetus’ is in the Galilean context to be understood as a capacity, acquired in falling down, to return to its previous height. Torricelli inferred from this principle his conclusion 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 derivation of 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. But this 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. And that is where Torricelli left the issue pending.

It was taken up again in the late 1660s by Huygens. His search was after second-order variables. These he decided in the end 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. Taken together, these four hidden variables should account for the optimal conditions specified by Torricelli. But 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 (and a somewhat shaky one to boot), rather than being “demonstrated by reason”. In other words, Huygens no longer accepted as sufficiently persuasive the analogy with freely falling bodies originally invoked by Torricelli; in addition, he required 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 is false. 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 satisfied Huygens’ requirement of being 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.

At the period we are addressing in the present session nearly all disciplinary boundaries were in flux – in the literal sense, the theme of the present conference hardly applies. Instead, there were approaches, attempted ways of knowing, investigation practices that were all alike in revolutionary ferment yet in mutually quite different ways. Beside atomism now enriched with this insistence on particle motion, and beside mathematical science now experimentally directed toward the real world, there was also the revolutionary enrichment of accurate observation and description with experimentation of the fact-finding kind. In this third, almost equally revolutionary current of modern science in the making, again other checks upon fancy were sought for. This concerns what Thomas Kuhn has dubbed the Baconian sciences, where experimentation was not so much directed toward validation of mathematical rules as toward the exploration of how nature would behave under artificial conditions. In this domain there were two principal impediments standing in the way of arriving at truthful conclusions. One concerns the question of how to convince others of the veracity of one’s observations. Here techniques of ‘virtual witnessing’ were gradually worked out, as Shapin & Schaffer have argued in innovative ways familiar to all of us. The other impediment, on which John Heilbron has focused in his study of electrical research, concerns nature’s whimsy – the utterly unexpected and mysterious behavior of nature that the patient observer may at times be faced with. This happened for instance when changed weather conditions or the softer or more leathery constitution of the experimenter’s own hands might enigmatically cause sudden failure in the collection of electric charges collected with apparent ease days or even hours before.

As my time is almost up, I just jump to my conclusion. The exploration of viable ways and means to make assertions about facts and properties of natural phenomena stick, and to rein in fancy and arbitrariness, naturally took different forms in different modes of revolutionary science-in-the-making.

In *realist-mathematical science* a delicate and variously weighted interplay emerged between, on the one hand, mathematical relation (functional dependence expressed as equality or proportion) and, on the other, experimental outcomes meant to check it. The interplay yielded valuable pointers toward possible correction and/or falsification, in that for the first time in history it provided systematic feedback against the very realities of nature.

Insofar as the natural philosophy of particles in incessant motion kept being handled in the dogmatic way of the Greeks, four checks served to some small extent as bridles upon fancy unlimited – foundational certainty, consistency, analogy with the macro-world, and ‘physical intuition’.

In *fact-finding experimental science*, finally, nature’s whimsy held the upper hand and practitioners found themselves burdened with the task of facing it as best they could. Here feasible checks upon outcomes proved to reside in little beyond (1) systematic efforts at purification of substances and at compensation for apparently consistent measurement errors; (2) ways and means to ensure actual and/or ‘virtual’ witnessing,

and (3) such coherence as was yielded by a rather tenuous blend of background worldview, explanatory theory, and experimental outcome.

Behind this variety of 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. There are no hard and fast rules for such balancing acts, as if the feedback gained from the messy world could be taken into account just automatically, as a routine matter. *Yet taken into account it can be.* How to do that in the best possible way, is one core issue first explored over the Scientific Revolution. When that Revolution was over, 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 Newton's unique case, it was not until the early 19<sup>th</sup> century that the two realms were made to fuse to such an extent as to become regularly susceptible to overall somewhat smoother, a little more routinely applicable ways to do the balancing.