Truth, rigor and common sense

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MPI 95-107

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To Misha Saveliev on the occasion of his 50th anniversary

§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: Gödels 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 $F(t; x_1, \dots, x_n) = 0$ 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* $\epsilon(1), \epsilon(2), \epsilon(3), \dots$ *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]

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

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.*

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[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.

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