TWO TALKS ON MATHEMATICS AND PHYSICS

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INTERRELATIONS BETWEEN MATHEMATICS AND PHYSICS

I. Foreword

I would like to start with an explicit description of the conceptual framework of this study.

To render it concisely, it is useful to look at the case of comparative linguistics. The history of a language is not a history of all, or even of "the most important," utterances (oral or written) in this language. Rather, it is a history of evolution *of the language as a system.*

Hence we need a preliminary description of the system(s) whose genesis we are studying.

An application of this Saussurian scheme to the history of mathematics (which, incidentally, I do not consider to be a mere language) was probably particularly appealing to Jean Dieudonné who, as an active member of the Bourbaki group, participated in the creation of a systematic picture of modern mathematics.[1] In this talk I follow his example, on a much humbler scale. Needless to say that restrictions of time, space, and competence, force me to choose a thin chain of connected ideas and present them in a highly selective way.

Thus I refuse (somewhat reluctantly) to discuss the history with Rankean insistence on *wie es eigentlich gewesen ist.* One reason for this refusal is that the history of contemporary mathematics tends to degenerate into credit and priority assignments, lacking pathetically the dramatic appeal with which the history of struggles for real power is charged. A more personal and compelling motive is succintly put by Joseph Brodsky in his autobiographical essay "Less Than One": "The little I remember becomes even more diminished by being recollected in English."

A last word of warning and apology is due. Any system is, of course, a theoretical construct. As such, it is at best relative and culture dependent, at worst subjective. It is precisely in this function that it can serve as material for the history of mathematics of the XXth century.

II. Mathematical Physics as a System

a) **Physics.** Physics describes the external world, and in its domain of competence, does this in two complementary modes: classical and quantum.

In the classical mode, events occur to the matter and fields which reside and evolve in the space-time. Physical laws directly constrain observables. They are basically deterministic and expressed by the differential equations which (sometimes demonstrably, sometimes hypothetically) satisfy appropriate uniqueness and existence theorems.

[1]Jean Dieudonné, as I remember him, had a strong voice, strong hands, and strong opinions. In particular, he insisted on using tensor products and commutative diagrams instead of classical subscripts and superscripts in calculations involving tensors. I used to believe his jugdement that this was a chalk-saving device, until one day I had to calculate with tensors myself. Then I found out that subscripts were much more economical.

A statistical submode of the classical mode of description deals with probabilities and averages which (sometimes demonstrably, sometimes presumably) can be deduced from an ideal deterministic description. The need for a statistical treatment arises from two basic premises: too many degrees of freedom and/or instability. (Metaphorically speaking, instability means that each consecutive decimal digit is a new degree of freedom.)

A fundamental physical abstraction is that of *an isolated system* which evolves in oblivion of the rest of the world, and of *interaction* between potentially isolated systems, or one isolated system and the rest of the world.

In one of the most remarkable flights of fancy of classical physics, *space–time itself* appears as such an isolated system governed by Einstein's equations of general relativity (perhaps, with an energy–momentum tensor summarily responsible for everything which is not pure space–time.)

In the quantum mode of theoretical description, the observable world is inherently probabilistic.

Moreover, and more significantly, the basic laws — which are in a sense deterministic — govern an unobservable entity, *the probability amplitude*, which is a complex valued function on a quantum path space. Roughly speaking, the amplitude of a composite event is the product of the amplitudes of its constituents, whereas the amplitude of an event which is a sum of alternatives is the sum of the amplitudes of these alternatives.

The probability of an event is the modulus squared of its amplitude. Physical observables are the appropriate averages, even if one speaks about an elementary act of scattering of an individual particle. The observable wave behavior of, say, light is only an imperfect reflection of the inherent wave behavior of the amplitudes (wave functions) of an indeterminate number of photons described by the Fock space of the quantized electromagnetic field.

Partly as a result of historical development, many quantum models contain as an intermediate stage a classical model which is then quantized. The word "quantization" rather indiscriminately refers to a wide variety of procedures of which two of the most important are operator, or Hamiltonian, quantization, and path integral quantization. The first is more algebraic and usually has a firmer mathematical background. The second possesses an enormous heuristic and aesthetic potential. I haven chosen the latter for my more detailed subsequent discussion.

If I had included the first one, the picture of the divergence of Mathematics and Physics in the first half of this century sketched below in Sec. IV would appear less pronounced. Nevertheless, the main results of my analysis would survive.

One more subject matter deserving a separate historical and structural study is the duality between these two approaches. It started with classical mechanics, Lagrange, and Hamilton, and continued via Heisenberg–Schrödinger wave mechanics to the path integral/scattering matrix controversy, On the fringes of physics it contains such recent mathematical gems as Virasoro algebra representations on the moduli spaces of curves.

b) **Mathematics.** If there is one most important notion of mathematical physics, it is that of *action* functional. It encompasses the classical ideas of energy and work, its density in a domain of space–time is the Lagrangian, and multiplied by $\sqrt{-1}$ and exponentiated, it furnishes the basic probability amplitude. Action is measured in absolute Planck units, and therefore can be thought of as a real number.

More precisely, we will consider the following scheme of description central for both modes of physical description referred to above.

The modeling of a physical system starts with the specification of its kinematics. This includes a space $\mathcal{P}$ of virtual classical paths of the system and an action functional $S : \mathcal{P} \to \mathbf{R}$. For example, $\mathcal{P}$ may consist of parametrized curves in a classical phase space of a mechanical system, or of Riemannian metrics on a given smooth manifold (space–time), or of triples *(a connection on a given vector bundle, a metric on it, a section of it)* etc. The value of the action functional at a point $p \in \mathcal{P}$ is usually given in the form $\int_p L$, that is a volume form integrated over one of the spaces figuring in the description of p.

Classical equations of motion specify a subspace $\mathcal{P}_{cl} \subset \mathcal{P}$. This subset consists of the solutions of the variational equations $\delta(S) = 0$, i. e. of the stationary points of the action functional.

If the classical description is the statistical one, then $\exp(-S)$ is the probability density.

In the quantum description, we choose physically motivated subsets $B \subset \mathcal{P}$, typically determined by boundary conditions, and define the average of an observable O in B by a path integral of the type

$$\langle O \rangle_B := \int_B O(p) e^{i \int_p L} Dp. \qquad (1)$$

These are our main actors. In the following, I present some musings about the history of this picture as seen through the eyes of physicists and mathematicians.

I will be most interested in the idea of the integral and its final incarnation, in the form of *the path integral.*

III. The Integral

The notion of an integral is one of the central and recurring themes in the history of mathematics for the last two millennia. The ardent problem solving is periodically followed by the anxious definition seeking, only to be replaced by new non–rigorous but amazingly efficient heuristics leaving a logically–minded fundamentalist in each of us baffled.

Richard Feynman who created the hierogram (1) (still lacking a precise mathematical meaning exactly in those cases when it is most needed by physicists[2]) used to boast that (1) allowed the calculation of the anomalous magnetic momentum of the electron, which coincided with its experimental value up to ten digits ([F2], p. 118): "As of 1983, the theoretical number was 1.00115965246, with an uncertainty

[2]For a more positive view, see [GJ], a remarkable book which influenced the structure of this essay. On p. 313 however the authors say: "... it is a theoretical puzzle whether a *theory* of electrodynamics exists in the sense of a mathematical framework ..."

of about 20 in the last two digits; the experimental number was 1.00115965221, with an uncertainty of about 4 in the last digit. This accuracy is equivalent to measuring the distance from Los Angeles to New York, a distance of over 3000 miles, to within the width of a human hair."

This feat was recently matched by physical calculations (even called "predictions", cf. [COGP]) of various interesting numbers in algebraic geometry, such as the number N_d of rational curves of degree d on a generic three–dimensional quintic (e. g. 70428 81649 78454 68611 34882 49750 for $d = 10$, a theoretical(?) number still unchecked in an experiment(?) involving a mathematical definition of N_d and a computer.) The ideology of path integration played an essential role in these calculations, leading to an interpretation of an instance of (1) as a sum over instantons in a sigma–model, which in this particular case are rational curves on a quintic.

The intuitive physical picture of an integral is *the quantity of something in a domain.* If the first calculations of this "something" are later interpreted as, say, the volume of a pyramid, one can hardly doubt that they were used for estimating the actual quantity of *stone* (and slaves' labor) needed for the building of an Egyptian pharaoh's tomb. Kepler's *Stereometria Doliorum* mentions *wine casks* in its title. The domain in question acquired a temporal dimension when *the length* of a path was calculated as an integral of velocity, and the notion of energy was gradually replaced by that of *action.* In the twentieth century, *topology* became one of the substances the quantity of which could be measured by integration of closed differential forms (De Rham theory of periods anticipated by Poincaré). *Probability* turned out to be another such substance, and Wiener's treatment of Brownian motion as a measure in a space of continuous paths paved the way both for Kolmogorov's axiomatic treatment of probability and our present reluctant acceptance of Feynman's integral. (This is at least partially supported by the successes of constructive field theory and stochastic integration. However, the random surfaces inherent in string path integrals present considerable difficulties.)

Mathematically, any calculation (or definition) of an integral is based upon two physically intuitive principles: additivity with respect to domains and integrands, and a form of limiting procedure. There are at least two archetypal forms of passing to a limit.

One is represented by Cavalieri's indivisibles, Riemann sums etc. It is connected with the topological structure of the domain of integration, specifically with the idea of boundary and thin layers of $(d+1)$–dimensional objects surrounding a d–dimensional object. The Stokes formula in all its modifications belongs to this circle of ideas, while the De Rham complex is its linear dual form.

Another form of limiting procedure is measure–theoretical rather than topological one. There are basic domains filled with well measured quantities of the substance of interest (volume, action, probability ...) We try to approximate other distributions by using mosaic portraits of them and allowing the size of local discrepancies to tend to zero. However, locality is not topological anymore, and the image of boundary becomes useless or irrelevant. Instead, we have to deal with measurable sets which must only form an algebra with respect to intersections and unions. Infinite–dimensional constructions are usually of this type. The well known effect "volume in high dimensions tends to concentrate near the boundary" prevents using the image of indivisibles effectively. Even in finite dimensions, the boundary can fail to serve the role of Cavalieri's indivisible if it is very rough (fractal). The subtle measure theoretic studies of the beginning of this century had much to say about it.

There are *two* integrals in (1), of quite different nature. The action $S = \int_p L$ is usually a classical entity, L being a local Lagrangian. A beautiful recent idea due to a collaboration of physicists and mathematicians (E. Witten [W] and M. F. Atiyah [A] playing leading roles, A. S. Schwarz having supplied a crucial first example) consisted in considering those path integrals in which the action is a topological invariant of p. Locally this means that classical equations of motion $\delta(S) = 0$ are identically satisfied. An example of such an action functional is the Chern–Simons invariant defined on the space of connections on a vector bundle over a three-dimensional manifold. The quantum observables (whose choice and name was motivated by the theory of strong interactions) are Wilson loops: averaged traces of monodromy representations along closed curves in the base.

In this context, the algebraic properties of the path integral reflected in the additivity of $\int_p L$ and resulting "multiplicativity" of the whole of (1) become so strong that they can be used to define a sufficiently rigid mathematical structure of "Topological Quantum Field Theory" which can then be studied by precise mathematical means. This was done by G. Segal and M. F. Atiyah. See [RT] and [BHMV] for some recent mathematical developments in this area.

The history of the integral seen from our vantage point can be conceived in terms of a Toynbeean challenge/response scheme. Challenges come from physics broadly construed, including geometry. It can be convincingly argued that even Euclidean geometry is in fact just the kinematics of rigid bodies in the absence of a gravitational field (curved the space–time), and both the invention and the development of the first non–Euclidean geometries (of constant curvature) was inextricably connected with physics. Gauss wanted to know what was the *actual* geometry of interstellar space. Hilbert's return to axiomatics was a mathematical response to the challenge of the discovery of multiple *possible* geometries of the physical world.

IV. The Schism

In this section of my talk I argue that the main event in the relationship between mathematics and physics in the first half of this century was their estrangement, after several centuries of close alliance.

The divergence started in the last two decades of the last century and was connected with the deepening understanding of two microworlds: a mathematical one embodied in the idea of the classical continuum of real numbers, and a physical one open to experiment.

Roughly speaking, around the turn of the century Peano, Jordan, Cantor, Borel, Stiltjes, and Lebesgue discovered and displayed with great subtlety the new properties of continuum, continuity and measurability. They have given a series of definitions of integration of increasing generality, and invented constructions and existence proofs for many strange mathematical objects which did not belong to the world of classical geometry and analysis but had to be accepted as a consequence of classical ways of mathematical reasoning stretched, as it seemed, to their limit.

The growing reaction against many counterintuitive discoveries led mathematicians to self–analysis centered around several basic problems: What is a mathematical proof? What meaning can be given to a statement about existence of a mathematical object? What is the status of mathematical infinity?

The outcome of this is well known. Fifty years of introspection were quite fruitful from the mathematical viewpoint: they produced mature mathematical logic, including theory of proof, theory of computability, and a clear picture of the hierarchy of expanding languages and axiom systems that mathematicians have had to adopt consecutively in their quest for mathematical truth.

In the meantime, physicists were engaged in a totally different quest. Planck's discovery of a quantum of action announced on December 14, 1900, initiated the quantum age. Physics needed sophisticated mathematics to formulate newly discovered non–classical laws, but new mathematics was of no help. Whatever was needed was hastily invented or reinvented: matrix algebra, spinors, Fock space, the delta function, the representation theory of Lorentz group. None of the pioneers (Bohr, Einstein, Pauli, Schrödinger, Dirac) needed the Lebesgue integral, or was interested in the cardinality of continuum. Logic interested them even less.

This does not mean that physicists had no philosophical preoccupations; in fact they had. But if mathematicians discussed the relationships between language and thought, physicists were troubled by the relation of language to reality. The basic problem confronted by the critics of classical mathematics was the inexpressibility of infinity, related to the inherently finitary syntactic structure of language. The basic problem confronted in the Bohr–Einstein controversy was the inexpressibility of quantum indeterminacy, related to the inherently classical semantics of language. Philosophy of mathematics and philosophy of physics almost completely lost contact with each other. Such ardent critics of the alleged inadequacies of contemporary research as Brouwer in mathematics and Pauli in physics shared not a single common idea. Mathematical criticism tended to become deeply autistic, while physical criticism strived to find better ways to express complex reality.[3]

A gap formed in traditional professional interactions as well. From the first successes of the quantum electrodynamics in the thirties until the renewed interaction in the sixties, mathematicians contributed almost nothing to the main physics research program of this century: Quantum Field Theory. Similarly, physicists payed no attention not only to mathematical logic (understandably) or analytical number theory (traditionally), but also to the emerging algebraic topology. Thirty years later, topology was to become the new common ground for the two communities. Somewhat paradoxically, mathematics gained from this renewed interaction more than physics: new invariants of three– and four–dimensional manifolds, quantum groups, quantum cohomology were its fruits.

The following well known empirical observation fits well into the picture we have sketched. Whenever a fresh mathematical tool for understanding physics is needed, physicists are very quick at inventing new or transforming already existing *algebraic* formalism to deal with it. We have already mentioned Heisenberg algebra,

[3]It is characteristic that G. H. Hardy's Rouse Ball Lecture [H] on "Mathematical Proof" delivered in 1928 does not even mention existence of quantum physics.

spinors and Dirac delta function. One can add the Schwinger–Dyson equation (for an otherwise undefined path integral), the Berezin integral on supermanifolds and Witten's topological invariants expressed as path integrals of a Topological QFT. All this constitutes only a small sample of inventions which are by now thoroughly absorbed and transformed into honest mathematics.

It is "only" when one has to deal with infinitary constructions, that is, limits of various kinds, that mathematicians do their job unassisted. According to Bourbaki's Chapters on Integration [B], mathematicians contributed to the theory of integral in the last century exclusively careful analysis of limits.

After the creation of the modern notion of a topological space and the discovery of limiting procedures basic to measure theory, the next major package of startlingly new infinitary constructions was introduced by Alexander Grothendieck with his treatment of Homological Algebra, derived categories and functors, Topoi and Sites. But this is another story.

V. Discussion

Direct contact between mathematical and physical modes of thought more often than not creates a tension. The basic values are different, the accepted types of social behavior clash, time scales for a problem to keep attention of the public tend to be incommensurable.[4] In a remarkable piece of introspection, F. Dyson [D] has shown how impenetrable the walls between mathematics and physics can be in one and the same mind. We would be much more tolerant to each other if we could discern in ourselves the two personalities so convincingly displayed by Dyson. A recent discussion (cf. [JQ] and [R]) shows the vulnerability of our community, when in a period of renewed fruitful interaction we try to harmonize our attitudes to what is and what is not a proof, what may and what may not be published, and who should be credited for what.

All of this is fortunately restricted to our social life. It seems that deep insights survive however we mess them up, and it is precisely the complementarity of mathematical and physical thinking that makes their interaction creative.

The crucial distinction between the ways we present our ideas in the last half of this century lies not so much in our attitudes towards a rigorous proof as towards exact definitions.

Mathematicians have developed a very precise common language for saying whatever they want to say. This precision is embodied first of all in *the definitions* of the objects they work with, stated usually in the framework of a more or less axiomatic set (or category) theory, and in the skillful use of *metalanguage* (which our natural languages provide) to qualify the statements. All the other vehicles of mathematical rigor are secondary, even that of rigorous proof. In fact, barring direct mistakes, the most crucial difficulty with checking a proof lies usually in the insufficiency of

[4]The relevant psychological difficulties are not often expressed in print. For an interesting recent reaction see S. MacLane's contribution in [R] of which we cite only one sentence: "Thus, when I attended a conference to understand the use of a small result of mine, I heard lectures about 'topological quantum field theory', without a slightest whiff of a definition; I was told that the notion had cropped up at some prior conference, so that 'Everybody knew it.' "

definitions (or lack thereof.) In plain words, we are more deeply troubled when we wonder what the author wants to say than when we do not quite see whether what he or she is saying is correct. The flaws in the argument in a strictly defined environment are quite detectable. Good mathematics might well be written down at a stage when proofs are incomplete or missing, but informed guesses can already form a fascinating system: outstanding instances are A. Weil's conjectures and Langlands's program, but there are many examples on a lesser scale.

The etymology of the term *de–fin–itio* shows that its primary function is to set strict limits. In the course of a given study, we agree to consider only locally compact topological spaces satisfying the countability condition, only finite–dimensional Lie algebras, only coarse moduli spaces of stable algebraic curves and so on. If we fail to mention a relevant restriction in the course of presenting a professional seminar, we will be politely reminded about it. If we claim to having done anything serious, our work will be carefully scrutinized for all the necessary caveats.

Of course, our definitions are far from being arbitrary. One function of a good definition is to be a carrier of analogies between various situations, and to this end the cage of a definition must be of optimal size. For example, one can convincingly argue that by far the most important result of the group theory is exactly the definition of an abstract group and its action on a set, because it describes a structure reappearing again and again in geometry, number theory, probability, the theory of space–time, theory of elementary particles, and so on. The whole ideology of Bourbaki's treatise consists in representation of mathematics as a building supported by a strict system of good definitions (axioms of basic structures). And since a good definition is sometimes the work of generations of good mathematicians, the temptation to believe that we already know them all can be great.

To the contrary, an inexperienced reader of the most interesting physical papers is often left in a vacuum about the precise meaning of the most common terms. Physicists are undoubtedly constrained by their own rules, but these rules are not ours. What is a current algebra? a supersymmetry transformation? a Topological Field Theory? a path integral, finally? They are very open concepts, and it is precisely their openness that makes them so interesting.

Here is what the history of our two metiers teaches: we cannot live without each other. At least for some of us, life becomes dull if it goes on for too long without contacts with good physics.

It is the interaction with a wildly differing set of values that counts most.

As a perceptive study by Hardy Grant [G] shows, in terms of cultural history of Isaiah Berlin's variety, mathematics is a very classical endeavour. In fact, it is based upon a commonly accepted idea of truth and ways to achieve it, forming a stable system. The Romantic Revolution of a century and a half ago did not really influence mathematics mainly because there was little place in it for personal whims.

In this century romantics comes from physics: the vast expanses of the Universe, the wonderfully erratic behavior of the microworld, the observer's subjectivism and the power of the unobservable, the Big Bang, the Anthropic Principle, our in turn humble and megalomaniacal attempts to cope with irreverent Nature.

Mathematics supplies hygienic habits and headaches.

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TRUTH, RIGOR, AND COMMON SENSE

§0. Preface

The main difficulty of discussing the nature of mathematical truth in 1995 as I see it is that no new insights into it were gained since the epoch of deep discoveries crowned by Gödel's results of the late thirties.

To avoid repetition and to enliven the discourse one can try to put the matter into a broader context and add a personal note. Both solutions tend to divert the reader's attention to vaguely related topics, and I offer my apology for choosing these dubious tactics.

This talk is divided into three parts: a) musings on the history of mathematics perceived as a genre of symbolic (or semiotic) games; b) a discussion of truth and proof in the context of contemporary research (centering on a recent controversy prompted by a letter by A. Jaffe and F. Quinn [JQ]); c) materials for three case studies (it being understood that the study itself will be carried out by the interested reader).

We adopt for this talk most naive philosophical background.

Naively, a truthful statement is a statement that could be submitted to verification, and would then pass this test. Verification is a procedure involving some comparison of the statement with reality, i.e., invoking an idea of *meaning.* (This applies equally well to "evident" statements whose verification is skipped.) The reality in question can be any kind of mental construct, from freely falling bodies to transfinite cardinals. We will pass over in silence the problem of how to verify statements about transfinite cardinals which surely will be addressed by other speakers.

The statement itself is a linguistic construct. As such, it must be grammatically correct in the first place, and meaningful in the second, before it can be submitted to a verification procedure.

Logic teaches us that certain formal constructions produce truthful statements when applied to truthful statements (syllogisms were the earliest examples). Mathematics uses such constructions recursively. All comparison with reality is relegated to comparatively scarse encounters with applications and, possibly, foundational studies. The main body of mathematical knowledge looks like a vast mental game with strict rules.

We might also contemplate the notion of truth applied not to isolated statements but to entities like a novel, a scientific theory, or a theological doctrine. The ideas of grammatical correctness, meaning, reality, and verification procedures acquire new dimensions, but seemingly do not lose their heuristic value. A new phenomenon is what can be called their non–locality: neither meaningfulness nor truthfulness of a theory resides entirely in its constituent statements, but rather in the whole body of the doctrine.

All the common sense notions mentioned above were submitted to fine theoretical analysis in many philosophical works. All of them, including the idea of reality, were also thoroughly criticised, to the extent of complete annihilation. One pertinent example is that of the idea of verification of a theory: it was argued that a theory can never be verified, but only falsified.

In what follows I will try to be commonsensical and to avoid extremist views. Some truth creeps even into the wildest deconstructions of this notion, but weaknesses of such attacks usually become apparent as soon as we start judging them by their own standards.

§1. Mathematical truth in history

The modern notion of mathematical truth goes back to ancient Greece; as Bourbaki succintly puts it, "Dépuis les Grecs, qui dit Mathématiques, dit démonstration."

It is the demonstration that counts, which is understood as a chain of well–organized consecutive standard steps, not as a physical act of showing, contrary to what the etymology of the word "demonstration" suggests.

Among other things, this means that modern mathematics is an essentially linguistic activity relying upon language, notation, symbolic manipulation as a means of convincing even when dealing with geometric, physical et al. realities. Consistency of argumentation free of contradictions and avoiding hideous gaps plays a major role in establishing that a given utterance proves what it purports to prove. The status of the postulates P upon which the demonstration/proof of the statement S is built strictly speaking need not be discussed in mathematics, which is responsible mainly for the structure of the deduction.

This idealized image had a long pre–history, and we will try to briefly review some archaic modes of protomathematical behaviour.

The economic and military life of early human collectives was correlated with accounting and keeping track of food resources, size of the tribe, seasons etc. Elementary arithmetic as we know it only gradually emerged as a subdialect of language supporting such activities.

Whereas the main (and for millenia the only) form of existence of natural languages was oral speech, the oral and then written language of elementary arithmetics must have slowly cristallized from many archaic forms including counting by fingers and other body parts, collecting stones and sticks, tying knots etc. (This process is now reversed as we observe how electronic arithmetics takes over the written one.)

If a mathematician is inclined to stress the "isomorphism" of all these realizations describing the universe of natural numbers and operations on them, he must understand that this is an appalling modernization.

In terms of the classical Saussurean dichotomy Langue (as system) vs Parole (as activity), we observe a slow and difficult emergence of "language" from "speech," the latter involving direct manipulation of things and body parts as symbols of something else. Whatever notion of truth can be read into such activity, it must be in the final account a function of the efficiency of social behaviour supported by it. Exchange and trade furnish obvious examples. Correct counting means just exchange and profitable trade, pure and simple.

This is not however the whole story. It is important to realize that not only materially profitable, but virtually any form of organized behaviour can have a special meaning for a human being or a human collective. This puts archaic arithmetic on a par with rites, music and dance, and all sorts of magic. The traces of this undifferentiated perception of mathematics as a form of magic are registered quite late in the history. A person who efficiently predicts an eclipse, or an outcome of an uncertain situation, is not necessarily a sage, but more appropriately a trickster who *makes* things happen by manipulating their symbolic representations.

Many philosophers tried to demythologize the image of mathematics as predominantly intellectual activity. A. Schopenhauer for one, already in the days of modern institutionalized mathematics, wrote: "Rechnungen haben bloß Werth für die Praxis, nicht für die Theorie. Sogar kann man sagen: wo das Rechnen anfängt, hört das Verstehen auf."

Citing this , S. Hildebrandt ([Hi], p. 13) continues: "Die Anbetroffenen lesen es staunend und denken sich, daß Schopenhauer schwerlich einen Blick in die Arbeiten von Euler, Lagrange oder Gauß getan haben kann."

However, taken literally, Schopenhauer is right. Not only does computation temporarily interrupt thinking, but an ultimate justification of the act of computation is that it replaces the act of thinking (or a stage of it) by a virtually mechanical interlude, in order to support a much higher level of competence for the next act. If thought is an interiorized and tentative action, then computation is an exteriorized thought, and the degree of possible exteriorization achieved by modern computers is stunning.

In the same vein, during the previous era of biological evolution, emergence of conscious thinking served to stop instinctive action and to replace it by planned behavior. An animal brain calculates in order to keep the animal body alive and kicking, running, flying, seeing, hearing. A human brain does the same, and this activity is the main content of the (non–Freudian) individual subconscious which must not allow any intervention of consciousness in order not to break the complex architecture of the relevant computations. Otherwise correct (biologically optimal) results cannot be secured.

The arrival of language and consciousness in a sense allowed the human brain to elevate this unconscious computation to the level of commonsense thinking and later to the level of theoretical thinking. A price paid was a loss of spontaneity of action and emergence of less and less biological patterns of individual and collective behaviour. In short, civilization was made possible.

This complementarity of action/thought/computation tends to reproduce on various levels.

The new alienation of thought in computerized systems of information processing is a grotesque materialization of the (non–Jungian) collective unconscious. Its running out of control is a recurring nightmare of our society, as well as the condition of its efficient functioning.

The abstract nature of modern mathematics understood not as its epistemological feature but as a psychological fact, supports our metaphor. The gaping abyss between the habits of our everyday thinking and the norms of mathematical reflection must remain intact if we want mathematics to fulfill its functions.

The heated battles about the foundations of mathematics which continued for several decades of this century did not resolve any of the epistemological problems under discussion. Let me remind you that at the center of attention and criticism was Cantor's theory of infinity.

Cantor's tremendous contribution to XXth century mathematics was twofold. First and foremost, he introduced an extremely economical and universal language of sets which subsequently proved capable to accomodate the semantics of all actual and potential mathematical constructions. This was understood only gradually, and full realization came only somewhere in the mid–century. What I mean is a kind of Bourbaki picture: every single mathematical or even metamathematical notion, be it probability, Frobenius morphism, or a deduction rule, is an instance of a *structure* which is a construct recursively produced from initial sets with the help of a handful of primitive operations.The formal language of mathematics itself is such a structure. (Sometimes, as in categorical constructions, classes instead of sets are allowed, but from the viewpoint I am advocating here this is a minor distinction).

I believe that Hilbert when he spoke with prescience about "Cantor's Paradise" had this grandiose picture in mind.

But second, Cantor produced some deep and unconventional mathematical reasonings about orders of infinity, thus spurring a long and heated controversy. As we now see it, he discovered probably the simplest imaginable and natural undecidable problem, the Continuum Hypothesis (CH). (For a penetrating discussion of the meaning of undecidability in this context cf. [G], p. 162.)

The austere and barren world of unstructured infinite sets of various orders of magnitude undoubtedly has a magic charm of its own, and reflections about this world in turn attracted and repelled philosophically–minded mathematicians and mathematically–minded philosophers for several decades. Cohen's famous proof of the consistency of the negation of CH, completing Gödel's earlier proof of the consistency of CH itself, came already when the fascination with mysteries of infinity was waning, precisely because by that time the language of sets had become the language of virtually every mathematical discourse.

Rethinking these old arguments, recalling the birth of intuitionism and constructivism, I am struck by the utterly classical mindset of some of Cantor's critics. A considerable part of the discussion concentrated on the principles of thinking about infinite sets. The Axiom of Choice was considered basically as a wild extension of mundane experience of picking randomly individual objects from heaps of them. Both the constructivist and intuitionist view of this picture revealed a deep emotional revulsion towards such an action involving infinite choice (in a later Essenin–Volpin decadent ultraintuitionistic world even imagining finite and rather small collections of things became an unbearable strain.)

Of course, the idea of a collection of distinguishable and immutable objects belongs to layman's physics. Many actors of the great Foundation Drama seemingly were convinced that the axiomatics of Set Theory must be understood as a direct extension of this naive physics.

The fact that even small sets of quantum objects behave quite differently was never taken in consideration. (It probably should not be.) The fact that working infinities of working mathematicians (real numbers, complex numbers, spectra of operators ...) were efficiently used for understanding of the real world was deemed irrelevant for foundations. (It probably is.)

In any case, the uneasiness about Cantor's arguments led Hilbert to start a deep formal study of the syntax of mathematical language (as opposed to the semantics of this language), thus preparing the ground for Tarski, Church, Gödel (and prompting philosophical platitudes like Carnap's view of mathematics as "systems of auxiliary statements without objects and without content", cf. [G], p. 335).

What these studies taught us was a highly technical picture of the relationships between the structure of formal deductions, their naive (or formal) set–theoretical models, and degrees of (un)solvability and (un)expressibility of the relevant precisely defined versions of mathematical truth. Popularizations ("vulgarizations") of Gödel's work rarely manage to convey the complexity of this picture, because they cannot convey the richness of its mathematical (as opposed to epistemological) context.

It is this richness that fascinates us most.

§2. Truth for a working mathematician

The Bourbaki aphorism cited at the beginning of the previous section does not imply two millenia of common agreement on what constitutes a proof. Moreover, the following quotation from A. Weil's talk at the 1954 International Mathematical Congress in Amsterdam leaves an impression that the notion of "rigorous" proof is quite recent, perhaps even due to the efforts of Bourbaki himself. "Rigor has ceased to be thought of as a cumbersome style of formal dress that one has to wear on state occasions and discards with a sigh of relief as soon as one comes home. We do not ask any more whether a theorem has been rigorously proved but whether it has been proved." ([W], p. 180).

Alas, this seems to be only wishful thinking. In the individual psychological development of a mathematician and in the social history of mathematics both the understanding of what constitutes a proof and the perception of its role greatly vary.

Below I collected a sample (A–F) of quite recent opinions of actively working mathematicians, taken from [JQ], [T] and [R]. The reader is urged to read the whole discussion; it is quite instructive. It was sparked by the letter of A. Jaffe and F. Quinn *"Theoretical Mathematics": towards a cultural synthesis of mathematics and theoretical physics* ([JQ]). The authors were worried by the local situation in the very active domain of mathematics bordering with mathematical physics. It seemed to them that the standards of physical reasoning (which are considerably lower than those in mathematics) tended to unfavorably influence standards of today's mathematical research. At the same time they fully recognized the value of cross–fertilization, and suggested some rules of conduct that should be imposed upon all players, in particular the rules of credit assigning. (The word "theoretical" in the title in the present context is a neologism, and not a very lucky one, because the authors have in mind a mixture of educated speculations, examples, and computer outputs, as opposed to theorems with proud quantifiers).

A."When I started as a graduate student at Berkeley, I had trouble imagining how I could 'prove' a new and interesting mathematical theorem. I didn't really understand what a 'proof' was.

"By going to seminars, reading papers, and talking to other graduate students, I gradually began to catch on. Within any field, there are certain theorems and certain techniques that are generally known and generally accepted. When you write a paper, you refer to these without proof. You look at other papers in the field, and you see what facts they quote without proof, and what they cite in their bibliography. You learn from other people some idea of their proofs. Then you're free to quote the same theorem and cite the same citations. You don't necessarily have to read the full papers or books that are in your bibliography. Many of the things that are generally known are things for which there may be no known written source. As long as people in the field are comfortable that an idea works, it doesn't need to have a formal written source." (W. Thurston, Fields Medal 1983, [T], p. 168. Thurston eloquently argues that the principal goal of the proof is understanding and communication, and that it is most efficiently achieved via personal contacts. His opponents in particular notice that transgenerational contacts can be achieved only via written texts of sufficient level of precision, and that the fate of Italian algebraic geometry should serve as a warning.)

B. "We must carefully distinguish between modern papers containing mathematical speculations, and papers published a hundred years ago which we, today, consider defective in rigor, but which were perfectly rigorous according to the standards of the time. Poincaré in his work on Analysis Situs was being as rigorous as he could, and certainly was not consciously speculative. I have seen no evidence that contemporary mathematicians considered it "reckless" or "excessively theoretical" (*in the JQ sense. Yu. M.*). When young Heegard in his 1898 dissertation brashly called the master's attention to subtle mistakes, Poincaré in 1899, calling Heegard's paper "très remarquable", respectfully admitted his errors and repaired them. In contrast, in his 1912 paper on the Annulus Twist theorem (later proved by Birkhoff), Poincaré apologized for publishing a conjecture, citing age as his excuse." (M.W. Hirsch, in [R], p. 187.)

C. "Intuition is glorious, but the heaven of mathematics requires much more [...] In theological terms, we are not saved by faith alone but by faith and works [...] Physics has provided mathematics with many fine suggestions and new initiatives, but mathematics does not need to copy the style of experimental physics. Mathematics rests on proof – and proof is eternal" (S. Mac Lane, in [R], 190–193).

D. "Philip Anderson describes mathematical rigor as 'irrelevant and impossible.' I would soften the blow by calling it besides the point and usually distracting, even when possible." (B. Mandelbrot, in [R], p. 194. Mandelbrot's contribution is a vehement attack not only on the abstract notion of rigorous proof, but also on a considerable part of the American mathematical community, "Charles mathematicians," who allegedly are totalitarian, concentrate on credit assigning, and strive to isolate open–minded researchers).

E. "Before 1958 I lived in a mathematical milieu involving essentially Bourbakist people, and even if I was not particularly rigorous, these people – H. Cartan, J.-P.

Serre, and H. Whitney (a would-be Bourbakist) – helped me to maintain a fairly acceptable level of rigor. It was only after the Fields medal (1958) that I gave way to my natural tendencies, with the (eventually disastrous) results which followed. Moreover, a few years after that, I became a colleague of Alexander Grothendieck at the IHES, a fact which encouraged me to consider rigor as a very unnecessary quality in mathematical thinking." (R. Thom, in [R], p. 203. Thom's irony requires a slow reading. In what sense did following his natural tendencies have eventually disastrous results? How exactly did becoming a colleague of Grothendieck's influence Thom's thinking? An outsider may remain puzzled whether Grothendieck himself shared Thom's convictions, or whether it was the other way around. Later in the same contribution Thom invokes *rigor mortis* as an appropriate connotation to the idea of mathematical rigor.)

F. "I find it difficult to convince students – who are often attracted into mathematics for the same abstract beauty and certainty that brought me here – of the value of the messy, concrete, and specific point of view of possibility and example. In my opinion, more mathematicians stifle for lack of breadth than are mortally stabbed by the opposing sword of rigor." (K. Uhlenbeck, in [R], p. 202).

I would like now to summarize, contributing my own share to the general confusion.

First, individually, producing acceptable proofs is an activity that takes arduous training and evokes strong emotional response. A person feels aversion if required to do something contradicting his or her nature. Innate or acquired preference of geometric reasoning or algebraic calculations can inform our career. When we philosophize, we unavoidably rationalize and generalize these basic instincts, and the whole spectrum of our attitudes can be traced back to the feelings of bliss or frustration that overwhelm us during confrontations with intellectual challenges of our metier.

Second, socially, we have to rely upon our contemporaries and forebears even when devising a very rigorous proof. Authority in mathematics plays a two-fold role: we acquire from our fathers and peers a value system (what questions are worth asking, what domains are worth developing, what problems are worth solving), and we rely upon the authority of published and accepted proofs and reasonings. Nothing is absolute here, but nothing is less important because of the lack of absoluteness.

Third, epistemologically, all of us who have bothered to think about it, know what a rigorous proof is. It has an ideal representation which was worked out by mathematical logicians in this century, but is only more explicit and not fundamentally different from the notion Euclides had. (In this respect, Bourbaki was quite right.) This ideal representation is an imaginary text which step by step deduces our theorem from axioms, both axioms and deduction rules being made explicit beforehand, say in a version of axiomatic set theory.

If this image arouses in your heart a strong aversion, or at least if you want to be realistic, you may (and should) object that this ideal is utterly unreachable because of the fantastic length of even the simplest formal deductions, and because the closer an exposition is to a formal proof, the more difficult is to check it. Moreover, since formal deduction strives to be freed of any remnant of meaning (otherwise it is not formal enough), it ends by losing meaning itself.

On the contrary, if this image arouses your enthusiasm, or once again if you want to be realistic, you will agree that the essence of mathematics requires daily maintenance of the current standards of proof. Whether we are engaged in the mathematical support of a vast technological project like moon–landing, or simply nurture a natural desire to know what assertions have a chance to be true and what do not, we have to resort to the ideal of mathematical proof as an ultimate judge of our efforts.

Even the use of mathematics "for narrative purposes" as is nicely put by Hirsch is not an exception, because such a narration is built of blocks of solid mathematics to a non–mathematical blue–print.

"An author with a story to tell feels it can be expressed most clearly in mathematical language. In order to tell it coherently without the possibly infinite delay rigor might require, the author introduces certain assumptions, speculations and leaps of faith, e.g.: 'In order to proceed further we assume the series converges — the random variables are independent — the equilibrium is stable — the determinant is non–zero —.' In such cases it is often irrelevant whether the mathematics can be rigorized, because the author's goal is to persuade the reader of the plausibility or relevance of a certain view about how some real world system behaves. The mathematics is a language filled with subtle and useful metaphors. The validation is to come from experiment — very possibly on a computer. The goal in fact may be to suggest a particular experiment. The result of the narrative will be not new mathematics, but a new description of reality (*real* reality!)." (M. Hirsch, in [R], p. 186–187).

A beautiful recent example of such a narrative use of mathematics is furnished by D. Mumford's talk at the first European Congress of Mathematicians [Mu]. About mathematical metaphors see also [Ma].

§3. Materials for three case studies

In this section, I present three cases relevant to our discussion: Gödel's proof of the existence of God (1970), the tale of the faulty Pentium chip (1994), and G. Chaitin's claim (1992 and earlier) that a perfectly well and uniformly defined sequence of mathematical questions can have a "completely random" sequence of answers. For all their differences, these arguments represent human attempts to grapple with infinity by finitary linguistic means, be it infinity of God, real numbers, or mathematics itself.

Whatever moral lessons (if any) can be drawn from these materials, the reader is free to decide.

Gödel's Ontological Proof

The third volume of K. Gödel's Collected Works recently published by Oxford University Press contains a note dated 1970. It presents a formal argument purporting to prove existence of God as an embodiment of all positive properties.

An introductory account by R.M. Adams ([G], p. 388–402) puts this proof into a historical perspective comparing it in particular to Leibniz's argument and discussing its possible place in theoretical theology.

The proof itself is a page of formulas in the language of modal logic (using Necessity and Possibility quantifiers in addition to the usual stuff). It is subdivided into five Axioms and two Theorems. A photocopy of the published version of this page (p. 403) may help the reader.

What Does a Computer Compute, or Truth in Advertising

In the Jan. 1995 issue of SIAM News the front page article "A Tale of Two Numbers" started with the following lines:

"This is the tale of two numbers, and how they found their way over the Internet to the front pages of the world's newspapers on Thanksgiving Day, embarassing the world's premier chip manufacturer."

Briefly, it was found that the Intel Corporation's newly launched Pentium chip (the Central Processing Unit in Pentium machines) contains a bug in its Floating–Point–Divide instruction so that e.g. calculating

$$r = 4195835 - (4195835/3145727)(3145727)$$

it produces $r = 256$ instead of the correct value $r = 0$.

Now, this is not something very unusual. In fact, in all computers the so called real number arithmetics *is programmed in such a way that it systematically produces incorrect answers (round-off errors).* In this particular case a (slightly inflated) public outrage was incited by the fact that in some cases the error was larger than promised (simple–precision when double–precision was advertised).

Completely precise calculations with rational numbers of arbitrary size can be programmed in principle (and are programmed for special purposes). This requires a lot of resources and might need also specialized input–output devices. The ideal Turing machine is highly impractical to implement, and real computers are not designed to facilitate this task.

It is not difficult to imagine a computerized system of decision–making which is unstable w.r.t. small calculational errors. Stock–market or military applications are sensitive to such problems. Here is one more example.

A recent study of sexuality in USA purportedly designed to support epidemiological models of the spread of AIDS did not include 3 percents of Americans who do not live in households, i.e. who live in prisons, in homeless shelters, or on the street. A critic of this study (R. C. Lewontin, the New York Review of Books, April 20, 1995) reasonably remarks: "The authors do not discuss it, and they may not even realize it, but mathematical and computer models of the spread of epidemics that take into account real complexities of the problem often turn out, in their predictions, to be extremely sensitive to the quantitative values of the variables. Very small differences in variables can be the critical determinant of whether an epidemic dies out or spreads catastrophically, so the use of inaccurate study in planning counter–measures can do more harmt han does total ignorance."

The problem of understanding what is computed by a computer becomes also more and more relevant with the spread of computer assisted proofs of mathematical theorems. I quote M. Hirsch once again ([R], p. 188): "Oscar Lanford pointed out that in order to justify a computer calculation as a part of proof (as he did in the first proof of the Feigenbaum cascade conjecture), you must not only prove that the program is correct (and how often this is done?) but you must understand how the computer rounds numbers, and how the operating system functions, including how the time-sharing system works".

Randomness of Mathematical Truth

Following A. N. Kolmogorov's, R. Solomonoff's and G. Chaitin's [Ch] discovery of the notion of complexity and a new definition of randomness based upon it, Chaitin constructed an example of an exponential Diophatine equation $F(t; x_1, \ldots, x_n) = 0$ with the following property. Put $\epsilon(t_0) = 0$ (resp. 1), if this equation has, for $t = t_0$, only finitely (resp. infinitely) many solutions in positive integers x_i. *Then the sequence* $\epsilon(1), \epsilon(2), \epsilon(3), \ldots$ *is random.* (Chaitin in fact has written a program producing F. The output is a 200-page long equation with about 17000 unknowns).

This is a really subtle mathematical construction, using among other tools the Davis–Putnam–Robinson–Matiyasevich presentation of recursively enumerable sets. The epistemologically important point is the discovery that randomness can be defined without any recourse to physical reality (the definition is then justified by checking that all the standard properties of "physical" randomness are present) in such a way that the necessity to make an infinite search to solve a parametric series of problems leads to the technically random answers.

Some people find it difficult to imagine that a rigidly determined discipline like elementary arithmetic may produce such phenomena. Notice that what is called "chaos" Mandelbrot–style is a considerably less sophisticated model of random behavior.

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Ontological proof
(*1970)

Feb. 10. 1970

$P(\varphi)$ φ is positive (or $\varphi \in P$).

Axiom 1. $P(\varphi).P(\psi) \supset P(\varphi.\psi)$.[1]

Axiom 2. $P(\varphi) \vee P(\sim\varphi)$.[2]

Definition 1. $G(x) \equiv (\varphi)[P(\varphi) \supset \varphi(x)]$ (God)

Definition 2. $\varphi \,\text{Ess.}\, x \equiv (\psi)[\psi(x) \supset N(y)[\varphi(y) \supset \psi(y)]]$. (Essence of x)[3]

$$p \supset_N q \;=\; N(p \supset q). \quad \text{Necessity}$$

Axiom 3. $P(\varphi) \supset NP(\varphi)$
$\sim P(\varphi) \supset N\sim P(\varphi)$

because it follows from the nature of the property.[a]

Theorem. $G(x) \supset G\,\text{Ess.}x$.

Definition. $E(x) \equiv (\varphi)[\varphi\,\text{Ess}\,x \supset N(\exists x)\,\varphi(x)]$. (necessary Existence)

Axiom 4. $P(E)$.

Theorem. $G(x) \supset N(\exists y)G(y)$,
hence $(\exists x)G(x) \supset N(\exists y)G(y)$;
hence $M(\exists x)G(x) \supset MN(\exists y)G(y)$. ($M$ = possibility)
$M(\exists x)G(x) \supset N(\exists y)G(y)$.

$M(\exists x)G(x)$ means the system of all positive properties is compatible. 2
This is true because of:
Axiom 5. $P(\varphi).\varphi \supset_N \psi :\supset P(\psi)$, which implies

$$\begin{cases} x = x & \text{is positive} \\ x \neq x & \text{is negative.} \end{cases}$$

[1] And for any number of summands.

[2] Exclusive or.

[3] Any two essences of x are *necessarily equivalent.*

[a] Gödel numbered two different axioms with the numeral "2". This double numbering was maintained in the printed version found in *Sobel 1987*. We have renumbered here in order to simplify reference to the axioms.