*H. F. COHEN**

MUSIC AS A TEST-CASE

How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?
Th. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1970), p. 9.

I. Introduction

THE AIM OF THIS PAPER is to inquire what a few selected general theories on the progress of science are capable of accomplishing when confronted with a particular set of historical data that have never gone into either the making or the testing of any metatheory of science. The data utilized in this paper are taken from a part of the history of a science that has fairly consistently been overlooked by historians of science, namely the science of music. During the period I am concerned with — the Scientific Revolution in its first stage — certain quantitative aspects of music theory were felt to be no less a legitimate part of the scientific enterprise than disciplines we at present take for granted, such as mechanics, optics, astronomy and so on. Yet many of those who shaped the Scientific Revolution, men like Kepler, Galileo, Descartes, Mersenne, Huygens, Newton, devoted part of their attention to the science of music, as natural participants in a tradition that goes back at least to Pythagoras, and has never entirely died out; witness contributions to the same field by such later luminaries as Euler, D'Alembert, and Helmholtz.

In my book *Quantifying Music. The Science of Music at the First Stage of the Scientific Revolution, 1580–1650* (Dordrecht: D. Reidel)[1] I have defined the issues involved, and given a detailed account, entirely based on primary sources, of the radical transformation the science of music underwent during the period covered at the hands of nine particular scientists: Kepler, Stevin,

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[1]D. Reidel Publishing Company granted me permission to insert some passages from *Quantifying Music* in the present article.

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Benedetti, Vincenzo and Galileo Galilei, Mersenne, Beeckman, Descartes and Huygens.[2]

The main problem they were all concerned with was the (in essence still Pythagorean) problem of consonance. What this was all about will be explained presently; here it should be noted that to the legacy of the Renaissance belonged one particular theory of consonance — the *senario*; that Kepler invented an account of his own; and that virtually all others either put forward or adopted a new explanation of consonance, which I have termed the coincidence theory. On the face of it we have here a rather clear-cut case of theory replacement: from about 1600 on the *senario* was universally rejected; Kepler's solution never convinced anyone; the coincidence theory carried the day for quite some time to come. What makes the historical case philosophically interesting, however, is that certain formal properties of the victorious theory (in particular its failure to solve any of the core problems of consonance it was supposed to provide the solution for) seem to defy many, if not all, current theoretical accounts of how theory replacement comes about. According to the inductivist account, science grows through the continuous expansion of known facts generalized into ever more-embracing theories. Popper's metatheory states that theories are replaced by new ones in case that the latter are capable of passing tests the earlier ones fail to stand up to. According to Kuhn, theory replacement is part of a revolutionary process; it comes about as a consequence of a preceding crisis, and results in a new 'paradigm', or 'disciplinary matrix', that is more capable than its predecessor of solving current problems — even though anomalies continue to exist. Finally, Feyerabend maintains that when more than one theory is available, there are no rational criteria for choosing between them: one theory replaces another for reasons that cannot possibly derive from external criteria, but ultimately come down to matters of taste and fashion.

In the main body of this paper we shall investigate, first through an historical (section 2), and then through a theoretical analysis (section 3), to what extent our historical case of theory replacement fits any of these four metatheoretical descriptions.[3] What makes such an exercise particularly

[2]Musical theories of some of these have been discussed before by a few historians, notably by D. P. Walker, *Studies in Musical Science in the Late Renaissance* (Leiden, 1978); C. V. Palisca, 'Scientific Empiricism in Musical Thought', in *Seventeenth Century Science and the Arts*, H. H. Rhys, (ed.) (Princeton, 1961); and M. Dickreiter, *Der Musiktheoretiker Johannes Kepler* (Bern, 1973). See for a more detailed discussion of the historiographical situation the preface of *Quantifying Music* (Dordrecht: D. Reidel, 1984).

[3]I refrain from including in this list Lakatos' 'methodology of research programs', since in my view it adds little of substance to Popper's ideas on the increase and decrease of the empirical content of theories (cp. K. R. Popper, 'Replies to my Critics', in *The Philosophy of Karl Popper*, P. A. Schilpp (ed.) (La Salle, Illinois, 1974), pp. 993 – 1013). I also do not think that my broad use of the term 'research program' further on in this paper has much in common with the Lakatosian notion.

worthwhile, I think, is that it straddles the borderline between pre-modern and modern science. Much more, therefore, was at stake than just the passage from one theory to the next: a fundamental shift in scientific standards was involved. Thus this period is a vital one for metatheories on the progress of science to prove their mettle: their worth is tested by means of data from a period when science itself underwent a drastic upheaval. Issues like the demarcation of science and non-science; the nature of revolutionary science; continuity versus break in scientific progress, and the amount of rationality involved in theory replacement thus come to the fore with particular urgency. This has in fact been recognized for a long time: witness the frequency with which Copernicus and Galileo, in particular, have been invoked to support one metatheory or another. In this paper I intend to draw a somewhat different conclusion, in that the uniqueness of the Scientific Revolution itself will be invoked to account for the overall failure of our four metatheories to fit all the facts of our historical case (section 4).

One side issue, finally, of the theory of consonance that may also be of special interest to philosophers is that it provides a rather unexpected perspective from which to look at the mind – body problem and its history. The perception of light is usually treated as the paradigm case of sense perception; yet the way regular vibrations of the air are perceived as sound provides a no less valid entrance to the same problem, particularly in the case of musical intervals, since these not only represent a very special kind of sound, but are also easily quantifiable. To explain why and how they are provides us at once with the necessary introduction to the historical part of this paper.

II. The transformation of the science of music at the first stage of the Scientific Revolution

2.1. The problem of consonance before 1600

The basic problem is very simple. When two arbitrarily chosen musical notes are sounded together, the resulting interval will not, generally speaking, be perceived as pleasant. Such an aesthetically displeasing interval is called a 'dissonance'. In the entire continuum of possible intervals only a very few do strike perceiving man as 'quiet', 'restful', 'beautiful'. These few intervals are called the 'consonances', and it is to this defining feature that they owe their having functioned throughout history as the 'building-blocks' on which virtually all of musical composition was based.[4] Hence the distinction

[4]Twentieth-century Western music provides one of the few exceptions to this rule.

Table 1.

Interval	Unison	Octave	Fifth	Fourth	Major third	Minor third	Major sixth	Minor sixth
Ratio	1:1	1:2	2:3	3:4	4:5	5:6	3:5	5:8

between consonance and dissonance is fundamental to music. Now Pythagoras is credited with the discovery that this fundamental distinction is exactly matched by another one. He found experimentally that the consonances can be generated by dividing a sounding string into several ratios, all of which are made up of a few simple integers. Thus, for instance, the consonance now called octave is sounded by a string and its half; hence it is represented by the ratio 1:2. Similarly the fifth is given by 2:3. In contrast, an obvious dissonance like the second is given by such a relatively complicated ratio as 8:9. Now the basic question is, and has always remained: Why is this so? Where does this apparent match between aesthetically pleasing sound and the ratios of the first few simple integers come from?

This question has repeatedly been reformulated, both from the musical point of view, in that in the course of more than twenty-five centuries of harmonic development the range of the intervals accepted as consonant has been extended, and from the side of science, in that the type of answer that was supposed to provide the solution to the Pythagorean riddle of consonance has repeatedly been shifted, from the numerical and the mathematical, through the physical, to the physiological and the neurological. For present purposes it is sufficient to sketch the problem situation as it looked around the middle of the sixteenth century, just on the verge of the period of transformation that is to occupy us.

At that time eight specific intervals were generally accepted as consonant, and functioned as such in virtually all musical composition (Table 1).[5] The Pythagorean problem now presented itself in the following form: What do the integers that make up these few consonance-generating ratios have in common? What distinguishes them from all other numbers, which serve only to generate dissonances? What is it that enables those few simple integers to generate consonances? In other words, the problem was to find a numerical distinguishing criterion between consonance and dissonance and to explain how number could bring forth aesthetic pleasure. The authoritative answer was given by a learned Renaissance scholar and musician, Gioseffo Zarlino, who in 1558 expounded his theory of the *senario*. In his view, the *senario*, that is, the range of the first six positive integers, provided the clue to the riddle, as all integers that make up the consonance-generating ratios appear to be

[5]For simplicity's sake I exclude throughout the 'replicas', that is, the consonances that exceed the range of one octave, such as the twelfth (1:3), which is the first replica of the fifth.

confined within the *senario*. Nor is this due to chance, in view of the properties of the number six. Isn't six the first perfect number (the first, that is, of those numbers that are themselves the sum of all factors into which they can be resolved, as $1 \times 2 \times 3 = 1 + 2 + 3$)? Aren't there six planets? Aren't there six directions? Aren't there twice six apostles? Didn't God create the world in six days?, etc. And the *senario* not only thus distinguishes the consonant from the dissonant ratios, it also functions as the 'sonorous number', the 'harmonic number', that transforms outer numerical regularity into inner aesthetic pleasure through the correspondence between the harmony of this number and the harmony that governs the perceiving soul. Such is Zarlino's solution to the problem of consonance.[6]

Now before going further I wish to make it clear that this solution was not quite so ridiculous as it almost inevitably appears to us. It was entirely in keeping with the scientific climate of the time, exemplifying both Platonically inspired number mysticism and Aristotelian-type distinctions and unrefined empiricism. As such it provided a plausible explanation that covered the entire range of the problem to be solved.

Yet there were a few drawbacks, too. One is particularly obvious: the ratio of the minor sixth is 5:8, hence it falls outside the *senario*. Zarlino managed to get out of this difficulty by applying a typically Aristotelian distinction: while the other consonances are actually contained in the *senario*, the minor sixth is so only potentially, which is really a rather pompous way of saying that in this special case eight should please be conceived of as twice four. But carrying out such a trick was forced on Zarlino because he could not possibly desert the *senario* and settle instead for the *ottonario*, the range of the first eight integers. For in that case the intervals whose ratios contain the number seven would have to be admitted as consonances as well. But these have no place in our musical scale at all, and the intervals nearest to them were traditionally regarded as very harsh dissonances.

But a much more fundamental objection is of course the extreme arbitrariness with which Zarlino handled the wondrous properties of the number six. Evidently it would not at all be difficult to carry out a similar exercise with, for instance, the number seven: Didn't God need seven days for the creation (for why not include His day of rest as well)? Aren't there seven heavenly bodies in between the Earth and the fixed stars (also including the Sun)? Aren't there seven wonders of the world? and so on. In short, it is precisely this sort of Renaissance number juggling that was to be altogether unpalatable to people with an altogether different conception of science, such

[6] G. Zarlino, *Istitutioni harmoniche* (Venezia, 1573), pp. 28 – 34; (facsimile edition, Ridgewood, 1966; the first edition of the book is from 1558).

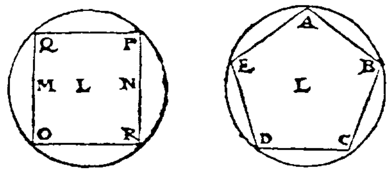

Fig. 1.

as were to appear only one generation after Zarlino and were to tackle afresh the old and venerable problem of consonance. One of these was Johannes Kepler.

2.2. Kepler's geometrical solution

Music was an essential ingredient of Kepler's scientific research program. The consonant intervals, and their derivation from one basic distinguishing criterion were to provide him with the knowledge of the mathematical proportions that had guided God in creating the universe and, hence, were to reveal the ultimate principles of the Harmony of the world. Kepler was convinced that the clue to the riddle of consonance could not be found in number, as Zarlino had taken for granted, but rather in geometrical figures:

> Geometry [. . .], coeternal with God, and radiating in the divine Mind, supplied God with the models [. . .] for establishing the World so as to make it the Best and the Finest, in short, the most similar to its Creator.[7]

Kepler found his geometrical criterion distinguishing the consonances from the dissonances in the arcs that are cut off the circumference of a circumscribed circle by regular polygons. Take, for instance, the square and the pentagon. The inscribed square divides the circle into four equal parts, the pentagon into five, and so on (see Figure 1, taken from *Harmonice Mundi*).

These divisions generate proportions which result from a comparison of the various arcs thus cut off. In order to make such a comparison a distinction has to be made between *part* and *residue*. 'Part' here means either one or more arcs subtended by one or more sides of the polygon in question, such that the sum of the lengths of the arcs does not exceed one half of the circle. What remains of the circle is called the 'residue', which thus is either equal to or greater than the 'part'. Now three kinds of comparisons can be made, namely: part to whole; residue to whole; part to residue. Thus the pentagon generates the following proportions:

[7] J. Kepler, *Gesammelte Werke*, M. Caspar *et al.* (eds), Vol. 6 (München, 1940), pp. 104–105.

part to whole	1:5; 2:5
residue to whole	3:5; 4:5
part to residue	1:4; 2:3.

Evidently this procedure is much too broad to generate only the eight consonances, but by judiciously introducing three successive limiting conditions Kepler managed in the end to exclude all those regular polygons that would have left him with dissonance-generating proportions. (One of his rules is that only those polygons are admitted that are constructible by means of compass and ruler, and this is where his entire, unbelievably elaborate and ingenious edifice is now known to break down: for in Kepler's system there is no use at all for the regular 17-gon that in 1796 was shown by Gauss to be constructible by compass and ruler after all).

For Kepler, the entire construction served to solve only part of the problem of consonance, since it remained to be explained what property was capable of turning these proportions of the consonances into pleasing sounds. This process, Kepler explained, essentially takes place within the soul, and is performed by the soul. After certain musical sounds have reached the ear they are somehow taken inwards and are subjected to a more or less conscious comparison by the soul. In Kepler's fine metaphor, they are 'brought before the tribunal of the soul'. If the soul's judgment is positive, they are turned into 'sensile harmonies'. This happens only to the consonances. All other musical intervals are rejected by the soul, and are thus experienced as disharmonious.

In short: the consonances exist outside of the sense of hearing. Through it they reach the soul, which, in comparing the terms of the proportions of which they consist, turns them into harmony. Thus harmony is not something given from outside; rather harmony is an activity of the soul.

Comparing something to something else is possible only if there is a common entity to which both terms of the comparison can be related. What entity enables the soul to compare the terms of the consonances, thus turning them into sensile harmonies? Kepler:

> To find a proper proportion in the sensile things is to discover and to recognize and to bring to light the similarity of this proportion in the sensile things with a certain Archetype of a most true Harmony, which is present in the Soul.[8]

This archetypical, or 'intellectual', harmony is the ultimate ground and explanation of the experience of musical beauty. Just like the sensile harmonies, the intellectual harmony needs reference points with regard to

[8] J. Kepler, *op. cit.* note 7, p. 215.

which it can carry out its comparisons, and these reference points are by now familiar to us: they are the circle and the arcs cut off its circumference by the constructible regular polygons.

Since the proportions of these arcs are independent of the size of the circle, size can be abstracted from the circle. The circle shrinks into a point; the soul becomes a 'potential circle', a 'circle provided with directions', a 'qualitative point'. This archetypical circle is not an image of an archetype outside of the soul, but it is itself the archetype, that is to be conceived of as an abstract, geometrical idea. And Kepler also provides the final step: the soul is itself harmony; harmony is itself divine, and man takes part in it.

Summing up, then, Kepler's geometrical solution to the problem of consonance, we end up with the two following propositions: the arcs cut off a circle by certain well-defined inscribed regular polygons provide the criterion for distinguishing consonance from dissonance; these same geometrical figures, as archetypes present in the human soul, turn the consonant intervals into the experience of harmony.

Thus the criterion found is indeed of a mathematical nature, in keeping with Kepler's deepest convictions about the structure of the universe. Yet he was not totally blind to the possibility of *physical* considerations being also taken into account in dealing with matters pertaining to music. In fact from a brief discussion of the phenomenon of sympathetic resonance he derived a possible alternative explanation of consonance:

> If the velocity of one string has the ability to move another, proportionate string that to the eye appears not to be moved, then would not the equal velocities of two strings have the ability pleasantly to titillate the ear, since in a certain sense it is excited uniformly by both strings, and since the two strokes of both tones or vibrations coincide at every moment?

But he at once rejected the idea:

> No, I say, this is too simple a way to settle the matter [. . .] For please, what relationship could there be between the titillation of the sense of hearing, which is a corporal thing, and the incredible delight that we perceive deep within our soul through the harmonic consonances? If the delight comes from the titillation, would not then the main part in this delight be played by the organ that sustains the titillation? [. . .] Add to this that the explanation taken from motion is applied in the first place to the unison; but sweetness lies not primarily in the unison, but in the other consonances and the combination thereof. Much [more] might be adduced in order to destroy this alleged explanation of the sweetness that comes from the consonances; but for the moment I prefer to desist from a more detailed disquisition.[9]

[9] J. Kepler, *op. cit.* note 7, pp. 106 – 107.

In other words, Kepler's arguments against an explanation of consonance by the coincidence of vibrations are the following:

(1) The theory leaves unexplained what happens in between the pulses reaching the ear and the experience of beauty within us.

(2) The theory, if valid, would only explain the consonance of the unison, not of the other consonant intervals and chords.

It may at first sight seem conceivable that Kepler might have extended the theory so as to comprise the other consonances as well, thus invalidating his own second argument; and this is precisely what, as we shall see presently, Beeckman, Mersenne and Galileo were to do. But it is clear why Kepler did not: even apart from his primarily mathematically oriented interest discussed above, his first argument must have appeared to him to be an insuperable stumbling block. Most of his fellow musico-scientists were to accept this 'coincidence theory of consonance' even in the absence of an explanation of how a physical property of nature, like coinciding pulses, can bring forth the human sensation of musical beauty. But Kepler already had such an explanation: he bridged the gap between the natural phenomenon and human sense experience by means of his theory of harmony. He rightly perceived that the coincidence theory could not close the gap; therefore, it had no attraction for Kepler, whose own theory could and did.

2.3. The coincidence theory of consonance

Thus we get acquainted with the coincidence theory of consonance through the man who anticipated it in rejecting it. Another, even earlier anticipation is to be found in the works of Giovanni Battista Benedetti (1585). But the coincidence theory was really established by Isaac Beeckman (in 1614 – 1615), who communicated it to René Descartes (in 1618 – 1619), and to Marin Mersenne (in 1629), whom he guided in elaborating the theory further, and also, independently of this cluster, by Galileo, who published a brief account in his 1638 *Discorsi*.

In its simplest form, such as expounded by Galileo, the coincidence theory of consonance runs as follows. Sound is conceived of as a succession of 'strokes', or 'percussions', which are supposed to be transmitted in a way quite similar to what happens in a quiet pond when a stone is thrown into it. The strokes are produced by the vibrating of a string or other musical instrument. Now every musical interval is characterized by a definitely patterned succession of such strokes. For instance, at the octave every single stroke of the lower string coincides with every second stroke of the upper string, owing to the fact that the upper one vibrates twice as rapidly as the lower one. (Both Beeckman and Galileo attempted to prove this new-found proportionality.) A similar argument can be set up for the fifth (3:2), the fourth (4:3), and so on. This regular coincidence, in being transmitted through the air to our sense of hearing, is what explains the experience of consonance; if the strokes do not, or only very irregularly coincide, we perceive the intervals in question as dissonant. In Galileo's own words:

> The Offence [the Dissonances] give, proceeds, I believe, from the discordant and jarring Pulsations of two different Tones, which, without any Proportion, strike the Drum of the Ear: And the Dissonances will be extreme harsh, in case the Times of the Vibrations are incommensurable.[. . .] Those Pairs of Sounds shall be Consonances, and will be heard with Pleasure, which strike the *Timpanum* in some Order; which order requires, in the first Place, that the Percussions made in the same Time be commensurable in Number, that the Cartilage of the *Timpanum* or Drum may not be subject to a perpetual Torment of bending itself two different Ways, in submission to the ever disagreeing Percussion.[10]

In the history of the problem of consonance the coincidence theory has been of the utmost importance. One reason is that, in providing a first precise, quantitative link between musical and acoustical phenomena, the coincidence theory gave much stimulus to drawing other phenomena of musical sound into the theory of consonance as well, such as beats and overtones (this was done in the main by Beeckman and Mersenne). Nor is this all, for the overall effect of the coincidence theory was a radical break of the problem of consonance with the entire past. Though superficially the theory looks like Zarlino's *senario* in that exactly the same first few integers still appear to define the ratios of the consonances, in fact they do so in a decisively different way. The age-old problem could now be reformulated as follows: Why is it that the simple ratios of the vibrational frequencies of sounding bodies correspond to the human sensation of pleasure and beauty in hearing the consonant intervals? The original question had thus undergone a striking transformation in that now an empirical physical phenomenon rather than, as before, an abstract mathematical ratio appeared to be responsible for our sense experience. In the course of the centuries the problem was to be transformed again and again; but no transformation could be so radical as this one: having once entered the realm of the empirical, it was never to leave it again.

Yet, when we take a somewhat closer look at the coincidence theory, and try to explore how far its explanatory power went, unexpected troubles appear to crop up. True, the theory had a certain inherent appeal, that was exploited to the full by Galileo. But when the theory was made a little bit more precise, was thoroughly quantified, and was made to generate predictions, its many

[10] G. Galilei, *Le Opere di Galilei,* A. Favaro (ed.) (Firenze, 1890 – 1909), Vol. 8, pp. 146 – 147 (The English is taken from Weston's 1730 translation of the *Discorsi*, p. 151).

shortcomings soon became apparent, to the extent that the theory turned out not even to be able to solve the two problems for the solution of which it had been devised in the first place.

The first of these two was the problem of how to find a clear-cut distinguishing criterion that matched the distinction between consonance and dissonance provided by our sense of hearing. The minor sixth (8:5) sounds evidently consonant, the second (9:8) equally evidently dissonant, but why in the world should the coincidence of every eighth vibration of the upper string still be perceived as pleasant, but no longer every ninth vibration? Yet this is where, twenty centuries earlier, the entire problem of consonance had started;[11] apparently the fresh approach along physical lines could not yield the clear-cut criterion already searched for for so long. In fact it tended rather to blur the original distinction, as indeed was conceded rather reluctantly by Mersenne.

The other fundamental problem had been to explain how a numerical regularity was capable of bringing forth the human experience of musical beauty. Just like the one above, this question had now been transformed from a question about numbers into one about vibrational frequencies. Galileo apparently felt satisfied by referring to the more or less harmonious way the eardrum was bent by the more or less coinciding strokes. Of all adherents to the coincidence theory only the two thorough-going mechanicists, Beeckman and Descartes, realized that in between the affected eardrum and the perceiving soul something remained to be explained (just as Kepler was doing at the time by means of his theory of Intellectual Harmony). Both attempted to show how the regularly coinciding vibrations were transmitted through material particles in the air, the eardrum, the middle ear, and the auditory nerve up to the seat of our perceptions, the brain. Both thought up intricate mechanisms of particulate 'spirits' running up and down the nerves, and, in so doing, opening or closing certain pores in the brain. To us it may be fairly evident that in this way the gap between the material mechanism and the perception itself cannot be closed, but can at best be shortened somewhat (which in itself is certainly no mean achievement). But the relevant point here is that even in Beeckman's and Descartes' own terms their effort at solving the auditory part of the body – mind problem failed, since there appeared to be no way to preserve the regularity of the original motion of the sounding body and of the air right to the end in the brain. In other words: Beeckman's and Descartes' corpuscular mechanisms made it intelligible (to their own satisfaction) how vibrations bring forth sound, but not how regularly coinciding

[11] In fact, since the Greeks did not admit the thirds and sixths as consonances, to the Pythagoreans the problem appeared in a slightly different guise: they had to explain why consonance seems to stop after the number 4.

Table 2.

	Ratio (frequencies)	Product
Unison	1:1	1
Octave	2:1	2
Fifth	3:2	6
Fourth	4:3	12
Major sixth	5:3	15
Major third	5:4	20
Minor third	6:5	30
Minor sixth	8:5	40

vibrations bring forth consonant sound. Beeckman was much more aware of this crucial gap in his theory than Descartes, yet both felt in the end compelled to appeal to certain *aesthetic* attributes of our sense perception, such as a preference for unity-in-variety, alternation-in-identity, and so forth. The invocation of these very principles makes it clear that no *material* principle had been found to connect outer regularity with inner pleasure. Thus the coincidence theory appeared not to be capable of solving the second fundamental problem of consonance either.

Nor was it capable of solving a host of derived lesser problems. The entire list, as discussed in *Quantifying Music*, comprises eight such problems; here I shall restrict myself to a brief sketch of some of these.

First of all, the coincidence theory implied, and at the same time seemed to solve, a problem that had never been stated before, namely that of providing a scale of degrees of consonance. Since an interval is the more consonant the more often the 'strokes' coincide, such a table is given simply by multiplying the terms of the frequency ratios of the consonant intervals.

Unfortunately this neat table (Table 2), that follows so inexorably from the coincidence theory, in its turn creates a great many new problems, all blissfully ignored by Galileo, but tackled head-on by Beeckman and Mersenne. For instance, in musical practice the status of the fourth as a consonance had become rather questionable, and certainly it was considered by most musicians to be less consonant than the major third. But Table 2 places the fourth higher in the hierarchy of the consonances than its rival, so how to account for that? Both Beeckman and Mersenne had to set up very involved arguments in order to get out of this difficulty. Also, the order given by Table 2 for the thirds and sixths is much more explicit than musical practice warranted. In particular, musical experience provided no reason for giving the major sixth a higher status than the major third. An even more compelling objection to be made against the above table is the following. If the table is valid, the intervals represented by the frequency ratios 7:4 and 7:5 should be considered consonances as well, the former in preference even to the minor third (as $28<30$), the latter preceding the minor sixth (as $35<40$). But these intervals had always been considered as very harsh dissonances (Kepler, in his geometrical approach to the problem of consonance, succeeded in getting rid of them through the inconstructibility of the heptagon). For adherents to the coincidence theory there were only three ways out of the dilemma: to declare it insoluble, to manipulate the table, or to pronounce the intervals with seven to be consonances after all. Again: each of these solutions found its defenders; virtually none tended to make things in the end less rather than more complicated.

Not only the scale of degrees of consonance, but the coincidence theory itself also ran into serious difficulties. One is the problem of *tempering*. No one who has ever attempted to tune a musical instrument can have failed to notice that in musical practice hardly any consonance sounds really pure; for instance, the major third, when struck on an equally-tempered piano, sounds quite perceptibly impure, though still consonant. Now 'tempering' means slightly altering the frequency ratio of the interval in question, e.g. from 5 to 4 for the pure major third to 5.04 . . . to 4 for the equally-tempered major third. But if consonance is supposed to result from frequently coinciding strokes, how, then, can tempered consonances be perceived as consonant, rather than as thoroughly dissonant?

The theory predicts that the incommensurable ratios of precisely the least-tempered intervals would entirely destroy the consonant effect: the smaller the deviation, the worse the effect. But in fact the ear appears to put up with small deviations very well, and the smaller the better. It notices only some beating, that increases with the rate of deviation from the true ratio. All this was noted only by the one man whose version of the coincidence theory was least affected by it, namely, Beeckman. All other adherents to the theory would have had to acknowledge that this was an insuperable objection, if only they had become aware of it.

Finally, the coincidence theory presupposes that the strokes of the two notes that make up the consonant interval start at exactly the same moment. But why should the strokes of, for example, the octave, proceed as shown in Fig. 2,

FIG. 2.

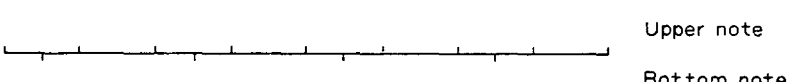

FIG. 3

rather than as shown in Fig. 3, and thus never coincide? No one, at the time, appears to have noticed this fatal objection to the coincidence theory. The first to make this point seems to have been Newton, who considered it sufficient reason for rejecting the coincidence theory.[12]

And so we might go on. Altogether a list can be drawn up of no less than eight new issues the coincidence theory was virtually powerless to solve. Add to this that it failed equally to provide satisfactory answers to the two original problems of consonance, and it becomes rather urgent to ask why in the world this theory was ever adopted by any scientist in his right senses. Yet in fact, within 20 – 30 years after it had been conceived of by Beeckman, the coincidence theory was to become the dominant explanation of consonance, entirely replacing the *senario* that had reigned supreme for half a century, and also defeating Kepler's geometrical explanation in the process. After the coincidence theory had been elaborated in the twenties and thirties by Beeckman himself, by Mersenne, and by Descartes, and had been proclaimed to the world by Mersenne (in 1636 – 1637) and by Galileo (in 1638), the theory was adopted by Gassendi, by Hooke, by Huygens, by Leibniz, and a host of others, while the only one to reject it, Newton, never even attempted to frame a better hypothesis. Not until the middle of the 18th century was the coincidence theory to be replaced by the overtone theory of Rameau and D'Alembert, which in its turn would be overthrown in 1863 by Helmholtz, whose explanation is basically still considered the best available. It is based on beating phenomena, and also preserves some defining features of the seventeenth century coincidence theory of consonance.

2.4. Why did the coincidence theory beat its rivals?

Let us now try to formulate as drastically as possible the paradoxical situation our historical survey has led us into.

Around 1640 three rival theories of consonance were available. One was the *senario*. For those who were willing to accept the explanatory principles on which it was based, it explained everything there was to explain. One detail (the minor sixth) had to be dealt with in an *ad hoc* manner; for the rest no problems remained. The second available theory of consonance was Kepler's. Despite all the many and important conceptual differences between Zarlino's and Kepler's theories, in a structural sense the two had much in common. Kepler's theory, too, took into account all available data. It, too, covered the entire problem, from the production of musical sound through various transmitting agencies up to and including the perceiving soul. It, too, left no

[12] H. W. Turnbull (ed.), *The Correspondence of Isaac Newton* (Cambridge, 1960), Vol. 2, pp. 206 – 207 (I owe this reference to Penelope Gouk).

questions open. In short, both the *senario* and Kepler's theory at least succeeded in explaining the problem of consonance in accordance with their own standards.

Now compare these two with the coincidence theory, which, according to *its* own standards, clearly failed to do so. Not only did the coincidence theory fail to solve the two original problems of consonance it had set out to solve; it also entailed no less than eight predictions that were all incompatible with the available data. What, then, made this seemingly hopeless failure carry the day for more than a century, in the presence of two rival theories which displayed none of its glaring deficiencies?

Admittedly this way of presenting the issue forces the historical picture somewhat. Some of the weaknesses of the coincidence theory were not perceived by anyone at the time, and others were solved in ways which may seem to us clearly *ad hoc*, but which were apparently capable of satisfying contemporary scientists. Yet such amendments do not really change the picture: the coincidence theory did leave open obvious gaps precisely on the most basic issues. *Kepler had anticipated so much, and had warned against adopting the coincidence theory for this very reason.* Why did no one else care?

Let us cautiously adduce some considerations that may serve to disentangle the apparent paradox. One important clue is provided by an inquiry into what our main personages themselves had to say on the issue. What objections were advanced by the adherents to the coincidence theory against their competitors? It is abundantly clear from their works that Zarlino-type number juggling no longer had much attraction for them. But, unfortunately, none of them ever took the trouble of pointing out explicitly what, in his view, was wrong with the *senario*.[13] Therefore we have to restrict ourselves to objections made against Kepler's explanation of consonance. Let us take a close look at two statements that seem to me particularly revealing:

> There is no number of motions or percussions of the air that is not commensurable to all other numbers of motions. Which is why I am surprised that Kepler has dared to introduce the comparison of [geometrical] figures with the Consonances, with the aim of deriving from them their number and their quality; which could have been tolerated if he had been content with comparing the said figures with the Consonances and the Dissonances by way of analogy and for entertainment.[14]

[13] Yet slightly more implicit criticisms abound; for some examples see *Quantifying Music* (Dordrecht: D. Reidel, 1984), p. 107 and note 8 to II.

[14] M. Mersenne, *Harmonie Universelle* (Paris, 1636 – 1637), 'Livre premier des consonances', prop. 33; p. 86.

> [I posit] this foundation, that [. . .] the sounds of [. . .] consonances actually cause, through motion, something inside us to change place, which is not done by Kepler.[15]

The first criticism is from Mersenne, the second from Beeckman. The point they are both making here is that Kepler's explanation of consonance was insufficient, not for inherent reasons, but because it relied on explanatory principles that were scientifically inadmissible. Mersenne is saying here that Kepler had explained consonance by appealing to geometric figures, rather than starting from the predominant feature of musical sound, which is *motion*.

This is a very relevant point for two distinct reasons. In the first place it calls attention to the fact that the coincidence theory was directly based on an elementary property of musical sound, namely the exact quantitative relationship between vibrational frequency and pitch that had just been discovered. A theory such as Kepler's, that simply passed by the most basic property of the phenomenon to be explained, could not be accepted, however ingenious it might otherwise be. Nor is this all. Motion, to Mersenne, meant much more than vibrational frequency. For the new scientific approach Mersenne was endorsing so enthusiastically, motion was the key to the analysis of many more phenomena of nature. Hence Kepler's, or, for that matter, Zarlino's explanations of consonance were already *a priori* untenable in that they had employed purely mathematical concepts that could not be used outside of the domain for which they had been designed in the first place.

A closely related point seems to be expressed by Beeckman's criticism. In explaining body – mind interaction, Beeckman is saying here, Kepler appealed to immaterial agencies such as 'Intellectual Harmony', rather than invoking the true scientific principle of the motion of particles of matter. Kepler's theory of consonance is unacceptable, not because it does not explain *consonance*, but because it does not *explain* consonance. Kepler's explanatory principles had been designed specifically for dealing with this one problem. The principles proposed by Mersenne and Beeckman, on the contrary, were being applied to various and sundry other phenomena of nature: to free fall, to the nature of light, to air pressure, and so on. Compared to the overall fertility of these new scientific principles it did not matter so much to what precise extent the coincidence theory of consonance could accomplish what it might ideally be hoped to accomplish. Whether or not it ran perfectly in all respects, its adherents felt themselves to be on the right track. The possibility of

[15] I. Beeckman, *Journal tenu par Isaac Beeckman de 1604 à 1634*, C. de Waard (ed.), (Den Haag, 1939 – 1953), Vol. 3, p. 69.

applying the same brand-new explanatory principles to other domains of science with at least some measure of success was there to prove it.

This, I think, goes a long way towards explaining the odd fact that so many seemingly obvious clashes of the theory with experience were simply not noticed at all, or at least not felt to be really consequential. Had the attention of our musico-scientists been directed towards an active search for countervailing evidence, they would not have failed to notice, for instance, that the coincidence theory presupposes a synchronicity of strokes that is, in fact, highly unlikely to occur. But they had no real motive to set out on such a search, precisely because they were confident that the coincidence theory rested on secure foundations that were proving their mettle in other domains of science.

And this was still not all. Precisely by answering all relevant questions and by accounting for all known relevant data both Zarlino and Kepler had failed to generate new questions which migh have led to the finding of new data, or to discovering that certain phenomena known of old were indeed relevant to the problem. But such service was abundantly performed by the coincidence theory. Precisely by leaving open so much it inspired the search for more. The *senario* and the theory of the regular polygons had not led to the discovery of the overtones, or to the realization of the importance of beats; the coincidence theory did. The *senario* and Kepler's theory had to be treated on a 'take it or leave it' basis; each had to be accepted wholly, or to be rejected wholly. One might or might not believe in the ontological significance of number; if one did not, there was no more room left for anything like the *senario*. One might or might not accept Kepler's view of the Creation; if one did not, such concepts as 'Intellectual Harmony' lost their entire *raison d'être*. In other words, for these theories, the basis of rejection was not really the one that has since become proper to the criticism of scientific theories. In modern science, criticism usually takes the form of pointing out that a theory (provided it is internally consistent) leaves certain relevant data unexplained, or that it clashes with relevant facts, or that it entails consequences that clash with other phenomena. Kepler's criticism of the coincidence theory had been of such a nature. But Kepler's own theory could not be criticized in a similar way. Much later it was discovered to be faulty in that there are more constructible regular polygons than Kepler had imagined. Yet it is clear that Gauss' discovery of the constructibility of the 17-gon was not destructive of Kepler's theories of harmony in the normal way. Much more, in judging Kepler's theory, depends on whether or not one shares Kepler's highly personal, partly Platonic, partly Christian view of the Creation, which we have seen to underlie his entire theory of harmony. Therefore, the basic distinction to be made is that, while conforming to the scientific standards of their own time, these very standards permitted both Zarlino's and Kepler's theories of consonance to be essentially metaphysical rather than scientific in the modern, post-Galilean sense.

This is true of Zarlino's theory in a very obvious way: the way he operated with numbers had rapidly become incompatible with the scientific methods and approaches that came to the fore at the beginning of the seventeenth century. But it was also true, in a much more subtle way, of Kepler's geometric concepts. Kepler's explanation of consonance was unlike more common metaphysical constructions in that it made much use of refined scientific concepts. Precisely in this wonderful mixture lies the enduring fascination exerted by his work. Yet it was not really scientific in the sense this word was acquiring in the course of precisely the period we are dealing with.

As against all this, the coincidence theory could be accepted on a tentative basis. Its adherents committed themselves, not to a fixed theoretical structure, but rather to a research program. To either Zarlino's or Kepler's theories one could respond only with a 'yes' or a 'no'; neither provided a motive for subsequent inquiry. But the coincidence theory did, precisely because it was such an inspiring mixture of right and wrong. There was no decisive reason for considering its numerous deficiencies as so many falsifying instances. Rather the theory held out the prospect of turning its numerous shortcomings into so many confirmations. And this is precisely what, in the course of the seventeenth century, its adherents set out to do.

III. Four metatheories of science historically tested

Let the above interpretation of one particular historical case of theory replacement be granted some measure of plausibility. Then our next business is to assume successively four different, theoretically given patterns of theory replacement; to investigate what our actual historical event would have looked like in each of these four cases; and to weigh the resulting differences and correspondences. As announced in the Introduction, I choose the following metatheories: the inductivist view, Popper's 'falsifiability' account, Kuhn's schema of paradigms and revolutions, and Feyerabend's 'theoretical pluralism'. Two of these, I think, are fairly obviously at variance with the historical facts of our case.

To begin with, the inductivist view that science grows through the accumulation of ever more facts generalized into ever more-embracing theories is clearly incompatible with our findings. The coincidence theory was certainly not occasioned by new facts having been found in the first place (although, indeed, it served to generate new facts; but that is a different issue). If the number of facts explained were a valid criterion for choosing between theories, the coincidence theory, which failed even to solve the core problems of consonance, would have lost the battle right from the start.

True, the inductivist could reply that I have said myself that the *senario* and Kepler's theory did not properly belong to science. Therefore, only one scientific theory of consonance was available, and the question of why it became the reigning theory appears to be spurious by definition. But this goes a little too far. When discussing the 'unscientific' elements in Zarlino's and Kepler's theories of consonance I took care to qualify: they only fall short according to the new standards that came to the fore in the course of the Scientific Revolution; according to Renaissance standards both make fine, scientific theories. For the inductivist the only way out is to let the 'history of the inductive sciences' start with the Scientific Revolution, which seems to me a very unhistorical thing to do. Of course, the distinction between modern and pre-modern science plays an important part in the discussion of our historical case, and will continue to do so in the theoretical analysis. But the point is that one needs criteria for making the distinction, rather than achieving it by definition. The inductivist account, however, fails to provide these. Even if we were to take the exclusion of the *senario* for granted, the coincidence theory would seem a very unlikely candidate to start the inductive science of music with: as we have seen, there was scarcely any musical fact that it really managed to account for (as, again, Kepler was urgently aware). But is not covering the known facts the first and foremost requirement for an inductive theory? So the inductivist would wind up stating that the two theories that did cover the known facts were unscientific, whereas the one that did not was the only scientific one. But this is incompatible with the inductivist criterion itself.

The other metatheory that is far from easily reconciled with our historical case is Feyerabend's. In his view *each* theory sets its own standards, and it would be useless to look for external criteria to choose between one theory and another. The business of choosing between theories is irrational, and subject ultimately to matters of taste and fashion. Now if it were indeed true that no objective, rational criteria are applied by scientists in opting for one theory rather than for another, then it is not clear how such a clear-cut pattern of theory choice as we have observed in our historical case could possibly have come about. To begin with, the rejection of the *senario* and of Kepler's theory was universal. No scientist who took part in the Scientific Revolution ever adopted Zarlino's theory of consonance; even Gassendi, who followed Zarlino closely in every other aspect of musical theory, replaced the *senario* by the coincidence theory.[16] And no student of the science of music has ever adopted Kepler's explanation of consonance. Obviously the scientists involved did so because they applied certain external criteria, and it is our business to identify

[16] P. Gassendi, *Opera Omnia* (Lyon, 1658), Vol. 5, pp. 643 – 645.

these, rather than to evade the issue by denying their existence. Yet that is what would be done by ascribing this universal preference to strangely corresponding 'tastes' or to a common urge to run after the fashionable. The rejection of the *senario* and of Kepler's theory was achieved before the coincidence theory even had had a chance to become the latest fashion. And as for 'tastes', these differed widely, from Galileo's mathematical and experimental approach, through Mersenne's sceptical experimentalism, to Beeckman's and Descartes' adherence to the mechanical philosophy. Nevertheless, they all concurred in opting for one and the same theory of consonance. Nor were they lacking in criteria for doing so: our quotations from Mersenne and Beeckman, in particular, have made it quite clear what external criteria they applied in rejecting the approach of their predecessors. These criteria derived from the place the category of motion and the corpuscular hypothesis had acquired in the explanation of the phenomena of nature. As such these were at the heart of what the Scientific Revolution was about, and whether or not they are capable of convincing us, they are certainly based on considerations that go far beyond what at the time could qualify as 'taste' or 'fashion'.

The two remaining metatheories under discussion here appear to make more sense in view of our problem, Popper's even more so than Kuhn's. Popper's metatheory has its origin in his interest in two basic, and related problems: to find out what makes science grow and what distinguishes scientific from non-scientific (in his parlance: 'metaphysical') statements. His answers have centered on the notion of falsifiability. That is, what distinguishes scientific from non-scientific theories is their being amenable, in principle, to falsification. For a theory to be accepted as scientific it must be possible to spell out under what specific conditions it will be given up. As it is always possible to evade threatening falsification by adopting some *ad hoc* hypothesis in order to save the theory, a conscious methodological decision on the part of the scientist is required to ensure that science will continue to grow. In Popper's view, the progress of science lies in the continuous process of advancing ever bolder theories that survive ever harsher empirical tests, until they are refuted as a result of an even harsher test and replaced by an even bolder theory. 'Bold' in this connection means 'forbidding many possible states of affairs'. 'Metaphysical' theories, in Popper's view, are compatible with any conceivable state of affairs; there is no fact in the world that might be used to refute them. But in science, the bolder a theory is, the more possible states of affairs are ruled out by it, and thus the more it says about our world.

Let us suppose for the moment that this metatheory of science is correct. What, then, would have been the truly scientific decision to make for a musico-scientist, confronted around 1640 with our three different theories of consonance? First of all he would have been perfectly justified in dismissing

Zarlino's *senario* as unscientific, since it cannot be refuted. Second, he would have perceived the essentially metaphysical nature of Kepler's theory behind its numerous truly scientific analyses, and therefore he would have discarded it as well. Third, he would have adopted the coincidence theory on a tentative basis, and have tried to derive testable consequences from it.

So far Popper's metatheory fits the historical facts admirably, for this is precisely what we discovered indeed to have happened. Also let me emphasize, before proceeding to the more questionable part of our metatheoretical testing procedure, that this in itself is no mean achievement, since none of the competing metatheories has even come close to offering an explanation, on a logical basis, of what enabled the coincidence theory to beat its rivals. Nor, to be sure, does Kuhn's metatheory, for the crisis in puzzle-solving that, in his view, precedes any revolutionary change of paradigms is conspicuously absent from the events that led to the adoption of the coincidence theory. Its originators surely never struggled with puzzles that could not be solved within the framework of the *senario*; rather they tacitly dismissed it. The *senario* was overthrown as a result of a shift in explanatory principles; it was rejected because it was incompatible with the new principles. The trouble with the *senario* was precisely that it did *not* give rise to 'puzzles'; only its being dismissed paved the way for posing new and fruitful scientific problems.

Nevertheless, Kuhn's metatheory will appear to contain some quite helpful elements when we proceed now to the second stage, and inquire what, according to Popper's logic of scientific discovery, our scientist was to do once he had adopted the coincidence theory, and, hence, was to confront its implications with musical reality.

Here we tread disputed epistemological ground. It has often been held against Popper that scientists do not really submit to falsifying evidence, let alone actively seek it. If it were indeed Popper's view that any apparent clash of the empirical consequences of a theory with some observation statement would constitute an actual falsification of the theory, then his metatheory would clearly be at odds with what happened in the case of the coincidence theory. As we have seen, this theory clashes with no less than eight of such contrary facts, and even if allowance is made for the ones that were not noticed at the time, enough of them remain to call into question Popper's alleged account.

But this is demonstrably *not* Popper's view on the actual process of falsification. Take, for instance, what he wrote in 1974, in an attempt to combat this particular Popper legend:

> . . . no test of any theoretical statement is final or conclusive, and [. . .] the empirical, or the critical, attitude involves the adherence to some 'methodological rules' which tell us not to evade criticism but to accept refutations (though not too

> easily). These rules are essentially somewhat flexible. As a consequence the acceptance of a refutation is nearly as risky as the tentative adoption of a hypothesis.[17]

In fact, from 1934 on Popper has devoted much effort to pointing out under what conditions a theory should be considered definitely falsified. Broadly speaking, this is the case when hypotheses introduced in order to save the theory tend to reduce the empirical content of the theory, that is, tend to reduce the range of possible facts forbidden by the theory. An example is provided by the celebrated occasion when Newton's theory of gravitational attraction was threatened with falsification because of certain incompatible phenomena observed in the orbit of Uranus. In Popper's view the hypothesis that this was due to the calculable presence of another, unknown planet nearby (later identified as Neptune) was a quite admissible one, because it increased the testability of the original theory. Therefore, theories should not be considered falsified until it appears that they can only be saved by introducing *ad hoc* hypotheses that tend to make them irrefutable.

Now the point I want to make here is that even such a much more sophisticated metatheory of the falsification of theories fails to account for what happened with the coincidence theory. The hypotheses introduced in order to save the coincidence theory from refutation on the basis of, for instance, the anomaly of the fourth (the fact, that is, that in musical practice the major third is treated as more consonant than the fourth, whereas the theory would have it the other way round) *were* clearly of such an *ad hoc* nature. If Popper were entirely right here, the musico-scientists who originated or adopted the coincidence theory — and they were among the very best scientists of their time! — would have had to give it up in face of the fact that it could only be saved on an *ad hoc* basis. Since no scientific rival theory was available the truly scientific thing for them to do would have been to declare the coincidence theory wrong, and to use it only as a kind of working hypothesis in the search for a better theory. Yet this they did *not* do. Why not?

To the Kuhnian reader the answer to this question will be obvious. For we seem now to be dealing with precisely the situation that is typical for 'normal science' once it has started afresh after a new paradigm has been adopted. According to Kuhn a new paradigm never promises to provide beforehand the solution to all conceivable problems in the field in question. Rather it functions as the framework within which these problems, anomalies, etc. may be tackled as 'puzzles', i.e. as the routine business of the scientific community

[17]K. R. Popper, 'Intellectual Autobiography', in *The Philosophy of Karl Popper*, P. A. Schilpp (ed.) (La Salle, Illinois, 1974), p. 79 (cp. K. R. Popper, *op cit.* note 3, pp. 986–987).

that has adopted the paradigm. And thus the Kuhnian will have read with great satisfaction my account of how Beeckman, Mersenne, Descartes, Huygens *e tutti quanti,* immediately after adopting the coincidence theory set themselves tasks of solving the anomaly of the fourth, the problem of tempering, the objection arising from the absent synchronicity of the strokes that according to the theory make up the consonant intervals, and so on.

And yet this orthodox Kuhnian 'rational reconstruction' will not quite do, for two distinct reasons. The first is that, in historical fact, so many of these 'puzzles' were not treated as puzzles: only the anomaly of the fourth was tackled by a great many adherents to the coincidence theory, whereas most other 'puzzles' were either totally ignored or just noticed by one or two, at best, of all those quite numerous adherents to the theory. And second, even more importantly, there were not only these eight derived problems, or 'puzzles'; the situation was much more serious in that, as we have seen, the coincidence theory failed even to provide a solution to the two classical core problems of consonance. Not even the Kuhnian concept of paradigm can be stretched so far as to provide an explanation of why a 'paradigm' that, like the *senario, was* able to solve the fundamental problem at hand, was replaced by a new 'paradigm' that was not. Against this the Kuhnian might argue that the entire point is irrelevant because the *senario* never belonged to science, and, hence, what happened to it is unrelated to the problem of how scientific progress comes about. But Kuhn is far too much of a historian thus to exclude pre-modern science from science altogether;[18] and in fact his overall schema would not yield any applicable criterion for doing so.

We conclude that neither Popper's nor Kuhn's metatheories of science appear to be capable of explaining wholly the behavior of our musico-scientists in rejecting both the *senario* and Kepler's theory and adopting, instead, the coincidence theory. The *historical* explanation of this apparent paradox has already been given in the above, and it reads: our musico-scientists acted as they did, because the coincidence theory was based on explanatory principles that operated successfully in many other branches of science. A new comprehensive research program had become available, according to which the phenomena of nature had to be analyzed in terms of matter and motion. This broad research program met with so much initial success throughout the various domains of science that it prevented the rejection of a particular theory in one particular field that probably would have been considered hopeless had it stood on its own.

It must be acknowledged at once that this answer is not entirely foreign to Kuhn's metatheory. On the one hand, we have now seen that Kuhn's claim

[18] Th. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1970) (in particular Ch I – II).

that 'probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis'[19] is wide of the mark in our case (there having been neither a crisis of the older paradigm, nor this ability of the new one to solve the problems left open by its predecessor). A conceivable way out of the difficulty would be for the orthodox Kuhnian to say that the concept of paradigm should be given much wider scope in that not the *senario* as such, but rather the entire set of Renaissance explanatory principles served as a paradigm to the scientific community at the time. According to such an hypothetical view not the victory of the coincidence theory, but the entire Scientific Revolution (or at least its first stage) would yield the proper framework to account for what happened in our case of theory replacement. Now this view indeed makes eminent sense; but one should be aware that it is incompatible with the mainstream of Kuhn's argument, since Kuhn unambiguously describes quite a few cases of theory replacement in the course of the Scientific Revolution as separate, individual examples of paradigm shift (for instance, Kepler's work on planetary orbits, or Galileo's new view of pendular motion, or the mechanical philosophy, or seventeenth century optics). And in fact he is forced by his own position to do so, because he aims at establishing an unchanging pattern of scientific progress, whereas appealing to the Scientific Revolution, which was the one unique historical event that divides much of traditional science from much of science in the modern sense, would not yield the kind of generalizable structure he is looking for.

Despite all this, there are indeed some fleeting passages in Kuhn's enumeration of reasons for the replacement of one paradigm by another that border on the historical account given in the above:

> Sometimes the looser practice that characterizes extraordinary research [i.e. research in a time of paradigm shift] will produce a candidate for paradigm that initially helps not at all with the problems that have evoked crisis. When that occurs, evidence must be drawn from other parts of the field as it often is anyway.[20]

Again, this one remark is scarcely allowed by its author to influence his overall schema of how progress in science comes about. Yet it does remind us of one important, though under-exploited, advantage of Kuhn's concept of paradigm, which is that it need not be restricted to theories, or to coherent sets of theories, but may also cover those fundamental assumptions regarding what

[19]Th. S. Kuhn, *op. cit.* note 18, p. 153.
[20]Th. S. Kuhn, *op. cit.* note 18, p. 154.

science is and how it should be done that, whether scientists are aware of it or not, underlie their theories.

I do not think that a notion comparable to this is to be found in the numerous discussions Popper devoted to the problem of why scientists prefer one theory over another;[21] but one of my aims in the next and final section of this paper will be to argue that such an idea can easily by incorporated into his overall account of scientific progress without destroying any of its main points.

IV. Summary and conclusions

Let me first sum up how far the argument by now has got us. I have argued four successive points. I have demonstrated how strange it is, at first sight, that the coincidence theory was generally favored at the expense of its two rivals. I have tried to explain the apparent paradox by showing that the two rivals were metaphysical rather than scientific theories in the modern sense, and that the coincidence theory was based on explanatory principles that were proving their mettle in many domains other than the science of music. I have pointed out that all this is incompatible with either an inductivist or a 'pluralist' metatheory of science. Finally I have shown that the first part of my historical explanation fits in exactly with Popper's metatheory, while the second part has more in common with elements derived from Kuhn's. Even though Kuhn's metatheory has appeared to be incompatible with the results of my research insofar as the replacement of theories is concerned, I do believe that it fills an important gap left open by Popper's logic of falsification procedures.

On the basis of this result we may perhaps formulate a tentative hypothesis which states that in the process of deciding whether or not a given theory should be considered falsified an important element is the extent to which the explanatory principles that underlie the theory in question are themselves part of a larger, successfully operating research program.

At this point we might well stop, leaving both the Kuhnian and the Popperian reader somewhat dissatisfied with the mixed result thus reached. Yet in the following I wish to enhance their dissatisfaction even more by asking now what reasons there could be for either to care at all about this result of our metatheoretical testing procedure. Why, the Kuhnian may well ask, why

[21] See in particular K. R. Popper, *The Logic of Scientific Discovery* (London, 1959), pp. 108 – 111 (Section 30), and K. R. Popper, *Objective Knowledge. An Evolutionary Approach* (Oxford, 1972), I, sections 7 – 9.

should I give up Kuhn's brilliant, convincing, and historically reliable schema of how science progresses just because one crazy little Renaissance theory, the *senario*, was not quite capable of giving rise to crisis before being discarded for good by men of the stature of Kepler and Galileo? And why, I hear the Popperian within myself asking, why should I be prepared to give up a philosophy that has led to so many brilliant and important insights into such various subjects as, for example, democracy and totalitarianism, the nature of rationality, the writing of history, Plato, Marx and Freudianism, just because three centuries ago a few scientists failed, in their behavior towards one particular theory, to comply down to the last detail with the fine structure of Popper's ideas on falsification procedures?[22] Or, formulated more broadly, is it reasonable to expect a metatheory of science to be in exact conformity with every single case of scientific progress in the entire history of science? Does this not make all these metatheories much too easy prey for counterinstances to be dug up by the diligent historian? Aren't we asking a bit too much from the philosophy of science if we require it to provide us with explanations that aim to cover the performance of, say, a few isolated Greek philosophers of nature just as well as the interactions of twentieth century scientists who operate in such a different scientific climate; socially, culturally, financially, institutionally, so entirely different surroundings? Is it *a priori* reasonable to expect that, whereas over the past twenty-five centuries so many things have changed so fundamentally in science, yet the overall structure of the way it progresses should have remained the same?

I do not feel capable of giving a definite, let alone definitive, response to these questions; yet the way I just phrased them may make it clear that I tend to answer them in the negative. To my historian's mind it seems really too rash to suppose that one and the same model could possibly account for all scientific progress, irrespective of time, place, and circumstances. In recent years such models have been cropping up at an alarming rate, each providing a hasty generalization supported by a few historical pet examples.[23] It seems to me that these attempts at improving on existing models like Popper's and Kuhn's really tackle the problem from the wrong end, and that it would be much better for philosophers of science to work along slightly more inductive lines, by investigating in depth a few historical cases from one and the same period, cautiously looking for tenable generalizations, and carefully attempting to apply these to cases from adjacent periods, using changes over time in the overall scientific and social setting as variables that might help to

[22] Popper has briefly discussed the testability of his own metatheory in his 'Replies to my Critics' (*op. cit.* note 3), p. 1010; see also his *Conjectures and Refutations. The Growth of Scientific Knowledge* (London, 1965), pp. 197–200.

[23] To mention just one example: C. Dilworth, *Scientific Progress. A Study concerning the Nature of the Relation between Successive Scientific Theories* (Dordrecht, 1981).

explain the no doubt limited applicability of the original model.[24] I now intend to do something like this at the end of the present paper by asking why it is that both Popper's and Kuhn's models turned out to fit our historical case to some extent, but neither of them entirely so? We have seen that, partly, Popper succeeded where Kuhn failed: the *senario* was not overthrown as a result of a crisis in puzzle-solving, but rather because it was felt (together with Kepler's theory of consonance) to be metaphysical instead of being open to really scientific discussion. It seems to me that this result need not at all be universally valid, since in fact it is closely linked up with the period in question, that of the first stage of the Scientific Revolution. This was precisely the unique turning point in history when in a number of key sciences such ultimately metaphysical explanatory models as Aristotelian qualities, Renaissance sympathies and antipathies, number mysticism and the like, were replaced by modes of explanation that have ever since been regarded as really scientific. Thus the period chosen for our historical case is precisely the one to which Popper's demarcation criterion between science and non-science might *a priori* have been expected to be particularly applicable. To be sure, the fact that it does fit the case so neatly is no mean triumph for the validity, or at least the fertility, of Popper's falsifiability criterion (which I continue to regard as one of the most important and fruitful ideas of our time); yet clearly it is not to be expected that the criterion is equally applicable, if at all, in historical cases where the issue was indeed the choice between two theories of equal scientific standing in the modern sense. Popper's success in this respect also accounts for Kuhn's failure: there needed not arise a crisis out of the *senario*, because the crisis that gave rise to the Scientific Revolution was of a much more general nature. As soon as the explanatory modes on which the *senario* implicitly rested were felt to be unsatisfactory, the implausibility of the *senario* itself became at once apparent.

We have also seen that the new explanatory model that gained currency in the first decades of the seventeenth century, that of particles of matter in motion, in encompassing the coincidence theory as one of a great many applications of the model, *ipso facto* guarded this new theory against the

[24] Thus I agree with Marxist critiques of the metatheories discussed here insofar as these metatheories are based on 'an unargued assumption, namely, that there is a single, timeless, correct scientific method' (A. F. Chalmers, *What is this Thing called Science?* (Milton Keynes, 1978), p. 141). But the approach suggested by Chalmers in order to draw the positive consequences from this point of view seems to me empty insofar as his introduction of the concept of 'scientific practice' is concerned (same book, IX and XI), and historically unreliable insofar as some particularly crude form of 'historical materialism' is invoked to do the job (XII). My appeal to the overall social setting does *not* imply any *a priori* view on what parts of it are the only relevant ones: this has to be discovered afresh for every specific case (as I have demonstrated in *Quantifying Music*, Section 7.2.).

falsification it so obviously seemed to deserve by its own merits. Here Kuhn's metatheory turned out to provide more room for an explanation of our scientists' behavior than Popper's. This is so, I think, because Kuhn's account of scientists' reasons for deciding to choose one particular theory as the framework for their research in the future is much more historically oriented than Popper's.[25] Popper tends, rather, to formalize these matters somewhat, looking for 'degrees of verisimilitude', and the like, as means towards a more objective basis for theory choice. But the degree of verisimilitude of the coincidence theory was very close to zero, hence objectively there should have been no reason for pursuing it further. The value of Popper's formalisms is certainly made more relative by an appeal to the overall situation of science at the time in question; yet I do not think that such an element of historical relativism is entirely foreign to his overall model. After all, the history of science abounds with actual falsifications,[26] and my only point here is that the question as to under what conditions these occur cannot be solved formally, but requires a mode of analysis that regards any given historical period as ultimately unique.[27] In this particular sense I do indeed believe that the philosophy and the history of science have much to contribute to each other.

[25] Th. S. Kuhn, *op. cit.* note 18, XII.

[26] Kuhn doubts this (*op. cit.* note 18, p. 146); but just consider Kepler's celebrated rejection of his own *'hypothesis vicaria'* for the orbit of Mars because of a difference of 8 min of arc between a predicted observation and the one actually made by Tycho. (This example is conspicuously absent from the historical treasures tapped by Kuhn in *The Structure of Scientific Revolutions*.)

[27] This statement is not intended as an expression of the belief that any given historical period is governed by one specific historical law; a belief that is combated so convincingly in K. R. Popper, *The Poverty of Historicism* (London, 1957). My point is that generalizations of cases of scientific progress should first be restricted to one definite period of time and duly linked to the overall situation of science in that period, before being generalized further. One example of such an approach is provided by N. J. Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Den Haag, 1984).