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Een eeuw wiskunde en werkelijkheid

Do mathematical models tell the truth?

Waar een bezoek aan een planetarium al niet toe kan leiden. Toegepast wiskundige Gerke Nieuwland belicht enige aspecten van wiskundige modellen die stellig de aandacht verdienen. Hij leidt echter ook in diep filosofisch vaarwater waar hoge eisen aan de lezer worden gesteld. Nieuwland beschrijft hoe een bezoek aan het planetarium van Eise Eisinga in Franeker hem leidde tot de filosofische vraag naar de preciese relatie tussen een wiskundig model en de erin gemodelleerde werkelijkheid. De opvattingen van Eisinga's tijdgenoot, de Duitse filosoof Immanuel Kant, impliceerden een oplossing: de werkelijkheid zoals wij die ervaren is in belangrijke mate een produkt van de menselijke geest en de in de natuurwetenschappen onthulde wiskundige structuur van de werkelijkheid is die werkelijkheid zelf; een model is dan in het algemeen een vereenvoudiging van (een deel van) die werkelijkheid. Na de introductie van de niet-euclidische meetkunde door Gauss, Bolyai en Lobachevski, in de eerste helft van de negentiende eeuw, was die oplossing in deze vorm niet langer houdbaar. Twee elkaar uitsluitende meetkunden kunnen immers niet alle twee dezelfde unieke werkelijkheid weergeven. Nieuwland ziet vervolgens twee ontwikkelingslijnen. Enerzijds is er in de wetenschappelijke en maatschappelijke praktijk een enorme toename van het gebruik van wiskundige modellen varierend van macro-modellen voor de kosmos tot modellen die slechts een klein deel van de werkelijkheid representeren. Anderzijds is er een grote filosofische hulpeloosheid met betrekking tot de relatie van de wiskunde tot de werkelijkheid. Niet ten onrechte zoekt Nieuwland tenslotte een antwoord bij gerenommeerde Amerikaanse filosofen van de wiskunde. Paul Benacerraf stelde in 1973 het probleem als volgt: Wil het begrip 'wiskundige waarheid' überhaupt zin hebben, dan moeten wiskundige uitspraken ergens naar verwijzen in de werkelijkheid — en de vraag is naar wat — en bovendien moeten wij die waarheid kunnen weten — en de vraag is hoe we dat kunnen. In 1979 deed Penelope Maddy een alom bewonderde poging om de twee vragen te beantwoorden. Ze betoogde dat aan onze intuïties met betrekking tot verzamelingen een causaal perceptuele relatie met de ons omringende wereld ten grondslag ligt. In 1992 was Maddy echter weer terug bij af. In het volgende nummer van het Nieuw Archief besluit Nieuwland zijn beschouwingen

Arces attigit igneas — *he attained the luminary vaults of the heavens*. This quotation from the Odes of Horace is written over the portrait of Eise Eisinga (1744–1828) in the Townhall of Franeker, a township in Friesland in the northern Netherlands. In the family tradition, Eisinga became a woolcomber and trader in worsted yarns, but he also shared the family's amateur interest in mathematics, astronomy and building of mechanical instruments. He received a rudimentary training in elementary geometry, spherical trigonometry and cosmography, without access to proper textbooks. In 1761 he assisted a well-known instrument maker, who was a competent mathematician, at astronomical observations in the Frisian capital. This sufficed for him to compute at the age of eighteen, practically from scratch, all the eclipses of the sun and moon from 1763 to 1800.

In 1773 Friesland witnessed among the less educationally privileged a general unrest, fed in the marketplaces by popular singers and pamfletteers, when a conjunction of four planets and the moon in the sign of Aries was predicted on the 8th of May of the next year. A local clergyman kindly made it known that such a portent spelled at least catastrophy, possibly phase 1 of the Last Judgment. Eisinga, a devout Christian, coolly checked the conjunction by computation and decided that the commotion would vanish if, and only if, the fact that the Creator had designed the universe along Copernican lines became more generally appreciated. He then proposed to his wife his plan to build a model of the solar system hung from the ceiling of her drawing room. She gave her permission on condition that a termination date be fixed in advance, and so he kept rigorously to a seven-year schedule, doing the design, machining, construction work and painting all by himself. The design of Eisenga's *Planetarium*, as it came to be known, was entirely original: he had never seen such a thing or even read a description.[1] Moreover, the location chosen posed many peculiar problems. Perhaps the least was an unusual basic time unit, resulting from a last-minute modification of the length of the pendulum of the clockwork driving the model, to keep the weight from moving to and fro in his cupboard-bed. What challenged Eisinga's engineering skills was his decision to build the hidden mechanism between the beams supporting the ceiling and the upper-story floor.

The orbits of the five planets as were then known, and of the earth, are on the scale of the model sufficiently closely approximated by circles, with the sun in the correct off-center position. The orbital revolutions in the model are in real time; the velocity is uniform, but the variation of speed as a function of position can be read off from a differential grading of the circles. The height of the planetary orbit with respect to the ecliptica is indicated on a curve along each of the circles, and the ascending and descending nodes are marked. The lunar movement is modeled by the mainly wooden clockwork to a surprising average accuracy of 0.7%, the moons of Saturn and Jupiter are shown but do not move.

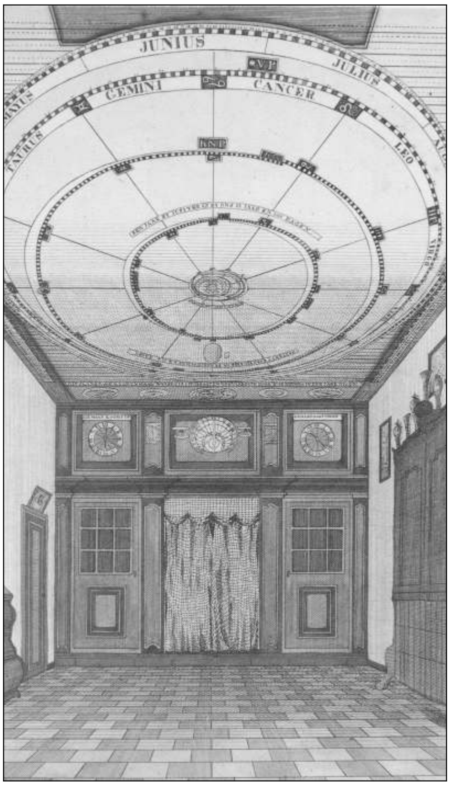
Further astronomical information is presented on the walls. There is a display of a projection of the stellar hemisphere as currently observable from Franeker, with the solar position on the ecliptica shown. Dials present the times of sunrise and sunset, and the position and phase of the moon, accurate to the extent that solar and lunar eclipses can be predicted.

The beauty of Eisinga's brilliantly simple solutions to all problems of display, engineering and approximation to the astronomical data has been one of the marvels of Friesland ever since. The Planetarium served its maker's intended purpose of harmonizing faith and enlightenment. During his lifetime entrance was free and especially after the timely publication of Van Swinden's description it was much visited by scientists, theologians and laypersons. Eisinga lived to see his work become a State monument in 1826.[2]

Eisinga's planetarium is everybody's idea of a *model*. According to Merriam-Webster's Collegiate Dictionary the word is in use in English since 1575. No. 4 of this lemma's definitions is *a usu. miniature representation of something*, also appropriate is no.11: *a description or analogy used to help visualize something (as an atom) that cannot be directly observed*. Webster would not count the planetarium as a *mathematical* model[3], because this is given a more abstract connotation in definition 12: *a system of postulates, data, and inferences presented as a mathematical description of an entity or state of affairs.* Eisinga, on the other hand, would not have objected to this adjective for his work. What he modeled were the quantitive, geometrical and kinematical properties of the physical world and this was in his time, and long afterwards, for most people exactly what mathematics was all about. *Mathematical description*, with its postulates and inferences, suggests some interpretive distance from its object, rather than the immediate view of reality that Eisinga doubtless wanted to convey.

The untying of the Galilean knot that held mathematics and reality together had already begun during Eisinga's lifetime. In 1781 Kant had published the first, more idealist, version of the *Critique of Pure Reason*. Faced with the equally unpalatable alternatives offered by a Leibnizean Platonism and a Lockean empiricism, he boldly took position in the midst between the horns of this dilemma. He decided to attack the problem of the structure of thought itself by application of the new scientific method of Galileo and Newton. As a working hypothesis he proposed the viewpoint that the temporal, spatial and causal structures of the physical world were not the given properties of an external reality, but in fact necessary preconditions for human sensibility, impressed by the mind onto amorphous matter. Still shining through the nearly impenetrable formal discussions that were the philosophical vogue of his time is the leading idea: there is first primal matter senselessly wandering through a chaotic universe — then the inspiration of order and clarity enter into this barren image through mathematical analysis.

Newtonian mechanics was the prime example of the new science in Kant's time and remained so for nearly another century. By 1891 Heinrich Hertz had already provided the experimental keystone to Maxwell's electromagnetic theory. He then wrote the 29 page introduction to *The Principles of Mechanics*[4] in the Kantian tradition, a scientific testament that was to become the seminal philosophical text during the heyday of the era of classical physics, and beyond. Around the beginning of the twentieth century its basic ideas were adopted by the last of the Kantians, the Marburg school in philosophy. A transcendental analysis of thought was supposed to show that knowledge as embodied in the mathematized natural sciences is not a *description* of reality, but reality itself. Such a rarified conception of reality would not have appealed to either Galileo or Eisinga, but the implication of this viewpoint for the notion of a mathematical model is clear. If the structure revealed in the mathematical analysis of the physical world is reality itself, a *model* of the physical world must involve a deliberate simplification — in the sense that the continuum model in fluid dynamics simplifies the discrete, but mathematically awkward atomic picture of kinetic gas theory. Today, although many Kantian notions are still around in various philosophical discussions of science (one can think of ideas of Putnam, Dummett, Van Fraassen), a return to a systematic neoKantianism is only considered occasionally as a feasible option in the aftermath of postmodernism. However, Hawking's beckoning ideal of a Theory of Everything that will sound the ultimate of physical reality, and will be the gauge for all future models, is a relic of this line of thought.



Eise Eisinga's planetarium in Franeker

Arguably, Kant's clearly defined position influenced the course of science at least as strongly by way of the negative reactions it provoked. In 1826 N.I. Lobachevsky announced his hyperbolic geometry. He refused to accept the Kantian view of mathematics as a synthetic *a priori* judgment. Instead he stated explicitly that, after the construction of his alternative geometry, the geometrical structure of physical space must be considered an empirical matter. After Riemann, Maxwell, Mach and Hertz, its definitive formulation from the classical, macroscopic point of view was laid down by Einstein.

Since Kuhn's work[5], this episode in the history of science has become the paradigm case of a *scientific revolution*. Mathematicians have been inclined to stress in this debate the continuity in the history of ideas rather than the conceptual overturn. Indeed, what revolution, if the earlier mathematical model is asymptotically retained as a limiting case in the new theory? However, as a philosophical stance Kuhn's position has

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been widely accepted. The issue was already raised before Lobachewski by Gauss, who had put it in one word: he "doubted the *truth* of geometry". He used truth, of course, in the classical sense of *adequatio rei et intellectus*. Then, if the intellect can think of a catalogue, instead of being offered the one Euclidean structure, there is a twofold problem. One can presumably be decided by science — which option is applicable in the physical world. But the other has no scientific answer — what is the ontological status of the discarded alternatives? They cannot be *true*, that is: do not correspond to a *res*. But, obviously, they also cannot be called *false*. (In today's technological society one would be hard put to come up with a mathematical concept without potential application, but this is the problem as it appeared at the time.) From the beginning of the twentieth century, the problem of the existence of mathematical entities dominated the discussions in the philosophy of mathematics for fifty years or so. Frege and Russell, Brouwer, and Hilbert offered their conflicting approaches, all of which were to be organized at the end of this period in the imposing building of mathematical logic — as a branch of mathematics, not philosophy. Traces of their individual contributions, perhaps in a somewhat uneasy coexistence, can still be identified in a much more varied picture than any of them could have imagined.

There is yet more to the model story than Webster allows. Gauss never published his theory, but Lobachewski also knew that his geometry was a valid alternative only if free of contradiction. Both men had to leave the point open because the conceptual development of mathematics at the time was insufficient to give a formal proof. After Riemann's input, Beltrami (1860) and Klein (1871) took on this problem and opened up an entirely new way of mathematical thought. They introduced a notion of model in the sense of a mathematical representation designed to study another mathematical structure. The construction of models of non-Euclidean geometries embedded in Euclidean space proved that if the latter theory was supposed to be consistent, the former could not fail to be so. Curiously, this also meant that, if anyone wanted to maintain that the 'natural' or 'real' conceptual frame of the world is Euclidean, there is no mathematical objection to do so. After Hilbert's axiomatic studies, and Frege and Russell's development of mathematical logic, this approach paved the way to a rigorous version of *semantics*. Introduced as a linguistic notion in the 1890s, this became a blanket term for methods with the common denominator of comparing something with something else, in linguistics, logic, science and conversation. The varied apparatus also provided resources for the modern philosophical discussion of truth. In particular Tarski's formal study of the truth concept (1931) became after its English translation (1956) the focal point of much debate.

Such could be the meditations of a mathematician with a taste for history on a visit to Eisinga's planetarium. There is a philosophical question she also might reflect upon: what is it, over and above the fact that his dials still display accurate astronomical information, that makes us feel: Eisinga *had it right*. It is, I think, more than our recognition over the centuries of the clear voice of science in a world of obscurantism, let alone our appreciation for a late manifestation of that seventeenth-century idea: the Clockwork of the Heavens. It is rather Eisinga's modeling of the solar system as a *system*. This was the idea that gave access to the external viewpoint in which the conjunction of lunar and planetary objects is immediately revealed as an apparent phenomenon. One indication that he at least intuitively possessed the concept is that in his model each of the planets can be independently switched off the driving mechanism.

A unified whole

The word *system* is, again consulting Webster's, in use since 1603, presumably more or less in the modern sense of *a regularly interacting or interdependent group of items forming a unified whole*. It is worth our while to look somewhat closer at this definition.

If a system can be identified as a unit, there is implied an embedding in an environment, with at least nominal interaction. If the regular interactions of the constituting items are evolving in time, one can expect the whole unit's interaction with the environment to involve temporal variation too. This means that the definition is iterable. If a system can be isolated from its background, it often pays to look at the constituting items as systems in their own right. That is: one is at liberty to define subsystems, by keeping track of its internal and external interactions. On the other hand, any collection of items, with their connecting interactions, can be rearranged as the subsystems of a new supersystem. If for the purpose on hand one decides to disregard such a subsystem's internal structure and consider only its interactions with its environment as given, it is called a *black box*. Of course, a system can be stationary, its interactions then collapse into steady *interface conditions*.

In this general sense, the systems concept has become of almost universal application. Arguably, it is the interplay of systems thinking in science, technology and management that marks the modern era of civilization; through its computer implementation it is said now to propel us into postmodernity. However, historically the theoretical notion took a remarkably long time to crystallize. Its first implicit appearance in science was Newton's conception of a dynamical system. In systems language, Newton's third law tells us

Heinrich Hertz

that in a gravitational system, a *force* appears as an interaction if a body is isolated as a subsystem. The fact that the word was in use long before Newton's time points to the fact that in engineering an implicit systems thinking was already in place. Indeed, contemporary illustrations of machinery almost invariably show enlarged details as subsystems.

It is interesting to note that this lead in an implicit conceptual thinking of technology over science was to remain for many years: James Watt built a self-regulating energy system some eighty years before a detailed theoretical analysis of a thermodynamical system could be given. The mathematical idea of a control system is of a yet later date.

Hertz was probably the first to develop a clear philosophical conception of a mathematical system as a representation of physical reality. He shows his Kantian colors in that:[6]

> *"Hertz's laws of nature are less* descriptive *shorthand for experientially correlated perceptions than* prescriptive *interpretive symbolic systems in the Kantian sense."*

He talks of scientific concepts as the *innere Scheinbilder*[7] (mental virtual images) or *symbols* we form of external objects. Such models should be logically consistent, or as Hertz has it: *permissable*. Still in the Kantian spirit, empirical content is then defined as a formal condition on the modeling:

> *"the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the thing pictured."*

It is interesting to note that for this purpose the system is considered as a black box: Hertz emphasizes that this commutation relation of modeling and output is the one and only condition on the model. We will never know whether our conception is conformal to things in any other than this *one* fundamental respect — the Kantian *Ding-an-sich* remains hidden forever. Hertz calls such a model *correct*.

Given two both permissable and correct models, the one may capture more of the essential relations[8] of the external object than the other: if so, it is more *distinct*. Of two equally distinctive models, the most *appropriate* is the one that has "the smaller number of superfluous or empty relations — the simpler of the two". However, one need not think of avoiding empty relations altogether: images produced by our mind are "necessarily affected by the characteristics of its mode of portrayal".

Hertz' Kantian perspective was soon to be abandoned by the majority of the philosophers of science. Within twenty-five years mental images as the concern of philosophy became in general disrepute as being too highly speculative. Language was thought to offer better purchase on reality, and the Continental philosophers following Heidegger turned away from science altogether. Science was the main concern of the philosophers related to the Vienna Circle, many of whom were to emigrate to the USA. Since Hertzian mental models were no longer scientifically respectable, they decided to scientize the scientist by turning him into a black box. Inputs were sense perceptions, the expected output was a logically structured language, under the sole control of the Verification Principle (or its later relaxations) — hence *logico-empiricism*. The operationalist school went still further in an attempt to strip away the empiricist concerns by proposing the use of theory as a bare tool mimicking measurement, without further mentalistic trimmings. They had to give up an unequal struggle with the advent of quantum theory and its conceptual intricacies, including a subtle theory of measurement. These positivist schools took their cue from Mach and Hertz, insisting on minimization of ontological commitment. They got at least this point — *Occam's razor* — generally accepted.

The notorious difficulties of quantum mechanics for a realistic philosophy of science made the attempts to guarantee the objectivity of science in an empiricist and logicist metatheory dominate the field until the 1960s. However, also logico-empiricism was not to remain uncontested. Kuhn[9] declared, after inspection of many Hertzian boxes in the sciences of earlier and modern times, that their contents seemed to be the result of the vagaries of history as much as anything else. The same message came from Lakatos[10] , who reviewed the historical development of mathematical theories. In reaction, a revival of the realist movement[11] stressed that the fault had been Hertz's all the time: the sole condition on his box should have been the at least *approximate truth* of its contents. On the contrary, Quine, who renewed the logical empiricist tradition in the second half of the century, kept to the physicalist position that if the scientists of today think the contents of any box *is it*, the question after its truth is devoid of sense.

Thus, at the beginning of a new century, the use of mathematical models is daily practice in management, engineering, science and scholarship, without raising controversy of any import among its practitioners. At the same time, consensus among philosophers as to the relation of all this to the world, the mind, to language or to a responsible society, is still conspicuous by its absence. Such an evaluation suggests a momentary return to the practical philosophy of Eise Eisinga.

In the discourse with the world of his times, he did without mental images, sense data, or any other projection of purported universal compass. He began with deliberate judgment to select a specific context — for his further purposes it sufficed to model the purely kinematical relations of the solar system. Then, he listened to the best available experts of his time, interpreting information received to the best of his own lights. Next, he built his model, taking into account the interests of other parties — first of all his wife's. He had not the slightest doubt that in all this he was dealing, in so far as was his concern, with the world as he knew it. On the other hand, he felt not in need of any metatheory to found this conviction. Or so one might suppose. In the judgment of his Frisian contemporaries he had in doing so reached the vaults of the heavens — and we still can agree.

In the remainder of this article I try to develop such a point of view in a discussion with some recent voices in the philosophy of mathematics.

To be or not to be

Some years ago, Penelope Maddy published a remarkable paper: *The Legacy of 'Mathematical Truth'*.[12] The title refers to a much quoted article[13] published in 1973 by the Princeton philosopher Paul Benacerraf. Taken together, these articles comment on a major part of the discussions in the philosophy of mathematics in the second half of the twentieth century.

Benacerraf's paper was one of a number at the time that put shadows of doubt on a scene of prevailing philosophical optimism: the confidence that with the advent of a scientific philosophy, the old Kantian criterion for philosophical health, *assured progress*, could finally be met. Within a circle of one hundred miles radius was assembled a company of philosophical talent perhaps unprecedented in history, and while admittedly it was impossible that Gödel, Church, Carnap, Hempel, Quine, Putnam and Chomsky could simultaneously be right, between them these gentlemen could be relied upon to solve most

Penelope Maddy

of the major philosophical problems still extant. Skeptical questions had already been raised by Kuhn[14] and Lakatos[15]. However, what made Benacerraf's criticism disturbing was that this was a palace revolution, not one engaged in by parties bitten by the bug of an antiscientific historicism.

He began his argument by putting down two minimum conditions for the discussion of mathematical truth. Both require that a scientific account of truth obtains.

Condition 1 is, in the philosophical vocabulary, the *ontological* condition. That is, mathematical objects should be as much part of "the furniture of world" as are the familiar objects of everyday experience. Benacerraf's famous example is:

a. There are at least three large cities older than New York.
b. There are at least three perfect numbers[16] greater than 17.

If both sentences are to be 'true' in a sense that can be scientifically upheld, they must reduce to the same canonical form — a logical standard operation in terms of existential quantifiers, variables and predicates. Obviously, this is not the case if, e.g., one denies that a number is the name of an object. Now, (a) is true if there are in fact cities that stand in a certain relation to each other, and Benacerraf has become convinced that the same sort of truth condition should be applicable to (b). That is, mathematical truth must be analyzed by (an elaboration for natural language of) Tarski's semantical theory of truth.[17]

Condition 2 is what philosophers call the *epistemological* condition: we must have *access* to the truth. If there be mathematical truths, at least some of them should be *knowable* in the same way other truths about the world can be known. Specifically, says Benacerraf, a *causal* theory of knowledge should apply, of the same sort that accounts for our knowledge of medium-sized physical objects. Thus,

"for X to know that X is true requires some causal relation to obtain between X and the referents of names, predicates and quantifiers of S."

Benacerraf emphasized that in a reasonable theory of mathematical truth both conditions should be met — in accord with the old Kantian demand of a balance between the noetic and the epistemic.

What has been called his *dilemma* begins where he lets these conditions suggest a dual typology of philosophies of mathematics. In type 1, mathematical truth is tied to the 'obvious semantics' of mathematical language, but defined independent of any question of how to be decided upon; in type 2, the truth concept is made dependent on epistemic and syntactic considerations and related, e.g., to proof theory or implicit definition. He then reviewed several of the philosophies of mathematics current in the 1960s, to conclude that all of them could be classified as either of type 1 or 2 — that is, none of them gave *both* conditions 1 and 2 their balanced and proper due. Most prominently appeared type 1, in particular the *standard view*: Platonism. This is the doctrine that mathematical entities — sets, dynamical systems — exist, like physical entities, independently of human language or cognition. It passes of course condition 1 with flying colors, but must be inherently weak on condition 2 — in fact, if mathematical entities exist only up there in the sky, how on earth are we to *causally* interact with them? Gödel, Benacerraf's arch-Platonist, concludes from the awareness of mathematical experience to a special human faculty to cognize mathematical form, but for most philosophers this is one too fast. On the other hand, in type 2 of mathematical truth theory (Hilbert's, Hempel's) the truth conditions for (a) and (b) must come out as fundamentally different - so how to tie up the purported mathematical truth to the common or garden variety?

Of course, Benacerraf's dilemma immediately translates into a discussion of the status of mathematical models. On type 1 theories of mathematical truth, there is a dynamical system for the course of the planets, or for the antics of the economy, because such systems have all the time been sitting 'out there' in the physical world, to be discovered by stripping off their phenomenal disguise. However, if asked to put up an explanation how one came to know this much, there is trouble ahead if even strong *intuitions* are not taken as an answer. Philosophies of type 2 are considerably easier on the nerves. The mathematician is held to construct a model, supply existence proofs of solutions, perhaps devise means for practical computation — this is already a borderline case! — but there his responsibilities rest. All resemblance of the product to actual situations or persons is fortuitous, and any buyer proceeds entirely at her own risk. By pinpointing this noncommittal attitude the theory might also explain why, in the bustle of the present day global market place, there is some difficulty in keeping the business of mathematics going.

Enter Maddy. In 1979 she wrote an very interesting defense of mathematical realism along Benacerraffian lines: *Set Theoretic Realism*.[18] Cantor and Gödel have made *sets* the basic furniture of the mathematical universe, and so the Zermelo-Fraenkl axioms define a natural metalanguage for a mathematical object language, to obtain standardly a Tarskian truth theory. What now had to be done was to meet Condition 2, or alternatively, as empiricists prefer, to offer a causal theory of how sets are perceived. Starting from the homely example of three eggs in a carton, two of which are needed to bake a cake, she worked up to the thesis that Gödel's account of a mathematical intuition of sets is in fact supported by a causal perceptual relation. In her own assessment, the case was not proven, but at least made plausible by calling to her aid work by Kripke on linguistic reference, by Pitcher on psychology of perception and by Goldman on justification of intuitive belief.

Now on to her 1992 paper. Looking back on Benacerraf's article, her first remark is that Benacerraf's hope for causal theories of reference and knowledge, and her own for such a theory of perception, have not materialized. Indeed, such expectations were common in analytic philosophy in the 1970s. The process of disenchantment has been vividly described by Putnam[19], who earlier was one of the main advocates of this viewpoint. However, Maddy goes on, also under the later regime of various other theories of reference and truth, Benacerraf's dilemma, read as a critique of Platonism, has shown remarkable resilience. All the same, she has come to the amazing conclusion that:

"as a challenge to Platonism it has become irrelevant in the absence of a strong argument for Platonism."

Like Gauss, who doubted the truth of geometry, she now doubts the truth of mathematics — and belief in its truth is certainly prior to any attempt to embark on Benacerraf's project.

Ground for belief in the truth of mathematics for philosophers of many persuasions is the *indispensability argument* launched by Quine and elaborated by the earlier Putnam. Quine's argument is naturalist and physicalist: if mathematical physics needs set theory to "limn the true and ultimate structure of reality", we are committed to an ontology of sets, because beyond physics there is no *first philosophy*. Putnam's version of the argument, deplored by him at a later date, was more in the realist tradition: if sets are indispensable for fundamental science, they must be 'true' in the sense of "correspond to the facts of the world".

But this is what now has become questionable for Maddy. She discusses the theories of the continuum models of solids, fluids and electro-magnetic media. Between them they constitute a major part of our scientific control over the world around us, and most will agree that the nominalist thesis[20] that such mathematical theories are dispensable is an interesting intellectual exercise, but borders on sophistry. Still, these theories' ontology is flagrantly false. At this point in the discussion one might still argue that in principle a conceptual repair action could be undertaken — along the well-known lines of obtaining the conservation laws of fluid mechanics as an approximation, averaging the equations of kinetic gas theory. The philosophical implementation of this idea is the introduction of *approximate truth*. Such a move has been proposed by Boyd[21] as appropriate in a realist naturalized epistemology: he takes truth as an empirical measure of the relative historical success of scientific theories. However, if approximate truth, taken as a *metaphysical* notion, sounds already rather fishy on the level of the phenomenal theories, on the fundamental level the notorious conceptual difficulties of harmonizing the relativistic and quantum structures of the world have put in doubt the whole idea of an ontology of sets of the space-time continuum. In fact, the attempt to give a naturalized account of even an approximate truth of our theories of the ultimate structure of physical reality seems beset with circularities — as is admitted by Boyd.

Maddy's falling from her metaphysical realist beliefs is in itself not remarkable — she is neither the first nor will she be the last to do so. What remains slightly worrying is the idea of a philosopher of mathematics, at least at the beginning of her career, patently unfamiliar with the ways of thinking in applied mathematics. More interesting is the point she makes in passing, the fact that at the end of the century the naturalist projects of the 1970s to provide *causal* theories of perception or correspondence truth have generally been abandoned. This does not mean that the study of perception as a branch of cognitive science has been given up — far from it. But the ideas for the design of a philosophical metatheory on a causal basis, overarching all of science, became a forlorn hope. In a nutshell: the reason, as in various places explained by Putnam[22], is that perception cannot be analyzed in systems terms, as a purely perceptual input to be processed by the brain; in fact perception inextricably involves some degree of *judgment*, and this is not a causal notion. As Maddy remarks, realism is back where it started — one metaphysical option competing with physicalism, idealism, nominalism. Currently, all of these are still more or less live philosophical options. *More or less*, because most of the younger philosophers of science have lost the taste for the grand vista and specialize their view to one of the sciences: mathematics, physics, biology, often taking historical or sociological inroads. The latter is also the moral of the story for Maddy: at the end of her article she advises philosophers of mathematics to concentrate on the *practice* of mathematics. And so the real question turns out to be: *if a metatheory of science is hard to get, perhaps illusory, why do we need one in the first place?*

Notes

1 J.H. van Swinden, a professor in the physical sciences at the then Franeker Academy, gave an excellent description of Eisenga's model already in 1780 (J.H. van Swinden, *Beschrijving van het Eisinga-Planetarium te Franeker*, reprint Franeker (1994)); I draw on this work. He mentions, among others, astronomical clocks and planetary models by Copernicus (1573), Roemer (1680) and Huygens (1682). He refers to the many models made later in England following Huygens example. The name *orrery* came in use when George Graham, the famous watchmaker, had constructed a mechanical planetary model for the 4th Earl of Orrery, some fifty years before Eisinga.

2 It became a Municipal museum in 1859; its status is now under discussion.

3 An interesting introduction to the phenomenology of mathematical models is given by F. Verhulst, *The validation of metaphors*, in C. van Dijkum *et al.*, *Validation of Simulation Models*, Siswo Publ. 403, Amsterdam (1999).

4 H. Hertz, *Die Prinzipien der Mechanik in neuem Zusammenhang dargestellt*, Leipzig (1894). Quotations in the text are from the translation by D.E. Jones & E.T. Walley, *The principles of mechanics presented in a new form*, London (1899), reprinted 1956.

5 T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago (1962)..

6 Quoted from D.S. Cohen's introductory essay to the 1956 reprint of *The Principles of Mechanics*, ibid.

7 I agree with A. Janik & S. Toulmin, who in *Wittgenstein's Vienna*, New York (1973), argue that Hertz's *Bild* should be translated as *model*. In *Jaren van berekening*, Amsterdam (1998), G. Alberts opposes this view.

8 Hertz is in the main body of his book concerned with gravitational systems only, although he mentions other physical forces in his Introduction. In modern practice, the iterativity of the systems concept is employed to refine a specific system into, say, mechanical, fluid dynamic, thermodynamic, electrical, subsystems, each with its distinctive relations. In quantum mechanics it is essential to also integrate the instrumentation into the system. All this enters into a highly interesting network of systematic, semantic, mathematical and physical relations that together make up the systems concept.

9 T.S. Kuhn, *ibid*.

10 I. Lakatos, *Proofs and Refutations,* British J. Phil. of Science, vol. 14, 1963–64.

11 E.g., W. Sellars, *Science, Perception and Reality*, New York (1962).

12 In: A. Morton & S.P. Stich (eds.): *Benacerraf and his Critics*, Oxford (1996).

13 In: J. of Philosophy, vol.70, 1973, p.661–79, reprinted in P. Benacerraf & H. Putnam (eds.): *Philosophy of mathematics*, 2nd ed., Cambridge (1983), and in W.D. Hart (ed.), *The Philosophy of Mathematics*, Oxford (1996). Hart discusses his selection of papers from the point of view of Benacerraf's dilemma.

14 T.S. Kuhn, *ibid*.

15 I. Lakatos, *ibid*.

16 A positive integer equal to the sum of its positive divisors, except itself. E.g., $28 = 1 + 2 + 4 + 7 + 14$.

17 A critical introduction to the Tarskian paradigm can be found in Ch. 3 of K. Taylor, *Truth & Meaning*, Oxford (1998). Keywords in this theory are reference (of names, predicates, ...), satisfaction (the linguistic analogue of Hertz's 'sole condition') and Convention T (as an adequacy condition on truth predicates).

18 Doctoral dissertation, Princeton (1979). See also P. Maddy, *Realism in Mathematics*, Oxford (1990).

19 See the essays *Realism without Absolutes* and *On Truth* in H. Putnam, *Words & Life*, Cambridge (Mass) (1994).

20 Cf. Hartry Field, *Science without Numbers*, Oxford (1980).

21 R. Boyd, *Approximate Truth, and Philosophical Method* (1990), reprinted in D. Papineau (ed.), *The Philosophy of Science*, Oxford (1996).

22 H. Putnam, *ibid*.