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Quotes5

1. "There are no illustrations in The Nature and Meaning of Numbers, but a couple of illus- trations of chains here would help to explain the concept." (Joyce, Notes on Richard Dedekind's Was sind und was sollen die Zahlen?)
2. Note: Chains are one level further (e.g related to Peano's axioms) than I am ready to go right now, Dedekind used them to proof induction per example.
   "The most original contributions made by Dedekind in Zahlen are all related to the notion of mapping. This distinguishes him from Cantor, who certainly employed mappings, but never considered them explicitly nor even employed a common word for the different kinds of mappings. Dedekind was the first mathematician who focused explicitly on the general notion, whose algebraic origins (Par. III.2) and connection to natural numbers (Par. 1) we have already considered. The new ideas that Dedekind gradually forged and elaborated in his draft of 1872/78 are all related to the notions of mappings of chains (which depends on the former). Actually, the theory of chains was his most important and original contribution to abstract set theory." (Domainguez, Labyrinth of Thought: A History of Set Theory and Its Role in Modern Mathematics)
3. "In the 1920s, Hilbert was in the habit of referring to the work of Dedekind, Cantor and Frege as the origins of modern mathematics and foundational research. In a lecture course of 1920, he said that Minkowski and him had been the first in the younger generation of German mathematicians to take 'Cantor's side', the side of abstract set theory [Ewald 1996, vol. 2, 964]. As we have seen, there are reasons to think that by 1900 or even 1910 Hilbert identified the set-theoretical approach also with Dedekind. But it is not by chance that he later preferred to mention Cantor, since Dedekind's approach, in its concentration upon systematics and foundations, must have seemed somewhat one-sided to Hilbert. Characteristically, he decided to associate the name of set theory with the man who posed completely new questions in this area, opening up a pure, abstract paradise. This is, after all, the spirit of 'Mathematische Probleme' and Hilbert's decision has deeply influenced later perceptions of the emergence of the set-theoretical approach." (Domainguez, Labyrinth of Thought: A History of Set Theory and Its Role in Modern Mathematics) <Origins>
4. "Being an illogician is inherently futile. After your death, all your great mischief will slowly become undone. Let me explain. The person who first discovered the irrationality of √2, that person was one of the world's first illogicians. Sadly all the mischief has been stripped from his legacy: the result is now regarded with utmost gravity, and not considered to be mischievous at all.

   A number of open questions in mathematics are irrationality questions about certain specific numbers. Is e+pi rational or irrational? It's known that e and pi are both irrational by themselves, but nobody knows whether e+pi is irrational. It's generally thought to be irrational but nobody can prove it. Maybe part of the reason why these questions are open is because mathematicians take them too seriously. Existing irrationality proofs, from √2 to pi, are inherently mischievous and naughty. The only reason they don't seem that way is because age has granted them a solemn seriousness.

   In time, Gödel's Incompleteness Theorems and Hilbert's Tenth Problem will be routinely taught in high school. And it breaks my heart to say it, but I suppose it will be drained of all the mischief and all the playfulness, and will be drilled into those future high school students with crushing gravity and seriousness. And that is the lament of the illogician, for all his illogic will eventually become logic, augmenting the logic of old.

   And as much as he wants to destroy mathematics and kick over the mathematician's sand castles, in the end he will only make them stronger and more beautiful." (http://www.xamuel.com/illogician/)
5. "Is Mathematics Quasi-Empirical?
   [First a caveat. I'm about to discuss what I think is the significance of incompleteness. But it's possible to argue that the incompleteness theorems completely miss the point because mathematics isn't about the consequences of rules, it's about creativity and imagination. Consider the imaginary number i, the square root of minus one. This number was impossible, it broke the rule that x2 must be positive, but mathematics eventually benefited and made more sense with i than without i. So maybe the incompleteness theorems are irrelevant! Because they limit formal reasoning, but they say nothing about what happens when we change the rules of the game. As a creative mathematician I certainly sympathize with the point of view that the imagination to change the rules of the game is more important than grinding out the consequences of a given set of rules. But I don't know how to analyze creativity and imagination with my metamathematical tools... For the history of i, see T. Dantzig, Number—The Language of Science, and P.J. Nahin, An Imaginary Tale—The Story of the Square Root of −1.]

   What I think it all means is that mathematics is different from physics, but it's not that different. I think that math is quasi-empirical. It's different from physics, but it's more a matter of degree than an all or nothing difference. I don't think that mathematicians have a direct pipeline to God's thoughts, to absolute truth, while physics must always remain tentative and subject to revision. Yes, math is less tentative than physics, but they're both in the same boat, because they're both human activities, and to err is human.

Now physicists used to love it when I said this, and mathematicians either hated it and said I was crazy or pretended not to understand.

But a funny thing has happened. I'm not alone anymore.

Now there's a journal called Experimental Mathematics. At Simon Fraser University in Canada there's a Centre for Experimental and Constructive Mathematics. And Thomas Tymoczko has published an anthology called New Directions in the Philosophy of Mathematics with essays by philosophers, mathematicians and computer scientists that he says support a quasi-empirical view of math. I'm happy to say that two of my articles are in his book.

By the way, the name ``quasi-empirical'' seems to come from Imre Lakatos. He uses it in an essay in Tymoczko's anthology. I used to say that ``perhaps mathematics should be pursued somewhat more in the spirit of an experimental science,'' which is a mouthful. It's much better to say ``maybe math is quasi-empirical!''

And I've seen computer scientists do some things quasi-empirically. They've added P ≠ NP as a new axiom. Everyone believes that P ≠ NP based on experimental evidence, but no one can prove it. And theoreticians working on cryptography assume that certain problems are hard, even though no one can prove it. Why? Simply because no one has been able to find an easy way to solve these problems, and no one has been able to break encryption schemes that are based on these problems.

The computer has expanded mathematical experience so greatly, that in order to cope, mathematicians are behaving differently. They're using unproved hypotheses that seem to be true.

So maybe Gödel was right after all, maybe incompleteness is a serious business. Maybe the traditional view that math gives absolute certainty is false.

Enough talking! Let's do some computer programming! [Readers who hate computer programming should skip directly to Chapter VI.] " (http://www.cs.auckland.ac.nz/~chaitin/unknowable/ch1.html)

6. "Ramsden was able to divide circles to a repeatable accuracy of 3 seconds of arc and as his dividing engine became more refined it was said that it eventually could produce a reliable
accuracy of one second. In 1782 Jesse Ramsden commenced one of his greatest co mmissions, that of building the Great Theodolite for the triangulation between the observatories of London and Paris, to be carried out by General Roy. The project took much longer than estimated, as many problems had to be overcome to fulfil the required specification.
The completed instrument had a horizontal circle of 36 inches in diameter with 6 micrometers each capable of reading directly to one second of arc. The total weight of the assembled instrument was 200 lbs. (90Kg) " (Wallis, History of Angle Measurement)
7. "The use of a micrometer to subdivide the graduations of a circle or arc was the most popular method for obtaining a high order of accuracy. The work on the development of the
micrometer date back to the 1660's with the astronomers Auzout and Picard. Before that William Gascoigne had made a working micrometer in 1639 but unfortunately died at the
Battle of Marston Moor in 1644.before receiving due credit for his invention. It was therefore some 26 years later that an instrument was produced that combined the micrometer with an
astronomical telescope.
The first application of micrometers was in astronomy, to measure the diameter of heavenly bodies. Early models employed a knife-edge moved by a fine threaded lead screw, to which
was connected a graduated drum divided into 100 subdivisions of the main scale divisions positioned alongside the lead-screw.
In about 1659 Robert Hooke replaced the knife-edge indices with fine hairlines. Circa 1662 Cornelio Malvasia introduced grid lines with thin silver wires. Auzout and Picard introduced
one fixed and one moveable thread, such that the movement of the thread across the object could be recorded on the drum. Later they introduced more threads across the field of view.
In the middle of the 18th century Tobias Meyer expounded a method of angle repetition or the multiplication method for increasing the accuracy of determine angles. However it was 20
years before this method was applied to an instrument, which was produced by the French manufacturer E. Lenoir on the basis of a design by the geographical engineer. J.C. Borda. " (Wallis, History of Angle Measurement)
8. "The topological view of computational phenomena has been developed in intuitionistic and constructive mathematics, logic and recursion theory, do-

main theory, and type-two theory of effectivity. This goes back to Brouwer, who proved that, in his intuitionistic approach to mathematics, all functions f:R→R
are continuous. In these notes, our mathematics is classical, but his arguments can be exported to the theory of computation as developed in
classical mathematics to conclude that computable functions are continuous.
Work by Kleene, Kreisel, Myhill/Shepherdson, Rice/Shapiro, Nerode, Scott, Ershov, Plotkin, Smyth, Abramsky, Vickers, Weihrauch and no doubt
many others gradually exhibited the topological character of data types other than the real numbers, emphasizing the fact that computable functions
are topologically continuous generalizes to any domain of computation." (Escardo, Synthetic topology of data types and classical spaces)

9. "Dedekind cuts

   The first construction of the Real numbers from the Rationals is due to the German mathematician Richard Dedekind (1831 - 1916). He developed the idea first in 1858 though he did not publish it until 1872. This is what he wrote at the beginning of the article.

   As professor in the Polytechnic School in Zürich I found myself for the first time obliged to lecture upon the ideas of the differential calculus and felt more keenly than ever before the lack of a really scientific foundation for arithmetic. In discussing the notion of the approach of a variable magnitude to a fixed limiting value, and especially in proving the theorem that every magnitude which grows continuously but not beyond all limits, must certainly approach a limiting value, I had recourse to geometric evidences. ... This feeling of dissatisfaction was so overpowering that I made the fixed resolve to keep mediating on the question till I should find a purely arithmetic and perfectly rigorous foundation for the principles of infinitesimal analysis.

   He defined a real number to be a pair (L, R) of sets of rationals which have the following properties.

   Every rational is in exactly one of the sets
   Every rational in L is < every rational in R

   Such a pair is called a Dedekind cut (Schnitt in German). You can think of it as defining a real number which is the least upper bound of the "Left-hand set" L and also the greatest lower bound of the "right-hand set" R. If the cut defines a rational number then this may be in either of the two sets.
   It is rather a rather long (and tedious) task to define the arithmetic operations and order relation on such cuts and to verify that they do then satisfy the axioms for the Reals -- including even the Completeness Axiom.

   Richard Dedekind, along with Bernhard Riemann was the last research student of Gauss. His arithmetisation of analysis was his most important contribution to mathematics, but was not enthusiastically received by leading mathematicians of his day, notably Kronecker and Weierstrass. His ideas were, however, warmly welcomed by Jordan and especially by Cantor with whom he became firm friends. " (http://www-history.mcs.st-and.ac.uk/~john/analysis/Lectures/A3.html) <Origins>
   Note: The paper I am partially reading was published later: 1888 "Richard Dedekind (1831–1916) published in 1888 a paper entitled Was sind und was sollen die Zahlen? variously translated as What are numbers and what should they be? or, as Beman did, The Nature of Meaning of Numbers."
10. "During the 1880-s Poincare came across "manifolds" in several analytical or geometrical contexts, although he personally understood them at that time still in a rather vague way.
   One of these contexts arose from his work in the theory of automorphic functions that made him famous (and Klein nervous) at the beginning of the decade. The culminating problem
   of Poincare's and Klein's research was the uniformization "theorem". Poincare's points of departure were complex differential equations over algebraic curves (f) being a meromorphic function on an algebraic curve C given by f(x,y) = 0, which
   leads only to (finitely many) regular singularities.^^ If it could be solved by means of a pair of Fuchsian functions^^ x(^), y(^) taking as fundamental system the functions
   vi = ^/dxjd^ and V2 = ^^/dxjd^ (pushed down to C), Poincare called the equation a Fuchsian differential equation.^^ The quotient V2/v\ = ^ was then the inverse of a
   universal covering map of the algebraic curve C, branched in the (regular) singularities of 0 on C Poincare called two Fuchsian equations of the same type if there is a birational transformation between the underlying algebraic curves C and C' which transforms the singularities one into another such that the monodromy characteristics remain identical.^^ = v(x)0(x, j) ," (James, History of Topology) <Justif>
11. "Poincare gave a discussion which in fact spoke in favour of the surjectivity and was already sharper than Klein's, but still used highly intuitive ideas about continuous vari-
   ation of images in higher dimensional spaces in a symbohcally uncontrollable manner. Even the spaces themselves were not shown to be manifolds but taken as such, without
   further ado. For any critical reader (perhaps even including Poincare himself) the "conti-nuity proof" could

thus be taken at least as much as an indicator for the necessity of an improved understanding of higher dimensional geometry as it was an indicator for the truthof the uniformization theorem. And in fact a clarification of the topological proof strategy was given only later by Brouwer (191 la, 191 lb) who used enlarged (necessarily no longeruniquely) parametrizing spaces which indeed were manifolds and to which he could apply his domain invariance theorem for continuous injective mappings.'*^" (James, History of Topology) <Justif>

12. "During the last three decades of the 19th century Cantor had devel-oped his theory of point sets in R" in the framework of general set theory. He himself was shocked to realize that bijective maps between real continua of different dimensions can be conceived, and even Dedekind's comforting conviction that more specific maps, in this case bijective and (bi-)continuous ones, would respect the invariance of dimension left the prob- lem to prove (or disprove) such a conjectured invariance. Naive assumptions from space intuition were particularly deceptive in this field; that became even clearer about 1890 when Peano published his example of a "spacefilling" curve with the surprising effect, that the lack of injectivity would even for continuous maps not necessarily lead to a decrease of dimension (or keep it at most invariant), but could as well increase it. Early attempts by Liiroth, Thomae, Netto, and Cantor himself, to prove the invariance of dimension under bijective continuous maps, turned out to contain unclosable gaps and again (as in the case of the continuity proof for uniformization) it was only Brouwer who surmounted the dif-ficulties and indeed proved the correctness of Dedekind's suggestion (Brouwer, 191 la).^^" (James, History of Topology) <Justif>
13. "So it was in fact Brouwer's highly influential introduction of a "mized approach" of combinatorial and continuity methods, in which manifolds were defined by simplicial methods, that marked the next remarkable leap for a constructive underpinning of the manifold concept. It also pointed out in which direction one had to go if manifolds should be selected among the more general objects of abstract combinatorial complexes.^^" (James, History of Topology) <Origins>
14. "“Finally the "modern " axiomatic concept

    There was, of course, still another line of research, more closely linked to differential geometry, where manifolds played an essential role, and purely topological aspects (in- dependently of whether continuous, combinatorial, or homological ones) did not suffice and still needed elaboration. In North America Oswald Veblen and his students formed an active center in both fields of topology and modern geometry. Veblen and his student J.H.C. Whitehead, coming from (and going back to) Oxford, brought the axiomatization of the manifold concept to a stage which stood up to the standards of modern mathemat- ics in the sense of the 20th century (Veblen and Whitehead, 1931, 1932). Veblen was an admirer of the Gottingen tradition of mathematics, in particular, F. Klein and D. Hilbert, and cooperated closely with H. Weyl, the broadest representative of his own generation from the Klein and Hilbert tradition. Veblen and J.H.C. Whitehead combined a view of the central importance of structure groups for geometry (generaUzing the Erlanger program) with Hubert's embryonic characterization of manifolds by coordinate systems; and they took care that the topologization of the underlying set would satisfy Hausdorff's axioms for a topological space.
    ...
    Whitehead and Veblen presented their axiomatic characterization of manifolds of class G first in a research article in the Annals of Mathematics (Veblen and Whitehead, 1931) and in the final form in their tract on the Foundations of Differential Geometry (Veblen and Whitehead, 1932). Their book contributed effectively to a conceptual standardization of modern differential geometry, including not only the basic concepts of continuous and differentiable manifolds of different classes, but also the "modern" reconstruction of the differentials dx = {Ax\,..., dxn) as objects in tangent spaces to M}^^ Basic concepts like Riemannian metric, affine connection, holonomy group, covering manifolds, etc. followed in a formal and symboUc precision that even from the strict logical standards of the 1930-s there remained no doubt about the wellfoundedness of differential geometry in manifolds.
    Moreover they made the whole subject conceptually accessible to anybody acquainted with the language and symbolic practices of modern mathematics.” [3]" (James, History of Topology) <Origins>
15. " By the time Stevin proposed the use of decimal fractions in 1585, the concept of a number had developed little from that of Euclid's Elements. Details of the earlier contributions are examined in some detail in our article: The real numbers: Pythagoras to Stevin

    If we move forward almost exactly 100 years to the publication of A treatise of Algebra by Wallis in 1684 we find that he accepts, without any great enthusiasm, the use of Stevin's decimals. He still only considers finite decimal expansions and realises that with these one can approximate numbers (which for him are constructed from positive integers by addition, subtraction, multiplication, division and taking nth roots) as closely as one wishes. However, Wallis understood that there were proportions which did not fall within this definition of number, such as those associated with the area and circumference of a circle:-

... such proportion is not to be expressed in the commonly received ways of notation: particularly that for the circles quadrature. ... Now, as for other incommensurable quantities, though this proportion cannot be accurately expressed in absolute numbers, yet by continued approximation it may; so as to approach nearer to it than any difference assignable.

For Wallis there were a variety of ways that one might achieve this approximation, so coming as close as one pleased. He considered approximations by continued fractions, and also approximations by taking successive square roots. This leads into the study of infinite series but without the necessary machinery to prove that these infinite series converged to a limit, he was never going to be able to progress much further in studying real numbers. Real numbers became very much associated with magnitudes. No definition was really thought necessary, and in fact the mathematics was considered the science of magnitudes. Euler, in Complete introduction to algebra (1771) wrote in the introduction:-

Mathematics, in general, is the science of quantity; or, the science which investigates the means of measuring quantity.

He also defined the notion of quantity as that which can be continuously increased or diminished and thought of length, area, volume, mass, velocity, time, etc. to be different examples of quantity. All could be measured by real numbers. However, Euler's mathematics itself led to a more abstract idea of quantity, a variable x which need not necessarily take real values. Symbolic mathematics took the notion of quantity too far, and a reassessment of the concept of a real number became more necessary. By the beginning of the nineteenth century a more rigorous approach to mathematics, principally by Cauchy and Bolzano, began to provide the machinery to put the real numbers on a firmer footing. Grabiner writes [2]:-

... though Cauchy implicitly assumed several forms of the completeness axiom for the real numbers, he did not fully understand the nature of completeness or the related topological properties of sets of real numbers or of points in space. ... Cauchy did not have explicit formulations for the completeness of the real numbers. Among the forms of the completeness property he implicitly assumed are that a bounded monotone sequence converges to a limit and that the Cauchy criterion is a sufficient condition for the convergence of a series. Though Cauchy understood that a real number could be obtained as the limit of rationals, he did not develop his insight into a definition of real numbers or a detailed description of the properties of real numbers.

Cauchy, in Cours d'analyse (1821), did not worry too much about the definition of the real numbers. He does say that a real number is the limit of a sequence of rational numbers but he is assuming here that the real numbers are known. Certainly this is not considered by Cauchy to be a definition of a real number, rather it is simply a statement of what he considers an "obvious" property. He says nothing about the need for the sequence to be what we call today a Cauchy sequence and this is necessary if one is to define convergence of a sequence without assuming the existence of its limit. He does define the product of a rational number A and an irrational number B as follows:-

Let b, b', b'', ... be a sequence of rationals approaching B closer and closer. Then the product AB will be the limit of the sequence of rational numbers Ab, Ab', Ab'', ...

Bolzano, on the other hand, showed that bounded Cauchy sequence of real numbers had a least upper bound in 1817. He later worked out his own theory of real numbers which he did not publish. This was a quite remarkable achievement and it is only comparatively recently that we have understood exactly what he did achieve. His definition of a real number was made in terms of convergent sequences of rational numbers and is explained in [22] where Rychlik describes it as "not quite correct". In [28] van Rootselaar disagrees saying that "Bolzano's elaboration is quite incorrect". However in J Berg's edition of Bolzano's Reine Zahlenlehre which was published in 1976, Berg points out that Bolzano had discovered the difficulties himself and Berg found notes by Bolzano which proposed amendments to his theory which make it completely correct. As Bolzano's contributions were unpublished they had little influence in the development of the theory of the real numbers.

Cauchy himself does not seem to have understood the significance of his own "Cauchy sequence" criterion for defining the real numbers. Nor did his immediate successors. It was Weierstrass, Heine, Méray, Cantor and Dedekind who, after convergence and uniform convergence were better understood, were able to give rigorous definitions of the real numbers.

Up to this time there was no proof that numbers existed that were not the roots of polynomial equations with rational coefficients. Clearly √2 is the root of a polynomial equation with rational coefficients, namely x2 = 2, and it is easy to see that all roots of rational numbers arise as solutions of such equations. A number is called transcendental if it is not the root of a polynomial equation with rational coefficients. The word transcendental is used as such number transcend the usual operations of arithmetic. Although mathematicians had guessed for a long time that π and e were transcendental, this had not been proved up to the middle of the 19th century. Liouville's interest in transcendental numbers stemmed from reading a correspondence between Goldbach and Daniel Bernoulli. Liouville certainly aimed to prove that e is transcendental but he did not succeed. However his contributions led him to prove the existence of a transcendental number in 1844 when he constructed an infinite class of such numbers using continued fractions. These were the first numbers to be proved transcendental. In 1851 he published results on transcendental numbers removing the dependence on continued fractions. In particular he gave an example of a transcendental number, the number now named the Liouvillian number

0.1100010000000000000000010000...

where there is a 1 in place n! and 0 elsewhere.

One of the first people to attempt to give a rigorous definition of the real numbers was Hamilton. Perhaps, if one thinks about it, it is logical that he would be interested in this since his introduction of the quaternions had shown that there were new previously unstudied number systems. In fact came close to the idea of a Dedekind cut, as Euclid had done in the Elements, but failed to make the idea into a definition (again Euclid had spotted the property but never thought to use it as a definition). For a number a he noted that there are rationals a', a", b', b", c', c", d', d", ... with

a' < a < a"
b' < a < b"
c' < a < c"
d' < a < d"
...

but he never thought to define a number by the sets {a', b', c', d', ... } and {a", b", c", d", ... }. He tried another approach of defining numbers given by some law, say x goesto x2. Hamilton writes:-

If x undergoes a continuous and constant increase from zero, then will pass successively through every state of positive ration b, and therefore that every determined positive ration b has one determined square root √b which will be commensurable or incommensurable according as b can or cannot be expressed as the square of a fraction. When b cannot be so expressed, it is still possible to approximate in fractions to the incommensurable square root √b by choosing successively larger and larger positive denominators ...

One can see what Hamilton is getting at, but much here is without justification - can a quantity undergo a continuous and constant increase. Even if one got round this problem he is only defining numbers given by a law. It is unclear whether he thought that all real numbers would arise in this way.

When progress came in giving a rigorous definition of a real number, there was a sudden flood of contributions. Dedekind worked out his theory of Dedekind cuts in 1858 but it remained unpublished until 1872. Weierstrass gave his own theory of real numbers in his Berlin lectures beginning in 1865 but this work was not published. The first published contribution regarding this new approach came in 1867 from Hankel who was a student of Weierstrass. Hankel, for the first time, suggests a total change in out point of view regarding the concept of a real number:-

Today number is no longer an object, a substance which exists outside the thinking subject and the objects giving rise to this substance, an independent principle, as it was for instance for the Pythagoreans. Therefore, the question of the existence of numbers can only refer to the thinking subject or to those objects of thought whose relations are represented by numbers. Strictly speaking, only that which is logically impossible (i.e. which contradicts itself) counts as impossible for the mathematician.

In his 1867 monograph Hankel addressed the question of whether there were other "number systems" which had essentially the same rules as the real numbers.

Two years after the publication of Hankel's monograph, Méray published Remarques sur la nature des quantités in which he considered Cauchy sequences of rational numbers which, if they did not converge to a rational limit, had what he called a "fictitious limit". He then considered the real numbers to consist of the rational numbers and his fictitious limits. Three years later Heine published a similar notion in his book Elemente der Functionenlehre although it was done independently of Méray. It was similar in nature with the ideas which Weierstrass had discussed in his lectures. Heine's system has become one of the two standard ways of defining the real numbers today. Essentially Heine looks at Cauchy sequences of rational numbers. He defines an equivalence relation on such sequences by defining a1 , a2 , a3 , a4 , ... and b1, b2 , b3 , b4 , ...

to be equivalent if the sequence of rational numbers a1 - b1, a2 - b2 , a3 - b3 , a4 - b4 , ... converges to 0. Heine then introduced arithmetic operations on his sequences and an order relation. Particular care is needed to handle division since sequences with a non-zero limit might still have terms equal to 0.

Cantor also published his version of the real numbers in 1872 which followed a similar method to that of Heine. His numbers were Cauchy sequences of rational numbers and he used the term "determinate limit". It was clear to Hankel (see the quote above) that the new ideas of number had suddenly totally changed a concept which had been motivated by measurement and quantity. Similarly Cantor realised that if he wants the line to represent the real numbers then he has to introduce an axiom to recover the connection between the way the real numbers are now being defined and the old concept of measurement. He writes about a distance of a point from the origin on the line:-

If this distance has a rational relation to the unit of measure, then it is expressed by a rational quantity in the domain of rational numbers; otherwise, if the point is one known through a construction, it is always possible to give a sequence of rationals a1 , a2 , a3 , ..., an , ... which has the properties indicated and relates to the distance in question in such a way that the points on the straight line to which the distances a1 , a2 , a3 , ..., an , ... are assigned approach in infinity the point to be determined with increasing n. ... In order to complete the connection presented in this section of the domains of the quantities defined [his determinate limits] with the geometry of the straight line, one must add an axiom which simple says that every numerical quantity also has a determined point on the straight line whose coordinate is equal to that quantity, indeed, equal in the sense in which this is explained in this section.

As we mentioned above, Dedekind had worked out his idea of Dedekind cuts in 1858. When he realised that others like Heine and Cantor were about to publish their versions of a rigorous definition of the real numbers he decided that he too should publish his ideas. This resulted in yet another 1872 publication giving a definition of the real numbers. Dedekind considered all decompositions of the rational numbers into two sets A1 , A2 so that a1 < a2 for all a1 in A1 and a2 in A2. He called (A1, A2) a cut. If the rational a is either the maximum element of A1 or the minimum element of A2 then Dedekind said the cut was produced by a. However not all cuts were produced by a rational. He wrote:-

In every case in which a cut (A1, A2) is given that is not produced by a rational number, we create a new number, an irrational number a, which we consider to be completely defined by this cut; we will say that the number a corresponds to this cut or that it produces the cut.

He defined the usual arithmetic operations and ordering and showed that the usual laws apply.

Another definition, similar in style to that of Heine and Cantor, appeared in a book by Thomae in 1880. Thomae had been a colleague of Heine and Cantor around the time they had been writing up their ideas. He claimed that the real numbers defined in this way had a right to exist because:-

... the rules of combination abstracted from calculations with integers may be applied to them without contradiction.

Frege, however, attacked these ideas of Thomae. He wanted to develop a theory of real numbers based on a purely logical base and attacked the philosophy behind the constructions which had been published. Thomae added further explanation to his idea of "formal arithmetic" in the second edition of his text which appeared in 1898:-

The formal conception of numbers requires of itself more modest limitations than does the logical conception. It does not ask, what are and what shall the numbers be, but it asks, what does one require of numbers in arithmetic.

Frege was still unhappy with the constructions of Weierstrass, Heine, Cantor, Thomae and Dedekind. How did one know. he asked, that the constructions led to systems which would not produced contradictions? He wrote in 1903:-

This task has never been approached seriously, let alone been solved.

Frege, however, never completed his own version of a logical framework. His hopes were shattered when he learnt of Russell's paradox. Hilbert had taken a totally different approach to defining the real numbers in 1900. He defined the real numbers to be a system with eighteen axioms. Sixteen of these axioms define what today we call an ordered field, while the other two were the Archimedean axiom and the completeness axiom. The Archimedean axiom stated that given positive numbers a and b then it is possible to add a to itself a finite number of times so that the sum exceed b. The completeness property says that one cannot extend the system and maintain the validity of all the other axioms. This was totally new since all other methods built the real numbers from the known rational numbers. Hilbert's numbers were unconnected with any known system. It was impossible to say whether a given mathematical object was a real number. Most seriously, there was no proof that any such system actually existed. If it did it was still subject to the same questions concerning its consistency as Frege had pointed out.

By the beginning of the 20th century, then, the concept of a real number had moved away completely from the concept of a number which had existed from the most ancient times to the beginning of the 19th century, namely its connection with measurement and quantity. "

16. "So where does Euclid's Elements leave us with respect to numbers. Basically numbers were 1, 2, 3, ... and ratios of numbers were used which (although not considered to be numbers) basically allowed manipulation with what we call rationals. Also magnitudes were considered and these were essentially lengths constructible by ruler and compass from a line of unit length. No other magnitudes were considered. Hence mathematicians studied magnitudes which had lengths which, in modern terms, could be formed from positive integers by addition, subtraction, multiplication, division and taking square roots." (http://www-history.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm) <Origins>
17. "The Arabic mathematicians went further with constructible magnitudes for they used geometric methods to solve cubic equations which meant that they could construct magnitudes whose ratio to a unit length involved cube roots. For example Omar Khayyam showed how to solve all cubic equations by geometric methods. Fibonacci, using skills learnt from the Arabs, solved a cubic equation showing that its root was not formed from rationals and square roots of rationals as Euclid's magnitudes were. He then went on to compute an approximate solution. Although no conceptual advances were taking place, by the end of the fifteenth century mathematicians were considering expressions build from positive integers by addition, subtraction, multiplication, division and taking nth roots. These are called radical expressions." (http://www-history.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm) <Origins>
18. "Therefore the mathematical circle is rightly described as the polygon of infinitely many sides. And thus the circumference of the mathematical circle receives no number, neither rational nor irrational.
Not too good an argument, but nevertheless a remarkable insight that there were lengths which did not correspond to radical expressions but which could be approximated as closely as one wished." (http://www-history.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm) <Origins>
19. "A major advance was made by Stevin in 1585 in La Theinde when he introduced decimal fractions. One has to understand here that in fact it was in a sense fortuitous that his invention led to a much deeper understanding of numbers for he certainly did not introduce the notation with that in mind. Only finite decimals were allowed, so with his notation only certain rationals to be represented exactly. Other rationals could be represented approximately and Stevin saw the system as a means to calculate with approximate rational values. His notation was to be taken up by Clavius and Napier but others resisted using it since they saw it as a backwards step to adopt a system which could not even represent 1/3 exactly." (http://www-history.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm) <Origins>
20. "Stevin made a number of other important advances in the study of the real numbers. He argued strongly in L'Arithmetique (1585) that all numbers such as square roots, irrational numbers, surds, negative numbers etc should all be treated as numbers and not distinguished as being different in nature. He wrote:
Thesis 1: Thesis 2: Thesis 3: Thesis 4:
That unity is a number.
That any given numbers can be square, cubes, fourth powers etc.
That any given root is a number.
That there are no absurd, irrational, irregular, inexplicable or surd numbers.
It is a very common thing amongst authors of arithmetics to treat numbers like √8 and similar ones, which they call absurd, irrational, irregular, inexplicable or surds etc and which we deny to be the case for number which turns up.
His first thesis was to argue against the Greek idea that 1 is not a number but a unit and the numbers 2, 3, 4, ... were composed of units. The other three theses were encouraging people to treat different types of numbers, which were at that time treated separately, as a single entity namely a number."
(http://wwwhistory.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm) <Origins>
21. "According to van der Waerden (1985, p. 69), Stevin's "general notion of a real number was accepted, tacitly or explicitly, by all later scientists". A recent study attributes a greater role to Stevin in developing the real numbers than has been acknowledged by Weierstrass's followers.[11] Stevin proved the intermediate value theorem for polynomials, anticipating Cauchy's proof thereof. Stevin uses a divide and conquer procedure subdividing the interval into ten equal parts.[12] Stevin's decimals were the inspiration for Isaac Newton's work on infinite series.[13]" (http://en.wikipedia.org/wiki/Simon_Stevin)
22. "Cauchy himself does not seem to have understood the significance of his own "Cauchy sequence" criterion for defining the real numbers. Nor did his immediate successors. It was Weierstrass, Heine, Méray, Cantor and Dedekind who, after convergence and uniform convergence were better understood, were able to give

rigorous definitions of the real numbers." (http://wwwhistory.mcs.st-andrews.ac.uk/HistTopics/Real_numbers_1.htm) <Sep>

23. "Up to this time there was no proof that numbers existed that were not the roots of polynomial equations with rational coefficients. Clearly √2 is the root of a polynomial equation with rational coefficients, namely x2 = 2, and it is easy to see that all roots of rational numbers arise as solutions of such equations. A number is called transcendental if it is not the root of a polynomial equation with rational coefficients. The word transcendental is used as such number transcend the usual operations of arithmetic. Although mathematicians had guessed for a long time that π and e were transcendental, this had not been proved up to the middle of the 19th century. Liouville's interest in transcendental numbers stemmed from reading a correspondence between Goldbach and Daniel Bernoulli. Liouville certainly aimed to prove that e is transcendental but he did not succeed. However his contributions led him to prove the existence of a transcendental number in 1844 when he constructed an infinite class of such numbers using continued fractions. These were the first numbers to be proved transcendental. In 1851 he published results on transcendental numbers removing the dependence on continued fractions. In particular he gave an example of a transcendental number, the number now named the Liouvillian number 0.1100010000000000000000010000... where there is a 1 in place n! and 0 elsewhere." (http://www-history.mcs.standrews.ac.uk/HistTopics/Real_numbers_1.htm)
24. "Functions of several variables have their origin in geometry (e.g., curves depending on parameters (Leibniz 1694a)) and in physics. A famous problem throughout the 18th century was the calculation of the movement of a vibrating string (d'Alembert 1748, Fig.0.1). The position of a string u(x,t) is actually a function of x, the space coordinate, and of t, the time. An important breakthrough for the systematic study of several variables, which occured around the middle of the 19th century, was the idea of denoting pairs (then n-tuples)
(x1,x2) = : x (x1,x2,...,xn) = : x
by a single letter and of considering them as new mathematical objects. They were called "extensive Gro ̈sse" by Grassmann (1844, 1862), "complexes" by Peano (1888), and "vectors" by Hamilton (1853)." <Origins> (Hairer, Analysis By its History"
25. Here is a nice quotes about the usage of contradiction of a theorem when trying to prove its converse: "As mentioned before, this proposition is a disguised converse of the previous one. As Euclid often does, he uses a proof by contradiction involving the already proved converse to prove this proposition. It is not that there is a logical connection between this statement and its converse that makes this tactic work, but some kind of symmetry involved. In this case, if one side is less than another, then the other is greater than the one, and the previous proposition applies. So the relevant symmetry is between "less" and "greater." " (http://aleph0.clarku.edu/~djoyce/java/elements/bookI/propI19.html, D.E. Joyce)
26. "On October 2, 1759, Euler had written Lagrange: "I am delighted to learn that you approve my solution … which D'alembert has tried to undermine by various cavils, and that for the sole reason that he did not get it himself. He has threatened to publish a weighty refutation; whether he really will I do not know. He thinks he can deceive the semi-learned by his eloquence. I doubt whether he is serious, unless perhaps he is
thoroughly blinded by self-love" <Curious> (Kline, Mathematical thought)
27. "A traveller who refuses to pass over a bridge until he has personally tested the soundness of every part of it is not likely to go far; something must be risked, even in mathematics." <Wisdom> (Horace Lamb) (Kline, Mathematical thought)
28. "one sees that Lagrange had succeeded in reducing an arbitrary first order equations in x,y and z to a system of simultaneous ordinary differential equations. He does not state the results explicitly but it follows from the above work. Curiously, in 1785 he had to solve a particular first order partial differential equations and said it was impossible with present methods; he had forgotten his earlier (1772) work." <Curious> (Kline, Mathematical Thought).
29. "It is true that I don't understand how to calculate any curves, but I do know that 16,000 talers is more than 13,000" (Freidrich II to Euler, Euler and Modern Science edited by Nikolai Nikolaevich)
30. "with respect to the form of presentation of his work, it is to be remarked that Huygens shares with Galileo, in all its perfection, the latter's exalted and inimitable candor. He is frank without reserve in the presentment of the methods that led him to his dis-coveries, and thus always conducts his reader into the full comprehension of his performances. Norhadhe cause to conceal these methods." <Character> (Mach, The Science of Mechanics: A Critical and Historical Account of Its Development)
31. "In science, there are two sorts of revolutionary masterpieces: those rhetorically powerful enough to provoke open revolt and those rhetorically ingenious enough to avoid it." (Starring the Text: The Place of Rhetoric in Science Studies Alan G. Gross)
32. The first three sections present the classical material named after Cauchy, Courant,Fischer,and Weyl. The Courant-Fischer minmax theorem turned up in two different lecture courses that I took as a graduate student.On each occasion the lecturer tried to present the proof and became confused. As a result I concluded (erroneously) that the theorem was deep and difficult. Many years later I realized that the only hazard in presenting the proof lies with the indices. Are eigenvalues labelled i n increasing o r decreasing order? Are subspaces given by bases or byconstraints? (The Symmetric Eigenvalue Problem, Parlett)

33.