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EUCLID'S V,5

Preface

The thesis of this lecture is that Euclid had good reasons for preferring Definition V,5 to "the product of means and extremes" as a definition of sameness for relations of size between magnitudes. I had this in mind several years ago when the editors of Seven asked for a few pages on something which interested me. I wrote a couple of pages on V,5 suggesting a possible origin of this definition. I thought that this way of understanding the definition explained why Euclid prefered it. That is, there were numerous reflections on definition in mathematics which together with this account seemed to explain why he had prefered it.

I intended to elaborate various other comments which the definition led me to make for Seven. Frank Flinn has been kind enough to pester me regularly about putting the rest of these things down. They occupy the latter parts of this lecture.

You may wonder why I should be concerned with the question at all. It is because I am interested in questions about the non-formal reasons we may have for doing the formal things we do in mathematics. That is, I would like to see mathematics in a more general context. Also because the discussion of this question has suggested to me a view of mathematics which is perhaps a trifle heretical.

I shall begin with an amplified version of the statement which appeared in Seven.

Part I

A number of years ago I noticed a relation between Euclid's definition VII,20 of proportion among numbers and his definition V,5 of same ratios among magnitudes which I thought helped me to understand V,5. It may have been suggested to me by Winfree Smith and it may even be obvious, but I will try to state it in case it isn't obvious.

Definitions 20 and 3, 4, and 5 of Book VII which lead up to it, seem to me a clear and intuitive statement of what I mean when I say that two pairs of numbers have "the same relation of size." That is what I take Euclid to mean when he says that numbers are proportional, since he first defines the possible relations between the numbers of a pair and then defines proportion in terms of those relations. I take it that proportion (ἀνάλογον) and same ratio (ἐν τῷ αὐτῷ λόγῳ) intend the same kind of relation or an analogous one. In VII, 14 and various other propositions which follow in Book VII, Euclid uses the term ratio for the relations of two pairs of numbers. Similarly, in Definition V,6 and Proposition V,12 he uses the term proportional for magnitudes. In fact, in Proposition X,5 he allows a same ratio between a pair of numbers and a pair of magnitudes which seems to leave no doubt about the identity of meaning.

The relations of the numbers of a pair are <u>multiple of</u>, <u>part of</u>, and <u>parts</u>. Let me give numerical examples. VII,20 would say that in each of two pairs of numbers forming the same ratios we could have the first, or antecedant, double the second, or consequent; or the consequent in each pair triple the antecedant; or the antecedant triple the greatest common divisor of that pair, while the consequent is five times it.

In VII,2 and 3 Euclid distinguishes between numbers which have a number which is their common divisor and those which do not, numbers prime to one another. In Proposition 3, he proves that any two numbers that have a common divisor have a greatest common divisor and presents a method by which we may find it.

In the examples cited above I have stated in each case the respect in which the two pairs of numbers are "the same." This notion seems to me clear and fully intelligible.

However different my notion of magnitude may be from that of number, in the case where each of two pairs of magnitudes has a common measure I can grasp their relations of size and the sameness or difference of those relations with the same degree of clarity. Each of the magnitudes of the pair is a whole number multiple of their common measure, and I understand their relations of size by those numbers. I then compare the relations of size just as I would for numbers. In X,3 Euclid proves that if two magnitudes have a common measure they have a greatest common measure and gives a procedure by which we can find it.

Perhaps it's not clear why I should bother to make this point. We rarely have time in the Freshman Mathematics Tutorial to do much with Book X. Maybe we would see what the realm of magnitude is like if we reflected deeply on the existence of incommensurability; on the proof of the incommensurability of the diagonal and the side of a square. In any case Euclid shows us something of it in Propositions 111 and the last of the book,115. He proves that there are infinitely many kinds of irrational magnitudes, all distinct, none commensurable with any of the others. In the late nineteenth century proofs were found which imply that all of Euclid's irrations put together are as nothing in that vast realm.

It is a great amorphous sea in which the commensurable magnitudes, that is those magnitudes commensurable with some chosen unit, described above constitute a tiny island of intelligibility. Where there is no hope of finding a common measure, there can be no hope of gaining that kind of intelligibility.

Let us go on to ask whether the clear case we've been discussing can be a guide for the harder case of ratios for all magnitudes. I think it can. Relying on a metaphor I am going to speak of the examination of numbers or magnitudes by means of their greatest common measure (or divisor) as looking "inside" them. I mean, simply, that we compare them by comparing the multiples that they are of their greatest common measure; that's their "inner" structure. Since for incommensurable magnitudes there is no common measure, there is no way to look inside and we have to find a way to judge their relations or at least to judge the sameness or difference of their relations from the "outside." So I ask: Could I have judged in the easy cases "from the outside?"

I don't know the historical fact, so I will speculate. Someone (often said to be Eudoxus) had, I will guess, the happy insight that he could judge sameness of ratio for numbers, not by finding their greatest common divisors, but by multiplying them by other numbers. It is easy to see that this works if we first represent four numbers A, B, C, and D which have the same ratio so that A:B :: C:D, in terms of their greatest common divisors. Let us suppose that the greatest common divisors are the numbers E and F, so that $A = K \cdot E$ and $B = L \cdot E$ while $C = K \cdot F$ and $D = L \cdot F$. The multipliers K and L can have no further divisor in common or else E and F would not be the greatest common divisors of A,B and C,D respectively. Also the multipliers K and L are the same for both pairs just because the pairs have the same ratio. This is Euclid's VII,20. It actually encompasses all three of his cases in one if we allow K or L to be missing to include his cases of multiple and part. He states them seperately, I gather, because his definition of number doesn't include the unit as a number.

Now the sameness which is so obvious when looking inside shows up on the outside this way: take any equimultiples of the antecedants of the two pairs, $S \cdot A$ and $S \cdot C$, and any other equimultiples of the consequents, $T \cdot B$ and $T.D$ and we will have

$$S \cdot A = S \cdot K \cdot E \qquad S \cdot C = S \cdot K \cdot F$$

$$T \cdot B = T \cdot L \cdot E \qquad T \cdot D = T \cdot L \cdot F$$

When we compare $S \cdot K \cdot E$ and $T \cdot L \cdot E$ either they are equal or one is bigger than the other; whichever of these holds must also hold between $S \cdot K \cdot F$ and $T \cdot L \cdot F$ since they are the exact same multiples of another single quantity. This is clearly so for any of the infinitely many pairs of numbers we might have picked for S and T.

If two pairs of numbers have <u>different</u> ratios, such multiples may or may not agree. If we are forced to look from the outside, we cannot escape the infinite. For it is quite sufficient to find one pair of multipliers S and T for which they do not agree in order to establish the difference of their ratios; but to establish their sameness from the outside, we must check every possible pair of multipliers to make sure that $S \cdot A$ and $T \cdot B$, $S \cdot C$ and $T \cdot D$ always agree in 'less than', 'equal to' or 'greater than'. This is Definition V,5.

We should also prove the sufficiency of this criterion. That is we should show that if the ratios were not same then the multiples could not all agree. In particular if we take as multipliers L and K, then $L \cdot A = L \cdot K \cdot E$ and $K \cdot B = K \cdot L \cdot E$ which are clearly equal; so that if the ratios were not the same there would be other multipliers, say N and M, so that $C = N \cdot F$ and $D = M \cdot F$ and hence $L \cdot C = L \cdot N \cdot F$ and $K \cdot D = K \cdot M \cdot F$ which could not be equal unless N were K and M were L which they are not.

It is clear that Definition V,5 can serve as an alternative to VII,20 for numbers and commensurable magnitudes. Euclid applies it however, to all magnitudes which can have a ratio. Those capable of having a ratio are, by Definition V,4, just those which when multiplied, are capable of exceeding one another. That's a criterion which applies to magnitudes whether commensurable or not.

What kind of a justification could we hope for in the case of such an enormous extension of the definition? No formal justification, I think. Our intuitive grasp of the idea of magnitudes and of the relations of size of magnitudes is more fundamental than our attempts to articulate those ideas. Our intuitions may tell us that magnitudes and their relations are all fundamentally the same and that the difficulty is merely our problem of trying to state these relations. But the "merely" doesn't help me, since the whole enterprise of geometry seems to me to be one of making our intuitions articulate. The only way we have been able to make our intuitions clear has left a sharp demarcation between the commensurable and the incommensurable magnitudes. I don't see how such an extension comes about except by analogy to the cases where we can see that the new criterion works. But I don't see how I could ever demonstrate the justness of extending this definition to cover the case of incommensurables unless I already had some other <u>clear and articulate</u> conception of what I should mean by same relation of size for them. But then I wouldn't bother with this one. I will return to this in Part II.

It should be clear that if we were given four arbitrary magnitudes, with any information about their relations, that we <u>could not</u>, in general, use V,5 to test the sameness of their ratios. There is no way of checking <u>all possible</u> multiples of the antecedants and consequents, since there are infinitely many of them. I will return to this in Part III.

Part II

Why should Euclid have chosen to flirt with the infinite and all its difficulties? For many of us, even as first readers, it has seemed obvious, when we do reach Proposition VI,16, that "Product of means and extremes" is a possible and even preferable alternative to Definition V,5. But Euclid did not choose it.

I would suggest that he did not, because of more general considerations which motivate the organization of much of The Elements. There is evidence that the material in Euclid's Elements was mostly old and familiar in his day. I believe that there is clear internal evidence that the organizing force of most of the thirteen books is irrationality and the problems of intelligibility which surround it. If both of these statements are right, then I infer that the essential task for Euclid was one of careful selection and arrangment. He must have had good reasons for his choice.

We may wonder whether a transition as easy as that outlined in Part I for V,5 is possible for "product of means and extremes". It can be put quite clearly in the same terms used above. In the case of numbers A, B, C, and D in proportion; having greatest common divisors E and F with $A = K \cdot E$, $B = L \cdot E$, $C = K \cdot F$ and $D = L \cdot F$ we would have for the product of means and extremes $A \cdot D = B \cdot C$, the representation $(K \cdot E) \cdot (L \cdot F) = (L \cdot E) \cdot (K \cdot F)$. We very probably can suppose, as we did above, that Euclid regarded the properties of multuplication as obvious or as sufficiently well known from earlier writers. Similarly for the sufficiency of this criterion.

Now we might expect to be able to carry this over directly from numbers to commensurable magnitudes. However a difficulty arises even in this first step. What can we mean by a product of magnitudes? Euclid defines products for numbers in terms of repeated addition of the number multiplied to itself as many times as there are units in the multiplier. This won't work for magnitudes, since they don't in general have units. The other obvious sense available is that which Euclid uses in VI,16. That is, we should form the rectangles from the given magnitudes as sides.

Even that won't quite do since the theory of ratio and proportion should be able to handle magnitudes which are not linear. A proportion between a pair of linear and a pair of planar magnitudes is still manageable since we can form the solid from them on each side of the required equality. Euclid classifies numbers in this fashion in Book VII, Definitions 16-19. I guess it's clear where I'm headed. We're out of luck along this line of thought when it comes to proportions involving pairs of magnitudes both planar, one linear and one solid, one planar and one solid, both planar or both solid. Intuitively it makes no sense to exclude such ratios or their comparison in a proportion. There is as much sense in comparing such magnitudes with respect to size as there is in comparing linear magnitudes.

There are two ways which I can think of that might be taken to get out of this impasse. One would rely on the "application of areas" and the other on a construction, possibly Descartes', that would give the product of two linear magnitudes as a linear magnitude. Descartes' depends, of course, on the theory of ratio and so could not be given the normal justification. Also we would have to reduce every planar or solid magnitude back into its sides and operate with them to form the necessary products as linear magnitudes. At a certain risk to clarity I am going to spare you a detailed account of either of these suggestions. I think that I can make sense of my objection to them without the details.

Above we found a way to tie either of our accounts of a same ratio back to a conception that was fully graspable and directly expressive of our intuitive understanding of ratio and proportion. In both cases we were forced (though at a different point in each), in order to extend our understanding, to take a step in the direction of formalization, that is toward a criterion based on a manipulation and itself without a direct intuitive meaning. Either of the suggestions I can make for applying the "mean and extreme" definition forces us several steps further in that direction. It loses the simplicity which it appeared to have and more importantly it loses any single meaning that is applicable to all the ratios we would want to deal with in geometry. Even if the wording of the definition could be kept the same the meaning of "product" has to be tailored to each type of ratio.

If the discovery of irrationality has, as I suppose, robbed plane geometry of its priveledged place as the paradigm of intellectual clarity then the greatest care should be taken in introducing the theory which copes with irrationality into the elements of the subject. The parts of plane geometry that can be treated without it should be; hence some of the arrangement of the Books in the Elements and the avoidance of any dependence of Books VII, VIII, and IX on Book V, in spite of Mr. Heath and company. It seems to me eminently reasonable that the introduction of such a messy subject into an otherwise intellectually neat one should be done with the greatest caution. And caution, it seems to me, is rightly identified here with keeping the intuitive basis clear. Of course it involves the infinite in some way; that's inherent in incommensurability.

Also we should remember that we haven't sacrificed product of means and extremes altogether by accepting V,5 as our definition since it can be introduced when needed as Euclid does in VI,16. We might ask why he waited so long to introduce it. Why not accept V,5 for definition, immediately prove the acceptibility of means and extremes as a criterion of sameness and proceed to do all the proofs of Book V using it? For the same two reasons, I suppose. Book V is the basis of the whole geometric theory of similarity in Euclid and must be able to deal with all magnitudes whether commensurable or not, whether linear or not, in order to be truly general. Otherwise Book VI and later theorems on similarity would be only the vestige of a theory of similarity. And even if means and extremes can be saved by sufficiently sophisticated artifice in its full generality, it would be a most cumbersome way to introduce irrationality into geometry. So if we are to base the theory of similarity as solidly as possible, the definitions and basic propositions of the theory of ratio and proportion need to be as close to the intuitive basis as possible.

Part III

Now I would like to return to the infinite and V,5. I claimed in Part I that V,5 could only be generally applied to proportions of numbers or commensurable magnitudes, in which cases it is not needed since we have VII,20. Though true, that is an inadequate statement. There are two common ways in which we use the definition, well exemplified by Propositions V,4 and VI,1.

Let us consider V,4: If four magnitudes have a same ratio, any equimultiples of the antecedants will have to any other equimultiples of the consequents a same ratio. We start with a set of magnitudes and are given the sameness of their ratios. That is we are gauranteed that for <u>all</u> the infinitely many pairs of multipliers, the equimultiples of the antecedants will bear exactly the same relation (less than, equal to, or greater than) to those other equimultiples of their consequents. With so much given it's not so hard to draw a comparable conclusion. For the infinitude of those equimultiples in the conclusion is drawn directly from the given infinitude of equimultiples in the hypothesis.

Of course, we never really handle the infinitude of equimultiples at all. We do precisely what Euclid does in many other places. If a theorem deals with all polygons, Euclid proves it for one case and it is clear from the form of the proof that it applies equally well to all others.

The form of a proposition like V,4 is hypothetical. I mean that it has the form: given one same ratio another slightly altered same ratio can be derived from it. Most of the propositions of Book V are hypothetical.

VI,1 is interestingly different. That's the proposition that triangles and parallelograms under the same height are as their bases. Here we can establish the conclusion because we know certain relations among the magnitudes themselves, on geometric grounds, which gaurantee that any equimultiples of the antecedants and consequents will be related alike. Here we have established an actual case of sameness of ratios of magnitudes which is perfectly general (no assumption of commensurability is necessary). Again, of course, Euclid takes only one case of some equimultiple of the antecedants and some one other of the consequents and makes his arguments on them. The form of the argument makes it clear that the same argument works for any multiples we might choose and hence for all.

The infinite involved in V,5 now looks a little less impressive perhaps. In all cases like this there is a relation between the magnitudes of the one ratio and those of the other which is a finite and proper object of thought. Will Williamson, a tutor in Annapolis, asserts that Euclid never uses πας in the plural. That is, not only does he never entertain an infinite mathematical object, but he never deals with an infinite set in its multiplicity. He only acknowledges them through their form. I can't think of any case other than the two exemplified by V,4 and VI,1 in which I have been able to understand how we can handle the infinite and in those, in a very real sense, we don't! The calculus and fancier modern theories don't seem to me to change the situation. I suppose that some of the puzzling paradoxes in modern accounts arise from ignoring that fact. You can talk all you like about arbitrary infinite sets and functions, but the only thing that I can see that anybody ever actually thinks about is a finite set from which he makes certain analogies or a defining property even if a complicated one) or relation which characterizes the set or function. I don't really see that I have any definite object before my mind when Russell or someone else starts discussing <u>the set of all sets</u>.

If we were ever given four arbitrary magnitudes and asked to judge whether their ratios were the same, we would be stuck. Nothing in the two examples I've discussed is of any help. Here we would confront the infinite "in the raw." In a sense, it is demonstrable that almost all ratios of magnitudes are of this sort. This is a version of the theorem that almost all real numbers are transcendental irrationals.

Part IV

Now I want to consider our notions of definition in mathematics.

Because of the peculiar character of the sort of extension by analogy of a definition which I have speculatively attributed to Euclid (or Eudoxus), we have a situation in which it is natural to ask, "is the definition right?" At the end of Part I I mentioned that no formal answer seemed possible. For perhaps this very reason it is often supposed that such definitions are stipulative. As Humpty Dumpty said (in a rather scornful tone) "When I use a word it means just what I choose it to mean - neither more nor less." "The question is," said Alice, "whether you can make words mean so many different things." "The question is," said Humpty Dumpty, "which is to be master - that's all." Now "is it right?" often seems illegitimate when asked of a definition in mathematics, but I will maintain that it is always fitting even though often not of much use.

I can put this in another way by saying that I view mathematics as one of the purely intellectual empirical sciences. I may be misusing the word 'empirical' in its common connotation, but in my vocabulary it means something to be discovered by experience (though not in this case sensible experience). By saying this I intend to deny that mathematics is a kind of self-contained formal game and I assert that its goal is to discover important truths about our experience. But this description is far too general.

The project of The Elements is to make some of our simpler intuitions about magnitude and shape articulate. As in every attempt to articulate our notions, the result may fail to be adequate to our intention. It seems unlikely (at the very least) that anyone will turn up cases so pathelogical as to be counterexamples to V,5 at this late date, but I don't see that we have a right to convictions of certainty in this. The theory of similarity in Euclid's Book VI could have, in its theorems, come down to him directly from an age predating the discovery of irrationality. The arguments would, I suppose, have been based on an understanding of ratio and proportion like that of Book VII instead of Book V. A very different Book VI it would be. But it would have expressed the intentions of those mathematicians so far as they could see.

I guess I have already made the shift from suggesting doubt about the certainty of our definitions in cases where we are clear that they rely on extreme extensions by analogy to all our conceptions and definitions in mathematics. We may be quite certain in some cases that our conclusions do follow from our definitions, but we may at any time be forced (and profitably) to face a major revision in those definitions in the face of incontrovertable evidence. Such a difficulty occurred with respect to the classical analytic definition of dimension around the turn of the century and led to far reaching and helpful reconsideration of the definitions. Perhaps Einstein, Whitehead and others have forced such a reconsideration of our conceptions of space and time, among the most fundamental conceptions in theoretical physics. It seems that the longest standing and most fundamental of our "purely" mathematical notions are least likely to be subject to such a shake up, but they should also make the most interesting difficulties when and if they are overturned. The discovery of irrationality was surely one such great upset. Perhaps, as Dedekind suggests, the upset goes far deeper than even Euclid ever realized.

But then how should we view the subject of this perennial, if not universal, human propensity to mathematize? Is it only the desire for a neat, consistent intellectual scheme that motivates us? To what are we appealing when we claim to find a counterexample, not merely to a proposed theorem, but to a central definition? If we say that we know ratio and proportion only through V,5 how do we lay hands on anything that can confute it? And yet, if we really grasped a deeper understanding by which we could judge V,5, why not articulate that understanding of the matter and be done with V,5?

The Socratic paradox always reappears: we both know and do not know the same things at the same time. In a sense it is only our articulations of the direct intuitions which we "know" and yet those intuitions have a status not derived from the proffered account of them, and can unseat that account by providing evidence contrary to it.

I think we mislead ourselves about the present state of our understanding if we pretend that the only problem in the past was a hidden assumption we hadn't been careful enough to make explicit. That notion is much of the motivation of the attempts to formalize mathematics. There is no way to be careful in that sense. The very existence of a distinction between the commensurable and the incommensurable about which there was an assumption to be made seems not to have existed for the pre-pythagoreans - the very notion seems to have been discovered along with the discovery of the existence of incommensurability.

It is not very helpful to be warned not to make assumptions with respect to distinctions which you have never even conceived. Nor is it possible to formalize such an unconceived distinction as Russell discovered somewhat belatedly. Obviously, the warning would be good advice if only we could follow it.

This is one way to formulate the program of the late Nineteenth Century and Twentieth Century attempts to formalize mathematics. The hope was, I gather, to blame most of our past difficulties on our reliance on "intuition" and to make all assumptions and steps of proof or derivation so formal and explicit that no "hidden assumption" could remain lurking in the shadows. But, I confess, it has always seemed to me that that way of putting it wholly misses the problem. I thought that one of the main and most salient points of Plato's diatribe in the Phaedrus against those who believe in the power of the written word to express "permanent truths of great worth" was that the assumption of explicitness possible in language (even mathematical "language") is false. The power of conception of the reader or hearer is an inherent limitation in the situation. Likewise for the writer; there is no magic to be attributed to the process of writing or of formalization.

A similar point became a theorem for one of the masters of the formal game, the famous Mr. Goedel. One way of putting it is this: any formal system good enough to include a fair share of classical mathematics (please note my weasle words) must have a vocabulary and syntax rich enough so that statements can be correctly formed in its own terms the truth or falsity of which can never be determined by the formal machinery in it. This can be read as an inherent limitation to the process of formalization itself.

Part V

Finally I would like to consider our notion of proof in mathematics. It seems to me that we commonly consider it as a method by which we become certain about things concerning which we are in doubt. We suppose, with Aristotle, that proof always begins with truths about which we are certain and that in deduction we are able to transmit that certainty to the conclusions. I doubt that most of Euclid's predecessors, Plato or perhaps even Euclid himself thought that.

Let me suggest an alternative view. It's not the certainty at the end of the proof which we're after, but the very web of relations constructed during the course of it. That web of relations between the mathematical objects or the terms which enter the statement of the proposition lead us to "see" the relation stated by the proposition itself. Previously understood propositions help by stating sub-relations with which we can construct the web. When clarity finally comes, even with a long and complicated proof, it seems to me that I always see the thing whole, all at once. The proof becomes not a succession of steps, but a view seen in one looking.

It is clear that there may be several different sets of relations which would illuminate the relations stated by the proposition. It seems to me proper, particularly in the case of a difficult proposition that a geometer should give us all the proofs, not trivially different, which he knows. Not so that we might take our choice, but so we may put all of them together in a fuller understanding. I don't know that this was ever commonly practised among geometers, but it sometimes happens in a mathematics tutorial when several people present alternatives to the proof in Euclid.

Secondly, I would suppose that even though we had first proved a proposition A from some other proposition B, it might also be illuminating to go ahead and construct a proof of B which in some way depends on A. This is commonly called circularity, but that unkind suggestion that "it gets nowhere" seems to have already taken for granted that mathematical examination must proceed by a linear deduction. What's wrong with mutual illumination? I suppose it's obvious that the objection to circularity arises to a large extent from the notion of inheritable certainty. This kind of reciprocal proof was, according to the histories of mathematics, common before Euclid. It is often presented in a manner that suggests that those predecessors were idiots and hadn't at all understood what mathematics is all about.

If such procedure was common before Euclid why did he avoid it? Not necessarily because he shared our conviction about inheritable certainty. I would guess that he avoided it because of his concern about irrationality. If, as I suggested earlier, the existence of that beast was a threat to the whole edifice of plane and solid geometry then it seems eminently reasonable not to let it creep into the parts of geometry where it can be avoided. A linear deduction facilitates the seperation. A so called circularity increases the difficulty in keeping track of the line of descent of propositions. As Euclid arranged them Books V, VI, X and XIII could fall without the loss of I-IV, VII-IX, and some of XI and XII.

In conclusion I will not try to summarize the various things which I have said, but perhaps I can give a general characterization of the view of mathematics I have been suggesting. Mathematics seems to me much more like the dialectic of which Plato's Socrates is always talking than it does like a straightforward method of grinding out results. It's a bootstrap operation. You start in the middle and work in all possible directions at once. Without a solid rock to stand on how can we get the leverage to lift anything up? I guess like Socrates I would have to deny the description implicit in the question. He resorts to a theory which claims that we already know all the things we need to find out. That encompasses the claim that there is something to find out and the claim that wherever we may have gotten our results are provisional. I think that any of us is most apt to be close to this sense of the enterprise when we are trying to do an "original" or a difficult problem. It's hard to know where to begin or when you're getting anywhere. When you finally do make the connections which solve the problem for you, even if the relations are quite clear it will generally take a complete recasting of what you have understood to turn it into a proof. Why bother? Because even you might need a regular sequence of steps to lead you back into that insight at another time.