Principles for Analysing the Meaning of Megalithic Art

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Abstract.

The corpus of megalithic art dates from the neolithic period in Western Europe, and is apparently abstract in character. The problem of its interpretation has attracted considerable interest, but there is no agreed methodology. The essential difficulty is due to the finiteness of the total body of data. In this respect, the problem is essentially the same as other problems of interpretation, such as cryptanalysis, the analysis of ancient alphabetical inscriptions, and so on. The paper is a tentative attempt to establish a foundation and a procedure for the rational analysis of this problem. The approach taken is based on an adaptation of Popper's approach to scientific theories.

1. Introduction.

The term *megalithic art* normally[1] refers to the art found on the large structural stones of megalithic tombs, and especially on the kind of tombs known as passage–tombs, which date from the neolithic period. There is a rich corpus of such art, and a good deal of it has been catalogued, photographed, sketched and described in published studies [Borlase, Chadwick, Coffey 1892, Coffey 1912, Eogan 1984, Eogan 1986, Hartnett, Herity, Herity and Eogan, O'Kelly 1982, O'Kelly 1986, O'Kelly 1988, Ó Riordáin, Ó Riordáin and Daniel, Shee–Twohig]. In all, about 900 stones bearing this art are known in Europe, principally in Iberia, Brittany, Britain and Ireland. Over half the total lie in County Meath, and most of those are on the three great tombs at Knowth, Newgrange, and Dowth, in the megalithic cemetery at the bend of the Boyne [Eogan 1986,p.168]. Most of the work is carved in the stone, but some paintwork survives in Iberia. The work is approximately five thousand years old.

The art is fairly enigmatic. For the most part, to most people, the carvings do not appear to be direct, natural representations of anything sensible. This circumstance has led to much speculation.

The first major artwork came to light in 1699, and by 1912, Coffey could justly remark [1912, p.89]: "This question" — the meaning of the markings at New Grange — "has exercised the minds of many fanciful archaeologists for a long time, but little more than absurd guesses has been the result." He went on to suggest a possible connection with sun–worship, but thought it premature to draw firm conclusions. Since then, the 'fanciful archaeologists' have multiplied. People have not hesitated to appeal to the Rig–veda and the 'collective unconscious' for approaches to the art. Rather more serious approaches have been attempted using calendrical and astronomical ideas, using surviving folklore, and comparing artefacts of later Irish cultures [Brennan 1980, Brennan 1983, Macalister,

[1] The term is also, and justly so, applied to the architecture of megalithic structures, to the sculptural and other designed features associated with them, and to portable art linked to these structures.

McKenna–Lawlor, Ó Síocháin, Streit]. In perhaps the most logical approach, some antiquarians have tried to trace a continuous line from mesolithic naturalistic representations, through patterns of increasing abstraction or simplification, to the patterns discernible in megalithic art. This has been about as successful as attempts to trace the pedigree of *Homo sapiens* in the primate fossil record: there is a problem of missing links. Worse, the urge to interpret has led people to run ahead of the evidence, and even, as in the case of the Abbé Breuil (cf.[Herity, Mac White, O'Sullivan 1988]), inadvertently to distort the art when sketching it. As M. O'Kelly remarked, people are inclined to find in the art what they come looking for, and many interpretations tell us more about the interpreters than about the art.

Not surprisingly, this kind of thing has increasingly caused serious archaeologists and art–historians to shy away from the question of the meaning of the art. Most believe that the art had significance, but doubt that much may be known of that significance. Until recently, serious scholars still felt obliged to offer some conjecture as to meaning, even though, as Macalister [1921] pointed out, such examples as the art of Australian aboriginals warned of the pitfalls. Nowadays, such guesswork is discouraged. Archaeology is a science, and its practitioners wish to deal in facts, rather than opinions or speculations. In dealing with the art, they try to describe it in an objective manner (— this is not as easy as it sounds —), and to classify and compare it without regard to possible meaning. They try to determine the tools, materials and techniques used, and they compare the positioning of the art on different stones, and the positioning of decorated stones in their context. If they have opinions about meanings, then they keep them out of serious papers.

In the seventies, a modest amount of speculation was still accepted, and Herity was willing to offer this view [1974]: "It now seems clear that the ideals of the passage–grave artists included the portrayal of magical symbols on the stone slabs of the tomb, the combination of these symbols into decorative or artistic designs, some of them having a strikingly anthropomorphic appearance, and the effective display of them in the tomb with the desire of enhancing both tomb and art". Harbison [1976] stated that the carvings "must have been of great religious significance", but were "still an enigma. They could, however, be symbolic representations of the deities worshipped on the site, and of the world and life they created." Harbison, Potterton and Sheehy [1978, p.17] suggested that Fourknocks art might involve depiction of human figures.

Limited in content as those statements are, they seem almost rash by comparison with the austere style of the eighties. Writing of the Knowth art, Eogan (who, as excavator of Knowth, is responsible for bringing more megalithic art to our knowledge than anyone else) said [1986 p.146]: "wholly nonrepresentational[2], it consists of geometric and other abstract motifs...", and goes on to suggest that "the designs were a form of religious symbolism, connected with a cult of the dead and having significance in that context." This very cautious assessment of a possible significance of the Knowth art is similar to the remarks about Newgrange art made by M.J. O'Kelly [1982], and is based solidly on the archaeological evidence, which demonstrates that passage tombs were indeed used for burial (without necessarily excluding other functions). Shee–Twohig [1981] provided a

[2] Presumably nonrepresentational is used here in the sense of 'abstract', or 'non–naturalistic', rather than 'representating nothing'.

monumental inventory of all known art outside the Boyne valley, but her analysis of the art was essentially limited to a clinical statistical study of motifs.

Archaeologists avoid the question of meaning because they wish to deal in *facts*. What is a fact? A fact is what you and I and that hard–headed fellow over there will agree on, once the evidence is laid before us. There does not appear to be a standard and acceptable way in which to arrive at agreement on the merits of proposed interpretations of megalithic art.

That is what this paper is about. Is there a reasonable way to assess interpretations? Is the art inherently uninterpretable? Is there a science of interpretation? If so, what are the rules? How far may interpretation go? What can sensible men and women agree on, in relation to interpretation? Is the meaning of megalithic art to be classed with the chronology of the hebrew kings, described by St. Jerome [Migne, col. 676] as "more the occupation of a man of leisure than a scholar"?

I quite appreciate that some of those interested in the art are very practical–minded people, far more concerned with getting things done than with worrying about abstract principles. But it *might* be possible to glean some meaning from the art, and that would be interesting. Practical people will surely ask for the evidence in favour or against an interpretation, and we must begin by agreeing what kind of evidence is appropriate. Unless we can do this, no headway will be made.

Perhaps, to avoid futile argument, it is as well to remark that I operate exclusively in the western intellectual tradition, using Aristotelian logic, and the basic ontological assumption that there are different things in the world, essentially different from me and different from one another. I do not, however, assume that the neolithic Irish shared my world–view.

The problem of interpreting megalithic art is not an isolated one. It is in principle similar to the problems of deciphering ancient writing, of analysing the motives of byegone captains and princes, of interpreting the intended meaning of programme music, of breaking alphabetic, digital, and more amorphous ciphers, and of interpreting the sedimentary record in geology. In discussing it, it will be helpful to compare it with these problems, and also to contrast it with other problems of interpretation, such as the determination of the laws of physics.

I hope that what I have to say will be broadly acceptable and useful. I would not, however, be surprised if someone could extend and improve on my conclusions and suggestions. Criticism is welcome. All I ask is that the critics criticise what I actually say, and not what they think I say. It has to be said that I face a particularly acute language problem in writing this article, and I think it helpful to get the problem into the open. The topic relates to several disciplines, each with its own nomenclature and stylistic conventions. By training, I am a mathematician, most at home with mathematical usage. In some respects that usage conflicts with that of academic philosophers, and even more so with that of antiquarians. I will try to make my meaning clear to all, but there is one inevitable problem. It is the problem of the *dangerous sense (d.s.)* of words [Lewis]. The d.s. of a word for you is the sense you give it by default, in the absence of contextual indications to the contrary. C.S. Lewis was concerned with shifts in the usual d.s. over centuries, which can lead modern readers to completely misunderstand old texts *without realising that the*

reading offers any difficulty at all. This is much more serious than struggling with some evidently obscure passage, and coming down on the wrong side. The same problem looms between specialists in different areas. If you and I use some word in different ways, and I am aware of that, then I can be circumspect. But, due to long habit, I may use a word in a specialised sense, and, also due to long habit, you may read it in a different specialised sense. We can only avoid this problem by not talking to each other at all, and perhaps that is one more reason why the spirit of the age opposes those who would offer an opinion outside their own specialty. In the present context, I feel that mathematics has something to offer, and I beg pardon for the inevitable problems.

As long as I'm being disarming, I should address a word to those readers who experience a sense of outrage when someone, particularly a mathematician, starts being *logical* about *art*. Understood or not, megalithic art has a powerful emotive effect on many people, and logical analysis offends them because they feel that this art, and all real art, is above logic, or beyond logic. Now in fact I agree, as any sane person must, that there are non–logical aspects to art, even a non–logical essence. But that actually has nothing to do with the appropriateness or otherwise of using logic to discuss art. If there are any *truths* about a work of art, then logic may be applied to these truths. And, of course, there are truths about any work of art, such as who made it, how, and why. Some of these may be discoverable facts. They may not, in the last analysis, be the most interesting aspects of the work, but they are interesting enough.

2. What can we know of the meaning of the art?

The first problem I consider is this: What knowledge can we hope to attain of the meaning of megalithic art?

Take for instance, the great entrance–stone (commonly called kerbstone one, or K1) at Newgrange, with its familiar pattern of spirals. What did it mean, if anything?

Ideally, it would be interesting to know what the artist had in mind, and what effect the work might be expected to have had on its intended audience. It would be interesting, but I imagine it is not feasible. Even allowing for the fact that response to a work of art can be expected to vary enormously from individual to individual, we can scarcely hope even to identify the broad cultural themes with which the work is connected. We are not in a position to interview the artist. We know nothing of his or her language, except that it was eradicated and replaced by a celtic one over two thousand years ago. We know something of the way of life of the artist's people, but not what they thought about life or death. We are not much more likely to penetrate to this knowledge than our neolithic ancestors would be likely to guess the significance of a simple cross for their modern descendants. They would, perhaps, have looked at K1 and immediately recognised its reference to some great and well–loved tale or tales, told and retold at their firesides in the long Winter evenings. How would we know?

But a work of art is normally interpretable on many levels. It has layers of meaning, and perhaps on a modest level we might divine the artist's intention. Suppose we take a painting of St. George slaying the dragon. In this part of the world, many people would react by thinking of England. Many people who are outside this culture will at least see a man on a horse and a monster. Some people will see a man on a monster and another

monster. Some will be so remote from our culture and its representational conventions that they will see no sensible pattern at all, just shiny colours (It is said that the headhunters of New Guinea saw nothing intelligible when first shown photographs). We are at present at the minimal level in relation to megalithic art. Perhaps we can climb to the next level, at which we will discern some natural objects or some simple external reference here and there in these works. We might be able to do that, while remaining insensitive to all or most of the deeper significance of the works. Worse, unless we are careful, correct recognition of meaning at a basic level could lead us to completely misunderstand a deeper level. For instance, someone who knew nothing about our culture might correctly recognise a drawing of an arrow through a heart, but interpret it as a reference to a killing, or even a cult of human sacrifice rather than an indication of tender passion. In any case, we are unlikely to understand anything of deeper meanings, unless we can first attain some trustworthy knowledge of meanings on the most basic level.

This brings us perilously close to the question: What do we mean by knowledge, in the first place?

We are never going to get anywhere if we get bogged down in fundamental epistemology, and I have neither the expertise nor the inclination to give this question the space it deserves. All I want to do is make a few common–sense remarks about it, so that we understand one another. My objectives here are practical, and I am less interested in what 'knowledge' ought to be, as in what kind of knowledge is possible, and what kind of knowledge is regarded as acceptable by ordinary people, as opposed to specialists in theories of knowledge.

In the usual sense, knowledge (of propositions) is a species of true belief or conviction. A proposition is a sentence that is true or false. One might say that I know a proposition if (1) it is true, rather than false, and (2) I believe it. Such knowledge is not uncommon. Indeed, for most simple propositions you will find people in favour and people against, and one or other position must be correct. Many people would reject this definition of knowledge as too liberal, arguing that 'real' knowledge would be *justified* true belief. They would say, in other words, that it is no use being right, unless you can offer a decent reason for your belief[3] Belief is an act of the will, and they feel that the rational intellect should have an essential function in knowledge. The demand for justification is especially strong among those who believe that knowledge should be communicable in a foolproof manner between rational beings. But if knowledge is *fully justified* true belief, then knowledge is a rare commodity. With the possible exception of direct psychological perceptions (such as 'I am conscious of myself', 'I feel happy')[4], the only facts outside mathematics[5] that qualify are those (such as religious knowledge) justified on the basis of an act of faith. As

[3] Littlewood, relating anecdotes about Bertrand Russell, provides this instructive tale [Littlewood, p.128]: "He told me that he had conceived a theory that 'knowledge' was 'belief' in something which was 'true'. But then he met a man who believed that the Prime Minister's name began with a B. So it did, but it was Bannerman and not Balfour as the man had supposed."

[4] It is arguable that direct perceptions are not propositions, but something else, and that all propositions asserted have the character of hypotheses.

[5] I mention this as a possible exception only because some readers will feel sure about

Popper says [1972, p.24], "there are all kinds of sources of our knowledge, but *none has authority*." Consider the prime example of what people commonly call knowledge, namely our knowledge of scientific facts.

What is the character of our scientific knowledge? There are very few people left who regard it as a body of certainly true statements. Most scientists nowadays accept some version of Popper's thesis (cf. [1972, p.37], [1980]), that our natural science amounts to a series of *falsifiable, or refutable, or testable, or empirical* theories (i.e., loosely speaking, theories that might be refuted by experiments[6]). Scientific laws and theories, such as Ohm's Law or General Relativity, make assertions about the world which might prove to be wrong. At the next experiment or observation, any one of them might fall. This has in fact happened to many past laws and theories, and indeed, it is reasonable to expect that practically all present–day science is not only falsifiable, but actually false, in varying degrees, and will have to be adjusted as time goes by. Statements about the world that of their nature cannot be proven false are usually excluded from the field of science. (Also excluded, of course, are those statements which contradict themselves, i.e. which are false simply because of logic, quite apart from any reference to reality.) In other words, the essence of scientific knowledge is not that it can be proven true, but that it can be proven false. The only scientific statements that I can assert as facts are the results of particular experiments, such as the statement: I saw the Sun rise in the east today[7]. That the Sun will rise in the east tomorrow is a falsifiable theory. It is not considered essential that experiments or observations designed to test a theory be easy to set up or admit of frequent repetition. Many scientific conjectures are tested far less frequently than the conjecture that the Sun will rise daily. Some (such as cosmological theories) are next to impossible to test directly, and for many the ideal of the 'repeatable' experiment is unattainable.

Scientific knowledge is only conjectural, and is the result of successful speculation.

But that is not all. It shows only a first approximation to the real state of affairs about conjectural knowledge. The conjectures of science cannot be proven false with certainty, because it is always possible to find grounds to suspect any given evidence that appears to contradict them. An extreme example is provided by the Flat Earth Society, whose members remain unconvinced that we inhabit a globe, and who offer an entertaining account of the 'reality' underlying such empirical sense–data as the TV pictures from

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mathematical facts. However, there is a universal rule: the more expert, the less sure. Personally, I am in broad agreement with Lakatos [1977] about the 'facts' of Mathematics. I would summarise my view thus: Mathematics, like many other areas of knowledge or expertise, is a highly articulated, functioning social system that is 'up and running'. It might be compared to a living human body, or the solar system. If you 'stop' it to analyse it completely, then it falls apart, becomes something else, and cannot be started again. Foundation–work studies the corpse of Mathematics.

[6] Popper [1980, section 21] defined a falsifiable theory to be one that is inconsistent with some 'basic statement'. Basic statements [1980, section 28] are a kind of singular existential statement that might be asserted; they are not quite the same thing as 'results of experiments', or even '*a priori* conceivable results of experiments'. They are actually a kind of low–level hypothesis. See below.

[7] However, see footnote 6.

the Apollo Moon–mission. The real situation is that scientific conjectures are neither rigorously verifiable nor rigorously refutable. How, then, does science proceed? What happens, strictly speaking, is that conjectures are refuted on the basis of 'accepted facts'. Accepted facts (for example, the Sun rose this morning) are really another species of conjecture. In Popper's language, they are examples of singular existential statements, or basic statements of some field of application[8]

Usually, in practice, refutations are asserted on a *probabilistic* basis. Such assertions are, at least implicitly, based on a Bayesian distinction between *a priori* and *a posteriori* probabilities of observed events. Experimenters say things like:

(a) 'The world was not first created in 4004 BC.'

Pressed to justify this statement, they might (if they keep their tempers) revise it to:

(b) 'It is highly unlikely that the world was created in 4004 BC.'

Pressed further, they will almost certainly abandon the discussion, but if they are unusually patient they might revise it further to:

(c) 'At the 99.99% confidence level, the hypothesis of creation in 4004BC is rejected.'

Statements of the latter kind involve additional problems, because an experimenter cannot make such a precise, numerical assertion without invoking further hypotheses. Worse, these hypotheses are of a completely different order from the primary hypothesis under investigation, because they involve statements about the relative likelihood of hypothetical events. For instance, we would logically reject the 4004 BC creation if we found something in 1987 AD that was demonstrably at least 5991 years old. Suppose we dig fifteen old chair–legs out of the ground, and get radiocarbon dates for them all predating 50000 BC. Doesn't that settle the matter? Unfortunately not. Radiocarbon dating is not a completely reliable procedure. First of all, laboratory measurement of the radioactivity of the carbon in the chair–legs is subject to possible error. Such dates are usually reported with estimated standard deviations, such as $\pm$5000 years. That doesn't mean that 5000 years is the *maximum* possible error; there is a small possibility that the error is over 46000 years; small but positive. Second, unsuspected long–term variation in cosmic ray intensities could vitiate the date calculation. Third, the chair–legs could have been deliberately doctored by some agent in the past. Of course, none of these factors is at all likely to convert radiocarbon dates as old as that into calendar dates after 4004 BC, but that is not the point. The point is that the possibility exists, no matter how small, so that we cannot be certain. Further, in order to put a number on the probability that the chair–legs contradict the 4004 BC creation, we must make additional hypotheses about the probability of various laboratory errors, the probability of variations in cosmic–ray intensity, and the probability of deliberate treatment of the chair–legs in the past.

What it boils down to is that

scientific theories may be described as theories capable of rejection at reasonably high con-

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[8] His basic statements would also include statements such as 'two suns were observed in the sky this morning'. Any theory that rules this statement out is scientific on that account. The concept of a *field of application*[1980, section 38 and p. 285] allows the relativisation of falsifiablity, thus allowing us to speak of a biological theory or a chemical theory, and so on.

fidence levels on the basis of experimental tests and under generally acceptable hypotheses about the likelihood of various contingencies.[9]

To be clear about this: the 'generally–accepted hypotheses' I am talking about are not the scientific hypotheses under investigation, but the hypotheses made by the experimenter about the probability of various kinds of laboratory error and other contingencies which might affect the interpretation of the experiment. What hypotheses of this kind are generally accepted is simply a matter of convention or even fashion in the particular field of science concerned, although it must be said that this is a rather fluid business and that common hypotheses may well go unstated, or even unrecognised. For example, an archaeologist who is considering the significance of finding a Roman coin in an early Irish grave will cheerfully ignore the possibility that it flew there from Lutetia because a modest proportion of its molecules just spontaneously happened to move in the same direction. He ignores this, if he thinks about it, because the kinetic theory of heat says it is possible, but unbelievably improbable. But the kinetic theory says this on the basis of a (quite plausible, but gratuitous) *assumption.* (This is not intended to be a criticism of archaeological procedure. The hypothesis that coins have not flown about by themselves is, by any standard, one of the least controversial I can think of.)

What confidence level is considered reasonable also varies from field to field, with physics at the top and sociology (if you count sociology) at the bottom. A major factor, in practice, is what confidence level is attainable.

In this connection, a quite fundamental distinction may be drawn. There are theories for which rejection is theoretically possible at *arbitrarily–high confidence levels.* Essentially, these are theories for which an arbitrarily– large number of independent tests is conceivable. These are the blue–chip theories (— at least until they have failed a few times). Most theories in the fundamental sciences are of this type. The remaining theories have an upper limit on the confidence with which they may be rejected. This limit is usually due to an inherent limitation on the accuracy with which something can be measured, or to a finite limit on the total amount of relevant information conceivably obtainable. For example, the theory that space has finite volume is hardly scientific at all, since the possibility of a very very large finite volume is impossible to rule out even at any confidence

[9] This is an attempt to describe what is meant by a scientific theory in terms that a working scientist would recognise. Hence the inclusion of the references to confidence levels and likelihood. From a strictly logical point of view, it would be better to say that a theory is scientific if there is at least one conceivable experiment with at least one conceivable result that *if accepted* would lead to the rejection of the theory. This avoids dragging in the ground of rejection explicitly. Bayesian analysis of the 'likelihood of hypotheses' is widely practiced, but is nonetheless a controversial technique, not least because it depends on a subjectively–given way of assigning probabilities to propositions, but also for other reasons [Eells1982, 1988]. This is more–or–less exactly what Popper did. A significant point here is that by an experiment we do not just mean a single observation, made once and for all. It should really be something that could be repeated ad infinitum, and what has to be *accepted* is that repetition would always produce essentially the same result. This is part of the reason why 'experimental facts' are really a kind of hypothesis.

level. The more specific theory that space is finite with volume at most 10^{150} cubic metres is falsifiable, but with limited confidence, even assuming a modest capability for interstellar travel. The theory that space is finite with a volume of 1 cubic metre is already falsified with arbitrarily–high confidence on generally–accepted hypotheses (i.e. unless you deny the reality of space or its independence from your mind). As a final example, the archaeological theory that the Incas had no wheels is falsifiable with limited but fairly high confidence. The finding of a single wheel in any place would cause its rejection at whatever confidence attached to the place's link to the Incas, and there are a large number of places linked to the Incas with high confidence.

On this basis, I propose that we approach the meaning of megalithic art by making and testing conjectures, and that we restrict attention to falsifiable conjectures — conjectures that may be refuted by evidence.

In other words, I suggest we abandon the idea of trying to find interpretations that we can expect to prove definitely correct. I am almost sure that it is not feasible to prove an interpretation correct. At any rate, I have no idea how it might be done.

3. How do we recognise a refutable conjecture?.

If this approach is to be practicable, then we must be able to determine when a conjecture about the art is refutable. We need an objective definition of the concept of refutability. There is a real difficulty here, as will appear.

Some conjectures about the meaning of megalithic art are plainly refutable, and others are plainly irrefutable. Some can be refuted on grounds which refer only to the art, and others can be refuted using available archaeological or other data. To take a couple of simple examples, the conjecture that the Newgrange stone K1 is a reasonably accurate map of the Boyne Valley cemetery can be rejected on internal grounds, and the conjecture that the Newgrange stones carry blueprints for an iron foundry can be dismissed with reasonable certainty on external grounds. An example of a conjecture, irrefutable on the basis of the art alone, is that all the symbols are Sun symbols. This, or any other conjecture which treats the complexities of the symbolism indifferently, is incapable of internal disproof. Essentially, it is equivalent to saying that there is no message, or at most an extremely simple message, like the message in English: 'the Sun'. This is arguably implausible on external grounds, but not impossible. An example of a conjecture, irrefutable on any grounds, is that all the carvings are depictions of scenery on a planet of Arcturus, due to a group of space–travellers, who came, built and decorated the mound, slew and interred a few locals, and departed whither they came, leaving the awestruck natives with a healthy respect for the mound and with something to imitate.

Among the examples just given, the refutable conjectures are in fact refuted conjectures. This is the rub. There is no problem about declaring a conjecture falsifiable if it has already been refuted, but what exactly does it mean to say that an as–yet–unrefuted conjecture is falsifiable? If it is a matter of some tests, and those tests are carried out, and the hypothesis survives them, and there are no more tests to do, what does it then mean to say that the tests *could have* refuted the hypothesis?

We are here up against the problem of limited data. The total corpus of megalithic art is finite. Eventually, any hypothesis that is not refuted by the data will have passed

all possible tests. What we want is some measure of whether the data really tested the hypothesis.

In ordinary (blue–chip) scientific work, the main criterion for falsifiability concerns the quality of the *predictions* of a theory. A theory should make statements about the expected outcome of experiments that have not yet taken place. A similar criterion was applied in the famous test of the proposed deciphering of Akkadian, in which three experts submitted sealed translations of a previously untranslated tablet. In deciphering alphabetical ciphers, the criterion is that the decrypting procedure should produce intelligible words when applied to the message. In considering Hitler's motives in declaring war on the US, the criterion is that any hypothesis must fit with what is known of Hitler's actions about that time.

But there are problems with all of these areas. For instance, we have seen theories that looked scientific, and were widely accepted, and only gradually stood revealed as unfalsifiable. The prime example is the lumeniferous aether. In the end it was clear that "the aether has only negative properties", i.e. nothing could be envisaged that would disprove the existence of the aether. In spite of that, many people who had grown up with the aether continued to carry it, as part of their mental baggage, to the grave. In more recent times we have seen elementary particle theories which were said to be verified because they predicted the existence of one or two new particles, specifying their masses and other properties, and these particles were then found. What really happened is that the theories contained unspecified parameters, and the values of these parameters were to be fixed in accordance with the number and properties of the particles found. But this meant that the experiments could not actually prove the theories false.

A different kind of irrefutability occurs when all the inscriptions in some ancient tongue have been made intelligible by some hypothesis, and all the information contained therein has been acted upon and found to hold good. At that stage, there are no more tests to apply. If the number of inscriptions was great, and the hypothesis made perfect sense of all of them, then the fact that no more tests are available surely does not greatly reduce our confidence in the hypothesis. Thus we need a criterion of admissibility for hypotheses which views each hypothesis *sub specie aeternitatis* (i.e. which involves the entire 'life' of the hypothesis, including all its tests, both past and potential), and does not reject it simply because its examination is drawing to a close.

We cannot simply say, on the lines of Popper's criticism of the theory of Psychoanalysis: "Let the proposer tell us what could happen that would refute his hypothesis." This way of putting it has the unwanted temporal element, quite apart from the fact that it would seem to leave too much scope for partisan writing of the rules (— even though Popper was not trying to be kind to the psychoanalysts). But certainly, for a hypothesis to be refutable, there should be something that could happen that would refute the hypothesis. More accurately, there should be something that could happen or *might have* happened, that would refute the hypothesis.

I was tempted to approach a criterion of refutability suitable for art interpretation is in terms of *quantity of information*. By information I mean simply assemblages or sets of propositions. Suppose I am studying a work of art. Before I make any assumptions, I will have some information in the form of facts. Now suppose I make some hypothesis. The

hypothesis contains some information. When I combine it with the art, more information may emerge. One expects this to happen if there is some real interaction between the hypothesis and the art.

To illustrate, suppose I have a very simple inscription, a plain circle all by itself on a rock. To keep things simple, suppose the rock's original context is unknown, so that there is no help available there. Let's make the hypothesis that the circle symbolises the sun. What do we deduce? We deduce nothing new. We get out exactly what we put in.

On the other hand, suppose the inscription has two circles, more–or–less identical, and we make the same hypothesis. We now get some extra information: The work refers to two suns. More follows. At any time, there is only one sun, so it can only refer to the sun at different times.

This led me to define a technical term to encapsulate this idea.

Definition. *I say that a hypothesis is coupled to the art if the information extracted by reading the art in the light of the hypothesis exceeds the union of the information contained in the hypothesis itself, and the facts available before the hypothesis was added*[10].

Note that in my usage here, facts are things that we accept as true, whereas information includes hypotheses that may be true or false.

Note also that information may contradict itself. This is a very important and useful feature.

I proposed to limit the class of admissible hypotheses to the coupled ones. Although superficially reasonable, this approach was a mistake. It turns out that as soon as there are any facts, and the hypothesis is not already a fact, then the hypothesis is coupled[11]! Thus essentially all hypotheses are coupled, and the concept is vacuous.

This forced me to try again. Eventually, I settled on the following definitions.

First, I suppose that we have agreed on the contents of a class of *conceivable basic statements* about the art. These are statements that might or might not be true, and might or might not be accepted as true. Each is a statement of some isolated observation

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[10] This definition is informal. For the benefit of logicians, it needs to be expressed more precisely. First, by a *deductively–closed class* I mean a class of propositions that contains all propositions deducible from its elements. Given a class C of propositions, I denote by $\mathrm{cons}(C)$ the deductively–closed class generated by C (i.e. the logical content of C). Given two deductively–closed classes, A and B, I define

$$A \wedge B = \{p \wedge q : p \in A, q \in B\}.$$

Let F be the deductively–closed class generated by the 'facts', and let H be the deductively–closed class generated by the hypothesis. Normally, F is very large (containing everything deducible from everything we accept). Typically, H is generated by a single composite proposition. The hypothesis is coupled to the art if $\mathrm{cons}(H \wedge F)$ differs from $H \wedge F$.

[11] I am very grateful to the late Professor Popper for pointing this out to me. It is not hard to prove. Popper also drew my attention to extensive work of Tarski [1935, 1935, translated to English in 1983, Chapter XII] on cons(), and made other useful suggestions.

about the art (including its appearance, its technique, its context, or the culture of its period). For instance, the accepted statement that there is surface pick–dressing on some kerbstone at Newgrange, and the generally–rejected statement that bronze swords were in use at the time the art was created[12].

Definition. *I say that a hypothesis is refutable if there is a conceivable basic statement about the art that contradicts the hypothesis.*

I propose that we confine our attention to refutable hypotheses. I argue that this is reasonable, because an irrefutable hypothesis simply imposes some meaning on the art, without effectively paying any attention to its content or context. If a hypothesis is irrefutable, then for instance, it could accommodate the scenario in which any other rock carvings in any position on the monuments would have done as well.

Restricting to refutable hypotheses is a modest restriction, but one might wonder: Could the real, correct, original interpretation be irrefutable? I don't see why not, although I may be missing something. If it does, then one might stumble upon it, but there is certainly no way of knowing that one has done so. I have no comfort to offer on this point.

The concept of a *refutable hypothesis* does for megalithic art interpretation what that of *falsifiable theory* does for experimental science. The main difference is that, in order to accommodate the finite corpus of conceivable raw data, the class of conceivable basic statements is adjusted. It is still an infinite class, but the individual basic statements correspond to conceivable single observations, or finite sets of observations, and are not thought of as the conceivable results of indefinitely–repeatable experiments.

Now how may a refutable hypothesis be refuted, in practice? In other words, what, specifically, are the basic statements? Effectively, the *accepted basic statements* (i.e. the raw observational facts) provide the *tests* of a hypothesis. What are these tests?

It is helpful to come at this by looking at the analogous but simpler question of deciphering encrypted alphabetical messages. The main difference is not that the messages are now deliberately, rather than accidentally encrypted. That hardly matters. The big difference is that with the art the range of possible primary meanings is enormously greater than with an encrypted alphabetical message.

[12] The class of conceivable basic statements consists formally of pure existence statements about the art and its context.

4. Cipher problems.

It is the ideal of cipher–makers to give the opposition no chance at all of decrypting their encrypted messages. The best thing would be to make all decryptions of the messages *a priori* equally–likely, but this is hard to do. For instance, if I use a straightforward letter–for–letter cipher, then:

A has 26 equally–likely decryptions, namely A,B,C,...,Z.

AB must encrypt two different letters, so the 26 decryptions AA,BB,CC,... are ruled out. Thus only 650 of the 676 conceivable two–letter 'words' are possible.

ABC has 15600 (e.g. DOG, CAT, MAN, KGB, CIA, TCD, XYZ) possible decryptions, but 1976 three–letter words (e.g. ADA, SST, AAA) are ruled out, because of repeated letters.

ABCA has also 15600 possible decryptions (e.g. DEAD, TODT) but 441376 four–letter words (e.g. ABBA, DUCK) are ruled out.

Thus, with only a four–letter message, the vast majority of equally–long decryptions are ruled out. If we add the information that the decryption is supposed to make sense in, say, English, then the list of possible decryptions is cut drastically. As the message grows, with the cipher held fixed, the number of possible decryptions dwindles. With a message as long as a sonnet, it is a moral certainty that there is only one decryption that makes any kind of sense.

To combat this problem, cipher–makers complexify their ciphers. For instance, instead of letting single letters stand for single letters, they might let each group of five letters stand for a different group of five letters, so that the letter 'a' might stand for different original letters, depending on where it occurred in the encrypted message. This kind of cipher is such that each decrytpion of a five–letter message is equally–likely, and the cipher–book is gigantic (— there are over ten million entries), but it is still pretty easy to break if used to transmit a page or so of text. For perfect results, the complexity of the cipher should match the length of the message. The trouble is that encryption then becomes an extremely tedious business, even when computers are used to carry it out. In practice, every cipher used for a long–enough message can be broken, and ciphers are issued for use either to conceal information for a limited period of time (24–hour ciphers, 3–day ciphers, etc., cf. [Wiener]) or to conceal a strictly limited amount of information. It is important to note the latter possibility. It is often said that there are no unbreakable ciphers, but it is not true. A cipher cannot be broken unless it is used, and *used to encrypt enough material*.

In the absence of any assurances from the encrypter, it is strictly speaking impossible to decrypt an encrypted alphabetical message. The encrypter may have used a cipher as complex as the message, and in that case all decryptions are equally likely.

Thus, no progress can be made in decryption, except on the assumption that the cipher has been over–used. This assuption is sometimes expressed thus: the cipher is simpler than the message. Assuming that, one may then proceed to test hypothetical ciphers, beginning with the simplest kinds.

The use of the speculative method is unavoidable. Nothing can be deciphered without using hypotheses. Indeed, you cannot understand this paper unless you begin with the

hypothesis that it is written in English. *Hanc sententiam intellegere non poteris, nisi eam latinae scriptam assumpseris.*

In deciphering, hypothesis after hypothesis is tried, until some sense (in the form of recognisable words, or some other kind of organisation) emerges from a message. The emergence of sense means intuitively that the interaction of hypothesis and message has produced additional information. If recognisable words are the criterion, then, strictly speaking, what is happening is that hypotheses are being rejected on the basis of the acceptance of facts of the form: so–and–so is not a word. If no such rejection is possible, then the hypothesis is useless for decryption. This would be an irrefutable hypothesis.

The process of testing does not stop there. Given a hypothesis that produces recognisable sense, what do cipher–breakers do next?

They use common–sense, of course. They decrypt as much material as the hypothesis allows, and they look at the result to see if it looks like a reasonable message from its source. If you think about it, there are several distinct checks involved, which we may label consistency, veracity, and context.

Under *consistency*, the hypothesis should work on the entire message, not just on a few scattered words. If there are several messages from one source, it should work on all of them, or at least hypotheses of the same broad class should work on all of them.

Under *veracity*, any statements of fact should be likely to be believed by the encryptr.

Under *context*, the content of the messages should fit whatever is known of the context.

It may happen that, even though there is a refutable hypothesis, still the message is indecipherable.

Suppose we intercept the encrypted message: 'Zpo gjfsz mbe.'

The hypothesis that the cipher is a simple letter–substitution cipher yields a number of acceptable extensions, giving possible alternative decryptions:

Nut brown ale.
The first day.
Sid loves ham.

Each version is based on a refutable hypothesis. For instance, the first version is the hypothesis that the cipher–table includes the correspondences: z←n, p←u, o←t, g←b, j←r, f←o, s←w, m←a, b←l, e←e. This would be refuted if the message 'Ozp jfsgz ebm' were to be observed, because this message would decrypt to 'Tnu rowbn ela', which is not a fragment of English.

Each hypothesis plus the encrypted message produces some sense, so it now depends on whether this sense may be subjected to any test. If anything is known about the source, then it probably can. But it may well happen that the available data permit us to eliminate some decryptions, but leave two or more in contention. We are then stuck. There is no way to choose between the two meanings. The hypotheses are equally simple, and the information extracted is comparable and irrefutable.

Indecipherability is not the result of a lack of decryptions; it results from an excess of them.

There is more to deciphering than simply searching for hypotheses that produce intelligible information. There may be several such hypotheses, that have little in common

with one another. What determines whether or not a message can be deciphered with some confidence is not whether an intelligible decryption can be found, but whether the there is an intelligible decryption that far outshines the others in terms of the excess of information out to information in.

5. Ways of refuting conjectures about megalithic art.

Given a refutable hypothesis, there are three broad ways that it might actually be refuted:

Test 1 (consistency). *In so far as one work of art can be firmly linked to another, the hypotheses about both and the information read from both should be consistent.*

That is, different parts of the same work, and different but related works should not contradict one another. If we assume that spiral = eye in one part of a work, then we should not assume that spiral = worm in another part. This test may pose considerable technical problems, in practice. It cannot be assumed without question that there is no variation in the representational conventions in megalithic art. There may have been differences from region to region, and from tomb to tomb within regions. There may have been different conventions for the art on tomb passages than for the art on the surrounding kerbs. It cannot even be assumed that the art found on a single stone is coherent. There is considerable evidence to the contrary [O'Sullivan 1988]. Before the test can be applied, one must have the result of detailed technical examination of techniques and styles, to determine as accurately as possible which assemblages of art may be expected to cohere.

Test 2 (veracity). *In so far as any information input or extracted implies statements about the natural world, we should consider that these statements could have been believed by the artist and his people.*

In other words, the art should not tell deliberate lies about agreed matters of fact (see below for qualifications).

Test 3 (context). *In so far as any information input or extracted implies statements about the culture, these should be reconcilable with the archaeological record.*

By the archaeological record I mean the results of excavations on the sites of the art, the results from all contemporary and earlier sites with credible cultural links to the artists, and I include all data such as pollen analyses, paleoclimatic data from tree–cores, the ocean–floor and the antarctic icecap, contemporary motions of heavenly bodies, you name it. I wouldn't rule out things like astronomical alignments (e.g. [Lockyer, Patrick, Thom 1967, Thom 1971, Thom and Thom 1978, Hawkins and White]), although I believe they must be treated with great care, and most of those claimed have no convincing significance [Heggie 1981a, Heggie 1981b, Schaeffer 1985, Schaeffer 1986, Schaeffer 1987][13]

To illustrate, let's consider a few simple hypotheses.

[13] The problem of assessing the significance of astronomical sight–lines in megalithic monuments is another problem of the interpretation of limited data, and to that extent the general methodology appropriate to it is similar to that appropriate to the problem under discussion. The critical apparatus laid out in [Heggie 1981a] fits into that schema, except that it proceeds on the basis that, if monuments were not deliberately aligned on

Hypothesis 1. *The empty hypothesis : Megalithic art means nothing.*

This should be distinguished from the hypothesis that megalithic art consists of pure abstract decoration. Abstract decoration has a kind of meaning. The present hypothesis is that the work has no significance at all.

This hypothesis evidently does not interact with the symbolism of the art. It is not refutable by any actual or conceivable statements about the positions, motifs, etc. of the art.

The hypothesis, combined with the archaeological record, implies that someone went to a great deal of trouble for nothing. This is a statement one might well reject, so the hypothesis is refutable on that account.

What about the three tests?

Consistency: There is no problem reconciling the meaning of different works. There is nothing to reconcile.

Veracity: The hypothesis passes the test vacuously, as the mathematicians say. There is no scientific information to check.

Context: The archaeological record shows that some at least of the work was carefully executed, meant to be seen, and is associated with gigantic buildings which took a lot of work to erect. Taken with the assumption that human nature has not changed all that much in 5000 years, this makes the hypothesis very doubtful.

That's it. The result is that it might be so, but it's very doubtful.

Hypothesis 2. *The abstract decoration hypothesis.*

This was championed by Coffey in 1892, although later he had second thoughts. The hypothesis is that the various motifs and combinations of them are simply intended to be *decorative*. There is a kind of meaning implicit in this, namely : 'I am beautiful work', or perhaps 'I am beautiful work, as befits an important building'.

The first kind of information that emerges is a list of the motifs and patterns regarded as suitable for decoration. The second is that the people were interested in decoration. The third is that the artists were not very good at decoration. To quote Coffey [1912]:

"the artist was as yet unable to grapple with the complexity of spirals for running and overall treatment" (p.25),

(the cup–marks on Kerbstone 52 at Newgrange) "interfere with the ornament of the stone, which, especially in the case of the lattice–pattern, is cramped and broken ..." (p.41)

"...these primitive carvers had not reached the conception of ordered design, of relation of parts, or unity in treatment." (p.42)

Many people have shared Coffey's perception that the cup–marks on K52 do nothing for its purely ornamental quality[14]. One could add that only one full panel (the roof–box

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astronomical grounds, then their alignment relative to astronomical markers was random. Statistically–significant astronomical alignment might result from deliberate alignment on some other basis. Thus, it is necessary to consider non–astronomical hypotheses, before concluding that a particular hypothesis of astronomical alignment is to be preferred.

[14] The cup–marks do appear to be deliberate. Mrs. C. O'Kelly informed me (private communication) that such solution depressions do occur naturally in glacial erratics, and

lintel) admits a rigid Euclidean symmetry. The remainder sometimes have sub–units with symmetries, but these are not combined to produce any overall balance.

Clearly, the hypothesis is refutable.

Consistency: There is again no problem about reconciling the meaning of different works. A slight difficulty is the marked variation in the number of occurrences of the various motifs from tomb to tomb. This could be reconciled by assuming that fashions changed, or that different artists had different preferences, or in other ways.

Veracity: There is no scientific information to check.

Context: Coffey demonstrates that the motifs found in the art resemble motifs used in (subsequent) Bronze–age, Iron–age, and later decoration of a plausibly abstract nature. So far, so good. That the artists were unskilled in the concepts of formal decoration is unlikely, however, in view of the very fine smaller decorated stone objects that have come to light. The superb 'mace–head' found by Eogan at Knowth is almost sufficient on its own to rule out the hypothesis. The various stone 'phallus' objects found in stone–settings, and the large stone bowls also show strong symmetry. Given that the artist of K1 and K52 at Newgrange made largely asymmetrical and formally–unbalanced designs, the odds are that the reason was something other than incompetence.

It might be possible to rescue the hypothesis by rejecting the critical standard used by Coffey. Much megalithic art shows great sensitivity in the use of stone, especially in the way it exploits the pre–existing coutours of the material. When modern eyes see only a lack of balance and symmetry in an inscribed pattern, that may just be the result of our insensitivity to the whole stone, and our remoteness from an age in which man's contact with nature was on a more intimate level than today, and in which the less regular contours characteristic of living things had more appeal than formally–symmetric patterns. However, if the hypothesis is treated in this way, then it may easily become irrefutable.

The above discussion is highly abbreviated, and the abstract decoration hypothesis deserves much more extensive treatment, but it is probably false.

Hypothesis 3. *The extreme diversity hypothesis.*

This arises in a number of guises, but the essential idea is that every little variation in the shape and perhaps the position and orientation of each motif represents a separate meaning. Not only is a right–hand spiral different from a left–hand spiral, and a single different from a double, but a three– spiral done one way is different (in meaning) from a three–spiral done another way, a lozenge high up is different from a lozenge low down, etc. At its extreme, every single element in megalithic art has a unique meaning, so that the few dozen spirals have a few dozen meanings, etc. For simplicity, let's take the extreme version. It has two sub–versions.

First is one in which the meanings of the symbols are not hypothesised. This is almost functionally equivalent to the empty interpretation.

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both she and M. O'Kelly believed that some at least of the K52 cup–marks were naturally there, but were deepened and "improved upon" by the artist. She added: "All may have been natural originally, but probably not; he may have added others for reasons of symmetry."

In the second sub–version, meanings are attributed to one or more motifs. The nature of the governing hypothesis is such that the information that comes out is practically identical with that put in, so again the hypothesis is practically irrefutable. More precisely, it is refutable, but only very weak tests are possible.

Hypothesis 4. *The extreme identity hypothesis.*

As a final example, consider again the hypothesis mentioned above that all the symbols represent the same thing, say the sun. This is a universally–quantified proposition, so the number of simple propositions it contains equals the number of symbols, or at least the number of (visually) clearly–distinct types of motifs. At the very least, we can distinguish spiral, lozenge, line, triangle, circle, and cup–mark, so there are a conservative minimum of six simple propositions in the hypothesis. The information that seems to come out is that the artist was interested in the sun. How could one deny this? The hypothesis is not really susceptible to test.

It is worth remarking that some versions of the extreme identity hypothesis in the literature mask their essential vacuity in a welter of fascinating facts. It may be necessary to unravel a very complex web of connections to discover that the author is actually setting everything equal to everything else.

These simple examples show the way that the tests function. Some hypotheses are irrefutable because they fail to interact with the art, and the tests are the means by which the remaining hypotheses might be refuted (with more or less confidence, as usual). Furthermore, it may be expected that in more complex situations, the tests will need to be recycled. After a first pass through the tests, it will usually be found that the hypothesis must be sharpened or extended to 'save the appearances'. Usually, there will be a number of possible, mutually–exclusive sharpenings. Each of the revised hypotheses may then be resubmitted to the tests, yielding fresh information and increasing the chance of a refutation. The process may be usefully continued until (if ever) the point is reached at which a full pass through the tests produces no change.

The tests of veracity and context are open to the objection that the artist could well have told a deliberate lie. We all know that great art is not supposed to lie, but perhaps this art is not so great after all? No–one doubts that the many ancient Egyptian inscriptions larded with Ozimandian extravagance were indeed intended to tell the very obvious lies they seem to tell. A cynic might even say that truth needs no inscription to support it, and most inscriptions lie. Certainly, if there is any resemblance between the function of Newgrange and that of Westminster Abbey, then its inscriptions need to be treated with caution. It was partly with this in mind that I inserted the qualifying 'in so far as...' clauses in the statements of the tests. If a hypothesis is refutable and spectacularly successful, and appears to yield falsehoods about the world or the culture, then we are not going to let that stand in its way. But if a hypothesis is otherwise mediocre, and also yields lies, then it is headed for the waste–basket.

Procedure. *In summary, a refutable hypothesis may be tested by the repeated passage of the hypothesis through the tests for consistency, veracity and context. This process may be expected to lead to progressive refinement of the hypothesis.*

One or another test may, as we have seen, have little interaction with the hypothesis. The test for consistency actually tests when two different carvings are interpreted to refer to the same object or phenomenon. They thus give two independent approaches to statements about it. The test for veracity actually tests when a carving is interpreted as asserting some proposition, say P, to be a scientific or historical fact (or, for that matter a philosophical or theological fact), and we have an agreed view on whether the artist and his peers would have believed P. Test 3 actually tests when a carving is interpreted as implying some proposition P about the culture, and independent archaeological evidence provides a proof or disproof of P.

It is natural to be more impressed by hypotheses that really interact a lot with the tests — hypotheses that are really tested by them. One might think of insisting that the hypothesis should interact with all three kinds of test. But it could, for instance, happen that a hypothesis is only really tested by consistency. Such a hypothesis would have extracted no information that needed to be reconciled with either the artist's natural science or our knowledge of the culture. This might seem an unlikely kind of hypothesis, but, for example, one could imagine an extreme situation in which a hypothesis that the symbols were syllabic produced the information that a string of consecutive slabs carried a coherent threnody in proto– Iberian on the unexpected death of a character represented by a dot–in–circle:

> They told me, dot–in–circle, they told me you were dead.
> They brought me bitter words to hear, and bitter tears to shed.
> etc.

— and that there was simply nothing scientific or archaeological to get your teeth (or your trowel) into. Wouldn't that deserve consideration? Indeed, I imagine that intelligible inscriptions would take precedence over the findings of excavation, in the sense that even if they were apparently contradicted by such findings, the pressure would be to reconcile the two, rather than reject the hypothesis that produced the interpretation.

To take a related example from another field, suppose we intercept the following message from the Byzantine Secret Service:

> Pvs ebuft bsf csjfg, boe uifsfgpsf xf benjsf
> Xibu uipv eptu gpjtu vqpo vt uibu jt pme,
> Boe sbuifs nblf uifn cpso up pvs eftjsf,
> Uibo uijol uibu xf cfgpsf ibwf ifbse uifn upme.

The hypothesis that each letter represents the letter just before it in the alphabet converts the message to:

> Our dates are brief, and therefore we admire
> What thou dost foist upon us that is old,
> And rather make them born to our desire,
> Than think that we before have heard them told.

Now this is such a simple hypothesis, by comparison with the rich lode it reveals, that it is a moral certainty that we have found the right decryption, even though there is practically no conceivable reason why the Byzantine Secret Service would want to send

such a message (unless perhaps they just wanted to send our cipher experts off in the wrong direction).

It may be that there are tests of some kind other than consistency, veracity and context which have not occurred to me. I would welcome suggestions, but observe that I do not think it valid to test a hypothesis by saying: 'prove it!' There has been a tendency to criticise interpretations by saying: 'I don't accept your assumptions.' Such a remark is pointless. If hypotheses could be proven, they need not be made. They should be criticised on the basis of their efficacy. Also, *there is no reason why the criticism should not come from the proposer*. I believe we should have a regime in which people feel free to suggest hypotheses without committing themselves to them. Until now, the people who decided to embark on interpretation have felt obliged to be *right* all the time. This tended to lead them to bottle themselves in a corner. There is no reason why someone should not propose a dozen different hypotheses. It seems to me a perfectly valid and useful contribution to write a paper setting out, analysing, and finally refuting some hypothesis. It saves the next person working through the same argument. It is equally useful to set out two different hypotheses, analyse them, and find neither is refuted. The particular use of such a finding will become clear below.

5. Comparison of competing hypotheses.

If a refutable and unrefuted hypothesis is known for the meaning of a corpus of megalithic art, then how much confidence does it deserve? What if several are known? Why should one be preferred to another?

Let's begin with the first question. Common sense says that it depends simply on how hard the tests were on the hypothesis. But this comes down to the quantity of information extracted by the hypothesis. More precisely, it comes down to the quantity of information *forbidden* by the hypothesis. The greater the number of basic statements that contradict the hypothesis, the more there is to test. If the hypothesis is refuted, then the information extracted explodes to embrace all propositions, no basic statement is forbidden, and our confidence is nil. If the hypothesis is not refuted and testing is over, then our confidence is greater if we have garnered much consistent information, and excluded many hypothetical basic statements.

The situation is quite analogous in physical science. Falsifiable conjectures may be divided into those that have been already proven false, and those that still stand. For instance, Newton's Second Law has survived the advent of Relativity, whereas his chronology of ancient kingdoms is ruled out. But what is the basis for our relative confidence in one unrefuted conjecture in comparison with another? Why do we feel happier about the kinetic theory of gases than the Big Bang? Two reasons are obvious. We prefer a theory that has been thoroughly tested, and we prefer a theory that has no plausible alternative (other than its simple negation). These reasons are closely connected, because they both depend on the richness of the data available to test the theory. The data obtainable by experiment are the testable information in this situation.

In a closer parallel, consider Babylonian numerals as represented on cuneiform tablets. Neuegebauer [1969] gives a fascinating account of the deciphering, which began with the simple hypothesis that a certain mark denoted 1, two of them denoted 2, etc., and pro-

ceeded, naturally, by testing and refining this hypothesis, hypothesising symbols for 5 and 10, the use of place–order, missing places, and sexagesimal notation. There are vast numbers of these tablets, and the reigning hypothetical system has worked on all tested items. People thus have great confidence in it, and their confidence will not be shaken if we ever get to the point where the very last tablet has been studied. Indeed, the stage was soon reached at which an apparent contradiction was seen, not as contradicting the hypothesis, but as indicating that the Babylonians possessed some unexpected mathematical knowledge.

Similarly, the deciphering of Egyptian hieroglyphs began with Young's hypothesis that the three ovals on the hieroglyphic section of the Rosetta stone encircled the name of King Ptolemy V Epiphanes, and it progressed (very slowly, and with much revision) through the assumptions that the script was syllabic, and the language related to Coptic. The end result has been successful on so many inscriptions that we can hardly conceive that it is not correct.

By contrast, consider the Phaistos disc [Varga and Botos]. The inscription, running in spirals on two sides of a bronze disc, contains a total of 241 symbols or characters, very likely to be alphabetical or syllabic writing. No other writing in this script is known, and the language is unknown, so not many people would place much confidence in any deciphering, even if it were refutable and unrefuted.

Now consider the situation where there are two or more available hypotheses, each refutable, tested, and unrefuted.

I suggest that there are two grounds for comparison of hypotheses: their simplicity, or complexity, and their success, or explanatory power.

Simplicity is not a simple concept, nor is success.

To evaluate them (or, more accurately, to define them) we need to specify some way of measuring them.

A numerical (real, cardinal, or ordinal) measure would be ideal, but it is not easy to see where an acceptable numerical measure is to come from.

Perhaps the most obvious measure of success is the concept of *predictive power*, defined thus:

Definition. *The predictive power of a hypothesis, in relation to a corpus of art, is the excess information obtained by combining the hypothesis with the art, as compared with the information in the hypothesis and the information available without using the hypothesis*[15].

Predictive power is not a numerical measure. Power is information. More accurately, the predictive power of a hypothesis is a certain body of information.

This measure is not very satisfactory. It may be shown that even irrefutable hypotheses can have non–empty predictive power, and worse, each refuted hypothesis is all–powerful, in spite of being useless.

A more realistic measure of the value of a hypothesis is its *restrictive power*, defined as follows:

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[15] In terms of the symbolism introduced in footnote 10, the predictive power of the hypothesis H is the set $\mathrm{cons}(H \wedge F) \sim (H \wedge F)$.

Definition. *The restrictive power of a hypothesis, in relation to a corpus of art, and a field of (hypothetical) basic statements about the art and its context, is the set of basic statements that are incompatible with the hypothesis and the facts.*[16].

This measure has the advantage that a hypothesis has empty restrictive power if it is irrefutable or if it is refuted.

Restrictive power is information about what is prohibited.

The analogy with physical science illustrates the reasonable character of this measure. For instance, a really successful dynamical theory, combined with sufficient data, will completely specify the motion of a system of n particles, and will rule out all contrary behaviour of the system. The set of excluded states will thus be optimally large.

There are a number of ways in which one might go about quantifying the restrictive power associated to a hypothesis and a body of facts. Popper [1980] has indicated how a *dimension* concept can be associated to theories. This concept relates essentially to the number of independently–adjustable parameters that the theory allows. Information theorists have developed the concept of *entropy*, and this could be used as well. A Bayesian approach is also possible. I would not wish to discourage the investigation of such measures, but I am not inclined to support the adoption of any at this stage.

I have three reasons for this. For a start, the class of assemblages of information is not totally–ordered under inclusion. Thus any map of hypotheses to numbers will create relative orders which are spurious, or at least controversial. Also, any natural mathematically– or logically–constructed measure of power will assign various weights to propositions. But whereas logically a proposition is a proposition, in reality simple propositions carry varying weight. For instance, many people would agree that the proposition 'God loves me' carries rather more information than 'Eggs are a pound a dozen today', but a significant number of people would take exactly the opposite position, and this brings me to my third and crucial reason: I am aiming for consensus on a *modus operandi*, and the adoption of a numerical measure is probably inconsistent with this. I think there is no alternative to common sense, in judging the either the complexity of a hypothesis or the substance of a given body of information. Even if I could satisfy myself with some numerical measures of these things (and I can't), I don't suppose you (or that hard–headed fellow over there) would accept it. It will be clear in some cases that one hypothesis is substantially simpler than another, or substantially more powerful. In other cases, it will not be clear, and since my objective is consensus, I would just say: A and B are roughly comparably in complexity, or in restrictive power, as the case may be.

So I suggest that we compare hypotheses by looking at two things: the restrictive complexity of the hypotheses, and their power.

If we agree that hypothesis A is as simple as hypothesis B, but has substantially greater power, then we prefer A. If we agree that hypothesis C is much simpler than hypothesis D, and has roughly comparable power, then we prefer C to D.

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[16] In terms of the concepts and notation introduced in footnote 10, the restrictive power of the hypothesis H is the set of those basic statements $b \in B$ such that the negation of b is deducible from the hypothesis and the facts, and b itself is not so decucible, i.e. $b \not\in \text{cons}(H \wedge F)$ and $\text{not}(b) \in \text{cons}(H \wedge F)$.

If we have to choose between a simple, reasonably powerful hypothesis and a very complex, very powerful hypothesis, then it is hard to say what should be done. My instinct, like Occam's, is to prefer the simpler hypothesis, but it is a question of degree, and I would want to examine the details and weigh one thing against another. There is less chance of achieving consensus on such a choice.

Popper [1972, p. 232] offers a "rather unsystematic list" of six cases in which one would be inclined to say that one scientific theory seems to correspond better to the facts than another. Translated to the present context, these elaborate on ways in which a new hypothesis, H_2, could outshine another, H_1, in restrictive or predictive power:

(1) H_2 makes more precise assertions than H_1, and these more precise assertions stand up to more precise tests.

(2) H_2 takes account of, and explains, more about the art and its context than H_1.

(3) H_2 explains the art in more detail than H_1.

(4) H_2 has passed tests which H_1 has failed.

(5) H_2 has suggested new tests, not considered before H_2 was designed (and not suggested by, or possibly not even applicable to H_1), and H_2 has passed these tests.

(6) H_1 has unified or connected hitherto unrelated bodies of art.

In applying such criteria, the relative complexity of the two hypotheses remains important.

You may feel let down at this point, because I am not offering any absolutely objective way to assess hypotheses. I agree that it would be wonderful to be able to settle these questions using a mechanism involving 'no human hands', but I don't think it possible. To be precise, I believe it is impossible to remove the element of subjective human judgement from this question, and retain the assent of a broad spectrum of people. People have to assent for themselves to statements like : 'A is simpler than B', or 'C is much more impressive than D in the information it yields'. If a substantial body of people don't assent, then we have to accept that these statements are not obvious.

Now, consider the situation when a list of hypotheses have been proposed and examined. There are three possible scenarios.

The first is that all are irrefutable or refuted. In that case, I would say that we currently know nothing of the meaning of the art.

The second is that there is a refutable and unrefuted hypothesis that clearly out–performs the others, on a balanced assessment of complexity versus power. In that case, I would deem the information yielded by this hypothesis the most likely to be correct, *at present*.

The third is that there are (at least) two mutually inconsistent hypotheses, both irrefutable and unrefuted, performing roughly as well as one another, and at least as well as any other known. In that case, I would deem the art *currently uninterpretable*.

If I was ever satisfied that all untested hypotheses were necessarily more complicated and less powerful than those already tested, then under the same conditions I would deem the art absolutely uninterpretable, probably interpreted, or absolutely uninterpretable, respectively.

Observe that uninterpretability results from an absence or an excess of reasonable interpretations.

Uninterpretability might be proven, but an interpretation can only be probable.

For instance, people have given quite distinct readings of the Phaistos disc. If, in the opinion of the experts, the best of these efforts are of roughly equal merit, then the disc is currently uninterpretable. If all reasonable schemas have been explored, then it is absolutely uninterpretable.

There certainly are uninterpretable inscriptions. All sufficiently simple, isolated inscriptions are such. If I find the word 'Bog' written on a scrap of timber in the rubble of a camp for prisoners of the Third Reich, and the rest of the context is lost, then I can offer two hypotheses:

A: It's in English, and labelled a latrine, or swamp.

B: It's slavic, means God, and formed part of a religious message.

These are not the only possibilities, but they are as good as anything else. I conclude that the inscription is uninterpretable.

Similarly, in ancient inscriptions in which the script and the main points of the language are understood, it regularly happens that there are words that are used too infrequently to allow their meaning to be pinned down. For instance, the word xyz might occur in a sum: I buy 6 xyz's and 8 xyz's, how many does that make? If that is the only known use of xyz, we can say that xyz is grammatically a noun, the name of a saleable good, and that's all. Since we can offer more than one suggestion for the meaning, xyz is uninterpretable.

Is megalithic art uninterpretable?

Whether a corpus of art is interpretable or not depends on its complexity. In the case of megalithic art, it difficult to forecast the eventual verdict. Even as a whole, the 900 odd known stones represent a fairly limited corpus, by comparison with most deposits of material in ancient languages. Then there is the problem of coherence: the art may divide into several distinct coherent bodies. The smaller the bodies of coherent art, the greater the odds that they will prove uninterpretable though excess of interpretations.

Consider, for instance, the case mentioned earlier of two simple circles on a rock whose context is lost. We could offer two hypotheses: (1) circle = sun and (2) circle = eye. Both would be refutable and unrefuted. In the first case, the inscription might mean 'two days', and in the second it might be monitory. Both are equally–simple, and equally powerful. We could find many other refutable hypotheses, but it seems reasonable to suppose that none will be any simpler or effective. In that case, the inscription is uninterpretable.

The complexity of megalithic art is increased if we include the non–incised art, in the form of architecture, sculpture, and other features of the design of passage–graves, and this increases the chance of interpretability. It may be possible to integrate such features into hypotheses about the incised art. Examples of striking features that may well relate to the art are the characteristics of kerb–contours, passages and chambers, the two large water–rolled stones that lie in front of the eastern passage at Knowth, the so–called 'stone settings' that are commonly found outside passages, and so on. Hypotheses about the function or the symbolism of such features may be useful.

6. Procedures.

On the face of it, the analysis of the meaning of megalithic art requires the systematic consideration of a large number of hypotheses, the vast majority of which will be disproved in the course of the investigation. The mathematical structure best adapted to this kind of activity is the 'tree'. A tree, as you might expect, is something with branches. It is a connected network of lines and nodes, containing no loops. A real tree is an example as long as you idealise the trunk, branches and roots to lines with no breadth, and as long as pairs of branches do not coalesce. Another example that is becoming increasingly familiar is the type of subdirectory structure used for disc files[17]. Another is the structure of the telephone network at any instant in time. The kind of tree we are concerned with here is a logical tree. When you are exploring a hypothesis, you will be obliged to introduce mutually–incompatible subsidiary hypotheses, and explore those. Each time this happens, the tree will fork. You will proceed out along each branch, 'sub–branch' and 'twig' until you arrive at a refutation or the trail goes cold. Obviously, it would help to keep a tree–structured diagram of the investigation. Equally obviously, the papers you write will be easier to follow and check if they too are tree–structured.

As a little example, each identifiable work of art should be given some preliminary consideration on its own, to test the hypothesis that it means nothing, then to test the hypothesis that it had meaning but this has been alteredi or reduced by subsequent work or vandalism, then to test the hypothesis that it bears several unrelated messages, then to test the hypothesis that it bears a single coherent message. The irrefutable and the refuted hypotheses should be set aside, and the remainder carried forward to the consideration of the work nearby, etc.

It does not require genius to see that the comprehensive investigation of every possible hypothesis is not possible in practice. Some preliminary selection is a necessity. The situation is no different in physical sciences, and it is reasonable to follow the approach taken there, which is to develop the investigation largely within the boundaries of previous tradition, with an occasional foray into new territory when things start to look desperate. A great many suggestions about megalithic art are already on the table[18], and many could bear unprejudiced investigation.

Will it be possible to find a reasonably successful hypothesis? The only way to find out is to try. It depends on two things. First, there must be one, and second it must be possible to identify one. The existence of a powerful hypothesis depends roughly on the complexity of the art. If a more–or–less fixed body of symbolism was used in sufficiently many ways, then the correct hypothesis will pass all the tests. It is impossible to be sure, that we have enough coherent art, but that question will be settled when one powerful and unrefuted hypothesis is found, whether it is right or wrong. Whether we can find a powerful hypothesis depends to some extent on human ingenuity, but unless the the symbolism involves an idea that has totally and irretrievably vanished from the face of the earth and the mind of man, then it is only a matter of time.

Once one powerful refutable and unrefuted hypothesis is found, the key question then becomes: Is there a second one? Again, the determinant of this is going to be the

[17] excluding links

[18] and in the bin!

complexity of the art. If the art is sufficiently complex, it is unlikely to yield to an incorrect powerful hypothesis.

In order to eliminate hypotheses as rapidly as possible, it is preferable to begin the investigation on the most complex body of art that is plausibly coherent. This points to the Boyne valley. At present, the archaeologists are not in a position to assure us of the coherence of the work at any two tombs in the valley[19] The single largest body of art is on the great tomb at Knowth, and the second is at Newgrange. (The single most complex stone is perhaps the Newgrange K52, although this is approached by several stones in the Knowth kerb.) It appears that the art on the Knowth kerb was applied in at least two phases, but it may be possible to identify one coherent style which predominates, and which occurs on a great many stones of the kerb and passages[O'Sullivan 1988]. It seems clear that this is the place to start testing conjectures, but it may simply be premature at this stage, before the job of sorting out the superimposed styles has been carried as far as available methods allow.

Where are the hypotheses to come from, apart from tradition? Some may be found by the method mentioned earlier, of examining more ancient representational conventions. But it would be foolish to stop there, without examining more recent conventions, even from remote places. The ideas may have died here, and sprung up elsewhere. It is helpful to look at the schemes of motif–classification that have been proposed, and also to bear in mind that motifs aren't everything. Hypotheses may also be found by looking at things in the world anew, and at the art, and asking yourself: could one imagine representing that thing *thus*? There is no need to restrict to concrete things: we could also consider the possibility that abstractions are represented. It is not a good idea to rely on one person's ingenuity, and I think it useful to comb the many books about the art, however fanciful, and to ask the opinions of anyone who knows the art, however inexpert. Also, I have found it useful to prepare sheets of signs or 'motifs', and ask hundreds of people to label them as they please. In this way, I have gathered many reasonable suggestions that would not otherwise have occured to me. There is much more that could be done in this way.

That concludes what I have to say. I hope that these suggestions will be thought helpful. I believe there is little new about the analogous principles in relation to other areas of inquiry. I have simply transferred them to the context of megalithic art interpretation, and written them down explicitly. I have tried to be fair in the formulations, so as not to load the dice in favour of any particular interpretation. I hope the approach described here will take the interpretation of this art out of the realm of imaginative fiction, and allow the discussion to take place in a rational and scientific atmosphere. As is known, I have reported the discovery of one hypothesis (containing starry, fluid and fishy(!) elements), which may have some restrictive power in relation to the Newgrange kerbs. But (as is, perhaps, by now, clear to you) I am not committed to the truth of that hypothesis, and am perfectly willing to reject it if it is refuted, to set it aside if a simpler or more powerful hypothesis comes along, or to declare the art currently uninterpretable if a comparable hypothesis appears. I don't want to impose my prejudices on the art. What interests me

[19] However, O'Sullivan has pointed to stylistic grounds for associating the art of the most spectacular Newgrange kerbs with what he calls the 'plastic style' of much Knowth art.

about megalithic art is the possibility it may offer to form some specific idea of what our remote ancestors had in mind, especially in relation to important matters. It may have meant nothing much, but, given its location and the work involved, the odds are that it relates to fundamental human concerns. It may not be possible to find out much about that meaning, but it is worth a try, even though it will probably require a concerted effort by a lot of people.

Summary.

Until now, efforts to interpret megalithic art have gained little credence, because there is no agreed method of evaluating such efforts. There is no agreed theory of interpretive theories. This paper addresses that theoretical vacuum.

I propose that we adopt the following working principles:

A way to approach the meaning (if any) of megalithic art is to make and test conjectures that are refutable. These conjectures are characterised by the property that they rule out certain 'basic statements' or conceivable raw observations.

All methods are fair when it comes to assembling conjectures to consider, but it is reasonable to pay some attention to the traditional suggestions hitherto offered by various antiquarians.

Conjectures may be tested by examining them in the light of the 'agreed facts', under the headings of consistency, veracity, and context.

When this is done, the refutable conjectures will divide into the the refuted and unrefuted.

Only unrefuted refutable conjectures deserve any confidence at all.

Conjectures may be compared by comparing their complexity and their restrictive power. Quantitative measures of these are conceivable, but it is suggested that only common–sense comparisons of complexity and power are likely to produce consensus.

No meaning can be determined, and the art is currently uninterpretable if there are two (or more) wholly–incompatible refutable unrefuted conjectures, having comparable complexity and restrictive power, and no other conjecture has greater restrictive power.

When only one refutable but unrefuted conjecture is known, then it deserves more confidence if it is agreed to be simple and powerful.

When several refutable but unrefuted conjectures are known, then one deserves the greatest confidence if it is considerably simpler than the others, or has the considerably greater power.

I hope that on the basis of these principles, or something like them, it will prove possible to reach agreement about whether certain works are interpretable, and what weight may be given to suggested interpretations.

These principles, suitably translated, apply to many problems on the fringes of normal science, in areas in which the total number of data or experiments is limited.

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Naturally, the views expressed in the paper are my own, and it is not implied that any of these people agree with any of them.

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