Diálogos 77 (2001), pp. 7–24.

THE STRUCTURE OF THE COPERNICAN REVOLUTION[1]

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In the Preface to the 2nd ed. of his *Critique of Pure Reason* KANT compares his transcendental turn

> with the first thought of Copernicus, who, not being able to get on in the explanation of the movements of the heavenly bodies, as long as he assumed that all the stars turned round the spectator, tried, whether he could not succeed better, by assuming the spectator to be turning round, and the stars to be at rest. (BXVI, transl. M. Müller)

Often this passage has been understood as if Kant interprets his contribution to epistemology (or even to philosophy in general) as something like a *Copernican Revolution*. However, this is a widespread misunderstanding – as shown, *con gusto*, by I. Bernard COHEN.[2]

Nevertheless, the quoted passage tells us how highly Kant valued the new perspective created by the Copernican idea of a moving earth. It does not come with surprise therefore that it enforced the tendency to regard the development initiated by Copernicus as a *revolution*.[3] Whether this is justified, depends, of course, on the concept of (scientific) revolution one wishes to apply, and also on exactly which series of events it is one wishes the concept to apply to. For the time being, I would like to leave open both questions. Instead I am going to start with a rather coarse *description* of the development in question (part I) and continue by successively elaborating on this description (part II). This procedure should open up perspectives for an *explanation* of the process, which allows for a distinctive use of the label *revolution* (part III). Also, I'll shortly discuss why Thomas KUHN and I. B. Cohen have en-

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[1] A previous version of this paper was given at UPR in March 1997.

[2] Cohen [85], ch. 15: "Kant's Alleged Copernican Revolution" (237 ff).

[3] cf. *ibid.*

dorsed or criticized, respectively, the talk of a Copernican Revolution (part IV). Finally, I'll justify this labeling with respect to certain traits of the development in question - traits which may well be used to form a new conception of scientific revolutions in general (part V).[4]

I.

A first plain rendering of the process envisaged could read:
COPERNICUS claimed that

(C) The earth is a planet and, like all other planets, circles around the sun.

Then KEPLER, specifying this view, has shown that

(K1) The planets move on elliptical paths around the sun, which is located at one of the foci of the respective ellipse.

(K2) Each planet's velocity varies according to the 'area law'.[5]

(K3) Each planet exhibits the same proportion of the square of its period, T, and the cube of its mean distance from the sun, r, i.e., all have the same ratio T^2/r^3.

Finally, NEWTON explained Kepler's laws by his law of gravitation:

(N) Two bodies, having masses M and m, respectively, and being at a distance r from each other attract each other by a force proportional to $(M \cdot m)/r^2$.

There are, of course, a couple of qualifications to be made. To mention just a few of the more important ones:

If, in (C), you take 'planet' in the modern sense, (C) comes near to a tautology. More appropriate, though more clumsy, would be the formulation:

(C') Earth and the (then known) "starlike" planets (Mercury, Venus, Mars, Jupiter, and Saturn) circle around the (stationary)[6] sun.

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[4] For Kuhn's account of the Copernican Revolution cf. his [57], for his general concept of scientific revolutions mainly [62], but also [81]. For Cohen's concept of 'revolution in science' cf. his [85], with regard to the Copernican Revolution esp. ch. 7 (105 ff).

[5] The planet sweeps out the same area per unit time all over its orbit.

For Copernicus this does *not* mean that the six celestial bodies just mentioned move in plain circles around the sun as center (or are carried around by such orbits or spheres); rather, only their 'deferents' center the sun, and even these only approximately.[7] At least, the 'epicycles', carried around by the deferents, are small enough not to include the sun.[8] Hence, the expression 'circle' in (C) and (C') is to be understood in the sense that the planet's path roughly centers the sun. But it is important to keep in mind that such a phrasing is anachronistic, because the concept of an orbit as a certain trajectory in space seems to have emerged only in Kepler.[9]

Of the three laws (K1-3) KEPLER first found the 'area law' (K2). This law, then, must be independent of the special shape of an ellipse. In fact, in an early stage of his investigations of Mars's orbit Kepler only knew that it couldn't be a circle; i.e., the curvature must vary along the orbit. Evidently, the orbit was most sharply bent when Mars is either nearest to or furthest away from the sun. Furthermore, Kepler assumed that not only the planet's *angular* velocity varies with the distance from the sun, r, but also its velocity along the orbit, v, in fact, he assumed that v is inversely proportional to r:

$$v \sim 1/r.$$

However, since he did not yet know the orbit, he could not yet calculate the distance r. In this situation he looked for an – admittedly fictitious – orbit, which he *could* calculate and which, if Mars *would* follow this path, would show the same pattern of variation of velocity. Considerations of this kind led Kepler to the area law. *Post facto* we know that not only gravitation but every central force (i.e. a force always directed to a point fixed in space) causes a body to move in a way according to

[6] Since, in Copernicus's system, the sun is *not* in the "center of the world" (cf. n. 7), this system should be called *heliostatic* rather than *heliocentric*.

[7] The deferents of all planets refer to the center of Earth's orbit. In this sense this center is regarded as the *center of the world*. It is carried around on an epicycle the center of which revolves on a sun-centered circle (cf. Kuhn [57], ch. 5, p. 170). This movable center of the planetary system is also called the *mean sun* (cf. Stephenson [87], 11). The difference of "mean" and "true" sun turned out later to be an obstacle for planetary *dynamics*. Accordingly, Kepler who identified Sun itself with the *source* of planetary motion, would construct the orbits of the planets with respect to the *true sun*.

[8] The epicycles can be made responsible for the "thickness" of the spheres.

[9] cf. *Astronomia Nova* I.1, Fig. 2.

the area law (and *vice versa*); i.e. the area law's scope is much wider than Kepler could imagine. For the special case of a force obeying an inverse square law like gravitation the resulting path is a conic section, i.e. an ellipse, parabola, or hyperbola. Since of these only the ellipse is a closed curve the periodic motions of the planets must exhibit elliptical paths.

My coarse reconstruction of the development from Copernicus via Kepler to Newton, as given above, basically consists only in a list of propositions, (C) through (NG), which we attribute to these authors. This, of course, is not yet doing history. Nevertheless, before I historically elaborate on this sketch, let me do a bit of *logical analysis* of these propositions. To begin with, let me once more reformulate the proposition related to Copernicus, in order to make it better comparable to the others:

(C") Each planet's motion is decomposable into uniform circular motions.

In a certain sense this is not at all inconsistent with Kepler's laws! For there exists a *common model*: namely the circle as a special case of ellipse. Hence, if each planet would actually go in a circle centering the sun and with appropriate constant velocity, (C") as well as (K1-3) would be fulfilled. But, as both authors knew, the planets are not behaving this way; the common model is only an abstract possibility. Of course, Copernicus did not only claim that there is *some* decomposition according to (C"), but he claimed that a *specific* system of circular motion is the right one; i.e. he described a certain model which supposedly matches reality. – Realism is not really an issue here; but, to say the least, Copernicus claimed that his concrete model, not only some model according to (C"), saves the phenomena.

In the same vein, KEPLER gave a concrete description, obeying K1 through K3, which saves the phenomena. This system, of course, differs considerably from Copernicus's. Furthermore, Kepler could show that the data available to him do not allow for a Copernican solution in the sense of (C"). Thus one could hardly say that Kepler refined, or elaborated on, Copernicus's system; strictly speaking, he proved this system to be *false*. The only common ground for Copernicus and Kepler is the heliostatic thesis.

By contrast, it looks like Newton actually elaborates on Kepler: Newton's theory seems to *explain* Kepler's system in the sense that his laws are implied by Newton's theory of gravitation. Logically, however, Kepler's laws are not exactly but only *approximately* implied.[10] Hence, strictly speaking, Newton proved Kepler's system to be *false*. – If, however, one detaches Kepler's theory from the contingent traits of our planetary system, there is a compatibility of Newton and Kepler similar to the one we observed between Kepler and Copernicus: there are models common to both, Newton and Kepler.[11]

More telling than this brute logical "fact", however, is the *specific way* in which Newton's theory approximates Kepler's laws. For it is easily shown – cf. elementary textbooks of mechanics – that each planet *in isolation*, i.e. if it and the sun were the only material objects in the world, would move almost elliptically around the sun, the approximation to an ellipse being the better the larger the proportion of the sun's mass, M, to the planet's mass, m, is. But (fortunately?) we are not alone with the sun. Both, the sun and the earth, gravitate also with the moon and with the other planets (and their satellites) and so on. Newton's theory applied to more comprehensive systems (than just Sun/Earth), yields complex planetary motions, which, however, obey Kepler's laws pretty closely. (Otherwise Kepler would hardly have "found" them.) Hence, it is quite understandable that the rumor that Newton explained Kepler's laws is so long-lived. If, however, one insists that explanations are logical implications, as explicated in the covering law model, the most one could say is that Newton explains why the planets *roughly* obey Kepler's laws; i.e., in a way, these laws themselves are explained. More appropriate seems to be the view that Newton explains Kepler's laws in a different sense of 'explanation': Newton shows that Kepler is almost right, gives, in principle, the degree to which Kepler's laws deviate and thus demonstrates *why* Kepler's laws are false.[12]

Let's summarize: Kepler refutes Copernicus, and Newton in turn refutes Kepler – but each time instructively. Is this an adequate image of the Copernican Revolution: a chain of informative refutations? Is at least the last link in this chain, Newton's theory, "true", such that we conceive of the Copernican Revolution as culminating in a true theory? Well, by now we know that, if EINSTEIN is right, Newton is bound to be false as

[10] The elliptical shape of a planet's orbit presupposes that the influence of the other planets is negligible.

[11] Cf. Scheibe [73]

[12] *ibid.*

well![13] Is, then, the history of science a non-ending chain of interesting falsehoods? Does history, at least, *approximate* Truth (as POPPER will have it)? I do not want to continue this line of thought here; rather I now turn to the *historical analysis*.

II.

Newton's theory of universal gravitation and general mechanics was well established pretty soon after its publication (1687). Nowadays we are used to call this theory *the paradigm* of classical physics (and science in general). Newton's achievements are well documented and appreciated, e.g. by I. B. COHEN.[14] Newton's theory completes the development, which I have called Copernican Revolution: it incorporates Kepler's astronomy (the then maturest form of Copernicanism) as well as Galileo's terrestrial mechanics, extended to the celestial realm.[15]

History of science is especially concerned with the *dynamics* of scientific processes, e.g. the question why certain achievements took place at specific points in history. Answers to such questions may be internalistic, i.e. claiming some inner logic of the sequence of scientific results, or externalistic – nowadays usually rather called *contextual* –, i.e. considering also cultural and social circumstances.[16] Most historians prefer some combination of the two types of approach. I am going to follow a middle way in that I refer to the *perspectives* of the most important scientists involved, rather than looking at the process in a *post facto* perspective, i.e. only noticing what finally was endorsed scientifically ("Whig history").[17]

Nevertheless, I'll start with NEWTON. How did he arrive at his gravitational theory? Usually this process is conceived of as kind of "induction", namely by generalizing Kepler's third law:[18]

(K3) "induces" (N).

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[13] Einstein's theory gives, *e.g.*, a more exact account of the precession of Mercury's perihelion.

[14] cf. Cohen [80]

[15] cf., *e.g.*, Cohen [60]

[16] cf., *e.g.*, Shapin [96]

[17] cf. Osler [00]

[18] cf. Glymour [80], ch. VI, 203 ff, for a detailed analysis.

This is plausible only with Newton's general mechanics as a background, i.e. – to put it in a nut-shell – under the assumption of the three Newtonian axioms:

(N1) If no forces are exerted on a body, it moves uniformly, i.e. its acceleration $\boldsymbol{a}$ vanishes: $\boldsymbol{a} = 0$.

(N2) A force $\boldsymbol{F}$ exerted on a body with mass m causes an acceleration $\boldsymbol{a}$ according to $\boldsymbol{F} = m\boldsymbol{a}$.

(N3) The forces which two bodies exert on each other, $\boldsymbol{F}_{12}$ and $\boldsymbol{F}_{21}$, are of same amount, but in opposite direction: $\boldsymbol{F}_{12} = -\boldsymbol{F}_{21}$.

The argument from (N1-3) and (K3) to (N) roughly runs as follows. The Keplerian laws apply especially to the limiting case of uniform circular motions (cf. the common model of (C) and (K1-2) as described above). A purely kinematical consideration shows that in this case the body's acceleration is always directed to the center and has the amount

$$a = v^2/r,$$

where 'v' is the body's velocity and 'r' its distance from the center. This argument needs, however, some infinitesimal considerations.[19]

If the circling body has mass m, (N2) tells us that the cause of the body's acceleration is a force $\boldsymbol{F}$, directed to the center, with the amount

$$F = m \cdot v^2/r.$$

Since the distance covered in one period is $2\pi r$, velocity v and period T are connected by

$$v = 2\pi\, r/T,$$

hence

$$F = m \cdot 4\pi^2 r^2/(T^2 \cdot r)$$
$$\sim m \cdot r/T^2.$$

If we substitute, according to (K3), 'r^3' for 'T^2', we gain

$$F \sim m/r^2.$$

[19] Cf. standard textbooks on classical particle mechanics.

If M is the mass of the central body (the sun), we have, according to (N3), also

$$F \sim M/r^2,$$

and the constants of proportionality – call them C_1 and C_2, respectively – must fulfill

$$C_1 m = C_2 M;$$

abbreviating $\gamma := C_2/C_1$ we get

$$F = \gamma \, Mm/r^2$$

as amount of the gravitational force.

Newton generalized this example of a circular motion of a planet around a central body, the sun, to his law of universal gravitation (N), as formulated above. It has been corroborated in a huge variety of terrestrial applications (cf. later) and hence proved truly universal.

III.

Could KEPLER have found the law of gravitation? Hardly. To begin with, some necessary mathematical tools, especially the calculus, had not yet been developed at his time; Kepler himself, although a gifted mathematician, had taken only a few steps in that direction.[20] More important, Kepler still lacked the modern concepts of *force* and *mass*; his ideas were quite Aristotelian. He sticked, e.g., to the principle that in order to maintain a constant velocity, a force is needed which is the larger the larger the caused (or maintained) velocity is. This seemed quite natural for his assumption of an external force exerted on the planets, namely a force, emanating from the sun, which pushes the planets around. If this force is connected with the propagation of light, it should be straightforward to assume that the force obeys an inverse square law, since this is true for the intensity of light –, as Kepler knew quite well.[21] However, Kepler insisted on an inverse *linear* law. This was too well entrenched with the account of velocities that he had given in his *Mysterium Cosmographicum* (1596), namely that the velocities of the planets, compared to each other, obey an inverse linear law as well. His 3rd law,

[20] He once calculated the volume of a barrel.

[21] Cf. his works on optics.

which he found only much later, could have told him otherwise.[22] None of his tentative concepts of force, however, could have justified such a law of velocity. He tried to harmonize his linear account with his idea of propelling beams of light by strangely claiming that the light's forces are confined to the ecliptic, i.e. spread out only two-dimensionally, while the light itself, of course, spreads out three-dimensionally.

To sum up: Kepler's "law of distance"

$$v \sim 1/r$$

and his force law

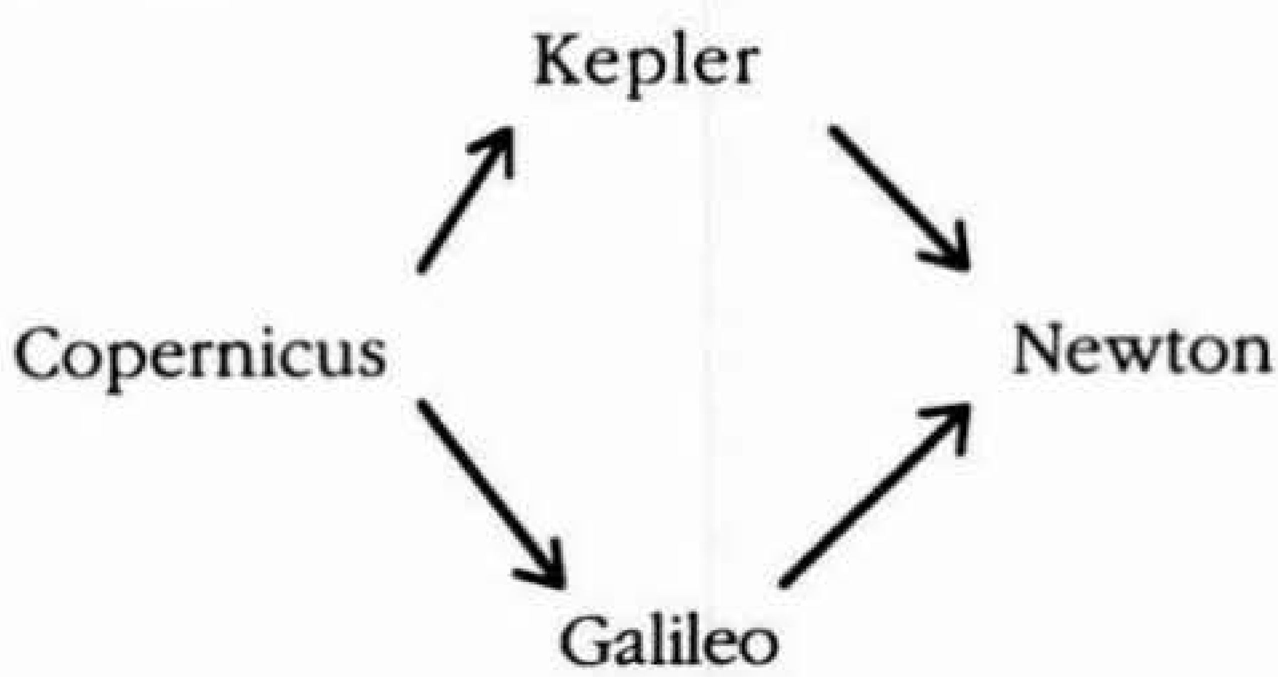
$$F \sim 1/r$$

obey, on the one hand, the Aristotelian doctrine

$$F \sim v\,;$$

on the other hand, they are pretty well confirmed empirically. Why should he have changed them?! Even *if* he could have found the inverse square law: *he* faced no problems, which could have urged him to do this step.

However, other Copernicans, contemporaries of Kepler, felt urged to revise Aristotelian physics, most prominently among them GALILEO. So far I haven't mentioned him in order to keep my account of the Copernican Revolution as simple as possible; for to include Galileo forces us to drop the linear scheme Copernicus – Kepler – Newton in favor of a "two-dimensional" one:



While Kepler concentrated on celestial physics, Galileo mainly promoted terrestrial physics. The latter's *law of free fall* was an early account of an *accelerated* motion, caused by a *constant* force (as gravity supposedly was). As a consequence one would expect a *uniform* motion

[22] Kepler found the 3rd law in May 1618, very shortly before he completed the manuscript of his *Harmonice Mundi*. (See *Gesammelte Werke*, vol. 6, p. 368.)

in case that *no* force is involved. Galileo is said to have demonstrated just that by experiments with an inclined plane. Interestingly enough, though, he did not infer a linear inertial motion, but a *circular* one (namely around the earth's center). A horizontally thrown ball would, if not subject to friction, fly around the earth and hit the throwing person – if patient enough to wait – from behind.

By way of simplification: Galileo's circular inertia kept him from a revision of celestial physics as long as he sticked to circular celestial motions. Was that a deeper motive for him to neglect Kepler's ellipses?[23] Astronomically, Galileo thereby kept behind Kepler. Nevertheless it was, at least temporarily, Galileo's Copernicanism, which forced him to revise physics.

Thus Galileo and Kepler fought in different battlefields, and with incompatible weapons, for the common goal of a Copernican physics – which none of them reached, but only NEWTON.

The central figure and turning point of the astronomical revolution, however, is KEPLER. What was *his* perspective? Certainly not something that is sufficiently expressed in (K1 – 3). Kepler's three laws are, so to speak, only the "nuggets" which later authors, in *their* perspective, have picked out. For Kepler his (later so-called) laws had a different weight. E.g. the "3rd law" was for him the cornerstone of his harmonic theory, not at all intended as a contribution to a force law. For Kepler this cornerstone is the last piece in a puzzle, which he began, when still pretty young, with his *Mysterium Cosmographicum*.[24] In Kepler's perspective, his life long work on the harmony of the world was by no means a side-aspect. Also, his combination of astronomy and harmonic theory wasn't that unusual if you look backwards in time: PTOLEMY, too, the great master of antiquity, had written a harmonic theory; in fact, Kepler first intended to reprint, and comment on, (parts of) Ptolemy's *Harmonics* together with his own *Harmonice Mundi*.[25] However, Ptolemy did not integrate his works on harmony and astronomy. Nor did he incorporate his astrological work, the so-called *Tetrabiblos*, into his *Almagest*. Furthermore, he also separated his celestial physics, laid down in his *Hy-*

[23] If Galileo took at all notice of Kepler's *Astronomia Nova*, he did so rather reluctantly.

[24] This is only an abbreviation of a lengthy programmatic title which Stephenson [94], p. 75, translates as *Introduction to the cosmographic treatises* [!] ...

[25] Cf. Stephenson [94], ch. VII.

potheses planetarum,[26] from his astronomical work. Kepler engaged in the same four fields: mathematical astronomy, physical astronomy, astrology, and harmonic theory; but he deliberately tried to integrate them as far as possible. Probably he conceived of his whole work as the final fulfillment of Ptolemy's general "research program".

Some words on *astrology* might be in place here. This wasn't just a side-aspect of Kepler's work, nor a mere job for making money. He did astrological work all his life: personally for himself, family and friends – e.g. before important steps like marriage – but as well publicly. His (pretty early) essay *De fundamentis astrologiae certioribus*,[27] written in Latin, shows that he also addressed an educated audience. (By the way, in this work Kepler shows himself still convinced that the planets spread their own light, rather than only reflect the sun's light. Shortly after, Galileo's telescopic evidence convinced him of the contrary.) Also, Kepler was highly critical of the astrological tradition, considering himself as an astrological reformer, or "Lutheran astrologer", as he once phrased.[28] He argues with the sharpest Renaissance critic of astrology, Pico DELLA MIRANDOLA, as well as with contemporaries, *pro* and *contra* astrology. Remarkably, Kepler also did extended work on the most empirical part of astrology of his time: weather prognosis. Over a period of several decades Kepler collected meteorological data. After realizing that these data conflicted with certain parts of the traditional (and his own early) theory of astrological "aspects", he revised his system considerably – thus proving to be a good Popperian!

That is all the more remarkable because this revision caused him extreme trouble for his system of harmonies: astrological and cosmological harmony no more coincided, and consequently are dealt with separately in two books (IV and V of *Harmonice Mundi*), although there is a certain common foundation in the geometrical part of his work (books I - II). This bifurcation of his system reflects the Copernican change in perspective: the cosmical harmonies are sun-centered (perceivable by Sun's inhabitants! – cf. *Harmonice Mundi* V.10), while the astrological harmonies are created for us as living on a moving planet. However, this moving *habitat* of ours is by no means a degradation: by placing us here,

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[26] Cf. Goldstein [67]

[27] Prague 1602. Engl. translation in Field [84].

[28] Cf. Field [84]

rather then in the center, God has given us a chance to find out about the mysteries of the universe![29]

In Kepler's perspective his work has a high degree of continuity and homogeneity. He sticked to the *polyhedra model* of the *Mysterium Cosmographicum* for the rest of his life, although he had to modify it. He even included it in his Copernican "textbook", the *Epitome*; and, of course, it is the basic structure of *Harmonice Mundi*. Here, as in *Mysterium Cosmographicum*, it is connected with astrological traits.

But isn't Kepler's *Astronomia Nova* (AN) quite different? He began this work as an assistant to T. Brahe. But we have to recall that it was Kepler's search for more accurate data, in order to confirm his polyhedra model of the *Mysterium Cosmographicum* (MC), that brought him into touch with Tycho. In the beginning, however, he got only the data for Mars, for this was the most "stubborn" planet, i.e. the one which fitted Brahe's own model least. Working on Mars, Kepler found his first two laws, published in AN, his "Commentary on Mars" (1609). Admitting elliptical paths made it necessary, of course, to modify the polyhedra model. But it still worked as a kind of gross approximation. The complications caused by his new insights finally led him to the more elaborated system of *Harmonice Mundi* (HM). Thus Kepler's own perspective might be put – by way of provocation – into the formula:

$$MC + AN = HM.$$

Was Kepler a *revolutionary*? It depends. If we take "revolutionary" in the sense of I. B. COHEN's 'revolution in science', Kepler himself should have regarded his work as revolutionary in order to call it such, historically. Darwin, e.g., fulfills this (necessary, in Cohen's sense) criterion,[30] but hardly Kepler. What *we* find revolutionary in Kepler (if something at all), is due to a *post facto* perspective which must not match with Kepler's own view. Take, e.g., the fact, that Kepler seems to be the first who focuses on the actual path a planet follows in space,[31] rather than be content with the description that the planet's motion is composed out of a number of simple (circular) motions. There had been no technical

[29] Copernican astronomy, unlike Ptolemean, allows to calculate the distances of the planets from the center and thus to detect the harmony expressed in these distances.

[30] Cf. Cohen [85]

[31] Cf. above

problem for astronomers before Kepler to draw the resultant curve on paper. They simply seem not to have been interested. The angular motion could be calculated without such a "detour", i.e. the system of circular motions is all that was needed "to save the phenomena". [If you actually draw the resultant curve for a planet's model according to Ptolemy – or Copernicus, for that matter! – you get something really strange! Hard to imagine that a realistically inclined person should not have taken offense!]

Kepler's (then) remarkable interest in the planet's paths themselves expresses his *realism* vs. the instrumentalism of the 'Save the Phenomena' tradition.[32] (Kepler has explicitly dealt with such methodological questions in his *Apologia*).[33] Already COPERNICUS had taken a realist stance, as is well documented in the Prologue to his *De revolutionibus* addressed to the Pope. (The effect of the Prologue is somewhat blurred, however, by OSIANDER's anonymous preface).[34] But still in Kepler's time realism was a minority position, although during the later parts of the 16th century it became harder to avoid astronomical realism, because the spheres carrying the planets lost their solidity, so to speak, especially by Tycho's proof that the comets go just through the alleged spheres. (Actually the dissuasion of the spheres was somewhat more complicated.) Thus realism in astronomical matters seems to be something that was only gradually accepted, not by somebody's revolutionary act.

A further "revolutionary" trait in Kepler is his "physicalism", i.e. his willingness to consider physical reasons in astronomical matters. This attitude, rather than his proposed physics itself, is revolutionary. He thereby inaugurates a *new discipline*, physical astronomy (or astronomy in the modern sense), as proudly announced in the title *Astronomia Nova Seu Physica Coelestis...* of his main astronomical work. Such a discipline is, of course, inconceivable in the Aristotelian tradition. That Kepler overcame respective obstacles is one of his greatest achievements. *How* he was able to do this, is a matter for further historical research. Part of the answer may refer even to the physical part of his astrology. This discipline may have made him methodologically more innovative than he was inclined to be in his still very Aristotelian physics

[32] Cf. Duhem [08]

[33] Cf. Jardine [84]

[34] Kepler was the first to publicly identify the preface's author with Osiander.

(see above). Thus he may be regarded as a revolutionary with respect to disciplinary structure. Especially he turns over the ranking order of physics (natural philosophy) and mathematics: it is physics that leads him to the mathematical form of an ellipse. Not *mathematica ancilla philosophiae*, but *vice versa*! There had been a tendency for this overturn already in Copernicus, who had proudly insisted that his work be written for mathematicians: these are the ones to settle questions of cosmology.[35]

To sum up: some elements of the new paradigm of classical physics were connected with programs and perspectives, which hardly are our own today, nor the ones of the successful new paradigm. Does this mean that there is no "Copernican Revolution"? (The question is, of course, not whether there is *something*, which in *some* respect can be labeled in such a way.) Confer again the Kantian revolution in epistemology (or metaphysics): it is connected with a change in perspective comparable to that connected with the interpretation of the apparent celestial motions by supposing a motion of the observer. Such a change in perspective, quite literally, has been a topic e.g. for Kepler in his last work, *Somnium* (Sagan/Frankfurt, 1634).[36] He there compared the Copernican change in perspective with the fictitious one which a traveler to the moon would have to undergo.

IV.

Why should we regard the development started or prompted by Copernicus as *revolutionary*? And, if it was, in which sense? Did the participants themselves act as revolutionaries? How about Copernicus himself? His biographers ensure us that he wasn't at all a typical revolutionary. Especially, there is nothing revolutionary in the fact that he uses the word *revolutio* in the title of his work: "re-volutio", in his time, means something like return to the origin, i.e. a circular motion like that of a planet. *If* the title bears some metaphorical meaning at all, it would likely be something like the Renaissance motive of restoring antiquity; but most probably "revolutio" is to be taken in its plain sense.

Is, then, the "Copernican Revolution" a revolution without revolutionaries? For I. B. Cohen this would be monstrous. For him, the only

[35] Cf. the end of his prologue to *De revolutionibus*.

[36] *Somnium ... seu opus posthumum de astronomia lunari*

revolutionary work in question is NEWTON's foundation of classical physics, i.e. the end of the development we are considering: we would have a revolution with the genuine revolutionary act at its very end!

I suppose we should be less concerned with the *heroes*. More appropriate are *structural* concepts of revolution like KUHN's. However, in case of the alleged Copernican Revolution Kuhn's concept faces a special difficulty. (Evidently, in his eyes, there *is* a Copernican Revolution – in fact, the first revolution discussed by him at length.) His account of the Copernican Revolution in his 1957 book with the very title *The Copernican Revolution* hardly bears the characteristics he attributes to scientific revolutions in the sense of his general account of 1962, *The Structure of Scientific Revolutions*. There is, e.g., no "crisis" which had to be overcome by Copernicus (or by one of his followers). Also, there hardly is the one *gestalt switch* that puts everything into the new perspective. Nevertheless, in his later work, the Copernican Revolution is still taken as an example for his general concept of a scientific revolution.

I would like to focus on a third peculiarity of the Copernican Revolution which seems to be too poorly described in Kuhn's general concept of scientific revolutions. According to his concept the pre- and the post-revolutionary paradigms are *incommensurable*. For the Copernican Revolution this certainly is the case. But at the same time it is important to realize what the two things are which are said to be incommensurable. For the two paradigms do not even belong to the same field, they are not separated just by a revolution *within* a certain scientific discipline. The pre-Copernican paradigm was one of mathematical astronomy while the resulting Newtonian one is one of physics in general. The two paradigms constitute quite different parts within a changing superstructure formed by mathematics and science and finally integrated by way of mathematization of natural philosophy (cf. Newton's title *Philosophiae Naturalis Principia Mathematica*). The sharp distinction between celestial and terrestrial physics vanishes, astrology is basically given up (as part of natural philosophy/physics), and – last but not least – physicists shift their attention from essential causes to some sort of mathematical functionalism. This change in interdisciplinary structure is, I would say, the most important structural feature of the Copernican Revolution.[37]

These considerations have a certain parallel in a more recent account of KUHN's where he introduces the concept of *local holism* to cover the

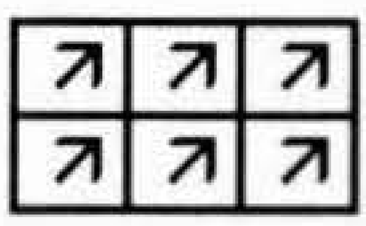
[37] Cf. my [86] for more details.

interdependencies between the various components of a scientific tradition.[38] He impressively reports how difficult it was for him as a physicist who had been socialized in Newtonian physics to make any sense of Aristotelian physics. In the beginning this would have appeared to him as no more than a bundle of falsehoods. Only after quite a while he would have become able to understand the inner coherence of the Aristotelian worldview.

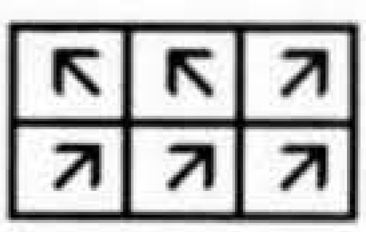
V.

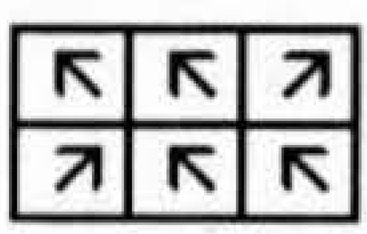
I do not pretend here to give a fair account of Kuhn's local holism. I would like instead, in my closing remarks, to present a somewhat similar view which I call the *magnet model* of scientific revolutions;[39] though understood as a general concept, I here restrict myself to an application to the Copernican Revolution.

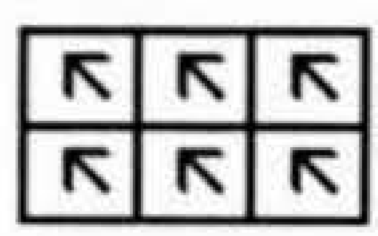
A lump of magnetized iron consists – as physicists tell us – of parts each of which is magnetically polarized and all of which are more or less oriented in the same direction.



If now the magnet is placed into an exterior magnetic field of a different direction, the homogenous parts of the magnet, one after another, switch into the new direction, e.g. *via* the states:







The two states at the beginning and at the end are stable, the ones in between unstable. Now associate with the four states the names of Ptolemy, Copernicus, Kepler, and Newton. Copernicus reorientates the relative motion of sun and earth (one arrow) and gains a coherence that allows him to calculate the distances of the planets from the sun (second arrow). Both innovations, however, clash with the rest of the old paradigm. Kepler switches the role of physics and thereby finds his laws of

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38 Cf. Kuhn [81]

39 Cf. again my [86].

planetary motion (two more arrows). Finally, Newton synthesizes Kepler's physical astronomy with Galileo's terrestrial mechanics and creates the new discipline of mathematical physics (arrows 5 and 6).

This model, of course, needs some qualifications; what it should show is the interplay between long-term constraints of the development (exterior magnetic field) and the somewhat narrower or different perspectives of the various authors involved. These do not "look outside" and probably have no definite idea of the direction the process will eventually take. Instead they are pretty absorbed by problems of coherence within their own respective conceptions. If they are lucky they solve *their* problems – like Kepler with his world harmony –, although *we* wouldn't say he has solved the "real" problems at stake. What is transitory in our view, may have been a quite authentic style of scientific reasoning which – for whatever reasons – proved out of date. If you are a realist you would say the exterior magnet field, which keeps the reorientation going, stands for nature itself. If not (if, e.g., you are a constructivist), you would want to replace nature by some social setting.

The magnet model also could support a certain *hermeneutics of historiography*. If you have a rough idea of a historical process and then dive into some details you easily get fascinated or even trapped by the inner logic of peculiar developments, thereby losing your initial more global perspective. The historian him- or herself is occupied with "switching" comparably small "parts" within the boundaries of the proponent's horizon. And this is necessary for grasping the inner dynamics of the process. If, however, you don't want to get lost in the historical process, you should eventually free yourself from all the details and again have a look at the whole process from an exterior perspective. This will then have changed considerably by the experiences you have made. It may well be that, in retrospect, you are more familiar with the, historically, earlier *gestalt* than with the later which had been all too familiar to you beforehand.

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