Jad > ThePlan >

Quotes1, 0-103

QUOTE TAGS

1. <Tag1> <Justif> Justifications (Usually philosophical sounding) of the 'ratios of vanishing quantities' (not-zero when dividing by them, but zero eventually) of 'differential calculus' before the more rigorous 'real analysis'
2. <Tag2> <Curious> Curious and/or Prolix.
3. <Tag3> <Origins> Origins. A historically direct or indirect origin of a concept that is usually taught without reference to it.
4. <Tag4> <PhilTake> 'Philosophical' take. A less formal -often zoomed out- but very useful take on a formal idea.
5. <Tag5> <AA>, The amazing ancients.
6. <Tag6> <IKIT>, I knew it!
7. <Tag7> <Sep> <Clueless>, Separate: do not confuse between excellent mathematicians and their work. They are two different things. An excellent mathematician, in another era, would still have been excellent, it is their excellence from which we need to learn, and that is not exactly their (polished) work. The work we still have to learn, with the goal of being on the boundary of course. We will use this tag especially on quotes that show how a brilliant idea solver a problem at hand, and many times, it was not realized by the (nevertheless brilliant as we argue for separation) mathematician that the concept is of significant importance for further developments.
8. <Tag8> <Humor>.

QUOTES

1. But the configuration spaces of physical systems were not handed down by Prometheus with fire, and on its face it is rather a mystery where they come from, what they do and why we need them. (Classical Mechanics Is Lagrangian; It Is Not Hamiltonian†, Erik Curiel).
2. “What matters to me,” Kepler explained in his Preface, “is not merely to impart to the reader what I have to say, but above all to convey to him the reasons, subterfuges, and lucky hazards which led me to my discoveries. When Christopher Colombus, Magelhaen, and the Portuguese relate how they went astray on their journeys, we not only forgive them, but would regret to miss their narration because without it the whole, grand entertainment would be lost. Hence I shall not be blamed if, prompted by the same affection for the reader, I follow the same method.” (http://homepages.wmich.edu/~mcgrew/sleepwalk.htm)
3. "For, if I had believed that we could ignore these eight minutes, I would have patched up my hypothesis accordingly. But since it was not permissible to ignore them, those eight minutes point the road to a complete reformation of astronomy: they have become the building material for a large part of this work. . . .” (Kepler) ((http://homepages.wmich.edu/~mcgrew/sleepwalk.htm))
4. "until "universal gravity" or "electro-magnetic field" became 1 Newton's First Law of Motion was in fact formulated by Descartes. verbal fetishes which hypnotised it into quiescence, disguising the fact that they are meta- physical concepts dressed in the mathematical language of physics." (The sleepwalkers, Koestler)
5. "And though the matter were divided at first into several systems, and every system by a divine power constituted like ours; yet would the outside systems descend towards the middlemost; so that this frame of things could not always subsist without a divine power to conserve it..." (Newton) (From the sleepwalkers)
6. "For ought we know," says Bertrand Russell, "an atom may consist entirely of the radiations which come out of it. It is useless to argue that radiations cannot come out of nothing... The idea that there is a little hard lump there, which is the electron or proton, is an illegitimate intrusion of commonsense notions derived from touch...'Matter' is a conve- nient formula for describing what happens where it isn't." (From the sleepwalkers)
7. "How did this situation come about? Already in 1925, before the new quantum mechanics came into being, Whitehead wrote that "the physical doctrine of the atom has got into a state which is strongly suggestive of the epicycles of astronomy before Copernicus." 22 The common feature between pre-Keplerian astronomy and modern physics is that both have developed in relative isolation as "closed systems", manipulating a set of symbols accord- ing to certain rules of the game. Both systems "worked"; modern physics yielded nuclear

energy, and Ptolemaic astronomy yielded predictions whose precision bowled over Tycho. The medieval astronomers manipulated their epicyclic symbols as modern physics mani- pulates Schroedinger's wave equations or Dirac's matrices, and it worked – though they knew nothing of gravity and elliptic orbits, believed in the dogma of circular motion, and had not the faintest idea why it worked. We are reminded of Urban VIII's famous argu- ment which Galileo treated with scorn: that a hypothesis which works must not necessari- ly have anything to do with reality for there may be alternative explanations of how the Lord Almighty produces the phenomena in question. If there is a lesson in our story it is that the manipulation, according to strictly self-consistent rules, of a set of symbols representing one single aspect of the phenomena may produce correct, verifiable predic- tions, and yet completely ignore all other aspects whose ensemble constitutes reality:" (The sleepwalkers, Koestler)

8. "And so in its actual procedure physics studies not these inscrutable qualities [of the material world], but pointer readings which we can observe. The read- ings, it is true, reflect the fluctuations of the world-qualities; but our exact knowledge is of the readings, not of the qualities. The former have as much re- semblance to the latter as a telephone number has to a subscriber. Bertrand Russell expressed this state of affairs even more succinctly: "Physics is mathematical not because we know so much about the physical world, but because we know so little: it is only its mathematical properties that we can discover." (The sleepwalkers, Koestler)
9. "There are two ways of interpreting this situation. Either the structure of the universe is indeed of such a nature that it cannot be compre- hended in terms of human space and time, human reason and human imagination. In this case Exact Science has ceased to be the Philosophy of Nature, and no longer has much in- spiration to offer to the questing human mind. In this case it would be legitimate for the scientist to withdraw into his closed system, to manipulate his purely formal symbols, and to evade questions concerning the "real meaning" of these symbols as "meaningless", as it has become the fashion. But if this be the case, he must accept his role as a mere technician whose task is to produce, on the one hand, better bombs and plastic fibres, and on the oth- er, more elegant systems of epicycles to save the phenomena. The second possibility is to regard the present crisis in physics as a temporary phenome- non, the result of a one-sided, overspecialized development like the giraffe's neck – one of those culs-de-sac of mental evolution which we have so often observed in the past. But if that is the case, where, on the three-centuries' journey from "natural philosophy" to "exact science", did the estrangement from reality begin; at what point was the new version of Plato's curse uttered: "Thou shalt think in circles"? If we knew the answer, we would, of course, also know the remedy; and once the answer is known, it will again appear as heartbreakingly obvious as the sun's central position in the solar system. "We are indeed a blind race," wrote a contemporary scientist, "and the next generation, blind to its own blindness, will be amazed at ours." 26" (The sleepwalkers, Koestler)
10. "A second example of the hubris of contemporary science is the rigorous banishment of the word "purpose" from its vocabulary. This is probably an aftermath of the reaction against the animism of Aristotelian physics, where stones accelerated their fall because of their impatience to get home, and against a teleological world-view in which the purpose of the stars was to serve as chronometers for man's profit. From Galileo onward, "final causes" (or "finality" for short) were relegated into the realm of superstition, and mechanical cau- sality reigned supreme. In the mechanical universe of indivisible hard little atoms, causali- ty worked by impact, as on a billiard table; events were caused by the mechanical push of the past, not by any "pull" of the future. That is the reason why gravity and other forms of action-at-a-distance did not fit into the picture and were regarded with suspicion; why ethers and vortices had to be invented to replace that occult pull by a mechanical push. The mechanistic universe gradually disintegrated, but the mechanistic notion of causality survived until Heisenberg's indeterminacy principle proved its untenability. Today we know that on the sub-atomic level the fate of an electron or a whole atom is not deter- mined by its past. But this discovery has not led to any basically new departure in the phi- losophy of nature, only to a state of bewildered embarrassment, a further retreat of physics into a language of even more abstract symbolism. Yet if causality has broken down and events are not rigidly governed by the pushes and pressures of the past, may they not be influenced in some manner by the "pull" of the future – which is a manner of saying that "purpose" may be a concrete physical factor in the evolution of the universe, both on the organic and unorganic levels. In the relativistic cosmos, gravitation is a result of the curva- tures and creases in space which continually tend to straighten themselves out – which, as Whittaker remarked, 27 "is a statement so completely teleological that it would certainly have delighted the hearts of the schoolmen." (The sleepwalkers, Koestler)
11. "The foregoing remarks are intended only to establish the fact that the definitive clarification of *the nature of the infinite,* instead of pertaining just to the sphere of specialized scientific interests, is needed for *the dignity of the human intellect* itself." Hilbert (http://www.math.dartmouth.edu/~matc/Readers/HowManyAngels/Philosophy/Philosophy.html)
12. -- "...it will not be possible to be and not to be the same thing" (Aristotle), "Every judgment is either true or false" (Leibniz), "Everything must either be or not be." (Russel), from (http://en.wikipedia.org/wiki/Law_of_excluded_middle)
13. Classical set theory accepts the notion of actual, completed infinities. However, some finitist philosophers of mathematics and constructivists object to the notion. If the positive number n becomes infinitely great, the expression 1/n goes to naught (or gets infinitely small). In this sense one speaks of the improper or

potential infinite. In sharp and clear contrast the set just considered is a readily finished, locked infinite set, fixed in itself, containing infinitely many exactly defined elements (the natural numbers) none more and none less. (A. Fraenkel [4, p. 6]) Thus the conquest of actual infinity may be considered an expansion of our scientific horizon no less revolutionary than the Copernican system or than the theory of relativity, or even of quantum and nuclear physics. (A. Fraenkel [4, p. 245]) To look at the universe of all sets not as a fixed entity but as an entity capable of "growing", i.e., we are able to "produce" bigger and bigger sets. (A. Fraenkel et al. [5, p. 118]) (Brouwer) maintains that a veritable continuum which is not denumerable can be obtained as a medium of free development; that is to say, besides the points which exist (are ready) on account of their definition by laws, such as e, pi, etc. other points of the continuum are not ready but develop as so-called choice sequences. (A. Fraenkel et al. [5, p. 255]) Intuitionists reject the very notion of an arbitrary sequence of integers, as denoting something finished and definite as illegitimate. Such a sequence is considered to be a growing object only and not a finished one. (A. Fraenkel et al. [5, p. 236]) Until then, no one envisioned the possibility that infinities come in different sizes, and moreover, mathematicians had no use for “actual infinity.” The arguments using infinity, including the Differential Calculus of Newton and Leibniz, do not require the use of infinite sets. (T. Jech [3]) Owing to the gigantic simultaneous efforts of Frege, Dedekind and Cantor, the infinite was set on a throne and revelled in its total triumph. In its daring flight the infinite reached dizzying heights of success. (D. Hilbert [6, p. 169]) One of the most vigorous and fruitful branches of mathematics [...] a paradise created by Cantor from which nobody shall ever expel us [...] the most admirable blossom of the mathematical mind and altogether one of the outstanding achievements of man's purely intellectual activity. (D. Hilbert on set theory [6]) Finally, let us return to our original topic, and let us draw the conclusion from all our reflections on the infinite. The overall result is then: The infinite is nowhere realized. Neither is it present in nature nor is it admissible as a foundation of our rational thinking - a remarkable harmony between being and thinking. (D. Hilbert [6, 190]) Infinite totalities do not exist in any sense of the word (i.e., either really or ideally). More precisely, any mention, or purported mention, of infinite totalities is, literally, meaningless. (A. Robinson [10, p. 507]) Indeed, I think that there is a real need, in formalism and elsewhere, to link our understanding of mathematics with our understanding of the physical world. (A. Robinson) Georg Cantor's grand meta-narrative, Set Theory, created by him almost singlehandedly in the span of about fifteen years, resembles a piece of high art more than a scientific theory. (Y. Manin [4]) Thus, exquisite minimalism of expressive means is used by Cantor to achieve a sublime goal: understanding infinity, or rather infinity of infinities. (Y. Manin [5]) There is no actual infinity, that the Cantorians have forgotten and have been trapped by contradictions. (H. Poincaré [Les mathématiques et la logique III, Rev. métaphys. morale 14 (1906) p. 316]) When the objects of discussion are linguistic entities [...] then that collection of entities may vary as a result of discussion about them. A consequence of this is that the "natural numbers" of today are not the same as the "natural numbers" of yesterday. (D. Isles [6]) There are at least two different ways of looking at the numbers: as a completed infinity and as an incomplete infinity. (E. Nelson [7]) A viable and interesting alternative to regarding the numbers as a completed infinity, one that leads to great simplifications in some areas of mathematics and that has strong connections with problems of computational complexity. (E. Nelson [8]) During the renaissance, particularly with Bruno, actual infinity transfers from God to the world. The finite world models of contemporary science clearly show how this power of the idea of actual infinity has ceased with classical (modern) physics. Under this aspect, the inclusion of actual infinity into mathematics, which explicitly started with G. Cantor only towards the end of the last century, seems displeasing. Within the intellectual overall picture of our century ... actual infinity brings about an impression of anachronism. (P. Lorenzen[9]) (http://en.wikipedia.org/wiki/Actual_infinity)

14. "[Impredicative definition's] vicious circle, which has crept into analysis through the foggy nature of the usual set and function concepts, is not a minor, easily avoided form of error in analysis." (Weyl)
15. "Continuous analysis and geometry are just degenerate approximations to the discrete world, made necessary by the very limited resources of the human intellect" (Doron zeilberger)
16. The best method of discussing any part of mathematics " is to regard the particular subject of that science in the most abstract and direct way possible ; to suppose nothing, and to assume nothing about that subject, that the properties of the science itself does not suppose. " (D'alembert, (Dugas, A history of mathematics)
17. If some body impinging upon another body changes the motion of that body in any way by its own force, then, by the force of the other body (because of the equality of their mutual pressures), it also will in turn undergo the same change in its own motion in the opposite direction.(Newton) (http://www.arcaneknowledge.org/science/causephy.htm)
18. "At this point we may pause in order to survey and contrast the different notions regarding space that have been mentioned. Until comparatively recent times, it was supposed that the geometrical theory expounded in the Elements of Euclid was an
intellectual construction belonging to the realm of Platonic Ideas, and was unquestionably true. But there was no agreement as to the philosophical relation of the abstract theory to the universe of sensuous perception. Timaeus and Newton both asserted the existence of an entity 'space', in and through which is comprehended all experience of external nature; but while the space of Timaeus is invariably associated with matter and has no existence
apart from it, the space of Newton is mostly vacant and is entirely distinct from the matter that moves within

it. Timaeus's space is finite,f while Newton's is infinite. Moreover, Newton introduces the new concept of 'absolute position', i.e. he considers that the position of a body may be referred to 'absolute space', without reference to other bodies. Leibnitz vigorously criticized the Newtonian doctrine, and in the last year of his life (1716) expressed his own conviction thus: \
'1 hold space to be an order of coexistences, as time is an order of successions. For space denotes an order of things which exist at the same time, considered as existing together.' The peculiar excellence of this definition did not become fully manifest until the twentieth century, when the implications of the word coexistence were more closely examined. To say that two things are 'coexistent' means that they exist simultaneously. But as was first recognized in 1905^ if we consider two observers P and Q, who are in motion relative to each other, then it is possible that two events, which are witnessed by both of them, may be simultaneous according to P's system of reckoning, and yet not simultaneous according to Q's system of reckoning. Thus it follows from Leibnitz's definition that Pys instantaneous space is not identical with Q's instantaneous space: as is indeed the case, these spaces being different three-dimensional sections of four-dimensional
space-time." (Whittaker, From Euclid to Eddington)

19. "Since our minds are so constituted that they cannot conceive of material bodies in any other way than under the condition of placing them in space, and moreover since we cannot conceive of the absence of space—we cannot 'think it away'—although we can conceive of the absence or non-existence of any of the objects that may be met with" (Kant) (Whittaker, From Euclid to Eddington)

20. "In one English translation, Hilbert asks:

> "When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. ... But above all I wish to designate the following as the most important among the numerous questions which can be asked with regard to the axioms: To prove that they are not contradictory, that is, that a definite number of logical steps based upon them can never lead to contradictory results. In geometry, the proof of the compatibility of the axioms can be effected by constructing a suitable field of numbers, such that analogous relations between the numbers of this field correspond to the geometrical axioms. ... On the other hand a direct method is needed for the proof of the compatibility of the arithmetical axioms."[1]

It is now common to interpret Hilbert's second question as asking in particular for a proof that Peano arithmetic is consistent.

There are many known proofs that Peano arithmetic is consistent that can be carried out in strong systems such as Zermelo-Fraenkel set theory. These do not provide a resolution to Hilbert's second question, however, because someone who doubts the consistency of Peano arithmetic is unlikely to accept the axioms of set theory (which is much stronger) to prove its consistency. Thus a satisfactory answer to Hilbert's problem must be carried out using principles that would be acceptable to someone who does not already believe PA is consistent. Such principles are often called finitistic because they are completely constructive and do not presuppose a completed infinity of natural numbers. Gödel's incompleteness theorem places a severe limit on how weak a finitistic system can be while still proving the consistency of Peano arithmetic." (wikipedia) (http://en.wikipedia.org/wiki/Hilbert%27s_second_problem)

21. "...for they [the Pythagoreans] plainly say that when the one had been constructed, whether out of planes or of surface or of seed or of elements which they cannot express, immediately the nearest part of the unlimited began to be drawn in and limited by the limit. And Stobaues in his turn, quoting Aristotle's now lost book wholly devoted to the Pythagoreans: In the first book of his work "On the Philosophy of Pythagoras" he writes that the universe is one, and that from the unlimited there are drawn into it time, breath and the void, which always distinguishes the places of each thing. This inhalation of the unlimited was what, in Pythagorean thought, made it possible to describe the world in mathematical terms. Quoting Aristotle again, commenting on Philolaus' teachings: The Pythagoreans, too, held that void exists, and that it enters the heaven from the unlimited breath it, so to speak, breathes in void. The void distinguishes the natures of things, since it is the thing that separates and distinguishes the successive terms in a series. This happens in the first case of numbers; for the void distinguishes their nature."

"About nature and harmony this is the position. The being of the objects, being eternal, and nature itself admit of divine, not human, knowledge except that it was not possible for any of the things that exist and are known by us to have come into being, without there existing the being of those things from which the universe was composed, the limited and the unlimited. And since these principles existed being neither alike nor of the same kind, it would have been impossible for them to be ordered into a universe if harmony had not supervened in whatever manner this came into being. Things that were alike and of the same kind had no need of harmony, but those that were unlike and not of the same kind and of unequal order it was necessary for such things to have been locked together by harmony, if they are to be held together in an ordered universe."

"A musical scale presupposes an unlimited continuum of pitches, which must be limited in some way in order for a scale to arise. The crucial point is that not just any set of limiters will do. We cannot just pick pitches at random along the contin- uum and produce a scale that will be musically pleasing"

"This ordering of the material, as well as the title ”Elements,” suggest that Euclid wrote his work as an expansion of Plato’s ”Timaeus.” This said, Eu- clid mentions nothing about elements, creation, etc., in his work. It seems the implicit connection to Plato’s work was meant to be understood only by an edu- cated audience familiar with both works. Allthough a work primarily in logic and mathematics, it is clear that the geometry Euclid tried to formalize was the intuited geometry of physical space."

"A common misconception is that analytic geometry was invented by Descartes. At least it is a misconception if we by analytic geometry mean drawing perpendic- ular coordinate axes and choosing an interval to serve as unit, so as to establish a one-to-one correspondence between the points of the plane and ordered pairs of real numbers."

(http://www2.math.uu.se/~thomase/GeometryoverFields.pdf) (FROM GEOMETRY TO NUMBER The Arithmetic Field implicit in Geometry Johan Alm)

22. "These ... considerations, however plausible, cannot be considered a rigorous proof of .... This proof also cannot be derived from the ... axioms stated thus far. On the other hand, the ... is of very great importance for the entire mathematical .... Therefore, it has to be introduced as a special axiom, which, for reasons to be explained later, is called the ,,, axiom." Modified from Zakon Analysis Basics. about the completeness axiom
23. "It must be acknowledged that all the sciences are so closely inter- connected that it is much easier to learn them all together than to separate one from the other. If, therefore, someone seriously wishes to investigate the truth of things, he ought not to select one science in particular, . . .[h]e should, rather, consider simply how to increase the natural light of his reason, not with a view to solving this or that scholastic problem, but in order that his intellect should show his will what decision it ought to make in each of life's contingencies. He will soon be surprised to find that he has made far greater progress than those who devote themselves to particular studies, and that he has achieved not only everything that the specialists aim at but also goals far beyond any they can hope to reach." DesCartes (http://scholar.valpo.edu/cgi/viewcontent.cgi?article=1933&context=vulr) (Descartes Analytic Method and the Art of Geometric Imagineering)
24. "He notes that whenever two persons make opposite judgments about the same thing, the only thing certain is that at least one of them is mistaken, and it is unlikely that one of them has knowledge. It is likely that neither has knowledge because if the reasoning of one of the two were compelling, the other should be convinced of the certainty of the asserted conclusion." (DesCartes) (see above)
25. "[O]nce writers have . . . heedlessly taken up a position on some controversial question, they are generally inclined to employ the most subtle arguments in an attempt to get us to adopt their point of view. On the other hand, whenever they have the luck to discover something certain and evident, they always present it wrapped up in various obscurities, either because they fear that the simplicity of their argument may depreciate the importance of their finding, or because they begrudge us the plain truth." (same)
26. "Nobody, according to Leibniz, could follow a long reasoning without freeing the mind from the ‘effort of imagination’.22" (Leibniz) (Method versus calculus in Newton’s criticisms of Descartes and Leibniz)
27. "In book 1 of the Géométrie Descartes explained how one could translate a ge- ometric problem into an equation. Descartes was able to do so by a revolutionary departure from tradition. In fact he interpreted algebraic operations as closed opera- tions on segments. For instance, if a and b represent lengths of segments the product ab is not conceived by Descartes as representing an area but rather another length. As he wrote: ‘it must be observed that by a2, b3, and similar expressions, I ordinarily mean any simple lines’, while before the Géométrie such expressions represented an area and a volume respectively (see Figure 3)." (Method versus calculus in Newton’s criticisms of Descartes and Leibniz)
28. "Fermat had observed that the tangent at a point *P* on a curve was determined if one other point besides *P* on it were known; hence, if the length of the subtangent *MT' could be found (thus determining the point* T*), then the line* TP *would be the required tangent. Now Barrow remarked that if the abscissa and ordinate at a point* Q *adjacent to* P *were drawn, he got a small* *triangle* PQR *(which he called the* differential triangle, because its sides *PR* and *PQ* were the differences of the abscissae and ordinates of *P* and *Q*)," (http://en.wikipedia.org/wiki/Isaac_Barrow)
29. "He illustrates this by the parabola, in which case m = 2. He states, but does not prove, the corresponding result for a curve of the form y = xp/q.
Wallis showed considerable ingenuity in reducing the equations of curves to the forms given above, but, as he was unacquainted with the binomial theorem, he could not effect the quadrature of the circle, whose equation is y = \sqrt{1 - x^2}, since he was unable to expand this in powers of x. He laid down, however, the principle of interpolation. Thus, as the ordinate of the circle y = \sqrt{1 - x^2} is the geometrical mean between the ordinates of the curves y = (1 - x^2)^0 and y = (1 - x^2)^1, it might be supposed that, as an approximation, the area of the semicircle \int_{0}^{1} \sqrt{1 - x^2}\, dx which is \begin{matrix} \frac{1}{4}

\end{matrix} \pi might be taken as the geometrical mean between the values of \int_{0}^{1} (1 - x^2)^0 \, dx\text{ and }\int_{0}^{1} (1 - x^2)^1 \, dx that is, 1 and \begin{matrix} \frac{2}{3} \end{matrix}; this is equivalent to taking 4 \sqrt{\begin{matrix} \frac{2}{3} \end{matrix}} or 3.26... as the value of π. But, Wallis argued, we have in fact a series 1, \begin{matrix} \frac{1}{6} \end{matrix}, \begin{matrix} \frac{1}{30} \end{matrix}, \begin{matrix} \frac{1}{140} \end{matrix},... and therefore the term interpolated between 1 and \begin{matrix} \frac{1}{6} \end{matrix} ought to be chosen so as to obey the law of this series[clarification needed]. This, by an elaborate method that is not described here in detail, leads to a value for the interpolated term which is equivalent to taking \frac{\pi}{2} = \frac21\cdot\frac23\cdot\frac43\cdot\frac45\cdot\frac65\cdot\frac67\cdots (which is now known as the Wallis product). In this work also the formation and properties of continued fractions are discussed, the subject having been brought into prominence by Brouncker's use of these fractions."
"The theory of the collision of bodies was propounded by the Royal Society in 1668 for the consideration of mathematicians. Wallis, Christopher Wren, and Christian Huygens sent correct and similar solutions"
"He is also credited with introducing the symbol {\infty} for infinity. He similarly used \frac{1}{\infty} for an infinitesimal"
(http://en.wikipedia.org/wiki/John_Wallis#Integral_calculus)

30. "The resolution of the equation is not, however, the solution of the problem. In fact, the solution of the problem must be a geometrical construction of the sought magnitude in terms of legitimate geometrical operations performed on the givens (‘Q.E.F.’!). We now have to move from algebra back to geometry again. Descartes understood this process from algebra to geometry as follows: the real roots of the equation (for him if there are no real roots, then the problem admits no solution) must be geometrically constructed. After Descartes, this process was known as the ‘construction of the equation’. This is where the synthetic, compositive part of the whole process begins. Descartes accepted from tradition the idea that such constructions must be per- formed by intersection of curves. That is to say, the real roots are geometrically represented by segments, and such segments are to be constructed by intersection of curves. As a matter of fact, the construction of the equation presented the geometer with a new problem: not always an easy one. One had to choose two curves, position and scale them, such that their intersections determine points from which segments – whose lengths geometrically represent the roots of the equation – can be drawn (see Figure 4).
The synthetic part of Descartes’ process of problem-solving gave rise to two ques- tions: which curves are admissible in the construction of equations? which curves,
among the admissible, are to be preferred in terms of simplicity? In asking him- self these questions Descartes was continuing a long debate concerning the role and classification of curves in the solution of problems. A tradition that, once again, stems from Pappus, and the interpretations of Pappus given by mathematicians such as Viète, Ghetaldi, Kepler, and Fermat. His answer was that only ‘geometrical curves’ (we would say ‘algebraic curves’) are admissible in the construction of the roots of equations and that one has to choose the curves of the lowest possible degree as these are the simplest. Descartes instead excluded ‘mechanical curves’ (we would say transcendental curves) as legitimate tools of construction."
(Method versus calculus in Newton’s criticisms of Descartes and Leibniz)
31. "To be sure, their [the Ancients’] method is more elegant by far than the Carte- sian one. For he [Descartes] achieved the result by an algebraic calculus which, when transposed into words (following the practice of the Ancients in their writings), would prove to be so tedious and entangled as to provoke nausea, nor might it be understood. But they accomplished it by certain simple propositions, judging that nothing written in a different style was worthy to be read, and in consequence concealing the analysis by which they found their constructions.12" (Newton) according to (Method versus calculus in Newton’s criticisms of Descartes and Leibniz)
32. "Newton was not alone in his battle against the algebraists. Similar statements can be found in the polemic works of Thomas Hobbes. But probably the deepest influence on Newton in this matter was played by his mentor Isaac Barrow. Newton’s quest for the ancient, non-algebraical, porismatic analysis led him to develop an interest in projective geometry (see Figure 5).
He convinced himself that the ancients had used projective properties of conic sections in order to achieve their results. Moving along these lines he classified cubics into five projective classes.13" (Same)
33. "Leibniz was thus praising the calculus as a *cogitatio caeca* and promoted the ‘blind use of reasoning’ among his disciples. Nobody, according to Leibniz, could follow a long reasoning without freeing the mind from the ‘effort of imagination’.
Leibniz conceived of himself as the promoter of new methods of reasoning, rather than‘just’ a mathematician. The calculus was just one successful example of the power of algorithmic thinking. The German diplomat was interested in promoting in Europe the formation of a group of intellectuals who could extend a universal knowledge achieved thanks to a new algorithm that he termed *universal characteristic*. He thus helped to form a school of mathematicians who distinguished themselves by their ability in handling the differentials and the integrals and by their innovative publication strategy. Thanks to Leibniz’s recommendation, they colonized chairs of mathematics all over Europe. The efficacy of this new algorithm was affirmed to be independent from metaphysical or cosmological questions. The persons who practised it

had to be professional mathematicians, rather than 'geometrical philosophers', able to teach and propagate knowledge of calculus." (Same)

34. "Figure 10. A portrait of Newton in old age (Source: [1], 831). He proudly opens the Principia at a page devoted to the attraction of extended bodies. In dealing with this problem Newton made recourse to his 'inverse method of fluxions' (the equivalent of Leibniz's integral calculus) which allowed him to 'square curves'. As a matter of fact, only by making recourse to his tables of curves ('integral tables'), see Figure 8, could Newton solve several problems in the Principia. Such analytic methods were not, however, made explicit to the reader. In the polemic with the Leibnizians – who claimed that absence of calculus from the Principia was proof positive of Newton's ignorance of quadrature techniques prior to 1687 – Newton was forced to maintain, with some exaggeration, that 'By the help of this new Analysis Mr Newton found out most of the Propositions in his Principia Philosophiae. But because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically that the systeme of the heavens might be founded upon good Geometry. And this makes it now difficult for unskillful men to see the Analysis by wch those Propositions were found out.' ([9], vol. 8, 599). On the issue of Newton's use of analytic methods in the Principia see [16]." (same)
35. "There is not only mathematics in this story, of course. Leibniz had to be opposed for a series of reasons that have to do with the Hannoverian succession. The German, in fact, who was employed by the Hannover family, wished to move to London as Royal Historian. The idea of having in England such a towering intellectual who was defending a philosophical view which contradicted Newton's voluntarist theology and who was promoting the unification of the Christian Churches was anathema for Newton and his supporters." (Same)
36. "For our purposes, it is interesting to turn to some passages that Newton penned in 1715 contained in an anonymous 'Account' to a collection of letters, the Commer- cium epistolicum, that the Royal Society produced in order to demonstrate Leibniz's plagiarism. In the 'Account', speaking of himself in the third person, " (Same).
37. "Hadamard ´(1945) further argued that even if Poincare's breakthrough was a result of chance alone, chance alone was insufficient to explain the considerable body of creative work credited´
to Poincare in almost every area of mathematics. The question then is how does (psychological) chance work?
The author conjectures that the mind throws out frag-ments (ideas) which are products of past experience. combined in a meaningful way. For example, if one reads a complicated proof consisting of a thousand steps, a thousand random fragments may not be enough to con-struct a meaningful proof. However the mind chooses relevant fragments from these random fragments and links
them into something meaningful. Wedderburn's theorem, that a finite division ring is a field is one instance of a unification of apparently random fragments because the proof involves algebra, complex analysis and number theory."
(The characteristics of mathematical creativity Bharath Sriraman (5.4 Incubation and illumination)
38. "The use of a single letter A to represent a matrix was crucial to the development of matrix algebra. Early in the development the formula det(AB) = det(A)det(B) provided a connection between matrix algebra and determinants. Cayley wrote "There would be many things to say about this theory of matrices which should, it seems to me, precede the theory of determinants."
(http://darkwing.uoregon.edu/~vitulli/441.sp04/LinAlgHistory.html)
39. "Vladimir Radzivilovsky: The brain is like the stomach: it can digest only stuff which is already inside."
40. "Leibnitz in effect says that first of all he prefers to write 10 for a, 11 for b, and so on; that, having done this, he can all the more readily take the next step, viz., forming every possible product whose factors are one coefficient from each equation,* the result being 10.21.32, 10.22.31, 11.20.32, 11.22.30, 12.20.31, 12.21.30; and thatj then, one being the number which is less by one than the number of unknowns, he makes those terms different in sign which have only one factor in common.
The last of these is manifestly the least satisfactory. In the first place, part of it is awkwardly stated. Making those terms different in sign ivhich have only as many factors alike a is indicated by the number which is less by one than the number of unknown quantities is exactly the same as making those terms different in sign which have only two f(jbctor different. Secondly, in form it is very unpractical. The only methodical way of putting it in use is to select a term and make it positive ; then seek out a second term, having all its factors except two the same as those of the first term, and make this second term negative ; then seek out a third term, having all its factors except two the same as those of the second term, and make this third term positive ; and so on.
" (The theory of determinants in the historical order of development. Thomas Muir"
41. "However, a very general rule when defining mathematical structures is that if a definition splits into parts, then the definition as a whole will not be interesting unless those parts interact" (The Princeton companion to mathematics page 20).
42. "If you have a general statement about structures of a given type and want to know whether is it true. then it is very helpful if you can test if in a wide range of particular cases.

If it passes all the tests, then you have some evidence in favor of the statement. If you are lucky, you may even be able to see why it is true; alternatively, you may find that the statement is true for each example you try, but always for reasons that depend on the particular features of the example you are examining.
Then you will know that you should try to avoid these features if you want to find a counterexample. If you do find a counterexample, then the general statement is false, but it may still happen that a modification to the statement is true and useful.
In that case, the counterexample will help you to find an appropriate modification" (The Princeton companion to mathematics page 23).

43. "Up to about the end of the 18th century, algebra consisted (in large part) of the study of solutions of polynomial equations. In the 20th century, algebra became a study of abstract, axiomatic systems. The transition from the so-called classical algebra of polynomial equations to the so-called modern algebra of axiomatic systems occurred in the 19th century." (The Evolution of Group Theory: A Brief Survey)
44. "The major problems in algebra at the time (1770) that Lagrange wrote his fundamental memoir “Réflexions sur la résolution algébrique des équations” concerned polynomial equations. There were “theoretical” questions dealing with the existence and nature of the roots (e.g., Does every equation have a root? How many roots are there? Are they real, complex, positive, negative?), and “practical” questions dealing with methods for finding` the roots. In the latter instance there were exact methods and approximate methods. In what follows we mention exact methods.

    The Babylonians knew how to solve quadratic equations (essentially by the method of completing the square) around 1600 B.C. Algebraic methods for solving the cubic and the quartic were given around 1540. One of the major problems for the next two centuries was the algebraic solution of the quintic. This is the task Lagrange set for himself in his paper of 1770.

    In his paper Lagrange first analyzes the Various known methods (devised by F. Víète, R. Descartes, L. Euler, and E, Bézout) for solving cubic and quartíc equations. He shows that the common feature of these methods is the reduction of such equations to auxiliary equations-the socalled resolvent equations. The latter are one degree lower than the original equations. Next Lagrange attempts 21 similar analysis of polynomial equations of arbitrary degree 11. With each such equation he associates a “resolvent equation” as follows: let f (x) be the original equation, with roots xl, Pick a rationál function R(x1, xg, . . . , of the roots and coefficients of (Lagrange describes methods for doing so.) Consider the different values which R(x17 assumes under all the n! permutations of the roots of f(x). If these are denoted by then the resolvent equation is given by g(x) (x - yz) » (x - yk). (Lagrange shows that k divides n !-the source of what we call Lagrange’s
    theorem in group theory.) For example, if f(x) is a quartic with roots then R(x1, , X3, X4) may he taken to be XIX2 X3 X4, and this function assumes three distinct values under the 24 permutations of X1, X2, X3, X4. Thus the resolvent equation of a quartic is a cubic. However, in carrying over this analysis to the quintic, he finds that the resolvent equation is of
    degree six!

    Although Lagrange did not succeed in resolving the problem of the algebraic solvability of the quintic, his Work was a milestone. It was the first time that an association was made between the solutions of a polynomial equation and the permutations of its roots. In fact, the study of the permutations of the roots of an equation was a cornerstone of Lagrange’s general theory of algebraic equations. This, he speculated, formed “the true principles for the solution of equations.”
    (He was, of course, vindicated in this by E. Galois.) Although Lagrange speaks of permutations without considering a “calculus” of permutations (e.g., there is no consideration of their composition or closure), it can be said that the germ of the group concept (as a group of permutations) is present in his work, For details see [12], [16], [19], [25], [33]." (The Evolution of Group Theory: A Brief Survey)
45. "The problem of representing integers by binary quadratic formsŕgoes back to Fermat in the early century. (Recall his theorem that every prime of the form 4n 1 can be represented as a sum of two squares x2 y2.) Gauss devotes a large part of the Disquísitiones to an exhaustive study of binary quadratic forms and the representation of integers by such forms." (The Evolution of Group Theory: A Brief Survey)
46. "A quadratic form representing all of the positive integers is sometimes called *universal*. Lagrange's four-square theorem shows that $w^2 + x^2 + y^2 + z^2$ is universal. Ramanujan generalized this to $aw^2 + bx^2 + cy^2 + dz^2$ and found 54 {*a*,*b*,*c*,*d*} such that it can generate all positive integers, namely,

    {1,1,1,*d*}, 1 ≤ *d* ≤ 7
    {1,1,2,*d*}, 2 ≤ *d* ≤ 14
    {1,1,3,*d*}, 3 ≤ *d* ≤ 6
    {1,2,2,*d*}, 2 ≤ *d* ≤ 7
    {1,2,3,*d*}, 3 ≤ *d* ≤ 10

{1,2,4,*d*}, 4 ≤ *d* ≤ 14
{1,2,5,*d*}, 6 ≤ *d* ≤ 10

There are also forms that can express nearly all positive integers except one, such as {1,2,5,5} which has 15 as the exception. Recently, the 15 and 290 theorems have completely characterized universal integral quadratic forms: if all coefficients are integers, then it represents all positive integers if and only if it represents all integers up through 290; if it has an integral matrix, it represents all positive integers if and only if it represents all integers up through 15." (http://en.wikipedia.org/wiki/Quadratic_form#Universal_quadratic_forms)

47. "The 19th century witnessed an explosive growth in geometry, both in scope and in depth. New geometries emerged: projective geometry, noneuclidean geometries, differential geometry, alge- braic geometry, n-dimensional geometry, and Grassmann's geometry of extension. Various geometric methods competed for supremacy: the synthetic versus the analytic, the metric versus the projective. At mid-century, a major problem had arisen, namely, the classification of the relations and inner connections among the different geometries and geometric methods. This gave rise to the study of "geometric relations," focusing on the study of properties of figures invariant under transformations. Soon the focus shifted to a study of the transformations themselves. Thus the study of the geometric relations of figures became the study of the associated transformations. Various types of transformations (e.g., collineations, circular transformations, inversive transfor- mations, affinities) became the objects of specialized studies. Subsequently, the logical connections among transformations were investigated, and this led to the problem of classifying transforma- tions and eventually to Klein's grouptheoretic synthesis of geometry. Klein's use of groups in geometry was the final stage in bringing order to geometry, " (The Evolution of Group Theory: A Brief Survey)
48. "In 1874 Lie introduced his general theory of (continuous) transformation groups-essentially what we call Lie groups today. Such a group is represented by the transformations where the fIl are analytic functions in the xl- and ai (the are parameters, with both xl- and aIA real or complex). For example, the transformations given by x' = f(x1,...,xn,a1,...an) , e.g: z'= ax + b / cx + d where cz, b, c, d, are real numbers and ad - bc != 0, define a continuous transformation group. Lie thought of himself as the successorI of N. H. Abel and Galois, doing for differential equations what they had done for algebraic equations. His work was inspired by the observation that almost all the differential equations which had been integrated by the older methods remain invariant under continuous groups that can be easily constructed. He was then led to consider, in general, differential equations that remain invariant under a given continuous group and to investigate the possible simplifications in these equations which result from the known properties of the given group (cf. Galois theory). Although Lie did not succeed in the actual formulation of a "Galois theory of differential equations," his Work was fundamental in the subsequent formu- lation of such a theory by E. Picard (1883 / 1887) and E. Vessiot (1892). Poincaré and Klein began their work on "autornorphic functions" and the groups associated with them around 1876. Autornorphic functions (which are generalizations of the circular, hyperbolic, elliptic, and other functions of elementary analysis) are functions of a complex variable Z, analytic in some domain D, which are invariant under the group of transformations z'= ax + b / cx + d (a, b, C, d real or complex and ad » bc 0) or under some subgroup of this group, Moreover, the group in question must be "discontinuous" (i.c., any compact domain contains only finitcly many transforms of any point). Examples of such groups are the modular group (in which a, b, C, d are integers and ad » bc = 1), which is associated with the elliptic modular functions, and Fuchsian groups (in which a, b, C, d are real and ad - bc = 1) associated with the Fuchsian automorphic functions. As in the case of Klein's Erlangen Program, we will explore the consequences of these works for group theory in section 2-(c)." (The Evolution of Group Theory: A Brief Survey)
49. "The crowning achievement of these two lines of development-a symphony on the grand themes of Galois and Cauchy-was Iordan's important and influential T des substitutions et des équations algébriques of 1870. Although the author states in the preface that "the aim of the work is to develop Galois' method and to make it a proper field of study, by showing with what facility it can solve all principal problems of the theory of equations," it is in fact group theory per se-not as an offshoot of the theory of solvability of equations~which forms the central object of study. The striving for a mathematical synthesis based on key ideas is a striking characteristic of J0rdan's work as well as that of a number of other mathematicians of the peńod (e.g., F, Klein) The Concept of a (permutation) group seemed to Jordan to provide such a key idea. His approach enabled him to give a unified presentation of results due to Galois, Cauchy, and others. His application of the group concept to the theory of equations, algebraic geometry, transcendental functions, and theoretical mechanics was also part of the unifying and synthesizing theme. "In his book Jordan wandered through all of algebraic geometry, number theory, and function theory in search of interesting permutation groups" (Klein, [20]). In fact, the aim was a survey of all of mathematics by areas in which the theory of permutation groups had been applied or seemed likely to be applicable. "The work represents. . . a review of the whole of contemporary mathe- matics from the standpoint of the occurrence of group-theoretic thinking in permutation-theoretic form" (Wussing, [33]). The Traité embodied the substance of most of Iordan's publications on groups up to that time (he wrote over 30 articles on groups during the

period 18604880) and directed attention to a large number of difficult problems, introducing many fundamental concepts." (The Evolution of Group Theory: A Brief Survey)

50. "He stated that what Galois theory and his own program have in common is the investigation of “groups of changes,” but added that “to be sure, the objects the changes apply to are different: there [Galois theory] one deals with a finite number of discrete elements, whereas here one deals with an infinite number of elements of a continuous manifold” [33, p. 191]. To continue the analogy, Klein notes that just as there is a theory of permutation groups, “ we insist on a theory of transformations, a study of groups generated by transformations of a given type” [33, p. 191]." (The Evolution of Group Theory: A Brief Survey)
51. "As M. Kline put it in his inimitable way [21]: “Premature abstraction falls on deaf ears, whether they belong to mathematicians or to students.” (The Evolution of Group Theory: A Brief Survey)
52. "In particular, Klein, one of the major contributors to the development of group theory, thought that the “abstract formulation is excellent for the working out of proofs but it does not help one find new ideas and methods,” adding that “in general, the disadvantage of the [abstract] method is that it fails to encourage thought” [33, p. 228]." (The Evolution of Group Theory: A Brief Survey)
53. "For fifteen days I strove to prove that there could not be any functions like those I have since called Fuchsian functions. I was then very ignorant; every day I seated myself at my work table, stayed an hour or two, tried a great number of combinations and reached no results. One evening, contrary to my custom, I drank black coffee and could not sleep. Ideas rose in crowds; I felt them collide until pairs interlocked, so to speak, making a stable combination. By the next morning I had established the existence of a class of Fuchsian functions, those which come from the hypergeometric series; I had only to write out the results, which took but a few hours." (Pointcare)
54. "Henri Poincaré est souvent présenté comme l’un des derniers mathématiciens universels. Ses travaux concernent en effet des domaines variés des sciences mathématiques.1 Cette variété thématique a souvent été mise au crédit des capacités individuelles exceptionnelles de Poincaré. Elle peut cependant égale- ment s’analyser comme témoignant d’organisations mathématiques antérieures aux découpages disciplinaires qui nous sont aujourd’hui familiers. De fait, le savant était contemporain de la montée en puissance de disciplines centrées sur des objets (groupes finis, groupes continus, algèbres associatives etc.) face aux "branches" en lesquelles se divisaient traditionnellement l’organisation des sciences mathématiques en France (Analyse, Géométrie, Mécanique, Physique mathématique etc.)." (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)
55. "Nous proposons dans cet article d’analyser les pratiques algébriques qui se présentent dans la première décennie de travaux de Poincaré. Ces pratiques sont mobilisées au sein de travaux variés, de l’arithmétique des formes quadratiques à la mécanique céleste en passant par les fonctions fuchsiennes. Plus encore, ces pratiques supportent souvent les liens qui sont établis entre ces différents do- maines. Elles témoignent ainsi d’une organisation mathématique dans laquelle l’algèbre joue un rôle transversal à différents domaines mais d’une manière diffé- rente du rôle qu’ont jouées ultérieurement les structures algébriques abstraites."
56. "Les Mathématiques sont les sciences des quantités. Elles se divisent en plusieurs branches, suivant la nature des grandeurs soumises au calcul. On y distingue principalement l’Arithmétique, la Géométrie, la Mécanique, la Physique mathématique, le Calcul des probabilités. Ces diverses branches ont pour lien commun l’Algèbre, qu’on pour- rait définir [comme] le calcul des opérations.[Jordan, 1882a, p.1]."
57. "Soulignons dès à présent quelques caractéristiques importantes de la note de 1884 : – La référence aux matrices de Sylvester – Le rôle joué par l’équation caractéristique – Le lien entre le problème de la non commutativité e tl’occurrence de racines caractéristiques multiples – Les rôles joués par les Tableaux comme moyen d’expression des résultats ainsi que comme procédé opératoire de décomposition des variables et de leurs indices afin de réduire les substitutions à leurs formes canoniques – La tension entre la généralité revendiquée pour des résultats sur n para- mètres et leurs présentations pour 3 à 5 paramètres."
58. "I have in previous papers defined a "Matrix" as a rectangular array of terms, out of which different systems of determinants may be engendered, as from the womb of a common parent ; these cognate determinants being by no means isolated in their relations to one another, but subject to certain simple laws of mutual dependence and simultaneous deperition. [Sylvester, 1851, p.296].
Cette définition d’une matrice comme mère des mineurs d’un déterminant a bé- néficié d’une circulation large sur le continent européen dès les années 1850. Dans ce contexte, le terme matrice a toujours été associé à des procédés d’extractions de mineurs et aux problèmes spécifiques posés par l’occurrence de racines carac- téristiques multiples. Les rares emplois de ce terme par Poincaré sont bien re- présentatifs de cet usage des matrices [Poincaré, 1884a, p.334], [Poincaré, 1884f, p.420]. Jusqu’au début des années 1880, les matrices n’ont au contraire que ra- rement été envisagées comme des nombres complexes. Poincaré, par exemple, se réfère plutôt à la notion de clef algébrique de Cauchy lorsqu’il utilise un calcul symbolique des transformations sur des systèmes de vecteurs au sens de Grassmann.[Poincaré, 1886d, p.101]."
59. "The argument, often heard, that the scientist is little concerned with the history of the concepts which he applies in his work, has lost much of its strength in view of the importance that modern physics attaches to concept-formation. Once held as a monopoly of antiquarian historians of science and of pedantic epistemolo- gists, the problem of concept-formation in scientific theory gained vital importance and notable prominence in modern research. The study of the historical aspects of concept-formation in physical

science is admittedly no easy task. In addition to a thorough historical and philological training, necessary for a skillful command of the source material, it requires comprehen- sion of physical theory, instrumental for the critical comparison and interpretation of the sources under discussion and indispen-sable for the evaluation of their significance for ssience as a whole. A serious difficulty in the study of the development of a scien- tific concept lies in the necessarily inherent vagueness of its defi- nition. This complication arises from the fact that the concept in question finds its strict specification only through its exact defini- tion in science. This definition, however, historically viewed, is a rather late and advanced stage in its development. To limit the discussion to the concept thus defined means to ignore a major part of its life history. The history of a concept has not yet run its course, it is true, even once it has achieved such a "defined" position, since it attains its complete meaning only through the ever-increasing and changing context of the conceptual structure in which it is placed. However, from the standpoint of the history of ideas, the most interesting and important part of its biography is passed, namely the period of its vigor and creative contribution to the advancement of scientific thought. When studying the de- velopment of a scientific concept one has, therefore, to cope with an essential vagueness of definition of the subject under discussion and one faces equally the danger of either drawing the limits too narrow or too wide." (Concepts of Force, Max Jammer)

60. "As a result of modern research in physics, the ambition and hope, still cherished by most authorities of the last century, that physical science could offer a photographic picture and true image of reality had to be abandoned. Science, as understood today, has a more restricted objective: its two major assignments are the description of certain phenomena in the world of experience and the establishment of general principles for their prediction and what might be called their "explanation." "Explanation" here means essentially their subsumption under these principles. For the efficient achievement of these two objectives science employs a conceptual apparatus, that is, a system of concepts and theories that represent or symbolize the data of sense experience, as pres- sures, colors, tones, odors, and their possible interrelations. This conceptual apparatus consists of two parts: (i) a system of concepts, definitions, axioms, and theorems, forming a hypo- thetico-deductive system, as exemplified in mathematics by Euclid- ean geometry; (2) a set of relations linking certain concepts of the hypothetico-deductive system with certain data of sensory experience. With the aid of these relations, which may be called "rules of interpretation" or "epistemic correlations," x an associ- ation is set up, for instance, between a black patch on a photo- graphic plate (a sensory impression) and a spectral line of a certain wavelength (a conceptual element or construct2 of the hypothetico-deductive system), or between the click of an ampli- fier coupled to a Geiger counter and the passage of an electron. The necessity for physical science of possessing both parts as con- stituents results from its status as a theoretical system of propo- sitions about empirical phenomena. A hypothetico-deductive sys- tem without rules of interpretation degenerates into a speculative calculus incapable of being tested or verified; a system of epistemic correlations without a theoretical superstructure of a deductive system remains a sterile record of observational facts, devoid of any predictive or explanatory power. The adoption of rules of interpretation introduces, to some extent, an arbitrariness in the construction of the system as a whole by allowing for certain predilections in the choice of the concepts to be employed. In other words, arbitrary modifications in the formation of the conceptual counterparts to given sensory impressions can be compensated by appropriate changes in the epistemic correlations without necessarily destroying the corre- spondence with physical reality. In consequence of this arbitrari-ness, scientific concepts "are free creations of the human mind and are not, however it may seem, uniquely determined by the external world."3 When science attempts to construct a logically consistent sys- tem of thought corresponding to the chaotic diversity of sense experience, the selection of concepts as fundamental is not un- ambiguously determined by their suitability to form a basis for the derivation of observable facts. In the first place, some element of contingency is introduced by the somehow fortuitous sequence of experimentation and observation, an idea recently emphasized by James Bryant Conant: "It seems clear that the development of our modern scientific ideas might have taken a somewhat dif- ferent course, if the chronological sequence of certain experimen- tal findings had been different. And to some degree, at least, this chronology can be regarded as purely accidental." * In the second place, a certain climate of opinion, conditioned by subconscious motives, is responsible to some extent for the specific character of the basic conceptions or primitive concepts. It is a major task of the historian of science to study this climate of opinion pre- vailing at a certain period and to expose the extrascientific ele- ments responsible for the finally accepted choice of those con- cepts that were to play a fundamental role in the construction of the contemporaneous conceptual apparatus. The history of sci- ence can often show in retrospect how alternative concepts have, or could have, been employed at the various stages in the devel- opment of the physical sciences in a provisionally satisfactory manner." (Concepts of Force, Max Jammer)
61. "Physics theories of the late 19th century assumed that just as surface water waves must have an intervening substance, i.e. a "medium", to move across (in this case water), and audible sound requires a medium to transmit its wave motions (such as air or water), so light must also require a medium, the "luminiferous aether", to transmit its wave motions. Because light can travel through a vacuum, it was assumed that even a vacuum must be filled with aether. Since the speed of light is so great, and as material bodies pass through the aether without obvious friction or drag, the aether was assumed to have a highly unusual combination of properties. Designing experiments to test the properties of the aether was a high priority of 19th century physics.[A 4]:411ff Earth orbits around the Sun at a speed of around 30 km/s (18.75

mi/s) or over 108,000 km/hr (67,500 mi/hr).
The Sun itself is travelling about the Galactic Center at an even greater speed, and there are other motions at higher levels of the structure of the universe. Since the Earth is in motion, two main possibilities were considered: (1) The aether is stationary and only partially dragged by Earth (proposed by Augustin-Jean Fresnel in 1818), or (2) the aether is completely dragged by Earth and thus shares its motion at Earth's surface (proposed by George Gabriel Stokes in 1844).[A 5] In addition, James Clerk Maxwell (1865) recognized the electromagnetic nature of light and developed what are now called Maxwell's equations, but these equations were still interpreted as describing the motion of waves through an aether, whose state of motion was unknown. Eventually, Fresnel's idea of an (almost) stationary aether was preferred because it appeared to be confirmed by the Fizeau experiment (1851) and the aberration of light.[A 5]
According to this hypothesis, Earth and the aether are in relative motion, implying that a so-called "aether wind" (Fig. 2) should exist. Although it would be possible, in theory, for the Earth's motion to match that of the aether at one moment in time, it was not possible for the Earth to remain at rest with respect to the aether at all times, because of the variation in both the direction and the speed of the motion. At any given point on the Earth's surface, the magnitude and direction of the wind would vary with time of day and season. By analysing the return speed of light in different directions at various different times, it was thought to be possible to measure the motion of the Earth relative to the aether. The expected relative difference in the measured speed of light was quite small, given that the velocity of the Earth in its orbit around the Sun was about one hundredth of one percent of the speed of light.[A 4]:417ff
During the mid-19th century it was thought that it should be possible to measure aether wind effects of first order, i.e. effects proportional to v/c (v being Earth's velocity, c the speed of light). However, it was clear that a direct measurement of the speed of light was not possible with the accuracy required. For instance, the Fizeau–Foucault apparatus could measure the speed of light to perhaps 5% accuracy, which was totally inadequate for measuring the direct, first order 0.01% change in the speed of light. Therefore, a number of physicists attempted to make measurements of indirect first order effects of light speed variations (see First order aether-drift experiments). For instance, the Hoek experiment intended to detect interferometric fringe shifts due to speed differences of oppositely propagating light waves through resting water. But the results were all negative.[A 6] This could be explained by using Fresnel's dragging coefficient, according to which the aether and thus light is partially dragged by moving matter. Partial aether dragging would cancel out the ability to measure any first order change in the speed of light.
As pointed out by Maxwell (1878), only experimental arrangements capable of measuring second order effects would have any hope of detecting aether drift, i.e. effects proportional to v2/c2.[A 7][A 8] However, the required instrumental sensitivity was simply too great for existing experimental setups.
Michelson had a solution to the problem of how to construct a device sufficiently accurate to detect aether flow. While teaching at his alma mater in 1877, the United States Naval Academy in Annapolis, Michelson conducted his first known light speed experiments as a part of a classroom demonstration. He left active U.S. Naval service in 1881 while he was in Germany concluding studies. In that year, Michelson used a prototype experimental device to make several more measurements. The device he designed, later known as a Michelson interferometer, sent yellow light from a sodium flame (for alignment), or white light (for the actual observations), through a half-silvered mirror that was used to split it into two beams travelling at right angles to one another. After leaving the splitter, the beams travelled out to the ends of long arms where they were reflected back into the middle by small mirrors. They then recombined on the far side of the splitter in an eyepiece, producing a pattern of constructive and destructive interference whose transverse displacement would depend on the relative time it takes light to transit the longitudinal vs. the transverse arms. If the Earth is traveling through an aether medium, a beam reflecting back and forth parallel to the flow of aether would take longer than a beam reflecting perpendicular to the aether because the time gained from traveling downwind is less than that lost traveling upwind. Michelson expected that the Earth's motion would produce a fringe shift equal to 4% of the fringe separation. Michelson did not observe the expected 0.04 fringe shift; the greatest average deviation that he measured (in the northwest direction) was only 0.018 fringes, while most of his measurements were much less. His conclusion was to rule out Fresnel's hypothesis of a stationary aether with partial aether dragging, confirming Stokes' hypothesis of complete aether dragging.[4] However, Alfred Potier (and later Hendrik Lorentz) pointed out to Michelson that he had made an error of calculation, and that the expected fringe shift should only have been 0.02 fringes. Thus, Michelson's apparatus had experimental errors far too large to say anything conclusive about the aether wind. For a definitive measurement of the aether wind, a much more accurate and tightly controlled experiment would have to be carried out. However, the prototype was successful in demonstrating that the basic method was feasible.[A 5][A 9]"
(http://en.wikipedia.org/wiki/Michelson%E2%80%93Morley_experiment)

62. "Most famous "failed" experiment Figure 7. Michelson and Morley's results. The upper solid line is the curve for their observations at noon, and the lower solid line is that for their evening observations. Note that the theoretical curves and the observed curves are not plotted at the same scale: the dotted curves, in fact, represent only one-eighth of the theoretical displacements. After all this thought and preparation, the experiment became what has been called the most famous failed experiment in history.[A 13] Instead of providing insight into the properties of the aether, Michelson and Morley's article in the American Journal of Science reported the measurement to be as small as one-fortieth of the expected displacement (see Fig. 7),

but "since the displacement is proportional to the square of the velocity" they concluded that the measured velocity was "probably less than one-sixth" of the expected velocity of the Earth's motion in orbit and "certainly less than one-fourth."[1] Although this small "velocity" was measured, it was considered far too small to be used as evidence of speed relative to the aether, and it was understood to be within the range of an experimental error that would allow the speed to actually be zero.[A 1] (Afterward, Michelson and Morley ceased their aether drift measurements and started to use their newly developed technique to establish the wavelength of light as a standard of length.[6][7])
From the standpoint of the then current aether models, the experimental results were conflicting. The Fizeau experiment and its 1886 repetition by Michelson and Morley apparently confirmed the stationary aether with partial aether dragging, and refuted complete aether dragging. On the other hand, the much more precise Michelson–Morley experiment (1887) apparently confirmed complete aether dragging and refuted the stationary aether.[A 5] In addition, the Michelson-Morley null result was further substantiated by the null results of other second-order experiments of different kind, namely the Trouton–Noble experiment (1903) and the Experiments of Rayleigh and Brace (1902–1904). These problems and their solution led to the development of the Lorentz transformation and special relativity (see Fallout)."
(http://en.wikipedia.org/wiki/Michelson%E2%80%93Morley_experiment)

63. "A well-known example is the Michelson-Morley experiment which revealed the independence of the velocity of light with respect to the motion of the earth, a phenomenon un- accounted for and inconsistent with the existing ether theory at the turn of the last century; this effect could have been fitted into the conceptual scheme of that time by certain ad hoc as- sumptions (the Lorentz contraction) which would, however, lead to serious complications and violate the principle of simplicity. Einstein's ingenious reinterpretations of space and time, ex- pounded in his special theory of relativity, are essentially a revi- sion of the conceptual apparatus of classical mechanics." (Concepts of Force, Max Jammer)
64. "Miller worked on increasingly larger interferometers, culminating in one with a 32 m (effective) arm length that he tried at various sites including on top of a mountain at the Mount Wilson observatory. To avoid the possibility of the aether wind being blocked by solid walls, his mountaintop observations used a special shed with thin walls, mainly of canvas. From noisy, irregular data, he consistently extracted a small positive signal that varied with each rotation of the device, with the sidereal day, and on a yearly basis. His measurements in the 1920s amounted to approximately 10 km/s instead of the nearly 30 km/s expected from the Earth's orbital motion alone. He remained convinced this was due to partial entrainment or aether dragging, though he did not attempt a detailed explanation. He ignored critiques demonstrating the inconsistency of his results and the refutation by the Hammar experiment.[B 4][A 16] Miller's findings were considered important at the time, and were discussed by Michelson, Lorentz and others at a meeting reported in 1928.[A 17] There was general agreement that more experimentation was needed to check Miller's results. Miller later built a non-magnetic device to eliminate magnetostriction, while Michelson built one of non-expanding Invar to eliminate any remaining thermal effects. Other experimenters from around the world increased accuracy, eliminated possible side effects, or both. So far, no one has been able to replicate Miller's results, and modern experimental accuracies have ruled them out.[A 18] Roberts (2006) has pointed out that the primitive data reduction techniques used by Miller and other early experimenters, including Michelson and Morley, were capable of creating apparent periodic signals even when none existed in the actual data. After reanalyzing Miller's original data using modern techniques of quantitative error analysis, Roberts found Miller's apparent signals to be statistically insignificant."
(http://en.wikipedia.org/wiki/Michelson%E2%80%93Morley_experiment)
65. "In this process the formation of a concept arises from the constancy of certain experimental relations, whereby the constant value obtained is given a special name.
Mach's well- known definition of "mass" is an important example: when two bodies, denoted by the subscripts i and 2, are acting on each other under the same external conditions, the constant (negative) inverse ratio of their mutually induced accelerations, (—02/01) is given a new name, the "relative mass" of the two bodies or, more precisely, the mass of the first body relative to the second. If, in particular, the second body is a certain standard body ("standard mass"), then the "relative mass" becomes the "mass" of the first body (with subscript 1)." (Concepts of Force, Max Jammer)
66. "As early as 1743 d'Alembert protested against this confused and indiscriminate use of the term "force" when he said : "When we speak of the 'force of a body in motion' either we form no clear idea of what this expression means or we understand by it only the property which moving bodies have of overcoming obstacles encountered in their path or of resisting them.
The nomenclature became even more ambiguous during the nineteenth century when the term "force" was used regularly also to denote our present-day notion of "energy" and "work," the reason being Leibniz's coinage of vis viva (our "kinetic energy")." (Concepts of Force, Max Jammer)
67. "n the first place, "force" is one of the first concepts, and, in many textbooks of physics, the very first nonmathematical concept that the student of science encounters. In the course of his study he meets this concept over and over again: he studies gravitational force, electromagnetic force, frictional and viscous forces, cohesive and adhesive forces, elastic and chemical, molecular and nuclear forces. If not clarified and subjected to critical analysis these concepts are liable to be misunderstood as mystic entities or even occult qualities playing a central role in present-day physics. Charles Sanders Peirce, who realized this situation, when interpreting the concept from his pragmatic point of view, said that force is "the great

concep- tion which, developed in the early part of the seventeenth century from the rude idea of a cause, and constantly improved upon since, has shown us how to explain all the changes of motion which bodies experience, and how to think about physical phe-
nomena; which has given birth to modern science, and changed the face of the globe; and which, aside from its more special uses, has played a principal part in directing the course of mod- ern thought, and in furthering modern social development. It is, therefore, worth some pains to comprehend it." Indeed, the clear comprehension of mechanical force and the conscious incorporation of it into the basic structure of physics can be regarded as the beginning of modern science." (Concepts of Force, Max Jammer)

68. ""Doppler cooling involves light whose frequency is tuned slightly below an electronic transition in an atom. Because the light is detuned to the "red" (i.e. at lower frequency) of the transition, the atoms will absorb more photons if they move towards the light source, due to the Doppler effect. Thus if one applies light from two opposite directions, the atoms will always absorb more photons from the laser beam pointing opposite to their direction of motion. In each absorption event, the atom loses a momentum equal to the momentum of the photon. If the atom, which is now in the excited state, emits a photon spontaneously, it will be kicked by the same amount of momentum but in a random direction. The result of the absorption and emission process is a reduced speed of the atom, provided its initial speed is larger than the recoil velocity from scattering a single photon. If the absorption and emission are repeated many times, the mean velocity, and therefore the kinetic energy of the atom will be reduced. Since the temperature of an ensemble of atoms is a measure of the random internal kinetic energy, this is equivalent to cooling the atoms.
The Doppler cooling limit is the minimum temperature achievable with Doppler cooling.
Detailed Explanation
The vast majority of photons that come anywhere near a particular atom are almost[1] completely unaffected by that atom. The atom is almost completely transparent to most frequencies (colors) of photons. A few photons happen to "resonate" with the atom, in a few very narrow bands of frequencies (a single color rather than a mixture like white light). When one of those photons comes close to the atom, the atom typically absorbs that photon (absorption spectrum) for a brief period of time, then emits an identical photon (emission spectrum) in some random, unpredictable direction. (Other sorts of interactions between atoms and photons exist, but are not relevant to this article.) ..." (http://en.wikipedia.org/wiki/Doppler_cooling)
69. "Dès l’introduction de la thèse qu’il a soutenue en 1860, Jordan a attribué un caractère "essentiel" à une méthode de réduction qu’il a rattaché au cadre de la "théorie de l’ordre" en revendiquant l’héritage de Poinsot. 24 Cette théorie
n’est pas centrée sur un objet mais vise au contraire l’étude des relations entre des classes d’objets. Elle se présente ainsi comme transversale à la théorie des nombres (cyclotomie, congruences), l’algèbre (équations, substitutions), l’ana- lyse (groupes de monodromie et lacets d’intégration des équations différentielles linéaires), la géométrie/ topologie (cristallographie, symétries des polyèdres et des surfaces - y compris de Riemann) et la mécanique (mouvements des so- lides). Tous les travaux de Jordan publiés dans les années 1860 se rattachent à ces thèmes. 25" (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)
70. "Dans ce cadre, Jordan a développé des procédés de réduction d’un groupe en sous-groupes sur le modèle des procédés d’indexation et de représentation analy- tique des substitutions. Ces procédés sous-tendent les relations entre domaines que vise la théorie de l’ordre. La réduction d’un groupe a ainsi été envisagée par Jordan comme une sorte de dévissage par analogie avec la décomposition du mouvement hélicoïdal d’un solide en mouvements de rotation et de transla- tion. Cette analogie a notamment été développée dans l’étude de 1868 sur les groupes de mouvement de solides polyédraux pour laquelle Jordan s’est inspiré des travaux de cristallographie de Bravais." (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)
71. "While the rest of humanity was seemingly occupied with the more practical uses of mathematics, the Greeks were among the first to develop a mathematical world that was not necessarily tied to real-world applications. It was during this time that the concept of axiomatic structure emerged—mathematical proof, in other words." (http://www.learner.org/courses/mathilluminated/units/1/textbook/03.php)
72. "The supposed motto of the Pythagorean group, "All Is Number," encapsulates their preoccupation with both mathematical and numerological concepts. For example, they ascribed a gender to numbers, odd numbers being male and even numbers being female. Much of the mathematical tradition of ancient Greece, and, thus, of the civilizations that followed, stemmed from the obsessions of the Pythagoreans." (http://www.learner.org/courses/mathilluminated/units/1/textbook/03.php)
73. "1.4 Une culture algébrique commune : l’équation séculaire Revenons à présent à la note de 1884 et à la question de la mise en relation des nombres complexes et des groupes continus. Nous avons déjà vu que cette mise en relation ne tient pas à l’identification d’une notion unificatrice entre deux grandes théories. Elle s’appuie en revanche sur l’explicitation d’une identité en partie commune entre deux pratiques distinctes : la pratique des matrices de Sylvester et la pratique de réduction canonique utilisée par Poincaré. Nous allons à présent montrer que cette identité commune présente une di- mension collective qui dépasse les travaux de Sylvester et Poincaré. Elle relève en fait d’une culture algébrique largement partagée dans la seconde partie du XIXe siècle et qu’identifie très précisément le titre du mémoire dans lequel Syl- vester développe, en 1883, la notion de racine latente d’une matrice : "On the equation to the secular inequalities in the planetary theory".[Sylvester, 1883] Ce titre reprend d’ailleurs celui d’un article

publié par Sylvester en 1851 lorsque ce dernier a introduit le terme matrice : "Sur l'équation à l'aide de laquelle on détermine les inégalités séculaires des planètes".[Sylvester, 1852] De telles références à l'équation séculaire interviennent dans de très nom- breux textes du XIXe siècle. Elles ne manifestent le plus souvent pas de préoc- cupations en mécanique céleste mais s'avèrent la manière dont était identifiée une pratique algébrique commune entre études des cordes vibrantes et méca- nique céleste, systèmes différentiels et géométrie analytique, arithmétique des formes quadratiques et algèbre des invariants. C'est d'ailleurs dans ce cadre que les notions de matrices et de mineurs ont été introduites par Sylvester à la suite de travaux de Cauchy et ont par la suite circulé entre des travaux de Cayley, Sylvester, Hermite, Riemann etc. On trouve au cœur de cette pratique algébrique un procédé consistant à donner une expression polynomiale des solutions (xi) d'un système linéaire sy- métrique de n équations par des quotients impliquant, outre le déterminant caractéristique S du système, ses mineurs successifs P1i obtenus par développe- ments par rapport à la 1re ligne et ie colonne. L'expression polynomiale donne P1i (∗) (x) S x−sj P1i s xi j = S x−sj (sj) s où xi j désigne le système de solutions associé à la racine caractéristique sj .[Brechenmacher, 2007b]. 19 La pratique algébrique associée à l'équation séculaire ne se réduit cepen- dant pas à l'usage de tels procédés polynomiaux. En effet, l'expression (*) est rarement utilisée en tant que telle mais le plus souvent incorporée au sein de dif- férentes méthodes élaborés dans divers cadres théoriques. Ainsi, dans l'approche de Joseph-Louis Lagrange (1766) sur les systèmes différentiels, cette pratique supporte la transformation d'un système de n équations différentielles linéaires à coefficients constants à la forme intégrable de n équations indépendantes (un système diagonal en termes actuels). Elle est indissociable d'une représentation mécanique selon laquelle des oscillations d'un fil lesté de n masses peuvent se représenter par la composition des oscillations propres de n fils lestés d'une seule masse. La nature des racines de l'équation caractéristique a ainsi été liée à la condition de stabilité mécanique des oscillations. Cette pratique a par la suite sous-tendu une mathématisation des oscillations séculaires des planètes sur leurs orbites. Pour cette raison, l'équation sur laquelle elle est basée (l'équation caractéristique) a été dénommée "l'équation à laquelle on détermine les inégalités séculaires des planètes". Dans ce contexte, Pierre-Simon Laplace a cherché à démontrer la stabilité du système solaire en déduisant la nature réelle des racines de l'équation séculaire de la propriété de symétrie des systèmes mécaniques. Plus tard, Cauchy s'est appuyé en 1829 sur l'analogie entre les propriétés de cette équation et celles des équations caractéristiques des coniques et quadriques pour donner aux procédés élaborés par Lagrange une interprétation géométrique. La transformation de systèmes linéaires a alors été envisagée en terme de changements d'axes en géométrie analytique. Plus tard encore, ces mêmes procédés ont été associés aux calculs de résidus de l'analyse complexe, aux suites de Sturm d'une équation algébrique, aux classes d'équivalence des couples de formes quadratiques etc. Ces divers travaux reconnaissaient cependant une nature algébrique sous- jacente aux différents cadres théoriques mobilisées. Cette identité algébrique tient principalement à trois caractères. – Le premier est la permanence sur plus d'un siècle des procédés opératoires (*). – Le deuxième est l'ambition explicite de généralité qui se manifeste depuis la prise en compte par Lagrange de systèmes linéaires à n variables. – Le troisième est le caractère spécial de l'équation séculaire dont on a long- temps pensé que les racines devaient être non seulement réelles mais aussi distinctes." (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)

74. " Le troisième est le caractère spécial de l'équation séculaire dont on a long- temps pensé que les racines devaient être non seulement réelles mais aussi distinctes." (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)
75. "A partir des années 1840, les références à l'équation séculaire apparaissent le plus souvent associées aux problèmes posés par l'occurrence de racines multiples dans la dite équation. Dans ce cas, l'expression (*) est en effet susceptible de 0 prendre une valeur 0 . Cette difficulté a suscité des critiques quant à la tendance de l'algèbre à attribuer une généralité excessive aux expressions symboliques. C'est d'ailleurs à partir de ce constat que Cauchy s'est détourné de l'algèbre au profit de l'analyse complexe : le calcul des résidus permet en effet de remplacer le problème de la multiplicité des racines par celui de la détermination des degrés des pôles de l'expression (*) envisagée comme une fonction méromorphe. À partir des années 1850, plusieurs approches algébriques du problème de la multiplicité des racines ont été développées de manière distinctes : matrices de Sylvester en géométrie analytique, réduites d'Hermite en théorie des formes quadratiques, diviseurs élémentaires de Weierstrass pour les couples de formes quadratiques et bilinéaires, réduction canonique de Jordan pour les systèmes différentiels linéaires à coefficients constants. Ces différents développements témoignent de la dislocation progressive de la culture algébrique commune qu'a longtemps porté l'équation séculaire. Jusque dans les années 1880, c'est cependant toujours par l'intermédiaire de cette équa- tion que ces lignes divergentes se sont rencontrées épisodiquement. C'est notam- ment dans ce cadre que Poincaré a réagi en 1884 aux difficultés posées par les racines multiples dans les travaux de Sylvester, sans pour autant s'intéresser aux problématiques sur les matrices-quantités complexes développées à cette époque par ce dernier." (Autour de pratiques algébriques de Poincaré : héritages de la réduction de Jordan)
76. "It is well-known that between 1880 and 1884 Poincare ́ brought together in a completely un- expected way the subjects of complex func- tion theory, linear differential equations, Rie- mann surfaces, and non-Euclidean geome- try (see, for example, [42] and [7]). Cauchy's approach to complex function theory and the theory of differential equations were main- stream topics in the education of a French mathematician at

the time, but Riemann sur- faces were not, largely because Riemann's way of thinking was not congenial to Charles Hermite, who dominated the scene in the 1870s.
Inthespringof1879theAcadeˊmiedesSci- ences in Paris announced the topic of the Grand prize for the mathematical sciences (see C.R. Acad. Sci., 88, 1879, p. 511), which was "to improve in some important way the theory of linear differential equations in a single independent variable". The topic had been proposed by Hermite, and his intention was to draw young French mathematicians to study the work of Lazarus Fuchs, who was
the German expert on the subject; in the af- termath of the Franco-Prussian War catching up with the Germans was on every patriotic Frenchman's mind." (Poincareˊ and the idea of a group, Gray)

77. "In a series of papers in 1866 to 1868, starting with [6], Fuchs had been able to characterise a class of linear ordinary differential equa- tions of arbitrary order that have the proper- ty that their solutions are meromorphic and have poles only where the coefficients of the equation themselves have poles. Among this class is the celebrated hypergeometric equa- tion, z(z−1)d2w/dz2 +(c−(a+b+1)z) dw/dz − abw = 0 and we may confine our attention to it, al- though by then Fuchs had moved on to study other problems. Poincareˊ took from the later work of Fuchs the idea that the quotient of a basis of solu-tions to equation (1) was an interesting ob-ject. If the solutions are denoted w1(z) andw2(z), and the quotient as ζ(z) = w1(z) , then w2(z) analytic continuation of the solutions around a path enclosing a singular point returns the quotient in the form ζ(z) = (a11w1(z) + a12w2(z)) / (a21w1(z) + a22w2(z)) = (a11ζ(z)+a12) / (a21ζ(z) + a22) where the coefficients ajk are constants that depend on the path. As this formula makes clear, the 'function' ζ is a multi-valued func- tion, but its set-theoretic inverse is a general- isation of a periodic function: z(ζ) = z(a11 ζ + a12) / (a21ζ + a22) Geometrically, the function ζ(z) maps the up- per half-plane to a triangle, the vertices of which are the images of the singular points 0, 1, ∞ on the real axis, and the angles of which are determined by the coefficients a, b, c of the hypergeometric equation. An- alytic continuation shows that the lower half- plane is mapped to another triangle (which will be congruent if the coefficients are all real) and thereafter the images of the half-planes form a net of triangles, provided the angles are of the form π/n for some integers n.Moreover,eachtrianglewillhavethesame three angles, although their sides will be cir- cular arcs and not straight, and Poincareˊ in- troduced a simple geometric transformation to straighten them out. This much, but lit- tle more, formed the content of the essay Poincareˊ submitted for the prize in May 1880. The extra concerned a discussion of the net of triangles and made some corrections to Fuchs's papers. He showed, for example, that if the angles of the triangles are π , π and π , 236 then the net covers the plane, eight suitably chosen triangles form a parallelogram, and z = z(ζ) is an elliptic function; but if the an- glesareπ andπ attheimageofz=0and 42 z = 1 respectively and π at ∞, then the net 6 lies inside a certain circle, and the sides of the triangles are circular arcs meeting this circle at right angles." (Poincareˊ and the idea of a group, Gray)
78. "Mittag-Leffler got the long pa- pers he wanted in which Poincareˊ set out the theory of the new functions, and Klein had to realise that the younger, less well- educated Poincareˊ was moving faster than he ever could. Their exchanges are both scholarly and personal. Klein objected to the name 'Fuchsian' for the new functions on the grounds that some ideas of Schwarz were much closer, and Poincareˊ agreed when he got round to consulting Schwarz's paper, which he had not known. But he could not agree to change the name, which he had al- ready used in publications, and Klein railed against this, doubtless because Fuchs, as a Berlin-trained mathematician close to Weier- strass and Kummer, was a rival likely to have a better career than him but with less talent. To shut him up Poincareˊ named the general- isation of Fuchsian functions that require 3- dimensional non-Euclidean geometry 'Kleini- an functions'. Klein protested, correctly, that he had had nothing to do with these func- tions and Schottky's name would be more appropriate; "Name ist Schall und Rauch" Poincareˊ replied in German ("Name is sound and fury", the quotation comes from Gretchen in Goethe's Faust)." (Poincareˊ and the idea of a group, Gray)
79. "The correspondence was also a competi- tion, and it was to cost Klein his health, but not before both men had come to the deep- est jewel in the field, what became known as the uniformisation theorem. Klein took the lead, because he knew Riemann's theory of the moduli of algebraic curves on Riemann surfaces according to which a Riemann sur- faceofgenusg>1dependson3g−3com- plex parameters. On the other hand, it was possible to show that the non-Euclidean poly- gons that are mapped around in the disc by a Fuchsian group in such a way that the quo- tient space is a Riemann surface of genus g also depend on 3g − 3 complex parameters. The implication was obvious: every Riemann surface arises as a quotient of the disc with its non-Euclidean metric under the action of a Fuchsian group. Not only that, but locally the map from the disc to the Riemann surface will be an isometry, so all but the Riemann surfaces of lowest genus locally carry non- Euclidean geometry. (This copies the case of elliptic functions, which are quotient spaces of the Euclidean plane.) " (Poincareˊ and the idea of a group, Gray)
80. "and after a short essay on what he had done on ce- lestial mechanics the next popular essay was a rather cryptic account of geometry. Here again it became clear that he put the group ahead of the space, or, as he preferred to say, the form ahead of the matter. " (Poincareˊ and the idea of a group, Gray)
81. "He asked his readers to imagine some ex- periment in which a seemingly decisive result had been obtained, for example the construc- tion of a figure with light rays marking out four equal sides meeting at four equal angles for which the sum of the angles was less than 2π . This would seem to suggest that space was non-Euclidean,but,said,Poincareˊ,thereis another interpretation, which was that space was Euclidean and light rays were curved. There could be no way of deciding logically between these two interpretations,

and all we could do would be to settle for the geometry we found most convenient, which, indeed, he said would be the Euclidean one. His reasons were, however, unexpected, and will be con- sidered shortly." (Poincare´ and the idea of a group, Gray)

82. "The best account he gave of his philoso- phy he published in an English translation in the Monist [29] in 1898. He began by rais- ing the fundamental question of how we con- struct a sense of space around us at all. This was much discussed by psychologists at the time, and Poincare´ observed that we can con- struct many spaces. A single motionless eye would construct a two-dimensional projective geometry with no sense of distance. A pair of eyes could construct a sense of depth. We have our sense of touch, and we could con- struct a high-dimensional space by recording the muscular sensations needed to put the tip of a finger somewhere. Out of this welter of experiences and before we are capable of formal instruction, we all construct a sense of three-dimensional space occupied by some bodies with predictable behaviour. These are the rigid bodies, and they are singled out by the fact that we can compensate for the mo- tion of a rigid body by a motion of our own. We can, for example, distinguish the motion of a glass from the motion of the wine swirling aroundinit.So,heargued,webuildupinour minds a sense of what rigid bodies are, and are able to handle them hypothetically. This mental construction gives us our idea of the isometries of a hypothetical rigid body, and from this we construct our concept of space – notice that rigid bodies and their motions came first in this analysis. The concept of dis- tance is derived from the behaviour of rigid bodies, which is why on this account Poincare´ could dispute Russell’s claim about London, Paris, and the millimetre. For Poincare´ the claim is true not because we know what dis- tance in space is, but because we know what isometries are. This knowledge is innate, it has evolved with the human species, and it is triggered by the experiences of every sentient infant.
    For Poincare´ it is only via the introduction of the group that the non-measurable ‘space’ of Helmholtz and Lie becomes a measurable magnitude “that is to say, a veritable space”. Therefore [29, Sections 21 and 22]:
    “What we call a geometry is nothing but the formal properties of a certain continuous group . . . so that we may say, space is a group. But the assertion is no less true of the notion of many other continuous groups; for exam- ple, that which corresponds to the geometry of Lobachevskii. There are, accordingly, sev- eral geometries possible, and it remains to be seen how a choice is made between them.”
    Poincare´ was adamant that different crea- tures, with a different history, might be non- Euclidean in the sense that their brains would find non-Euclidean geometry convenient. If we met such creatures, we would not share their sense of what is easy of natural, nor would they share ours, but neither side would be able to trap the other in a contradiction. Where we and they would differ would be in our innate understandings of rigid bodies, and we would be the ones whose brains ap- preciated that the translations in the group of isometries formed a normal subgroup, a statement that is false in the group of non- Euclidean isometries.
    This is fundamental epistemology (and quite different from his other convention- alisms). It accounts for the one wry observa- tion Poincare´ had on Hilbert’s foundations of geometry as they were set out in his Grundla- gen der Geometrie [12], in 1899. Hilbert gave a purely axiomatic formulation, with no pre- tence to being an account of how we can have knowledge of the external world, and when Poincare´ reviewed them he commented [33, p. 272] that:
    “The objects which he calls points, straight lines,orplanesbecomethuspurelylogical entities which it is impossible to represent to ourselves. We should not know how to pic- ture them as sensory images . . . Each of his geometries is still the study of a group. The logical point of view alone appears to interest him. With the foundation of the first propo- sition, with its psychological origin, he does not concern himself. His work is then incom- plete; but this is not a criticism . . . Incomplete one must indeed resign one’s self to be.”" (Poincare´ and the idea of a group, Gray)
83. "Poincare´ thought deeply about what it is to do good mathematics, to find what he once called the ‘soul of the fact’ around which a body of theory grouped itself in the most per- spicacious way and enabled to mathemati- cian to do the most effective work. (A fuller account of Poincare´’s work will appear in [8].) In both his pure mathematics and his work on mathematical physics he advocated the study of what he called form over matter, by which he meant the abstract system of rela- tions rather than specific objects that obey those relations. He looked for analogies that would enable a system of relations to be moved, with a degree of fidelity, from one field to another. In all of these ways the group idea was vital to him. In number theory it enabled him to place the modular equation in a rich- er setting, one that opened the way for some types of Fuchsian functions to do arithmetic work. The group idea was crucial to his anal- ysis of surfaces at the start of his career, and his work on 3-manifolds towards the end. It allowed him to classify domains of holomor- phic functions in C2. It allowed him to give an account of how we construct space around us, and also of the space and time of contem- porary physics (but not space-time, a step he refused to take). But in none of these areas did he then pause to study the groups in any detail. It was the idea of a group, not group theory, that animated him." (Poincare´ and the idea of a group, Gray)
84. "Furthermore Gauss's notational practices were carried one step further by Eisenstein [1], [2] in his efforts to develop further the general theory of forms envisioned by Gauss. Eisenstein observed that if linear substitutions (in any number of variables) are considered as entities and denoted by letters, then they can be added and multiplied much as ordinary numbers, except, as he stressed, the order of multiplication does matter: »ST need not be the same as TS. Eisenstein also introduced the common algebraic notation for products, inverses and powers of linear substitutions and used it to good advantage in his papers on the arithmetical theory of forms during the period 1844-1852 (the year of his untimely death). In the early

1850's, Eisenstein's contemporary, Charles Hermite, who was in contact with Eisenstein, continued the latter's use of the symbolical algebra of linear sub- stitutions, both in his work on the theory of forms and on the transformation of abelian functions. Thus by the mid-1850's the idea of treating linear substitutions as objects which can be treated algebraically much like ordinary numbers was not very novel." (The Theory of Matrices in the 19th Century, Hawkins)

85. "There is another reason why the Cayley-as-Founder view of the history of matrix theory is misleading. By focusing, as it does, upon the form of the theory, i.e., the matrix symbolism, it tends to ignore its content', the concepts and theorems that make it a bonafidetheory. For example : the notion of an eigenvalue, the classifica- tion of matrices into types, such as symmetric, orthogonal, Hermitian, unitary, etc., and the theorems on the nature of the eigenvalues of the various types and, above all, the theory of canonical matrix forms—in short, what I shall refer to as the spectral theory of matrices." (The Theory of Matrices in the 19th Century, Hawkins)
86. "Certain generalities seem to have been drawn from this, namely that a concern for rigor comes at the end of a mathematical develop- ment, after the "creative ferment" has subsided, that rigor in fact means rigor mortis. Weierstrass himself provides a good counterexample to this generality, for all his work on the spectral theory of forms was motivated by a concern for rigor, a concern that was vital to his accomplishments.
   Weierstrass was dissatisfied with the kind of algebraic proofs that were com- monplace in his time. These proofs proceeded by formal manipulation of the symbols involved, and no attention was given to the singular cases that could arise when the symbols were given actual values. One operated with symbols that were regarded as having "general" values, and hence such proofs were sometimes re- ferred to as treating the "general case", although it would be more appropriate to speak of the generic case. Generic reasoning had led Lagrange and Laplace to the incorrect conclusion that, in their problems, stability of the solutions to the system of linear differential equations required not only reality but the nonexistence of multiple roots. (Hence their problem had seemed all the more formidable !) In fact, Sturm who was the first to study the eigenvalue problem (1) proved among other things the "theorem" that the eigenvalues are not only real but distinct as well. His proof was of course generic, and he himself appears to have been uneasy about it; for at the end of his paper he confessed that some of his theorems might be subject to exceptions if the matrix coefficients are given specific values. Cauchy was much more careful to avoid what he called disparagingly "the generalities of algebra," but multiple roots also proved problematic for him. As he realized, his proof of the existence of an orthogonal substitution which diagonalizes the given quadratic form depended upon the nonexistence of multiple roots. He tried to brush away the cases not covered by his proof with a vague reference to an infinitesimal argument that was anything but satisfactory.
   It was to clear up the muddle surrounding multiple roots by replacing generic arguments with truly general ones that Weierstrass was led to create his theory of elementary divisors. Here is a good example in which a concern for rigor proved productive rather than sterile. Another good example is to be found in the work of Frobenius, Weierstrass' student, as I shall shortly indicate."
   (The Theory of Matrices in the 19th Century, Hawkins)" (The Theory of Matrices in the 19th Century, Hawkins)
87. "He had read a paper in Liouville's journal by Olinde Rodrigues in which the latter showed among many other things that the 9 coef- ficients of a linear transformation of rectilinear axes could be expressed rationally in terms of three parameters. This was the period—the early 1840's—when Cayley was preoccupied with learning and applying the theory of determinants, and he showed .....
   The significance of Cayley's solution was that a succinct symbolical representation can be given if both the operations of addition and multiplication are employed. I already pointed out that in the late 1840's and early 1850's Eisenstein and Her- mite recognized the possibility of a symbolical algebra of linear substitutions under addition and multiplication. But in their work they had—or saw—no occasion to make any symbolical use of the addition of substitutions." (The Theory of Matrices in the 19th Century, Hawkins)
88. "The solutions that Cayley and Hermite had given to the Cayley-Hermite problem were also generic; they had not obtained all possible solutions to the problem. Generic proofs were the order of the day in the 1850's, and no one raised any objections. The Cayley-Hermite problem thus sank into oblivion where it perhaps would have remained had it not once again been for the theory of numbers. Just as Hermite's interest in the arithmetical theory of ternary forms had reawakened Cayley's interest in the Cayley-Hermite problem, so now in the early 1870's it was Paul Bachmann's interest in ternary forms that led him to re-examine Hermite's solution to the Cayley-Hermite problem and to discover its completeness.
   Bachmann's observations brought forth a reply from Hermite in which he patched up his earlier solution to cover the singular cases—but only for the ternary case. Also Bachmann's colleague at Breslau, Jacob Rosanes, attempted to deal with the «-variable case by making use of the fact, observed by Hermite and Cayley concern- ing their solutions, that if X is a characteristic root so is Iß. Rosanes' results were, however, incomplete, especially because he could not handle the case of multiple roots." (The Theory of Matrices in the 19th Century, Hawkins)
89. "Topology, for many years, has been one of the most exciting and influential fields of re- search in modern mathematics. Although its origins may be traced back several hundred years it was Poincare who, to borrow an expression used of Mobius, "gave topology wings" in a classic series of articles published around the turn of the century. While the earlier his- tory, sometimes called the prehistory, is also considered, this

volume is mainly concerned with the more recent history of topology, from Poincare onwards." (The history of Topology, James)

90. "In essence, the three problems of defining, proving and explaining have been fundamen- tal to the growth of topological dimension theory:
• The problem of defining the concept of dimension itself.
• The problem of proving that the dimension of mathematical spaces is invariant under
certain types of mapping.
• The problem of explaining the number of dimensions of physical space.
The first and second problems, mathematical in nature, have been the most important direct influence on the growth of the theory of dimension. The third, a problem of physics or cosmology, has provided an indirect but significant motivation for the development of the theory from outside the mathematical domain." (The history of Topology, James)
91. "According to Euclid, a point is that which has no part, a line is breadthless length, and a surface is that which has length and breadth only (Book I). A solid is that which has length, breadth and depth {Book XI). Euclid's definitions show a concern for a rudimentary "theory of dimension" by the recognition of a "dimension" hierarchy in the sequence of primary geometrical objects: point, line, surface, solid. A passage from Aristotle's On the Heavens shows a similar motivation. In it, Aristotle is more definite, even if its tone is more metaphysical:
Of magnitude that which (extends) one way is a line, that which (extends) two ways a plane, and that which (extends) three ways a body. And there is no magnitude besides these, because the dimensions are all that there are, and thrice extended means extended all ways. For, as the Pythagoreans say, the All and all things in it are determined by three things; end, middle and beginning give the number of the All, and these give the number of the Triad [17, p. 104]." (The history of Topology, James)
92. "Bolzano sought precise definitions of geometrical objects. "At the present time", he wrote in 1810, "there is still lacking a precise definition of the most important concepts: line, surface, solid" [16, p. 271]. This dull essentiaUst problem of definitions, conceived within the limits of Euclidean geometry, led him to break from the bonds of traditional geometry. Bolzano stressed the theoretical role of mathematics and its "usefulness" in exercising and sharpening the mind. Rigour in pure mathematics was uppermost in his thoughts. For example, he regarded it a mistake to make any appeal to motion as it was foreign to pure geometry. This purge of motion from geometry is relevant to Bolzano's dimension-theoretic definitions of fine, surface, and solid. For instead of taking a Une as the path of a moving point, as for example was done by Abraham Kastner (1719-1800), Bolzano attempted to define the concept of line independently of any idea of motion." (The history of Topology, James)
93. "His youthful Betrachtungen is heavily imbued with philosophy and this illustrated his deep concern for the logical and foundational issues in mathematics. Bolzano's con- cern over definitions required that he seek the "true" definitions for the objects of geome- try. Undoubtedly, this essentialist philosophy is to blame for the main shortcomings of his geometrical investigations. The end product of his research, a seemingly endless string of definitions with hardly a theorem, must be regarded as disappointing. Yet if one asks "what is" questions - What is a line?. What is a continuum? - then one must expect essentiaUst answers. But, while definitions have a certain value in mathematics, no fruitful mathemati- cal theory can consist entirely of them. Theorems and their proofs which relate definitions one to another, are much more important. Bolzano's theory is unquestionably lacking in these.
Bolzano returned to geometrical studies in the 1830s and 1840s. In writings of 1843 and 1844, though not published in his lifetime, he revised and improved his youthful find- ings. In these, Bolzano's topological basis, derived from his concept of "neighbour" and "isolated point", is very deep. The concept of neighbour, which in effect uses the mod- ern notion of the boundary of a spherical neighbourhood allowed Bolzano to put forward some very clear definitions of line, surface, and soHd. Later, when he discovered his no- tion of isolated point, he was able to arrive at an even deeper understanding of the basic figures of geometry. His geometrical insights were far more penetrating than those of his contemporaries." (The history of Topology, James)
94. ""As much as you will not agree with me, I can only say: I see it but I do not beUeve it" [3, p. 44]. The strange result immediately called into question the very concept of dimen- sion. Was it well-defined or even meaningful?" (The history of Topology, James)
95. "Cantor's work on set theory arose out of his investigations into the uniqueness of rep- resenting a function by a trigonometric series. In 1874 he published his first purely set- theoretic paper, giving proofs that the set of real algebraic numbers could be conceived in the form of an infinite sequence: ..., This set is countable (abzdhlbar, to use Cantor's later term), while the set of all real num- bers is uncountable and cannot be listed in this way. Through these results on "linear sets" Cantor saw a clear distinction between two types of infinite sets of numbers on the real number line." (The history of Topology, James)
96. "In a letter to Dedekind dated 5 January 1874 he posed a tantalising new research question, a question which is basic to the growth of dimension theory:
Can a surface (perhaps a square including its boundary) be put into one-to-one corre- spondence with a line (perhaps a straight line segment including its endpoints) so that
to each point of the surface there corresponds a point of the line and conversely to each point of the line there corresponds a point of the surface? [17, p. 132]" (The history of Topology, James)

97. "It is likely that Cantor only worked intermittently on this question from May 1874 until April 1877 and indeed without success. However, he persisted in regarding it as important." (The history of Topology, James)
98. "Cantor ascribed his result of a ^-fold manifold being "coordinated" by a single coordinate to the "wonderful power in the usual real and irrational numbers" [17, p. 14]. Cantor's letter to Dedekind of 25 June ended: Now it seems to me that all philosophical or mathematical deductions which make use of this mistaken assumption [on the number of coordinates] are inadmissible. Rather the distinction which exists betweenfiguresof different dimension numbers must be sought in entirely different aspects than in the number of independent coordinates, which is normally held to be characteristic [17, p. 141]." (The history of Topology, James)
99. "Prior to Cantor and Dedekind, Bernhard Riemann (1826-1866) and Hermann Helmholtz (1821-1894) among others, had put forward a very informal theory of continuous mani- folds which included an impUcit theory of dimension based on the number of coordinates. Riemann and Helmholtz merely intended their theory of manifolds as a general framework for their investigations into geometry. Objectively one can only regard their theory as vague in its mathematical details. As Cantor saw, his result was a direct and devastating blow to the "coordinate concept of dimension". Another concept of dimension was needed." (The history of Topology, James)
100. "Cantor approached the concept of dimension from a position outside geometry. His point of view derived from his work on one-to-one correspondences and cardinality, and hence, from the viewpoint of the set theory which he was in the midst of creating. Consequently, he brought an entirely new set-theoretic approach to problems of geometry. Moreover, he recognised his result could easily be extended from ^-dimensional manifolds to infinite- dimensional manifolds, assuming that their infinitely many dimensions have the form of a simple infinite sequence (i.e. the dimension is countably infinite). Apparently the editors of Crelle's Journal found Cantor's results bizarre. They wished to ignore or even reject his paper; to use Imre Lakatos' term they were monster-barrers [25]. After a frustrating wait. Cantor heard his paper would be printed." (The history of Topology, James)
101. "Dedekind was not entirely persuaded by Cantor's claims of a revolution. While accepting Cantor's counterintuitive result whole-heartedly, he did not concur with Cantor's claims that the foundations of geometry were being undermined. He immediately saw a way out of the difficulty through continuity and gave credit to the older geometers for this means of escape from the consequences of Cantor's result. To a certain extent it is true that Riemann and Helmholtz included continuity (and differentiability) in their concept of dimension. However, Dedekind probably imputed a little too much to their informal theory in his desire to find "hidden lemmas" in the work of the great men of the past." (The history of Topology, James)
102. "A fresh examination of the vague informal concept of dimension was now an absolute necessity. Even Dedekind reahsed this and he quickly saw that a proof of some kind of theorem about dimensional invariance incorporating the idea of continuity was needed. Hence, he came to state very clearly the crucial problem of dimension which the paradoxi- cal correspondence result forced upon mathematicians. In a letter to Cantor of 2 July 1877, Dedekind arrived at the following statement, in effect a proposed invariance theorem:
    If one succeeds in setting up a one-to-one and complete correspondence between the points of a continuous manifold A oi a dimensions on the one hand and the points of a continuous manifold B of b dimensions on the other, then this correspondence itself must necessarily be discontinuous throughout if a and b are unequal [17, pp. 141-142]." (The history of Topology, James)
103. "In Cantor's study of linear point sets and point sets in the n-dimensional arithmetic con-
    tinuum (Euclidean n-space), the fundamental concept is that of Hmit point. The underlying theorem is the so-called Bolzano-Weierstrass theorem (that every infinite set of points in a bounded region of n-space possesses at least one limit point). From this fundamental concept and theorem flow all of Cantor's deep point set-theoretic concepts: the notions of a derived set, an everywhere dense set, an isolated point set, nowhere dense set and the Can- tor "middle-third" discontinuum. By introducing these notions Cantor opened up a new field of study. Analysts were the first to see the usefulness of Cantor's imaginative ideas. Point set theory offered wonderful new instruments for a detailed study of the nature of functions, with the result that the growth of real and complex function theory was greatly accelerated in the years after the publication of Cantor's great papers. Applications of the Cantorian toolkit to the fundamental notions of geometry came a little later." (The history of Topology, James)