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# CHRISTIAAN HUYGENS AND THE SCIENTIFIC REVOLUTION

(opening day of conference ‘Titan – From Discovery to Encounter’; Noordwijk, 13 April 2004)

Some three decades before Christiaan Huygens was born, a revolution occurred in modes of pursuit of knowledge of nature. It was not a revolution in the sense of an entirely fresh start being made (as if that ever happens in history). Rather, a variety of approaches to the phenomena of nature that had been there for centuries were now *transformed*, albeit in so radical and also sudden a fashion as fully to deserve the epithet ‘revolutionary’. After all, out of the upheaval came nothing less than “recognizably modern science”. For taking part in the upheaval, himself, Christiaan was born too late. When he came of age in the mid-1640s, the onset of the Scientific Revolution was already over, with the works that contain the basics published and (in good part) ready to hand in his father’s well-stocked, up-to-date library. These works to inaugurate (yet by no means to complete) the Scientific Revolution of the 17th century — which were they?

In the *first* place, books in mathematical science that betokened a radical departure from the very abstract manner in which, from the times of Archimedes and Ptolemy onward, and with the city of Alexandria for center, certain natural phenomena had been handled the geometric way. These and other ancient Greek geometers, but also such creative disciples in later civilizations to adopt the Greek legacy as Thabit ibn-Qurrah in Islam civilization or Francesco Maurolyco or Simon Stevin in postmedieval Europe, had been concerned to erect upon a very slender empirical basis an impressive building of abstract geometric derivation, confined to a very limited number of subjects — those to do with planetary trajectories, light rays, musical intervals, and equilibrium states of solids and in fluids, no more. The transformation in decades around 1600 to be accomplished principally by Kepler and Galileo came down, not only to a widening of subjects found susceptible to mathematical treatment, but above all to a newly *realist* way of treating them. For instance, Galileo posited his rule of uniform acceleration for the actual phenomenon of bodies falling, while granting the rule to apply, not to bodies as we watch them falling but to bodies imagined to fall under ideal circumstances, with all impediments removed, and in ways susceptible to experimental checking. For us, nowadays, this has become so standard an approach as to require quite some historical imagination to realize how paradoxical it once was — to help us recapture what, in what is now trivial, was once paradoxical is what you have historians for, that now more than ever endangered species, at least where the prospects for the history of *science* as cultivated in our own country are concerned. With regard, then, to this revolutionary transformation of ‘Alexandria’ into what I like to call ‘Alexandria-plus’, I add just two more things. One is that how bodies *move* began to be handled in quite novel, counterintuitive ways exemplified in Galileo’s principles of inertia and of relativity; as such, motion was turned into the fundamental unit of analysis of nature’s diverse phenomena. My other remark is that, even with the range of subjects handled the mathematical way expanding ever since, the approach has never added up to a comprehensive world-view. Nor was it aimed to do that; to construct a comprehensive view of the world had of old been the business of philosophers.

This remark brings us to the *second* revolutionary transformation to occur in early decades of the 17th century. This is the one in which ‘Athens’ (center in ancient times of no less than four systems of natural philosophy) was transformed into ‘Athens-plus’. However much the natural philosophies of Plato, Aristotle, the Stoa, and the atomists differed *qua* content, at bottom each displayed the same knowledge-structure. A few first-principles are held to determine how our world runs; an assortment of at least superficially fitting evidence is adduced both to illustrate and to sustain the empirical way the capacity of those principles to explain the world at large. In respect of its knowledge-structure, then, the philosophy of nature Descartes presented as brand-new to the learned world in his *Principia philosophiae* of 1644 was no different at all. The indubitable certainty he claimed for his first-principles, while grounded in a partly novel way involving his famous *cogito*, was still the certainty claimed for each prior philosophy by *its* adherents. Also, with Descartes empirical evidence took quite the same guise of loosely observed or hearsay facts pliable enough to fit his *a priori* principles or at least clear & distinct deductions therefrom. What *was* different with Descartes' variety of natural philosophy, as also with the similar ones put forward at the time by Beeckman and Gassendi, rather concerned its content. Whereas Lucretius and earlier ancient atomists had focused primarily on the various shapes and sizes of the particles postulated by them, in Descartes' doctrine emphasis was rather on the variety of movements these particles execute, and, above all, on the lawlike nature of their motions. To bring out the distinctive difference, I adorn the doctrine of a world run by the lawlike motions of particles with the ugly yet accurate label 'kinetic corpuscularianism'. As a comprehensive philosophy of nature out to grasp at once stroke the totality of things, and set up therefore in ways quite distinct from the knowledge-structure of mathematical science, it nonetheless held two features in common with Galileo's realist-mathematical science — a similar idea of motion as relative and inertial, and a shared conception of our world as different, really, from how our senses present it to us.

For present purposes I must ignore a *third* revolutionary transformation to take place at almost the same time — one in which a mode of nature-knowledge aimed at painstakingly accurate, also practice-oriented description condensed into fact-finding experimentation, in ways advocated by Francis Bacon and exemplified by Gilbert, van Helmont, and Harvey.

Our first acquaintance with these three revolutionary transformations jointly to mark the onset of the Scientific Revolution thus summarily completed, I begin by noting about the immediate sequel that the three partly novel modes of pursuit of nature-knowledge thus arisen kept being cultivated over the remainder of the century by and large in conformity to the programs laid down by the pioneers. Mathematical-realist science kept being pursued the Galilean way, e.g., by Torricelli and by Huygens; the natural philosophy of kinetic corpuscularianism the Cartesian way, e.g., by Rohault and hosts of novelty-inclined university professors; fact-finding experimentalism roughly the Baconian way, e.g., by van Leeuwenhoek and by Huygens.

That is to say, part of Christiaan Huygens' scientific achievement is best characterized as following in the footsteps of Galileo and (to a very much smaller extent, to be sure) of Bacon. It is certainly true that Huygens was far too great a scientist not to stamp almost everything he touched with his own brilliance; still, follow-up to the pioneers is what, in good part, marks an accomplishment deservedly characterized by Henk Bos therefore as tinged with 'conservatism' in the sense of surely virtuoso usage made of basic equipment already there.

In good part indeed, yet not wholly so. In his own, quiet way Huygens brought about a revolutionary transformation of his own; one that on completion seemed to him so self-evident as to omit to mark it as the historic breakthrough it can in retrospect be recognized to be. This *fourth* revolutionary transformation to mark the

Scientific Revolution, started in 1652 — about a decade earlier than a partially similar *fifth*, peculiarly British one, which Boyle and Hooke started and which I leave out of account here. To grasp Huygens' revolution, let us seek to recapture the make-up of his (in 1652) 23-years' old mind. Some eight years before, a youthful reading of Descartes' freshly published manifesto of kinetic corpuscularianism, *Principia philosophiae*, had overwhelmed him; had given him the very idea intended by Descartes, that the universe has become transparent to human reason for the first time and that no riddle remains. A few years later, as a mathematical prodigy under Frans van Schooten's tutelage he began to occupy himself, the way generations of mathematical scientists before him had, with elaboration and enrichment of well-known Alexandrian themes, like the floating of certain complicated three-dimensional figures. But such unworldly business quickly bored him, and by 1652 he had already transferred for good his allegiance from 'Alexandria' to Galileo's much more reality-bound, 'Alexandria-plus' mode of mathematical science. So in one department of his mind Huygens was now a natural philosopher of Cartesian persuasion; in another, a mathematical scientist of Galilean leanings. *Such a split mind was not at all uncommon*. Descartes himself, as earlier other men (Ibn Sina, or Thomas Harriot), had been natural philosophers in one mental department, Alexandrian mathematical scientists in another, without (and this is a crucial point) experiencing any desire to bridge the vast conceptual gap between them, let alone construct out of them some larger whole. The three modes of nature-knowledge transformed around 1600 had always been cultivated in splendid isolation from one another; in that particular regard their respective transformations had changed nothing at all. Prefigured in mostly superficial ways by Mersenne and by Kircher about a decade earlier, the first man *truly* to effect a creative merger of a kind between natural philosophy and mathematical science was ... well, you guess who. Here is how Huygens did it, with Descartes' account of *rebound* for his point of departure.

For an understanding of Huygens' difficulties with Descartes' account of impact, we must first note that its centrality in Descartes' world flew from his identification of matter and extension. Since wherever a particle turns it encounters other particles, any action in the Cartesian world whatsoever takes place solely through the particles' perennial crowding upon one another, i.e., through rebound.

In *Principia philosophiae* Descartes examined rebound for the sole case of perfectly hard bodies in central collision. He brought four variables to bear on his analysis — size; surface touched; speed, and 'determination' (a measure for direction). The principle to govern rebound is that the quantity of motion does not change before and after impact; that is, for the pair of bodies in impact the product of size and speed remains the same. This is presented as sufficient to yield nine rules under three distinct conditions — ($1^{st}$) the two bodies move in opposite directions; ($2^{nd}$) one body moves, the other is at rest; ($3^{rd}$) one body overtakes the other. A recent student of the — to the modern reader, quite baffling — rules that ensue, Peter McLaughlin, has unraveled the reasoning going on behind the scenes by working out the tacit analogy here adopted by Descartes. As earlier to Galileo when out to analyze falling bodies, no other analogy of motion presented itself to Descartes than the law of the lever. But Descartes' handling of analogies of motion was quite different — not mathematical in an idealized-real sense, but quantitative in a sense meant to be pertinent to the micro-world of corpuscles in incessant motion. Consequently, Descartes conceptualized impact in terms of a contest between opposite forces. These contests are laden with discontinuities; as with a balance, “the difference between equilibrium and disequilibrium may be almost infinitesimal, but the difference in outcome between winning and losing a conflict can be very great”.

Considered from within the frame of Descartes’ philosophy of nature there is nothing odd, incoherent, or arbitrary about the rules he came up with. Nor, inside that frame, is one entitled to object that a casual look thrown upon the billiard table suffices to see how wrong almost all his rules are. Descartes himself granted at once that, since there is no perfect hardness in the world, and since two bodies never collide in isolation from other bodies that surround them, “it often happens that experience may seem at first to conflict with the rules I have just explicated.”

Considered, however, from inside another frame, that of Galilean mathematical science, difficulties with these rules readily present themselves. And it was in reconsidering them thus that, in 1652, to his surprise and dismay, Christiaan Huygens famously found them all (with the sole exception of rule nº 1) to be dead wrong. It was a momentous discovery. On the one hand, from about the 15th year of his life onward he had been a faithful Cartesian. On the other hand, along the Alexandrian path taken by him as van Schooten’s student he had within a few years of flexing his mathematical muscles come to abandon his efforts at doing Archimedes one better, in favor of the kind of mathematical science of the real world he found embodied in Galileo’s work. But from that newly adopted, Galilean point of view, so he discovered in 1652, an issue at the heart of Descartes’ philosophy of nature looks thoroughly mishandled.

Huygens was not much given to self-reflection, and what he found so wrong about Descartes’ rules of impact is to be surmised from features of his immediately ensuing effort to derive better ones.

Little more than a guess is that (just as his pupil Leibniz was later to set forth with philosophical rigor) he may have come to feel ill at ease with those sudden jumps the rules display — as noted, the tiniest of changes in size or speed may alter the entire outcome. Kepler, the mathematical realist, had found occasion to reject a certain medieval account of vision as soon as he came to realize that it hinged on the — to Kepler — absurdity of light rays failing to contribute to vision if deviating but the smallest amount from perpendicular incidence, and so may have Huygens in the (at least in this respect) similar case of Cartesian impact.

More easily reconstructible is the significance, for Huygens, of the empirical aspect of Descartes’ rules of impact. For in what sense can one say that these rules are ‘wrong’? In Descartes’ conception the warrant for their correctness rested, as always with him, in the clarity and distinctness of the chain of reasoning through which they are connected to first-principles themselves erected upon the rock-bottom certainty of the *cogito*. That they look odd in the macro-world is entirely inconsequential — there are no perfectly hard bodies in the macro-world, so no empirical test is possible. What, then, enabled Huygens to take their apparent failure to obey the conduct of billiard balls as indicative, nonetheless, of their being false? In early drafts of his unpublished treatise ‘De Motu Corporum Ex Percussione’ (‘On the movement of bodies in impact’; first completed in 1656) Huygens, in an educational effort for the benefit of his Cartesian readers, dwelt at some length on the proper way (which of course is the Galilean way) of looking at empirical checks. True, there are no perfectly hard bodies in the macro-world, but there are bodies at least *approximating* perfect hardness. What one may ask of rules of impact is that they approximate the actual conduct of macro-world bodies, and also that they do so the more closely, the harder these bodies are. If, rather than approximating actual conduct on the billiard table, they go flat against it (as with Descartes' rule nº 4 in particularly glaring fashion, but with all the others except nº 1 as well), this may be taken as a sound sign of their being false.

Quite unambiguously the case, finally, is that Huygens came to realize the incompatibility of Descartes' rules of impact with a basic principle of motion first enunciated by Galileo — the relativity of motion and rest as illustrated captivatingly in Galileo's *Dialogo* by means of the, on board of a ship on its way from Venice to Aleppo, unobservable motion of its cargo. Since Descartes upheld the relativity of motion and rest, too, it was even possible for Huygens to regard Descartes' failure to bring it to bear on his analysis of impact as a sign of inner inconsistency of the latter's philosophy of nature at a critical point. The inconsistency is most glaring with Descartes' rules 4 and 5. If one takes the relativity of motion and rest seriously, the analysis of what happens when a small body hits a bigger one at rest ought to be symmetrical with what happens when a big body hits a smaller one at rest, since the two cases are identical but for a shift in the frame of reference; instead, with Descartes the two cases come out quite different.

Expedient shifting of frames of reference, then, so as to make each case identical to one already treated, is how Huygens went about the derivation of better rules, for which he, too, thought a boat to come in handy.

We leave aside the familiar story of how virtuoso usage of a boat imagined to coast smoothly down a canal enabled him to derive the rules of central, perfectly elastic collision as we know them still, as also his solution to the more difficult task of (under the self-imposed exclusion of dynamic considerations) finding proper axioms from which to derive his new rules. Significant for our purposes is rather this. As argued by Christopher Burch, in the treatise to come out of the entire effort Huygens can be seen to take care to present his argument such as to lure a Cartesian reader into nodding agreement as far into the treatise as it was humanly possible to get him, so as to entice him to go along the rest of the way as well. For the author himself the didactic device served as a marker that his departure from certain Cartesian tenets involved less than full rupture. Rupture of a kind it certainly was. To regard the matter as settled by means of local repair (the way Aristotelian philosophers used on occasion to plug apparent, empirical leaks in their master's system) clearly would not do. A philosophy of nature more emphatically than any earlier one presented to the world as indubitably certain and as fully consistent both with itself and with phenomena at large had now, in the light of Galilean mathematical science, proven to be both inconsistent and empirically wanting in one of its key points. From the mid-1650s onward, kinetic corpuscularianism ceased to command Huygens' allegiance *insofar as conceived as a system of natural philosophy*.

Precisely this is what was so revolutionary about his move. Never before had a system of philosophy of nature been treated the way it was now being treated by Huygens. It is certainly true that natural philosophy had been treated before in other ways than the customary mode of dogmatic adherence. It had often been treated *syncretically*, that is, as a source of individual tenets freely blended with tenets selected from rival philosophies for the concoction of some inevitably inconsistent brew, the way Huygens' Jesuit contemporaries were busily concocting a syncretic brew out of Aristotelian and kinetic-corpuscularian views. It had sometimes been treated *opportunistically*, that is, as a source of noncommittal stopgap arguments to help one out wherever mathematical reasoning proper appeared to come to an end, as with Ptolemy or Copernicus or Galileo. It had on occasion been treated as one resource among others for some vaguer, most often magic-tinged world-view, as with Paracelsus. But never before had natural philosophy been treated as *hypothetical*, that is, with its first-principles adopted wholesale yet employed solely as a resource for conceiving of some specific mechanism for some specific phenomenon under current treatment.

To bring home the point I am seeking to make here, let us focus for a moment on Blaise Pascal. Far too honest intellectually to settle for syncretism or even opportunism, the revulsion Pascal felt for Descartes' corpuscular fancies masked as strict deductions of reason left him no other choice but wholesale rejection; in his *Pensées* he famously wrote about "Descartes. — One must say at large: 'These things happen through their shapes and their motions'; for that is true. But to say which ones, and to construct the machinery — that is ridiculous, because it is useless and uncertain and awkward." Sharing with Pascal a stance in realist-mathematical science, Huygens nonetheless saw what his elder colleague failed to see, which was that, indeed, 'the' machinery cannot be constructed, but that, if set in a frame of mathematical science and of a hypothetical rather than dogmatic handling of natural-philosophical first-principles, individual mechanisms may on occasion be hit upon, be it in impact or with other individual subjects where the Galilean and the Cartesian legacies could be seen to stand athwart each other. It has been recognized by historians, notably by Richard S. Westfall, that an important portion of Huygens' scientific accomplishment rests in the creative, often successful resolution of a profound tension running between the Galilean and the Cartesian approaches. It may be observed in addition that, in so doing, Huygens creatively broke with a knowledge-structure that had meanwhile dominated thinking about the constitution of the natural world for twenty centuries. From the mid-1650s onward, Huygens was to operate in a manner first figured out over the half-decade (1652-1656) spent on and off on the problem of impact — to use kinetic corpuscularianism heuristically, as a possible source of specific problems or of specific hypotheses, in those cases, examined in depth by Joella Yoder, in particular, where sheer mathematical treatment appeared to him not to suffice on its own. This momentous move gave him the freedom to invoke Gassendi-like atoms in the void or rather some Descartes-like whirlpool mechanism wherever any one of such conceptions might suit him best; it also enabled him, in the 1670s, in ways recently clarified by Fokko Jan Dijksterhuis to hit upon the optical principle of wave propagation that still bears his name. Still, however revolutionary such a transformation of the standard way to handle a philosophy of nature was, it has always been recognized that at no time throughout his life was Huygens to abandon the broad conception of matter in motion as the only category suitable for arriving at some causal understanding of the phenomena of nature. It remained to his younger colleague, Isaac Newton, in the 1660s to join Huygens in the revolutionary transformation just outlined but then, in the 1680s, to break through those remaining limitations and, in so doing, bring about an even larger revolution.

So much for Huygens as an unwitting revolutionary. Now, by way of a bridge to the next item on the conference program, a word about Huygens and music. Son of the one-but-greatest composer the Dutch nation has ever produced, he was raised in an environment saturated with music. His contributions to musical theory are not epochal yet on a level with his scientific achievement as a whole. His construction, on the basis of his calculations, of a moveable keyboard to facilitate transposition on the harpsichord was admired in Paris (if we may believe his rare boasting) “by great masters”. His playing of music involved song, the viola da gamba, the lute, the harpsichord, and the soprano recorder, on a level of performance we know nothing about. His enjoyment of music, while directed more toward the charming than the profound, was varied, particularly so in his Paris years, but also at home when listening to father Constantijn’s moving *Pathodia sacra et profana occupati* (‘Affective songs, both sacred and profane, by a busy man’) or late in life when listening to work by his friend, the organist and composer Quirijn van Blankenburg. The letters Christiaan wrote back home from Paris have allowed the music historian, Rudolf Rasch, to reconstruct which composers Christiaan frequented in person and also a number of works at the performance of which he was actually present. It has even proved possible to put together a music program filled for the occasion with none but works listened to at one or another time in his life by Christiaan Huygens, or at least composed by musicians of his acquaintance. On behalf of Stephen Stubbs and Harry van der Kamp and Maxine Eilander, please imagine yourself now relocated in mid-17th century Paris or Voorburg, lean back at your ease, and enjoy.

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# CHRISTIAAN HUYGENS AND THE SCIENTIFIC REVOLUTION

(opening day of conference 'Titan – From Discovery to Encounter'; Noordwijk, 13 April 2004)

In my talk I address in quick succession the following topics.

- Three revolutionary transformations around 1600:
  (1) from 'Alexandria' (Archimedes, Ptolemy *et al.*; Thabit ibn Qurrah *et al.*; Stevin *et al.*) to 'Alexandria-plus' (Kepler, Galileo)
  (2) from 'Athens' (Plato, Aristotle, Stoa, atomism) to 'Athens-plus' (Descartes, Beeckman, Gassendi)
  (3) [condensation of practice-oriented, accurate description (Vesalius, Paracelsus *et al.*) into fact-finding experimentation (Bacon *et al.*)]
- Each transformed mode of nature-knowledge continued over 17th century, with Huygens prominent in (1) [and in some measure in (3), too]
- A fourth revolutionary transformation:
  ◊ make-up of Huygens' mind by 1652
  ◊ customary compartmentalization of modes of nature-knowledge
  ◊ Descartes on rebound
  ◊ exactly what was wrong?
  ◊ rupture, up to a point
- Varieties of natural philosophy: dogmatic, syncretic, opportunist, *hypothetical*
- Enlightening contrast with Pascal
- A fundamental tension resolved
- Enter (in the final sentence) Isaac Newton
- A musical postlude

[NB To avoid excessive complexity, I have left out a range of structural conditions jointly present in mid-17th-century Europe that help *explain* what, over and above Huygens' extraordinary perspicacity, enabled him to make the revolutionary move here identified.]

The passages I quote literally are taken from:

* Stillman Drake, *Galileo: Pioneer Scientist*. Toronto: University of Toronto Press, 1990; p. 98.
* Peter McLaughlin, 'Force, Determination and Impact'. In: S. Gaukroger, J. Schuster, J. Sutton (eds.), *Descartes' Natural Philosophy*. London: Routledge, 2000; p. 81-112; p. 101.
* René Descartes, *Oeuvres* **9**, p. 93.
* Blaise Pascal, *Pensées*, n° *79* in Brunschvicg's arrangement.
* Christiaan Huygens, *Oeuvres Complètes* **10**, p. 172.

My argument as a whole derives from a book I expect soon to complete, provisionally entitled 'How Modern Science Came Into the World. Six Revolutionary Transformations And The Dynamics Behind Them'. For individual issues raised in my talk I have benefited in particular from:

◊ Henk J.M. Bos, lemma 'Christiaan Huygens' in *Dictionary of Scientific Biography* **6**.
◊ Peter McLaughlin, 'Force, Determination and Impact'. In: S. Gaukroger, J. Schuster, J. Sutton (eds.), *Descartes' Natural Philosophy*. London: Routledge, 2000; p. 81-112.
◊ Christopher B. Burch, 'Christiaan Huygens: The Development of a Scientific Research Program in the Foundations of Mechanics' (unpublished doctoral dissertation, University of Pittsburgh, 1981).
◊ Richard S. Westfall, *Force in Newton's Physics. The Science of Dynamics in the Seventeenth Century*. London: MacDonald, 1971.
◊ Joella G. Yoder, *Unrolling Time. Christiaan Huygens and the Mathematization of Nature*. Cambridge UP, 1988.
◊ Fokko Jan Dijksterhuis, 'Once Snell Breaks Down. From Geometrical to Physical Optics in the Seventeenth Century'. *Annals of Science* **61**, 2004, p. 165-185.
◊ H. Floris Cohen, *Quantifying Music. The Science of Music at the First Stage of the Scientific Revolution, 1580-1650*. Dordrecht: Reidel, 1984.
◊ Rudolf Rasch (ed., transl., introd.), *Christiaan Huygens,* Le cycle harmonique *(Rotterdam 1691);* Novus cyclus harmonicus *(Leiden 1724), with Dutch and English translations*. Utrecht: Diapason Press, 1986.

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