METHODS AND AIMS IN THE EUCLIDEAN *SECTIO CANONIS*

For the writers of the imperial period, music theory presented a sharp dichotomy. One might be an Aristoxenean, or one might be a follower of the *mathematikoi* or Pythagoreans. The methods and doctrines of the two schools were thought of as radically incompatible with one another;[1] and it is true that from the fourth century B.C., when their respective doctrines were first formally articulated, members of each tradition held views which their opposite numbers denied. But their disagreements are easily misunderstood. I have argued elsewhere that Aristoxenus' attitude to the Pythagoreans has sometimes been distorted.[2] Here, in discussing the *Sectio*, which appears to be the earliest continuous treatise surviving from the other side of the fence, I shall try to show some of the ways in which its objectives differ from Aristoxenus', and thus the sense in which certain major disagreements between them reflect an oblique rather than a direct confrontation. I hope also to show that further study of this rather arid looking work for its own sake would repay both musicologists and philosophers.

I. The Authenticity of the Treatise, and the Arguments of the Introduction

The *Sectio*, as we have it, contains a short introduction, and twenty propositions, set out in the manner of the theorems of Euclid's *Elements*. Whether or not they are by Euclid himself, there is no good reason to assign at least the first eighteen propositions to a date later than Euclid's, or to suggest that they are the work of more than one hand.[3] Suspicion has occasionally been directed at the last two propositions. As I shall explain later, they presuppose a form of the scale different from that which is demanded by propositions 17 and 18. But I shall suggest that this difference is to be accounted for otherwise than by a difference of author or date. For the present I shall proceed as if the whole set of twenty propositions formed a unitary essay of the late fourth century.

As von Jan puts it, however, *alia vereor ne sit condicio prooemii*.[4] But though I endorse von Jan's judgement my reasons are not the same. The Introduction begins with an account of the dependence of sound on movement. More numerous and 'closely packed' movements generate higher sounds, less numerous and more widely spaced ones lower sounds. Sounds, the writer infers, are composed of parts, 'since they attain the required pitch through addition and subtraction'. Sounds of different pitch must then be capable of being related to one another by numerical ratios, since all things composed of parts can be so related. According to von Jan, this section is clear and well argued 'ut suspicio non oriatur'. But its clarity is not the important issue. The serious question is whether it has a role to perform in the sequel, and if so, whether it performs it adequately. I shall argue later that it does indeed have a function; but its fulfilment of it involves an anomaly. The present passage implies that the ratios involved in differences of pitch are ratios of numbers of movements. But the theorems do not mention movement: they relate ratios directly to lengths of strings. This distinction generates another. A vibration theory, such as that of the Introduction, must assign larger numbers to higher pitches and smaller ones to lower: but the practice of the theorems is the other way round, as is demanded by a scheme of measurement based on string-lengths. This anomaly may seem insufficient to justify suspicion.[5]

[1] See e.g. Porphyry's *Commentary on Ptolemy's Harmonics*, 23.24–31, 25.3–28.27 (Düring).

[2] 'οἱ καλούμενοι ἁρμονικοί: the predecessors of Aristoxenus', *PCPS* cciv (1978) 1–21.

[3] For a brief account of the controversies about authorship and date, see W. Burkert, *Lore and Science in Ancient Pythagoreanism* (Harvard 1972) 375 n. 22, and the references given there.

[4] K. von Jan (Carolus Janus), *Musici Scriptores Graeci* (Leipzig 1895) i 117.

[5] *Cf.* Burkert (n. 3) 379–83, esp. 380 n. 47; and some intriguing speculations on the development of the Pythagorean conception of an interval in A. Szabó, *The Beginnings of Greek Mathematics* (Dordrecht 1978) 107–34.

So it is: it is less a sign of separate authorship than of the existence of a lacuna. What is missing is a way of relating the primary ratios of movements to the reversed ratios of the lengths of strings. We should have expected the careful author of the theorems at least to make explicit the linking proposition that longer strings yield in a given time proportionally fewer movements. The fact that he does not suggests the fragmentary nature of this Introduction, of which we shall shortly find more evidence.

The author goes on to present, very briefly, a classification of ratios. All ratios are either multiple ($n:1$), superparticular ($n+1:n$), or superpartient ($n+m:n$, where m is greater than 1). In an obscure sentence to which I shall return immediately, he indicates that the first two classes of ratio have something in common, not shared by the third. He then passes at once to concords and discords. Concordant sounds are 'those which form a single blended union out of their two elements', and discordant sounds those which do not. He concludes that it is therefore to be expected that the ratios of concords will all be either of multiple or of superparticular ratio, in view of the common feature of these classes of ratio indicated in the obscure sentence mentioned above.

It is the obscurity, brevity, and argumentative feebleness of this passage which excites von Jan's suspicions. Though I am sure that he has misconstrued the argument, I agree with his conclusion, that this passage is not as it stands the work of the meticulous author of the theorems. But the passage contains, in however truncated a form, the gist of something essential to the subsequent argument. It is explicitly taken as a premise of certain of the theorems: further, as I shall explain, the methodological framework of the whole work would collapse in its absence.

Let us now try to establish the sense of the argument. On von Jan's interpretation the sentence I have stigmatised as 'obscure' would read: 'of these ratios, the multiple and the superparticular are spoken of together under a single name'.[6] The argument that concords should always be of one or the other of these classes of ratio would then be that since the notes of any concord form together a single union, concords should all belong to a class of ratios put together under a single classification. Von Jan complains that the author does not tell us what the single name covering the two classes of ratio is: but this is scarcely the point, for the argument, whether it named the single name or not, would be not so much feeble as ludicrous. Assuming that these ratios' possession of a common name is taken to point to their possession of a common feature, some semblance of an argument for assigning concords to these classes might be generated from the observation that all concords have something in common; though even that would be worthless without some way of relating the common feature of concords to the common feature of the ratios. But what is emphasised here is not that a certain feature is common to all concords: it is that each individual concord is itself in a certain sense a unity. And from the premises (a) Multiple and superparticular ratios are brought together under a single name, and (b) Each individual concord forms of its elements a single unity, nothing whatever can be made to follow.

Fortunately there is another way of construing the obscure remark about the ratios. In place of the translation given above I suggest the following, as an equally justifiable rendering of the Greek. 'Of these numbers, those standing to one another in multiple and superparticular ratio are related to one another under a single name.' Doubtless this is still obscure, but what it means, I think, is simple enough. Whereas, in Greek, superpartient ratios such as 5:3 can only be designated by compound expressions like 'five to three', there is a one-word name for every ratio in the other two classes. Multiple ratios are straightforward: the ratio 2:1 is called 'double' (*diplasios*), 3:1 'triple' (*triplasios*), and so on. Of the superparticulars, the ratio 3:2 bears the special name '*hemiolos*', that is 'half-whole', while all the others bear names generated by adding the prefix '*epi-*' to an ordinal adjective. Thus the ratio 4:3 is '*epitritos*' and 9:8 is '*epogdoos*'; and names for all the others are constructed similarly. If this is what our author means, it explains

[6] *Τούτων δὲ οἱ μὲν πολλαπλάσιοι καὶ ἐπιμόριοι ἑνὶ ὀνόματι λέγονται πρὸς ἀλλήλους*: p. 149, ll. 14–16 in von Jan (n. 4).

why he does not tell us what the 'single name' is: there is no single name for all these ratios (or if there is—von Jan suggests '*kreitton*' on the authority of Porphyry—it is irrelevant), but each one of them individually, unlike each superpartient ratio, bears a single name of its own. And we now have something which—though still feeble enough—does at least have the smell of an argument about it. Concords are unifications of two elements. Ratios of certain classes 'unify' the two numbers which are their elements into something single, designated by a single name, instead of leaving them distinct and unblended, as in the compound expression 'five to three'. We should expect the ratios of concords, since concords present themselves to us as unities, to be capable of bearing unitary names. Hence we should expect them to be multiple or superparticular, not superpartient, since superpartient ratios cannot be expressed except as relations between pairs of distinct and independent terms. It is not much of an argument, but equally it is not altogether nonsense.[7]

By way of a final preliminary, it is worth mentioning briefly the most general grounds for seeing this treatise as a representative of the 'Pythagorean' tradition in harmonic theory. According to the later compilers and commentators, there are two major distinctions between the Pythagorean and Aristoxenean approaches. They are (a) that the former treats intervals as ratios of numbers, whereas the latter treats them as distances on a linear continuum; and (b) that the former gives priority to reason over sensory perception as a criterion of musical judgement, while the latter makes perception the final court of appeal. This second point is hard to pin down precisely: I shall return to it later, and try to indicate something of its basis and origins. The first point, however, is unequivocal. The Pythagorean theorist operates with an entirely different conception of musical 'space' from his Aristoxenean counterpart, and his allegiance is immediately indicated by his use of the language of ratios. This alone is enough to place our present treatise, at least from the point of view of the later theorists, squarely in the Pythagorean camp.[8]

II. The Theorems: Exposition and Discussion

There is no space here to expound and discuss in detail the twenty theorems which constitute the bulk of the work. But a brief summary and a few comments may be helpful.

The first nine propositions (P1–P9) are designed to establish certain truths about ratios, without reference to musical phenomena. Their conclusions are as follows. P1: If an interval of multiple ratio is taken twice, the interval so formed will also be of multiple ratio. P2: If the whole interval made up of a given interval taken twice over is of multiple ratio, the given interval will also be of multiple ratio. P3: There is no mean in an interval of superparticular ratio, and neither one nor more than one number can divide it proportionally.[9] P4: If an interval which is not of multiple ratio is taken twice, the whole interval resulting will be neither of multiple nor of superparticular ratio. (This follows directly from P2 and P3.) P5: If a given interval taken twice makes a whole interval which is not of multiple ratio, the given interval is not of multiple ratio either. (This follows from P1.) P6: The interval of ratio 2 : 1 is composed of the two greatest

[7] Burkert (n. 3) 383–4 interprets the passage as I do, but without explanation or discussion. On the reasons behind the Pythagorean acceptance of the principle which assigns concords to these two classes of ratio, see also Ptolemy *Harmonics* 11.8–20 (Düring).

[8] That it was already in the fourth century a definitive mark of a distinct school of thought is clear from e.g. Aristoxenus *El.Harm.* 32.24 and Theophrastus *fr.* 89 (Wimmer) = Porphyry *op. cit.* 62.1 ff. (Düring). The Aristoxenus passage also explicitly associates the use of the language of ratios with a special attitude to the deliverances of reason. The fullest discussion of the wider implications of the two positions is Ptolemy *Harm.* 19.16–21.20.

[9] That is, if two terms, B and C, are in the ratio $n+1:n$, there is no term D, between B and C, such that $B:D=D:C$, and no series of terms $D, E, F, \ldots, N$, such that $B:D=D:E=E:F=\ldots=N:C$. The proof given is of some historical interest, being substantially identical to that ascribed by Boethius to Archytas. See T. L. Heath, *A Manual of Greek Mathematics* (Oxford 1931), republ. as *Greek Mathematics* (Dover N.Y. 1963) 136–7; and Burkert (n. 3) 442–7.

superparticular intervals, the ratios 3:2 and 4:3. (Here the author gives two proofs, one geometrical, the other arithmetical.) P7: The interval of ratio 2:1 together with that of ratio 3:2 make up the interval of ratio 3:1. P8: If the interval of ratio 4:3 is subtracted from that of ratio 3:2, the remainder is an interval of ratio 9:8. P9: Six intervals of ratio 9:8 are greater than one of ratio 2:1.

Two points about these theorems are worth making. First, there are three occasions, in P2, P3 and P9, where the author relies on proofs or principles not demonstrated in the present work. The demonstrative validity of the theorems cannot then be fully assessed except against the background of a prior mathematical system or course of education. Secondly, it should be noted that there are three ratios central to the scheme, 2:1, 3:2, 4:3; and that whereas we are given in P6 the product of 3:2 and 4:3, and in P7 that of 2:1 and 3:2, the product which would complete the sequence, that of 2:1 and 4:3 (which is of course 8:3) is not mentioned. As we shall see, the omission is significant.

The next group of propositions use the mathematical results so far obtained, but import independent musical data as well; and their conclusions relate to musical phenomena. P10–P16 are designed to yield the ratios of the most important musical intervals, those on the basis of which a system of the scale can be drawn up, and to prove certain properties of these intervals. I shall need to discuss some of them rather more fully.

P10: The octave is an interval of multiple ratio.

This is shown by taking three named notes of the scale, known to be such that the second lies at an octave below the first, and the third at an octave below the second. The interval between the first and the third is known to be a double octave. It is therefore concordant, and hence either of superparticular or of multiple ratio (from the second principle established in the Introduction). It cannot be superparticular, since there is no mean proportional in such a ratio (from P3), and in this ratio there is, the ratio of the second to the first being equal to that of the third to the second, since both are octaves. Hence the double octave must be multiple; and hence also the octave must be multiple (from P2).

P11: The interval of the fourth and that of the fifth are of superparticular ratio.

Again, we take three named notes on the scale. The second is known to be a fourth below the first, the third a fourth below the second. The interval between the third and the first is known to be discordant; the interval of the fourth itself is known to be a concord. Thus the double fourth is discordant, and hence, the writer asserts, is not of multiple ratio: and it follows that the fourth is not of multiple ratio (from P5). Hence, since it is concordant, it must be superparticular (from the second principle of the Introduction). 'And the same demonstration', the author goes on, 'applies also to the fifth.'

But the crucial inference that the double fourth is not of multiple ratio appears to involve our author's one serious argumentative lapse. The only principle he has stated which links concords with multiple ratios is that all concords are either of multiple or of superparticular ratio: to this principle von Jan refers us for the implicit premise of the present argument. But from this it does not follow that all intervals of multiple ratio are concords; and in fact they are not. According to the present scheme, the ratios 2:1, 3:1, 4:1 are ratios of concords: but the next concord greater than the double octave will be either the double octave plus a fourth (if it counts, which is doubtful, as I shall explain later), with a ratio of 16:3, or the double octave plus a fifth, with a ratio of 6:1. Each of these is greater than 5:1, and hence 5:1 is not the ratio of a concord. Nor is this an isolated example. Hence not all multiple ratios are ratios of concords. But in order to argue directly from the fact that the double fourth is discordant to the conclusion that it is not of multiple ratio, our author needs the premise that all intervals of multiple ratio are concordant. He is not entitled to any such premise: he has nowhere stated it; and it is false.

One might suppose that a different proof, adequate for the present argument, could be provided from propositions legitimately proved in the work. It could be shown that the double fourth is smaller than the octave: P12 shows that the ratio of the octave is 2:1; and there is no smaller multiple ratio. Hence the double fourth is not of multiple ratio. Something comparable could be constructed for the double fifth. This interval is greater than the octave but smaller than the octave plus a fifth, whose ratio, as P12 shows, is 3:1. Since no multiple ratio lies between 2:1 and 3:1, the double fifth cannot be of multiple ratio. But these arguments will not do. The proof of the ratio of the octave plus a fifth depends on the proof that the ratio of the fifth is 3:2, and this in turn depends on the present proof that the fifth is of superparticular ratio. Thus the demonstration would be circular. And the suggested argument about the double fourth is no better, since the proof in P12 that the ratio of the octave is 2:1 also hangs on the demonstration that the fourth and the fifth are of superparticular ratio. Hence the author cannot even rely on the simple argument that since the octave is of ratio 2:1, the smallest multiple ratio, the fourth and the fifth, being smaller, cannot be multiple.

I can find no adequate argument for P11 to which the author is entitled. I conclude that without the straightforward but quite false assumption, nowhere stated or argued for, that all multiple ratios are ratios of concords, the author cannot prove this proposition: and as my remarks above have shown, the proposition is indispensable to the arguments which follow. It seems most unlikely that the author consciously adopted the necessary principle as an independent assumption, or that he stated and somehow sought to justify it in his original Introduction; and almost equally incredible that he wrongly supposed it to be somehow derivable from anything stated or proved in the work as we have it. It would be astonishing if on any grounds he supposed it to be true. More probably he simply failed to notice what the premise is which his argument requires. I shall return later to this strange argumentative aberration.

P12: The ratio of the octave is 2:1, that of the fifth is 3:2, and that of the fourth is 4:3. (The reasoning is straightforward, and depends on the conclusions proved, or alleged to be proved, in P6 and P11.) Hence the fifth and the octave together make an interval of ratio 3:1 (from P7), and the double octave is 4:1 (no argument is given). The author concludes: 'We have demonstrated, then, for all of the concords, the ratios in which their bounding notes stand to one another.' But we should notice again that nothing is said of the interval of the octave plus a fourth, which must in fact be of ratio 8:3. I shall come back to this matter. It is perhaps worth reemphasising also the dependence of all the reasoning in this section on the conclusion of P11, the argument for which we found to be illegitimate. The conclusions of P12, in turn, bear the full weight of the theorems which follow.

P13: The tone is in the ratio 9:8. (It is taken as known that the difference between the fifth and the fourth is a tone. The conclusion then follows from what is established in P8 and P12.)

P14: The octave is less than six tones. (From P12, P13, P9.)

P15: The fourth is less than $2\frac{1}{2}$ tones, and the fifth is less than $3\frac{1}{2}$ tones.

Here the proof involves taking four named notes of the scale, the intervals between which are known. Call the notes, in order from the top, B, C, D, E. (These are of course merely labels of convenience, and in no way correspond to modern musical terminology.) Interval CD is a tone; interval BE is an octave, hence less than six tones (P14). DE and BC are equal, both being fourths. Together they must be less than five tones. Hence one of them taken separately—that is, one fourth—must be less than $2\frac{1}{2}$ tones: and hence CE, which is a fifth (a fourth plus a tone: see P13) must be less than $3\frac{1}{2}$ tones.

P16: The tone cannot be divided into two or more equal intervals. (From P13 and P3.)

This completes the propositions concerned with the ratios and other properties of intervals considered in abstraction from the scale. P17 and P18 deal with certain special named notes. The author does not say why these notes demand individual treatment; but part of the point is clear, though I shall argue later that the serious purpose behind these propositions is somewhat more recondite. What is clear is that these notes do not stand at any of the intervals previously discussed from any of the important fixed notes of the scale, and their positions must therefore be located less directly. Thus P17 establishes that the positions of two groups of these notes can be found 'by means of concords': this means that they can be related to other known notes of the scale by ratios already established. P18 proves a special theorem about notes falling inside the *pyknon*, that is, roughly and in certain forms of the scale, the residue of the tetrachord after the first interval downwards. In the form considered here, the first interval downwards in the tetrachord is a ditone, and the residue is therefore a little under half a tone (*cf.* P15). This form of the scale is known as the enharmonic genus, and it is implicit in P17 as well as P18, though not in P19 and P20. I shall discuss the reasons for this change of genus later. That discussion may help to clarify the point of the present propositions, and their relations to those which precede and follow them.

P17: The *paranetai* and *lichanoi* can be found by means of concords.

The *paranetai* and *lichanoi* are the second notes of the tetrachord, reading from the top down. In the enharmonic genus they stand at the interval of a ditone from the top note of the tetrachord. The problem the author sets himself is to construct a ditone downwards from a given note through the construction of a series of concords.

The construction is simple. Take the top note of the tetrachord (here *mese*) as given. Then find the fourth above it, the fifth below that, another fourth upwards, and another fifth downwards. Since the fifth is larger than the fourth by a tone, the pitch thus reached is two tones below the original note, *mese*. I shall later say a little about this 'method of concords', and the relation of its use here to its use by Aristoxenus.

P18: The *parhypatai* and *tritai* do not divide the *pyknon* into equal intervals.

At several points in the argument of this theorem I follow the readings of the MSS against von Jan's emendations.[10] The reasoning is a little involved, but not fundamentally obscure. Because it seems worth establishing, against von Jan, that the argument as the MSS have it makes sense, I shall quote it in full: clauses in parentheses are my own explanatory additions.

'Let *B* be *mese* (the top note of the tetrachord under consideration), let *C* be *lichanos* (the second note down, a ditone below *mese*), and let *D* be *hypate* (the lowest note, a fourth below *mese*. *Parhypate*, to which the present proof refers, lies between *C* and *D*, *lichanos* and *hypate*.) Construct a fifth downwards from *B* to *F*. *FD* is therefore a tone. Then construct a fourth upwards from *F* to *E*. Thus the intervals *FD* and *CE*[11] are both tones. (At this stage we have the following picture:

$$F - \text{tone} - D - ? - C - \text{tone} - E - (\text{tone}) - B$$

That the interval *EB* is a tone follows from the fact that *CE* is a tone and *CB* is a ditone. *DC* is the difference between a ditone and a fourth, that is, a little less than half a tone.) Let *DC* be added to each of them. (That is, consider the intervals *FC* and *DE*.) Then *FC* is equal to *DE*.[12] But *FE* is a fourth, and therefore contains no mean proportional, since the interval is superparticular. Now *DF* is equal to *CE*.[13] hence there is no mean proportional in *DC*, which is the interval from *hypate* to *lichanos* (because it would have to lie in the same place as the mean proportional of the interval *FE*, and there is no such mean). Therefore the *parhypate* does not divide the *pyknon* (i.e., in this case, the interval *DC*) into equal intervals. And in the same way, neither does the *trite* (which stands in the same position in the highest tetrachord as does *parhypate* in the tetrachord running down from *mese*).'

P19 and P20 are not propositions of the same kind as their predecessors. What they give is a method of construction, for so marking out a 'canon' or measuring rod that a string of the same

[10] Von Jan seems to have been unable to make sense of the argument as it stands in the MSS. His version can be reconstructed from the emendations recorded in the three following notes. See *Musici Scriptores Graeci* 163.

[11] Von Jan emends *CE* to *BE*.

[12] Von Jan emends *FC* to *FE* and *DE* to *DB*.

[13] Von Jan emends to '*DB* is equal to *FE*'.

length stretched above the rod, and stopped by a moveable bridge at each of the points marked, in sequence, will sound the intervals of the scale.[14]

P19 is headed 'To mark out the measuring rod according to the so-called immutable system'. The 'immutable system', in this context, is the series of 'fixed' notes; that is, those notes which stand to one another at intervals which do not change with a change of genus.

P20 is headed 'It remains to find the moveable notes', that is, those notes whose intervals in relation to the fixed notes do change between one genus and another. In fact the author gives us the construction for only one genus, the diatonic: hence, in particular, the enharmonic *lichanos* of P17 and P18 does not appear in P20.

The author's method is straightforward enough. The note sounded by the full length of the string is *proslambanomenos*, the lowest note of the scale. The intervals at which the other notes stand to one another are known. The author shows how to mark the divisions of the string which will yield these notes by making the lengths of string divided off stand to one another in the appropriate ratios. He does this using only the ratios already assigned to specific intervals, 4:1, 2:1, 3:2, 4:3, and—only in P20—9:8. This independent use of the ratio 9:8 is strictly speaking unnecessary. He has already shown in P17 how to find a note a tone, or two tones distant from a given note, by means of concords alone, and everything he does in P20 by reference to the ratio 9:8 could have been done as well without it. I shall suggest that this fact is a small but significant pointer to the purpose for which the treatise was written.

III. Classification of Principles

In discussing the methods and aims of the work we must distinguish three groups of principles which the author takes as given, and which form the foundations on which his arguments rest. The first consists of the mathematical principles employed in P1–P9 and relied on in the remaining arguments. About them I shall continue to say little or nothing. The second contains a rather miscellaneous collection of assumptions concerned with notes, intervals, and certain musical structures. In particular, there are assumptions about the notes of the scale, and their relations to one another expressed as intervals. It is assumed that the names of these notes, their order, and the intervals between them are known. Other assumptions concern intervals taken in abstraction from the structure of the scale. It is assumed that for any given interval we can determine, in advance of mathematical analysis, whether it is a concord, and whether it is equal to, greater than, or smaller than, any other given interval. It is also assumed that we can decide, in at least some cases, what interval is generated by the addition or subtraction of two intervals of known size.

By themselves, these two classes of proposition are not sufficient to form the basis for a reduction of scalar intervals to ratios. The principles of the second class do not mention ratios at all, whereas the conclusions derived in P1–P9 are expressed wholly in terms of ratios. We therefore require rules and grounds for translating into the terminology of ratios propositions about notes and intervals. I shall call these 'bridging' principles, and I shall discuss them first.

IV. The Bridging Principles

The author nowhere sets out explicitly a list of the bridging principles on which he relies (any more than he does for the principles of the other two classes), but the existing Introduction provides some traces of them. We shall find reason to doubt, however, whether any introduction could have performed this task satisfactorily, since the status of the principles which the author calls upon is shaky, at best. There appear to be three bridging principles. One is very

[14] The best description of the Pythagorean canon, or monochord, is at Ptol. *Harm.* 17.27–19.15 (Düring).

general: the third, unstated and perhaps unnoticed by the author, makes only a single contribution to the argument. The second is explicit, pervasive, and crucial, and like the third profoundly unsatisfactory.

We may state the first bridging principle (BP1) roughly as follows. The interval between any two notes can be expressed as a ratio of numbers, the ratio being the same as that between the lengths of a true string which yield the notes in question. (By a 'true' string I mean one of uniform thickness, constitution and tension.[15])

The principle evidently dissolves into two propositions: first, that an interval can be expressed as a numerical ratio, and secondly that this ratio is the same as that between the relevant lengths of a true string. The former proposition is one which the Introduction seeks to underwrite. It states as known facts that sound is produced by movement; that more frequent movements generate higher pitched sounds; and that by the 'addition and subtraction' of movement the pitches of sounds can be raised or lowered. It follows, so the author argues, that sounds are composed of parts—each part, presumably, being a single movement: and he concludes that differences in pitch can be expressed as ratios of numbers—the ratios, presumably, being the ratios between the numbers of parts of which the sounds compared are composed.

The argument has some merit, as far as it goes. But it does not go far enough. It can reasonably be inferred, from the evidence given, that the relation between any given pair of individual sounds can be expressed as a numerical ratio. But it does not follow that all intervals of the same size can be expressed by the same ratio. Though it is the case that intervals can be consistently expressed in terms of ratios, one ratio corresponding invariably to one size of interval no matter what the absolute pitch of its bounding notes, and though procedures such as the addition and subtraction of intervals can be represented by the corresponding operations with ratios, our author has not produced evidence sufficient to establish the fact.

Something similar can be said of the second proposition of BP1, that the ratios in question are the same as those between the relevant lengths of a true string. The Introduction makes no attempt to establish this, nor to justify the reversal of ratios involved in expression in terms of string lengths instead of numbers of movements. Suppose that we grant that intervals can be expressed as ratios of numbers, and that these numbers are the relevant numbers of movements. Assume also that a longer string vibrates more slowly than a shorter one. It still does not follow that a string of, for instance, twice the length of a given string will generate in a given time exactly half the number of movements. Our suppositions are consistent with any number of rival hypotheses.

We can conclude that though both propositions of BP1 are true, the author does not prove either of them. But we need not cavil too much at this. Any system of demonstrations requires starting points not themselves proved within the system: all we can properly require of such starting points is that they are such that the author can reasonably recommend our acceptance of them. That BP1 is true had long been accepted. It had not been demonstratively proved, but the empirical experience of practical experiment, together with the theoretical success of earlier 'divisions of the canon', such as that of Archytas,[16] was sufficient to make it a reliable basis for calculation. The author of our Introduction may be less concerned to prove BP1 than to sketch the physical facts which underlie it, indicating merely the general area—the physics of movement—in which a scientific demonstration should be sought. BP1 is evidently a presupposition which the author of the theorems must accept: if he does not see the need to prove it, that is because it is already a non-controversial *phainomenon*[17] in good standing among those familiar with the subject.

[15] *Cf.* Ptolemy *loc. cit.*, Nicomachus *Enchiridion* 246.22–247.4 (von Jan).

[16] DK 47, A16, A17.

[17] *Cf.* the discussion of Aristotle's use of the notion of a *phainomenon* in G. E. L. Owen, 'Tithenai ta phainomena' in *Aristote et les problèmes de la méthode* (Symposium Aristotelicum: Louvain 1961) 83–103, repr. in J. M. E. Moravcsik (ed.), *Aristotle: a collection of critical essays* (Macmillan 1968) 167–90.

The same cannot be said of BP2, the thesis that all concords are either of multiple or of superparticular ratio. We have already looked at the argument by which the author of the Introduction seeks to recommend this principle, and we have seen that it too falls far short of demonstrative proof. It is at best an argument to the likelihood of the principle, that is, to the claim that it is not an unreasonable proposition to hold as true. So far as this goes, the status of BP2 seems comparable to that of BP1: but differences will soon emerge.

I shall not consider further the argument used in the Introduction to support BP2: I want now to approach it from another angle. The project in hand evidently demands the assumption of some such principle, one which will limit the classes of ratio to which particular classes of interval may be assigned. The author believes that given BP2 (and perhaps BP3, to which we shall turn shortly) it is possible by means of propositions established within the mathematics of ratios, together with known truths about intervals between notes on the scale, to show that the octave must be of multiple ratio (P10) and that the fourth and the fifth must be superparticular (P11), and to go on to prove what the numerical values of these ratios are (P12). BP2 is assumed more or less explicitly, not slipped in unnoticed: it is not formally stated in the exposition of the theorems, but it is openly involved in the reasoning of these three crucial propositions.

But quite apart from the weakness of the argument by which it is supported, BP2 is in difficulties. The problem is simple enough: BP2 is false. What is more, the fact that it is false is not in the least obscure. The Greeks, as I have said, unanimously recognised three basic concords, the fourth, the fifth, and the octave. All intervals smaller than the fourth were reckoned discordant: assimilation of the major and minor thirds to the class of concords is a modern phenomenon. In addition to these three, they recognised as concordant multiples of the octave, and the interval of the octave or multiple octave plus a fifth. So far all schools of thought were in agreement. It is with the octave or multiple octave plus a fourth that difficulties arise. So far as the ear is concerned, the members of this group share the common character of the other concordant intervals. No one seems to have disputed this. Aristoxenus, like others later, takes it as acoustically obvious that the addition of an octave to any concord will generate a concord.[18] The problem does not arise from any dispute about what these intervals sounded like. It is a purely theoretical difficulty, affecting only those who commit themselves to the truth of BP2. It arises because whatever the auditory phenomena may be, the ratio of the octave plus a fourth, which is 8:3, is neither superparticular nor multiple, and neither is that of the double octave plus a fourth, which is 16:3.

It is therefore significant, as I mentioned in my remarks on P7 and P12, that the author omits any reference to the ratio which is the product of 2:1 and 4:3, and to the ratio of the octave plus a fourth. One might charitably argue that he is concerned with the ratios of concords, and that he omits these simply because, from his point of view, the octave plus a fourth is not a concord. But this will not do. His reason for accepting certain intervals as concords is not that their ratios are of the right kinds: it is not because it is of superparticular ratio that the fourth is a concord, since all except two of the superparticular intervals are discords. The criterion by which the concordant or discordant character of any interval is determined, even by the present author, is acoustic, not mathematical, and this character is assumed, not proved, in the theorems—*cf.* P10, P11, P12. The concords are those intervals which are heard in a certain way, as the definition given in the Introduction suggests. From this point of view the disqualification of the octave plus a fourth is plainly illegitimate. The grounds on which it is (silently) disqualified are much flimsier than the case for its inclusion, since these grounds are wholly constituted by BP2, and the unpersuasive argument which supports it. But if BP2 is abandoned, the author is left with no basis for his derivation of the ratios of the concords in P10–P12. One must conclude, it appears, that the omission of any reference to these awkward ratios is motivated more by embarrassment at the anomalies they present than by any more respectable considerations. (I shall shortly suggest, however, that there are ways in which this criticism may be somewhat softened.)

[18] Aristoxenus *El. Harm.* 20.17–22, 45.20–34. *Cf.* Ptol. *Harm.* 13.1–23 (Düring).

It is possible that this difficulty is one on which Plato had his eye in a well known passage of the *Republic*.[19] He argues that whatever else may be said for the Pythagoreans, they have failed to tell us which 'numbers' (i.e., probably, which ratios) are concordant and which are not, and why. It is this 'why' which is crucial. Empirical methods can establish with some accuracy what the ratios of the concords are; but they cannot trace to some special feature of the ratios the special acoustic character of these intervals. Our present principle, BP2, may be as old as Archytas,[20] and may therefore have been known to Plato. It will not do, because it is false. But if there is no way of marking off the ratios of the concords as a special class, our present author's project must founder, since he would have to rely on merely empirical evidence; and that evidence is not that the ratios of the concords are of some peculiar kind, but that they are the particular ratios which they are—precisely what our author wants to prove, not to assume. We may note in passing that even if BP2 were true and provable, it would hardly satisfy Plato: it cannot be *because* a given ratio is superparticular or multiple that it is the ratio of a concord, since many such ratios are not.[21] Hence BP2 could yield no explanation of why certain ratios are the ratios of concords.

This point leads us directly to the third and final bridging principle, BP3. It is nowhere stated, and used only once. As we have seen, the argument of P11 rests on the assumption that if an interval is discordant, it is not of multiple ratio. If we state this principle in its positive form, it is that all intervals of multiple ratio are concordant. This is false, and its falsity is even less controversial than that of BP2. There is, of course, nothing in the Introduction to suggest or support BP3, and its role in P11 is inexplicit. But the argument of P11 cannot work without it. Possibly the author thought that all he was using was BP2, which would convict him of logical incompetence, or at least extreme carelessness. If he consciously supposed that BP3 was true, this merely transfers his incompetence to another area. The remaining possibility is that he deliberately suppressed a premise which he knew to be both indispensable and false; in that case his 'demonstration' is simply dishonest.

If we adopt the more generous view that he has made an honest, if elementary mistake, and has supposed that BP2 is sufficient to licence the vital inference of P11, it becomes still clearer why BP2 itself is so tempting. BP1 is very general, and was more or less non-controversial. Given BP1, and given the mistake in P11, BP2 is the only premise the writer needs, over and above the standard propositions of mathematics and those giving the data about the intervals of the scale. It thus enables him to base his demonstrations on a most economical system of assumptions. It is worth adding that so far as our authorities suggest, no substitute for BP2 was ever found, at least until the time of Ptolemy, who treats BP2 as a fundamental premise of the Pythagorean system, and criticizes the system on the grounds of this principle's inadequacy.[22] It is perhaps partly their use of this principle which gave the Pythagoreans their reputation for preferring reason to perception as a criterion of musical judgement. They had found that intervals can be expressed as ratios; and empirical experience indicated that the ratios of the fundamental scalar intervals formed a neat pattern of ratios of whole numbers. It could hardly be merely fortuitous that the ratios of the three basic concords are 2:1, 3:2, 4:3. There must be a rational explanation for this pattern, and a way of demonstrating these values mathematically. But such a demonstration demands bridging principles, and nothing but BP2 could be found to do the job. Further, BP2 is perfectly satisfactory, so long as we ignore the anomalous interval of the octave plus a fourth. We can ignore it only if we deny that it is a concord; and we deny this on the grounds that it does not fit the criteria of BP2. That is, reason demands the truth of BP2, since otherwise the pattern of ratios cannot be demonstrated mathematically; and it must be so demonstrable, since the alternative is that it is fortuitous, a wholly incredible supposition. If the ear quarrels with reason over this one interval, there must be something wrong, not with reason,

[19] *Repub.* 531c1–4.

[20] *Cf.* my paper 'σύμφωνοι ἀριθμοί: a note on Republic 531c 1–4', *CPh* lxxiii (1978), esp. 339–40.

[21] *Cf.* Ptolemy's remarks at *Harm.* 13.23–14.2 (Düring).

[22] *Harm.* 13.1–23.

but with our—notoriously fallible—capacities of perception. I have already criticised a version of this argument on the grounds that it rejects as unreliable the source of our evidence for the belief that the admitted concords are concords. But it must now be said that this criticism is not final. Any induction which moves from the perception of particular cases to a general principle may be faced with anomalous instances. We then have the choice of rejecting the suggested principle, or denying that the anomalous instance is an instance. Which we do will depend on a number of considerations, among which the explanatory power of the suggested principle is important: in our present case its overwhelming economy and success as a principle of demonstration might be thought sufficient grounds for retaining it, and thus for rejecting the supposed anomalous instance. The octave plus a fourth, whatever it sounds like, cannot be a concord. But in that case we must be redefining, surreptitiously or otherwise, the notion of a concord. It is no longer something which is heard in a certain way, or not just that. It may be no accident that it is within the Pythagorean school that the concepts of harmonics, and particularly that of the concord, find their primary application outside the auditory realm, in the context of the 'harmony' of the planets.[23] If the term 'concord' applies directly not to something heard, but to a special kind of relation between the heavenly bodies, and only by some sort of extension to the perceived properties of sounds, it becomes possible to suggest that our ears may erroneously accept as a concord something which is not one. This can be true because 'C is a concord' no longer means simply 'C has a particular auditory character'. To carry these speculations further would be beyond the scope of this paper. But if it was the demonstrative power of BP2 which was taken to license the rejection of the anomalous interval, and indirectly the reshaping of the concept of concord, the case would have been severely weakened if the surreptitious role of BP3 had been detected.

V. Principles Derived from Auditory Experience

Let us consider first the propositions which state the order of the named notes of the scale, and the intervals at which they stand to one another. The author presupposes a fixed nomenclature, and a standard scale or set of scales. The history both of the naming of notes and of the standardisation of the scale is in doubt, but I do not propose to enter these controversies here: I shall take it for granted that at least by the late fourth century, both were more or less uncontroversially fixed.

But the scales which the author deploys exhibit one feature deserving further comment. As we have noted, the form of the scale presupposed in P17 and P18 differs from that in P19 and P20. P17 and P18 demand an undivided interval of a ditone between the upper two notes of a tetrachord, for instance between *lichanos* and *mese*; whereas the equivalent interval in P19 and P20 is a tone. P19 and P20, in fact, describe a scale in the diatonic genus, while P17 and P18 give a partial account of one in the enharmonic genus.

I see no reason to put this distinction down to a difference in authorship or date. Certainly all three genera, enharmonic, diatonic, and chromatic, were known at this period, and all are discussed by Aristoxenus. That the complete scale is given in its diatonic form is not surprising. It was probably the genus in commonest use.[24] In the form given here, it can be expressed in terms of the ratios of concords more readily than any of the others. Further, Plato's *Timaeus* suggests that at any rate within the Pythagorean tradition this genus was thought to represent the archetypal form of the scale, of which the others were perhaps to be considered as variants.[25] Other authors' remarks about the history of the genera tend to confirm that this was the commonly accepted belief.[26] What needs more explanation is the appearance of the ditone

[23] See e.g. Nicomachus *Enchiridion* section 3, pp. 241–2 (von Jan).

[24] E.g. Aristoxenus *El. Harm.* 19.20–29. This genus was also central to certain fundamental constructions of theory. *Cf.* e.g. J. Chailley, *La musique grècque antique* (Paris 1979) chs 5–7.

[25] *Timaeus* 35b–36b.

[26] E.g. Ps.-Plutarch *De Musica* 1134f–1135b.

interval and semitone *pyknon* of the enharmonic genus in P17 and P18. Given that the author makes no attempt at a complete account of all the genera and their variations or 'shades',[27] he presumably had a special reason for picking out this one. While certainty is impossible, the following suggestions may be somewhere near the mark.

We know from Aristoxenus that earlier, non-Pythagorean theorists whom he calls '*harmonikoi*' had attempted to give an accurate account of the intervals of the scale, and had concentrated exclusively on the enharmonic genus.[28] Their method was empirical, and their results were set out not in terms of ratios, but in terms of quasi-linear auditory intervals such as the tone, the semitone, and so on. The key-stone of the procedure was the establishment of a *metron*, or unit of measurement, which they may have supposed to be the smallest interval which the ear can reliably detect.[29] This *metron* was established as the enharmonic diesis, or quarter-tone, that is, the interval which appears twice in what is left of the interval of a fourth after the ditone, moving from the top of the tetrachord downwards. All other intervals, and the whole system of the scale, were expressed in terms of this interval and its multiples.[30]

These facts give the author of the *Sectio* two reasons for the notice he gives to the enharmonic scale. In the first place it was established as a focus of analysis, and a theorist might therefore be expected to say something about it. (By the same token, Aristoxenus suggests that the enharmonic genus is the most sophisticated and intellectually advanced of the three,[31] so that it would perhaps be improper for a reputable analysis to pass it by.) More importantly, what is established in P18 is not the value of an interval, but the thesis that the residue of the fourth after the interval of a ditone cannot be equally divided. I suggest that this argument is not there merely to establish a curious truth, but that its object is polemical, and that is it directed precisely at these earlier analysts of the enharmonic scale. Their method depended on taking as a fixed unit the enharmonic quarter-tone: what our author demonstrates is that there is no such unit, and that what pass for equal quarter-tones must in fact be intervals of different sizes, and hence useless for the purposes of measurement. Here, I think, we can see genuine traces of conflict between Pythagoreans and a more empirical school of thought. Though Aristoxenus himself rejects the methods of the *harmonikoi*, he too is committed to a system in which the tone can in principle be divided into as many equal parts as you like; in which the octave is six tones, the fourth two and a half, and the fifth three and a half; and in which the two lowest intervals of the enharmonic tetrachord are equal quarter-tones.[32] In all these claims he is directly in conflict with the present work, not only in P18, but also in P14, P15 and P16. None of these propositions is needed for the 'division of the canon' in P19 and P20: they are worth establishing only for the means they give of attacking theorists of another school. P18, and P17, which is necessary for its proof, are to be seen not as a gesture towards an enharmonic 'division of the canon', but as a continuation of the polemic implicit in P14–P16.

The claim that this is the object of the whole set of propositions from P14 to P18 might be thought to strengthen the argument that P19 and P20 are of different authorship. They plainly have a different purpose. But I think that this suggestion would be mistaken. The *harmonikoi* and Aristoxenus shared the primary objective of giving an accurate description of the scale. It seems unlikely that the rival school, if such it was, would have rested content with an attack on their methods and presuppositions, without offering an analysis of their own.[33] All the mathematical premises required for the derivation of P19 and P20 have been established in the earlier

[27] On the χρόαι or 'shades', see Aristoxenus *El. Harm.* 47–52.

[28] *Op. cit.* 2.6–11, 35.1–13.

[29] Cf. *Repub.* 531a7, Arist. *Metaph.* 1016b18, 1052b20, 1083b33.

[30] See my paper cited at n. 2, 10–12.

[31] Aristoxenus *El.Harm.* 19.26–9.

[32] Division of the tone, including reference to the enharmonic diesis, *El.Harm.* 21.20–31, 46.2–7, and elsewhere: the sizes of the fourth and the fifth, assumed elsewhere (e.g. 46.1–2) are derived at 56.13–58.6. The crux of the derivation lies, I think, in Aristoxenus' implied claim that the ear 'accepts' the difference between the fourth and the fifth (the tone) as identical with the difference between the fourth and the ditone taken twice.

[33] The passages cited at n. 16, as well as the scale in the *Timaeus*, are evidence of earlier attempts at such an analysis within the 'Pythagorean' tradition.

theorems. It would be remarkable if our author had not gone on to work the 'division' out. No suspicion would have fallen on these propositions if the form of the scale they presuppose had not differed from that of P17 and P18: if I have succeeded in explaining the appearance of the enharmonic genus in those propositions, there are no longer any grounds for doubting the authenticity of the final pair.[34]

Two further kinds of assumption can be identified.[35] The first is that certain kinds of phenomenon can be directly apprehended through perception: in particular, as we saw, it is assumed that we know without the need for argument that such and such an interval is a concord (e.g. in P10) and that such and such another one is not (e.g. in P11). Similarly, the author assumes that we can tell which of two intervals is the greater (e.g. in P12), and whether two given intervals are equal (e.g. in P11). It is not clear whether this knowledge is supposed to be based directly in auditory experience, or on a theoretical grasp of the interval structure of the scale. In P11 the latter seems more likely. We know that the interval between *nete synemmenon* and *mese* is equal to that between *mese* and *hypate meson* presumably because we know that according to the established system of the scale, both are fourths. In P12 no clue is given as to how we know that the fifth is greater than the fourth: ultimately, however, in both cases, our judgement must depend on auditory experience, that is, on the fact that we simply do hear two intervals as equal, or one as greater than the other. There are interesting philosophical issues embedded in our common talk of 'sizes' of intervals, but it would be inappropriate to pursue them here: our author is merely accepting the commonplaces of everyday experience and expression. But it is worth noticing that these commonplaces are rooted in a linear apprehension of musical space, a continuum which can be divided into equal or unequal segments, on which a pitch is a point, and an interval a length separating two points. It is a long way from this to a conception of intervals as ratios, and pitches not as points on a line, but as magnitudes between which ratios hold. The project which our author undertakes, since it must begin from commonplace apprehensions and formulae, involves *inter alia* the task of altering the entire conceptual framework within which these commonplaces are represented.

The second class of propositions in this group contains those which state, for certain cases, what interval is produced by the addition or subtraction of a given pair of intervals (addition, P12; subtraction, P13). These two cases should be distinguished. In the first case it is taken that we know that a fifth plus a fourth yields an octave. This proposition is presumably founded directly in auditory experience. The fifth, the fourth, and the octave can all be independently and fairly precisely identified, since all are concords, and as Aristoxenus tells us, concords can be determined accurately by ear.[36] Discords, however, cannot, or not so readily; and in the second example, which takes it that the difference between a fifth and a fourth is a tone, it is not at all clear that the tone is something which we are supposed to be able to recognise independently. It is, perhaps, simply *defined* as the difference between the fifth and the fourth. Aristoxenus specifies it in the same way, without seeing any need to produce evidence or give grounds:[37] it seems that the point is not that if you take the recognisable interval of a fourth and add to it the recognisable interval of a tone, the result is the independently identifiable fifth, but merely that 'tone' is the name given to the interval by which the (recognisable) fifth exceeds the (recognisable) fourth.

[34] The arguments against the authenticity of Propositions 19–20 have hinged on the supposition that Propositions 17–18 indicate the original author's primary concern with the analysis of a scale in the enharmonic genus. If I am right in suggesting that they indicate nothing of the kind, but are by intention largely polemical in the way I have outlined, these arguments can have no weight. If the early date of P19–P20 is admitted, van der Waerden's arguments, cited by Burkert, for the later invention of the monochord must also fail. See Burkert (n. 3) 375 n. 22, and Szabó (n. 5) 118–19.

[35] Burkert (n. 3) 384 states that the argument presupposes only two 'empirical observations'—that the octave consists of a fourth and a fifth, and that while a double octave is a concord, a double fourth or a double fifth is not. As will become clear, the empirical status of some of the presuppositions I shall mention is in doubt: but the discussion which follows will suggest that the system of presuppositions is rather more complex than Burkert implies.

[36] *El.Harm.* 55.3–10.

[37] *El.Harm.* 21.20–3, 45.35–46.1.

There is no need for an independently recognisable tone in the Greek system of tuning: it is implicit in what our author says in P17, and explicit in Aristoxenus,[38] that a note a tone distant from any given note can always be found by tuning operations involving concords alone.

VI. The Aims of the Treatise as a Whole

At a superficial glance, the general purpose of the work appears to be to establish, by means of the theory of ratios, a system of measurement by which pitches can accurately be determined in their proper relations; and hence to give a mathematically precise account of the relations between the notes of the scale. What makes the exercise worthwhile is presumably the fact that pitch relations are not precisely measurable by ear. The attempts of the *harmonikoi* to establish an auditory unit of measurement fail, both for the theoretical reasons already mentioned, and because there can be no clear agreement on the identity of the supposed minimum or unit interval.[39] There is no clearly determined auditory measuring rod. Hence if accuracy is to be attained, a way must be found of transferring pitch relations from auditory to visual space, where acceptable measures are available: and this is what the system of ratios, set in terms of lengths of strings which inhabit visual space, can evidently achieve.

But this brings us back to the need for a prior acceptance of the interval system of the scale as given. The system of ratios which the mathematical theory yields cannot itself determine the sizes and order of intervals which form the scale: the theorems of the *Sectio* have no tendency to establish *a priori* what the intervals of the scale ought to be. These must already be given, and given precisely, in terms of auditory intervals, that is, in the terminology of 'linear' musical space.

The project of determining by theoretical means what the scalar intervals ought to be sounds, in any case, nonsensical, though it is a piece of nonsense to which Aristoxenus, at times, almost commits himself. But Aristoxenus' enterprise can for the most part be seen as less ambitious and less absurd; since what he sets out to do in general is not to *determine* the interval system of the scale, but to explain and justify it by showing that it conforms to certain higher principles of harmonics, notably his so-called 'law of fourths and fifths'.[40] But the *Sectio* does not attempt to do even that. The laws on which it relies are mathematical, not musical. The scale is simply accepted as given, as being what the existing tradition tells us it is.

But this leaves a puzzle. The scale is known in advance, in terms of named auditory intervals. The sizes of these intervals are precisely determinable in relation to the fourth, the fifth, and the octave, each of which is identifiable directly by perception. The object of the enterprise is to establish a system of measurement by which the scale may in practice be accurately constructed. But if it is not precisely constructible in advance of the theorems, how can it already be known? How can we know what it is that we are to set about constructing? And if it is already constructible by reference to our auditory perception of the three fundamental concords, what role can the theorems perform? We seem to be caught by a version of Meno's paradox.

If the *Sectio* were directed at the musical performer, as a set of instructions about accurate tuning, this apparent difficulty would have more force than it does. The method by which Aristoxenus seeks to determine the discordant and compound intervals, the 'method of concords' referred to above, could indeed have been used for tuning a stringed instrument, and may well have been derived from the established practice of performers;[41] though for Aristoxenus it in fact remains a tool of theory. In the *Sectio*, something like the method of concords is used in P17, and in the divisions of P19 and P20. But three features of the case make it

[38] *El.Harm.* 55.10–56.12.

[39] Cf. *Repub.* 531a–c.

[40] Best explained at *El.Harm.* 53.34–54.21, which, with its continuation to 55.2, strongly suggests the 'less ambitious' form of the project.

[41] The procedure described in the passage cited at n. 36 is plainly at least capable of practical application.

clear that the author does not have the practical musician in mind. First, though all the divisions of P20 could have been made, in the 'practical' manner, by means of concords, the author in fact also employs the discordant, less readily identifiable interval of a tone. Secondly, it seems most unlikely that a practical musician would set about tuning his instrument, in the manner recommended, with the aid of a rule and compasses: the practitioner, like Aristoxenus, relies on his ear.[42] Thirdly, the method is in any case unworkable for anything but the monochord, which was never a performer's instrument, only an aid for theorists. To transfer the method to an eight-stringed lyre, for example, involves complexities in which our author is plainly not interested.

The *Sectio* is intended, then, for the theorist: and we may say that its aim is less that of making precise what was previously vague—since, as we have seen, it depends on a scale already precisely established—than of translating the system of the scale out of one terminological and conceptual framework into another, from a scheme of quasi-linear auditory relations into one of numerical and geometrical ratios.

There might be two reasons why such a project of translation would seem attractive. The Introduction might suggest the theory that numerical determinations stand as causal or explanatory principles to the auditory phenomena; that is, that differences of pitch are to be explained in terms of numbers of movements, so that an exposition of the relevant ratios would yield scientific understanding of the data given in perception. Such a theory might seek to explain, for example, why this interval added to that yields such and such another one; why the difference between an octave and a fourth is a concord; why there are no concords smaller than the fourth; and so on.

But the *Sectio* can explain none of these things. All of them are taken for granted. The only principle it employs which even looks like being able to explain why certain intervals are concords while others are not is BP2; and that, as I explained earlier, is not nearly strong enough for the task suggested here. As to the question why, for instance, a fourth plus a fifth yields an octave, this can be answered once the ratios of the three intervals are known; but their values cannot be established without already assuming the truth which is supposed to be explained. A statement cannot comfortably stand as a premise in its own explanation. Of course the *Sectio* does explain certain things, in particular why the tone cannot be equally divided, why the octave is less than six tones, and so on. But these are scarcely *phainomena* in any sense of that elastic term, and certainly not auditory data: they are puzzling and obscure consequences of the theory of ratios itself, which require no explanation—because no one will suppose them to be true—unless that theory is already accepted.

In general, the author of the theorems shows no consciousness of the explanatory or 'causal' role of the propositions he establishes. From the Introduction we could infer some such role, but nothing in the theorems depends on it: BP1, unlike BP2, is nowhere directly called upon as a premise. Further, no adequate explanatory theory could function without some elucidation of the reversal of ratios involved in an exposition in terms of string-lengths; and this is not given. Finally, the focus of the theorems is not upon the explanation of puzzles arising from the auditory phenomena, but partly on the demonstration of certain facts whose importance is almost wholly polemical, and partly on the translation of the scalar intervals into the language of ratios. The *Sectio* is not then an exercise in scientific explanation.

Its real aim, I suggest, is simply to enable the subject matter of music to be treated with precision by the mathematical philosopher, or indeed by philosophers of any kind. No doubt it is part of the Pythagorean programme to relate musical phenomena to those in other areas, in particular those in the field of astronomy; and if this is to be done, both must be expressed in a common terminology, admirably provided by the mathematics of ratios. But the point may be more general. It is all very well to say that the scale must be determinable in advance by means which depend ultimately on the ear: but these means, as Aristoxenus emphasises, are the

[42] See the passages cited in Burkert (n. 3) 383 n. 62.

province of the expert musician alone.[43] There is no auditory measuring-stick with which all rational and sensitive creatures are naturally endowed. But ratios and their relations fall within the province of reason, which is common to us all. Thus the translation is not necessarily from the imprecise to the precise, but rather from the specialist and esoteric into common coinage, into a language where the capacity precisely to determine the references of terms does not depend on a special biological endowment or a special kind of training, but on the universal faculty of reason. If this is the aim of the project, it is valuable and coherent, and does not depend on assuming an unreliability or imprecision in the auditory data which it accepts among its premises. And it gives a very tolerable sense to the claim that the Pythagoreans valued reason more highly than perception as a criterion of musical judgement, a sense which does not demand that they reject out of hand the data of perception from which their project must necessarily begin. Perception seems to confront reason in two main areas. The first is in the Pythagorean denial that the octave is exactly six tones, that the tone can be exactly halved, and so on. But these propositions do not deny what perception definitely demands: perception can hardly be thought so precise as to be able to determine whether these minute differences exist or not—a point which Aristoxenus occasionally seems to concede.[44] The Pythagorean claims are important, not because they show that perception is wrong, but because they show that perception cannot make the fine discriminations of which reason is capable, and where controversy arises is therefore a less reliable guide. The second point of conflict lies in the head-on collision between the two schools over the interval of the octave plus a fourth. Here the confrontation is direct, since the perception of experts, on which for both parties the initial list of concords depends, is unanimous in accepting as a concord what reason rejects. But this rejection is not due to the nature of the Pythagorean programme as such, only to the special claims of one principle on which they rely, but which could be abandoned in favour of some alternative—if such could be found—consistently with the general aims of the system. And the prospect which the programme holds out is irresistibly enticing. It is not only astronomy with which music might be united by mathematical methods: mathematics, as the *Timaeus* suggests, may also provide the link between music and the soul, and hence between music and those ethical conceptions with which philosophers in the past had persistently but loosely associated it: the ultimate, if quite unrealisable objective may be nothing less than a mathematically demonstrable system of ethics.[45]

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[43] *Cf.* especially the discussion of the criteria of musical judgement at *El.Harm.* 33.1–34.34.

[44] *El.Harm.* 55.3–7.

[45] I should like to thank David Fowler for his comments on an earlier draft of this paper. He has also pointed out to me recently sources for the principles not proved, but assumed from elsewhere, in P2 and P3. See Euclid *Elements* viii, Propositions 6–9.